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Science and its History

A Reassessment of the Historiography of Science

Joseph Agassi

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A Reassessment of the Historiography of Science

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For Robert S. Cohen

History of science without philosophy of science is blind.

(Norwood Russell Hanson)

In our society all that is known of the scientific work is summed up in tales of the extraordinary absent-mindedness of old professors, and a few witticisms … This is not much for a society with claims for culture. If society really loves science, scientists, and students …, our literature would long have been enriched by epics, legends, and tales — all of that unfortunately it lacks at the present.

(Chekhov, "Dull Story")

It is not that science is free from legends, witchcraft, miracles, biographic boosting of quacks as heroes and saints, and of barren scoundrels as explorers and discoverers. On the contrary, the iconography and hagiology of Scientism are as copious as they are mostly squalid. But no student of science has yet been taught that specific gravity consists in the belief that Archimedes jumped out of his bath and ran naked through the streets of Syracuse shouting Eureka, Eureka, or that the law of inverse squares must be disregarded if anyone can prove that Newton was never in an orchard in his life. When some unusually conscientious or enterprising bacteriologist reads the pamphlets of Jenner, and discovers that they might have been written by an ignorant but curious and observant nursery maid, and could not possibly have been written by any person with a scientifically trained mind, he does not feel that the whole edifice of science has collapsed and crumbled, and that there is no such thing as smallpox.

(Shaw, *Back to Methuselah*, Preface)

ABSTRACT

Two different false philosophies of science present it as infallible. Historians of science traditionally presented its history in their light, and so had to distort the facts. The history of science, as the history of our culture in general, is the history of noble and heroic efforts to push away a little the darkness in the middle of which we are doomed to live. In brief, the history of science, as the history of our culture in general, is the history of noble and wise errors. Errors, however wise and intelligent, still are errors, and no amount of casuistry will make them true. So many philosophers and historians of science struggle in vain in futile efforts to prove that since the past of science is noble and glorious, it is free of error. And then historians of science undertake the impossible task of sweeping all these fine errors under the rug. And when they fail to do so, they blame.

Science brings about worldly success. This is important, at least as long as we wish to combat hunger and child-neglect and similar ills. Yet we should not forget that the fathers of modern science made great stride by proudly refusing to make science the mere handmaid of technology. We should neither consider the worldly success of science as its chief goal, nor take it for granted. It is easier to take error for granted. If science is always right, then its success may be taken for granted; to allow that science is ridden with error is to challenge, to invite efforts to explain its past success, and not assume that the future will bring science more success. Now the explanation of the success of science will be scientific at best, and so it will be open to doubt. Scientific explanation should be put to test. The explanation can be of the success of science in general and of successful episodes, such as the great episodes in the growth of science. The concern of this study is with the difficulty to take seriously in public the view of science as fallible. It is easy to be on the side of science because it brings about worldly success; it is somewhat harder, it seems, to advocate it as an adventure, much less as the spiritual adventure that it is. This is an error, as we see from the popularity of space exploration. Yet science is stricken with the affliction of all successful movements: it has its share of hangers-on, the mixed multitude of joiners, among them those who serve as its public relations officers, chiefly the philosophers and historians of science. But not all historians and not all philosophers of science are public relations spokespeople. There are fascinating studies in the field, and these may be studied and emulated. The first step

in that direction is to take into account the influence of traditional philosophical errors on reports about historical events that both scientists and historians have succumbed to. This kind of exercise is common in all sorts of history, but in the history of science it is a relative novelty.

The major thesis that most of my writings illustrate is that science is a part of our culture. The histories of science that I read in my adolescence displayed a refusal to differentiate between Prometheus, Aristarchus, Archimedes, Copernicus, Galileo, Newton or Bohr. They viewed science the same if it was ancient, mediaeval, Renaissance, Enlightenment or twentieth-century. I hope readers of this volume will find it a bit difficult to do the same. Yet it is foolish to demy that this abstraction was inevitable. The more realistic the view of science became, the more its abstract nature became strange. This makes sense of the strange fact that Paul Feyerabend criticized severely as too abstract the philosophy of science of our joint teacher, Sir Karl Popper, although — or rather because — it is more realist by far than any other philosophy of science, since that philosophy of science is the only fallibilist philosophy and the only one that recognizes scientific controversies and scientific schools of thought. (It was C. S. Peirce who coined the term “fallibilism” and was in favor of what it stands for, the idea that nothing human is error-free; but he viewed science as verified theories.)

I cannot imagine what the history of science will look like when many historians of science will keep in mind as they write their histories the views of their heroes about science — and about God and the world. Nor is it easy: Lotte Mulligan has argued against efforts to anchor science in its cultural and socio-political background by showing how diverse it is. This book does not discuss that but illustrates manners of relating science to its cultural background. It is clear that historians of science can study new culture-related problems. My essays at the end of this volume come to illustrate this.

Herzlia, Israel, summer 2007.

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FOREWORD

By Kostas Gavroglu

To be able to witness the presence and, especially, the assertiveness of members of a species under the threat of extinction, gives one a sense of hopelessness, but, at the same time, the encounter is highly invigorating. Scholars who are in the tradition of history *and* philosophy of science, are few and becoming even fewer. Joseph Agassi, is one of those who helped mould this tradition and, in this volume, he has many and intriguing things to say. From Maimonides to Einstein, from Aristotle to Newton, from Priestley to Oersted, from the problems associated with the inductivist philosophy and history of science to the metaphysics of the historiography of science, the ideas that are so systematically unfolding in these papers, comprise surely the best samples of work of a person belonging to a generation whose main paradigm was the enterprise of science itself, the critical assessment of the grand narratives, without, however, loosing track of the big picture. While making his points in the history and philosophy of science, Agassi is struggling to make us sensitive to issues related to the public perception of science and the role of philosophers and historians of science in forming the various characteristics of such a perception –even, at times, accusing them of perpetrating the myth of science.

Joseph Agassi has been reminding us of the necessity for the meshing of history and philosophy of science, insisting that there is a single enterpise. His dogmatic insistence is not because he feels that historical knowledge on the part of the philosopher, and philosophical expertise on the part of the historian, makes a generally more comprehensive profile for historians and philosophers of science. He has been insisting on this overall approach, because he feels that unless one is well versed in philosophy as well as history, it is impossible to be self critical –and, for Agassi, being (self)critical is one of the pillars of the western scientific tradition. In addition the disinterestness of criticism, and the acquiring of all the epistemological prerequisites for becoming (self)critical, becomes a constitutive *moral* aspect of the very practice of history and philosophy of science, and, thus, it is "impossible to appraise it cleanly and synoptically while leaving out its critical dimension –which is what is done by historians and popularizers of science".

One of the many aspects Agassi has forced us to pay attention to, are the errors and mistakes in the sciences as an integral part of their development. In this way he has helped bring to surface the significance of errors as part of the history of science. Science has not been an uninterrupted success story, unfolding the truths of nature. The "bad" moments of science will have to be regarded as "part and parcel" of science itself. The errors, especially all those that many scientists flippantly consider as errors "which could have been avoided", Agassi tells us to consider them as additional instances in order to acquire a further insight into science. Few historians and, even fewer, philosophers of science have systematically studied this aspect of science. Let me add a few thoughts.

To discuss errors, especially in the work of those scientists whose activities were predominantly experimental, is somewhat uninteresting: errors happen all the time, and many experimenters either (eventually) discover them or they are pointed out to them. Errors, are in a way, an integral part of experimentation, it is important to record them, they have been discussed by the experimenters themselves in articles they wrote about their experiments, and they have, even, been commented upon in historical writings. Often, they have been part of the account of priority disputes. Nevertheless, such errors appear to be devoid of much historiographic interest, since, they are, in a way, of a technical character. Almost always their sources have been uncovered, the reasons for their creeping up have been well understood and, in most cases, they were not repeated by subsequent experimenters. Even though they are part and parcel of the experimenters' way of life, errors of this sort do not have any appreciable effect on the overall practice of the scientists involved, they do not seem to affect macroscopically what they do.

Alternatively, there is a state of affairs which is often confused with errors and which presents immense historiographic interest: going amiss. The study of cases of going amiss is a distressful process full of predicaments. On the one hand, such a study needs a healthy dose of anachronistic readings and, on the other, it is necessary to rise above the indignities inflicted by such indulgence with anachronisms. What is *not* meant by going amiss is getting nowhere. When one is going amiss, one does, in fact, get results – experimental, mathematical, theoretical, interpretational. We know someone was going amiss, because where he got did not turn out to be "correct" according to later developments. The study concerning going amiss of programs and persons working within research programs, is not a discussion about the false consciousness of the researchers, nor is it an attempt to discuss the psychology of discovery. [1]

The discussion concerning going amiss is rather challenging for the historian of science, who is obliged to understand as correct the results that later on we know they were either wrong or indifferent to later developments. Going amiss is not an accusation, but rather a characterization: it is an *a*

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[1] A systematic study of a lot of problems related to errors can be found in *Going Amiss in Experimental Research,* Edited by Giora Hon, Jutta Schickore, and Friedrich Steinle (to appear in 2009).

posteriori characterization of the overall framework within which theoretical or experimental practices are defined. It is conditioned by methodological choices, philosophical preferences, ontological commitments, beliefs which turn out to be prejudicial attitudes, self-evident assumptions which turn out to be unnecessarily constraining conceptions. Such are the ingredients of research programs and theoretical frameworks which, in a way, oblige the researchers, to insist that they unswervingly persevere in what it is they were doing. Going amiss is about such constraining frameworks and the ways the scientists' practices are accommodated within these frameworks. And understanding the character of constraints unfolds the reasons for the excessive rapport and the perceived affinity scientists have to specific theoretical schemata. This is something related to individual scientists, but it, also, displays social and cultural dimensions. Hence the discussion about going amiss, is a discussion about motives, and most significantly, about understanding practices –experimental or theoretical. Examining such practices help us clarify modes of obtaining experimental knowledge, the conceptualisation of prejudices, the extent of commitments to theories or theoretical schemata and the ensuing deadlocks of the programs.

In what has been said above there is a clear cut distinction: errors are of a technical character, going amiss can only be assessed after the event, ex post and anachronistically. Hence, in questions of what we call "wrong interpretations" errors do not creep in. Of course, there are cases when wrong interpretations are, in fact, interpretations full of errors. But what characterises wrong interpretations is that they are cases of going amiss, since most often they are formulated within conceptual frameworks conducive to going amiss.

Agassi's motto has been all along "let it be": let science be reflected through its philosophical and historical considerations as it was practiced, as it emerged through its social setting *and* through the intellectual efforts of its protagonists, take center stage. His sympathies definitely lie with those who consider the scientific enterprise as primarily an intellectual phenomenon, without dismissing the role of the social factors, even though in the long run what remains are the results of the intellectual effort. Agassi, averse to any kind of prescriptions of how to do history and philosophy of science, has been faithfully following Popper's (non)prescription: "take scientific theories at their face value as true or false and research as a process of explanatory conjectures and their tests – their attempted refutations."

Joseph Agassi has been arguing his views for many decades and those of us who had not dismissed him as an exotic voice, have always held Agassi's papers as providing a peculiar kind of quasi-ethical, quasi-professional criteria for our work. He has forced us to keep on asking for ourselves those idiosyncratic yet demanding, questions: have we been sufficiently (self)critical? Is what we are writing at least some kind of fun for others to read? Are we as historians (or philosophers) of science developing arguments, making points or raising issues which would not be taken to be trivial by the philosophers (or historians) of science? The various chapters in this book, provide all kinds of reasons of why it is so very important to continue asking these questions.

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PREFACE

It is a great pleasure to see this new selection of essays of mine, *Science and Its History*, appear in the highly esteemed *Boston Studies in the Philosophy of Science* series. It is a sequel to earlier selections of my essays there, *Science in Flux* (1975), *Science and Society* (1981), and *Science and Culture* (2003). My collections of essays (among them my *The Gentle Art of Philosophical Polemics,* 1988), include chapters that have appeared earlier in learned periodicals, but are hardly accessible; they all appear here in revised versions. Reading this book, as almost any other work of mine, requires no special prior knowledge. Let me repeat now all the philosophical information that I hope my readers will have some idea about as they read my works.

1. The oldest view of science is intellectualism: science rests on purely rational foundations, with no need to appeal to experience. Foremost thinkers from Plato through Galileo and Descartes to Kant have advocated it. Champions of the dominant view of science malign it. I will reluctantly ignore it here.

Intellectualism fails. Each guarantee for correctness invites a guarantee for its own correctness. This is the argument from infinite regress.

2. The dominant view of science is empiricism – science rests on experience – and inductivism (of Francis Bacon, the father of modernism). It is the view of science as error avoidance: science disregards all tradition and relies on facts alone to guarantee the truth of its theories (or at least their high probability).

Inductivism fails too, and for the same reason for which intellectualism does.

3. Consequently, instrumentalism (of Pierre Duhem) suggests that science is but a mathematical tool-kit. Depriving scientific theories of informative content, instrumentalists are left with no view of the world. They endorse some traditional or arbitrary views of the world that they shield from any possible impact from scientific discovery. Viewing science as merely practical (as applied mathematics) they cannot possibly explain its practical success.

4. The only serious alternative to these theories is the critical view (Karl Popper) that takes scientific theories at face value as true or false and research as the process of proposing explanatory conjectures and undertaking their tests – their attempted refutations. It is an endless process of error elimination. Technology is a social affair: to explain it we need theories from the social sciences to bridge between the theories that we apply and their

successful applications. The idea that applications of applicable technology are unproblematic is obviously false.

This is all the background information that I use. I will explain it repeatedly. My main task is to discuss in some detail the distortions repeatedly and systematically disseminated as expressions of admiration for science. Science has no need for these distortions – or for any other. When new research is promising, the public is informed about it with much fanfare. If the hope peters out, information about it is allowed to drop out of memory silently. Let me mention a few examples. The discovery of helium was an admirable achievement: it was observed on the sun before it was observed on earth. This is very exciting, as all we have here on earth is the radiation from the sun. Its color revealed the existence of a new element. Now after the discovery of the ability to identify elements by the color of their radiation, astronomers observed more than one element on the sun, but these observations turned out to be mistaken; only helium remained. The concealment of this fact is understandable, yet mentioning it, not from spite, makes it easier to see the daring of those researchers who sought the fingerprints of elements in the solar radiation. Similarly, when on theoretical grounds a physicist Hideki Yukawa claimed that there is an elementary particle other than the familiar electron, proton and neutron, the test of his theory led to the finding of such a particle. It did not fit all that he had expected of it. Usually his success is praised and his disappointment is suppressed – seemingly out of respect for him. He does not need this kind of respect: like Oliver Cromwell, he could ask to be portrayed truthfully, warts and all. Now the concealment of the warts is regrettable; when it is reinforced by historians of science, it vitiates their work. This is the main point of this volume.

Aspects of the history of science other than the stories of great discoveries came to light in recent decades. They deserve mention. The most important of these is the sociology of science – that is naturally accompanied by the history of the social settings of science. Science does not exist in a social vacuum, and this raises the question, what is the input of the social background of science to its activities and how much is the output of science independent of its social background? The traditional theory of rationality recognized as rational only what has a claim to be universal, independent of the vagaries of history. Tradition expressed the demand for rationality as the requirement that scientific research should be utterly independent of external factors. At most these were allowed to present challenges to science, express the wish to study this or that aspect of reality, but no more than that. The reaction to the rationalist demand from scientific research for utter independence of historical circumstances was the irrationalist demand from historians to show that it is utterly dependent. This is known as the Edinburgh school in the sociology of science or the strong program. Of course, no explanation of any intellectual development by reference to social circumstances alone is possible, so that what members of this school are doing is a mystery. One example will be discussed below, since it is historical and relates to the rise of modern science. Needless to say, commonsense prescribes the right attitude: we can study intellectual developments in their context and try to find out how much the context prescribes and constrains and what remains for thinkers to contribute as free creations of the human intellect.

Another new aspect of modern science is the growth of teamwork that began in the mid-nineteenth century but made its impact with the celebrated Manhattan project that gave birth to nuclear weapons. The herald of this development was Thomas S. Kuhn, who took it as axiomatic that there are leading researchers and the rank-and-file from whom total obedience to the leadership is expected, even to the degree of believing in their theories. Unfortunately, Kuhn and his followers did not examine sufficiently the social settings of research and the interactions between individuals and teams or between different teams, etc. This aspect of the social history of science still hides in the future. But Kuhn had a great and positive influence on research in the history of science as he allowed for differences between different research programs, even though he stressed less the programs and more their paradigms (although not every research program has a paradigm: it gains one with its first success), as well on the claim that theories that belong to different programs (or paradigms) cannot be compared. Of course, he admitted, in a sense comparison is possible. In what respect it is not possible is a mystery. This too will be discussed later to some extent.

The aim of this study is to encourage historians of science to write without beautifying, to examine the past without concealment of past errors and failings, especially important ones, and to worry less about the reputation of science and more about engaging their readers in exciting intellectual adventures. From the start of my work on this project, over half a century ago, I could and did make use of wonderful examples. Their number is on the increase, and my hope is that the present work is helpful in helping the trend to grow.

Herzlia, Israel, Summer, 2007.
WebPages: http://www.tau.ac.il/~agass/

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I. CHRONICLERS IN THE COURTS OF SCIENCE: PRELIMINARY ESSAYS ON THE TRADITIONS AND THE HISTORY OF SCIENCE

PREFACE

The English novelist W. Somerset Maugham pokes sarcastic fun in his *Cakes and Ale* at an official biographer who renders the simple but human biography of a celebrated author into a boring picture of a popular idol; the omission of the human failings from the portrait in an attempt to make its object look larger than life, suggests Maugham, deprives that portrait of all possible human interest, gratify as it may the readers with poor taste. Now Maugham himself is a very popular writer, and his own novel in which he thus scorns poor taste is perhaps his most successful, as well as his best. Thus, it seems, there is a wide market for both kinds of literature, the one Maugham pokes fun at, as well as the one to which Maugham's putdown of it belongs.

By the traditional theory of both art and science, development is of representation that moves ever closer towards realism of one kind or another but away from the idealized and the stereotype. This theory is itself an idealization of the situation that it depicts, but there is a significant truth in it. Representation was always pushed in different directions: some preferred it to develop this way, some the other, and some wanted it not at all. The following is eloquent evidence. The chronicle of King David is a monument of bold realism, and the chronicle of King Solomon of equally bold idealization. Both chronicles were preserved in the Hebrew Scriptures because they were valued — each for its own merit. Hence, it is an error to generalize and to view all idealizers as insincere and boring as the one that Maugham depicts. Yet by-and-large there is, indeed, a movement towards increasing realism, that would have pleased King David's chronicler very much. It would have also greatly surprised him. Realism in an invention; incentive for it must first exist. In the nineteenth century the Romantic movement was one of the worst movements of idealization, most of whose products are now deliberately forgotten. It included idealized nationalism and a beautified history of it, as well as narrative and plastic arts that accompanied it. This led to violent reactions to in the form of crude realism and expressionism. And there were mixtures of both. The realism in question often was defective, and so there was ample room for improvement; twentieth century thought excelled in its critical assessments of various forms of realism and in challenging people to attempt to improve upon them all, and in various fields, including not only the various representative arts but also biographical, historical, and social studies. And some of the results were magnificent. Yet it would be foolish to think that idealizing does not evolve today, or that its products are all worthless. The example that is most forceful is Karl Popper's theory of science (as a set of theories open to empirical criticism and overturn) that most of his peers declared too unflattering to be tolerable, yet whose erstwhile disciple Paul Feyerabend declared a dangerous idealization. He spoke as one who fears science, and so he won the sympathy of enemies of science of all colors, the vast crowds who will here be ignored as completely as possible. Yet he was not one of them.

Thus, realism and idealization are not mere matters of historical trends. It is largely a matter of one's view of life and of representation. King David's chronicler was the discoverer of a great idea that he boldly put to experimental test: he thought that the readers' sympathy towards his hero could be maintained or even enhanced by presenting the hero's human weaknesses. Idealizing chroniclers are not necessarily insincere: they often consider mentioning certain details matters of bad taste, or as cramming pictures with distracting irrelevancies. I confess I was myself rather disturbed when reading Thomas De Quincey's *The Last Days of Immanuel Kant* because I often felt both gratitude for the meticulous recorder and doubt as to his taste. Does this not expose me thus as one who prefers idealization? Is not idealization chiefly the omission of unpleasant or distasteful details? Is it so obvious that my dislike of the distasteful does not clash with my convictions and that my real intent is not therefore to ignore? The same question asked concerning allegedly distasteful details, may also be asked concerning allegedly irrelevant details. A simple representation may be exaggerated but bold and clear, or it may be idealization. Who can give a general criterion to distinguish between the two?

These questions are very difficult. They serve here as warnings to readers against a naïve view of idealization. These questions remain open and enable diverse authors to offer explanations and rationale for some bold idealizations. In some cases, idealizations comprise uncritical or unrealistic attempts at preserving pet convictions. In these cases, perhaps, there is room for introducing new modes of critical realism. But not always.

The aim of the present study is to illustrate and examine the criticism of the ideals and idols that many researchers advocate and their objections to this kind of criticism. They advocate untenable ideals and object to criticism of them, perhaps under the dangerous notion that science needs defense against the hostility towards it and that such criticism may encourage this hostility. Readers who share this attitude and readers who feel that the defects of this attitude are too obvious to deserve a whole detailed discussion, such readers may save their time by laying this work aside — unless they would find entertaining a discussion that has so little to say but that links it with a variety of studies from diverse fields.

Urbana, Illinois, Fall 1964,
Herzlia, Israel, Fall 2007

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PRELIMINARY ESSAYS: ON THE TRADITIONS AND THE HISTORY OF SCIENCE

Introductory Note: On Studies and Their Motivations

The following four preliminary essays concern the four obvious charges that have been made against my *Towards an Historiography of Science*: first, that on account of not being expert in the history of science I have not produced a work on the situation in that field that is up to the standard required of academic publications; second, that my work primarily consists of destructive criticism; third, that I am unjust to most historians of science whose primary aim is merely to bring the educated inexpert towards a little understanding and appreciation of science and its method; fourth, that I am flogging dead horses.

I readily plead guilty to all these changes. If this means that the present volume is of little value, so be it. But I do not think so. I cite here not only venerated historians of science like George Sarton, but also popularizers who may now be forgotten but who were immensely influential during my youth, long before I dreamt of writing anything, let alone anything on the methods of writing the history of science. Perhaps I live in the past: I still remain quite unimpressed by the charge that I am out-of-date: my aim is not to make any contribution, whatever this may precisely mean, but to share with my readers certain problems, solutions and criticisms, perhaps also some impressions of some studies. Though I am inexpert in the history of science, I hope to portray the history of science as it was at least until the late twentieth century, if it is not still very much alive, and to provoke some discussion about this field, its methods, etc. And although my criticism is destructive, I tried to make it interesting as much as I could, in the hope that perhaps it will provoke some new thoughts, some new constructive alternatives. My severest criticism of so many historians of science of the mind-twentieth century is my pointing out that their works are excruciatingly boring and almost entirely unreadable — as readers can find out for themselves with little effort. Yet I should add that they convey a philosophy, perhaps a mere feel, old-fashioned and intriguing, that only the defensiveness of my critics makes it impossible for me to convey and to provide the sense of charm that goes with it. And though to that end I am flogging some dead horses, I hope my readers may enjoy watching the exercise and get at least a whiff of the fascination that I find in it all.

It may puzzle prospective readers that I have found it appropriate to dwell on this in a whole volume. My aim in doing so is hardly to repudiate the charges of commentators; rather it is to reduce their popularity, to reduce the frequency of the future use of their kind of argument. This, however, is not a sufficient reason for my readers to spend their valuable time on wading through my work. My aim is to offer something that is interesting and pleasing, perhaps challenging them to try their hand in recreating old episodes from the history of science as artfully and as truthfully as possible.

To that end I offer some recipes. I describe and illustrate the impact of certain traditional views of the nature of science on the historians of science and of the writings of its records. I argue that Popper's view of science as the pinnacle of the western tradition of criticism is new and exciting. And I suggest that it may be a powerful tool in the hands of students of the history of science who are willing to try their hand in new exciting adventures of writing about exciting old material. In all this, however, my wish is not to advocate Popper's theory but to employ it myself, and to a different and more practical problem, one that will hopefully provide a unifying thread to the different discussions that appear in this volume. It is this. How can we increase the ability of educated people to enjoy fruits of specialized studies? The diversity of studies and the concentrated effort invested in each of them makes some measure of division of labor inevitable. But the enjoyment of the fruit of the labors of specialists need not be so limited. If it remains limited, it tends to disappear altogether: specialists who cannot enjoy the fruit of the efforts of colleagues in diverse fields of study lose all ability to enjoy all study, and then the rest of the community may lose it too. This is the general trend today; hopefully something will be done to change it.

It is hard to appraise the facts. There is a sharp increase of production and purchase of a variety of reading material, in diverse subject matters and in varied quality, specialized and introductory or popular. Many commentators and reviewers claim — rightly, I suppose — that these purchases often indicate nothing more than good intentions, that paradoxically the failure to execute one's intention to read increases the propensity to purchase books, in a sort of guilty conscience and in vain hope for better opportunities. These commentators and reviewers, as well as other well-wishers who have half assimilated C. P. Snow's *cri de coeur* about the two cultures, exhort the public to read, thereby increasing pressure to purchase, and thereby implicitly admitting failure to stimulate pleasurable reading: we do not force people to do things we expect them to enjoy; rather, we appeal to their wish to have fun. Can we revive the fun of reading even histories of science?

That purchasing books is no evidence for pleasurable reading is obvious in the case of increased sales of books that are neither readable nor enjoyable. The example that comes readily to my mind is the literature on the history of the natural sciences, especially books by the (justly) celebrated historian of science George Sarton. His books are compilations so unreadable that even the few specialist historians of science who use them with profit admit inability to read them ─ yet these books are still on sale.

This evidence is inconclusive. Not all purchased books are so unread or so unreadable. The facts, to repeat, are not easy to appraise, and in the circumstances one can hardly do more than convey a general impression and personal experiences. These, however, are notoriously highly biased, since they depend on personal background. My personal background, partly traditional Jewish as it is, strongly colors my view in this matter: reading for fun is, or rather was, taken for granted there, where the importance and the desirability of study are deeply felt by all members of the community. This has made me particularly aware of, and uncommonly puzzled by the view prevalent among so many of my acquaintances and colleagues outside that tradition, of most study as a chore rather than as a pleasure. I have the impression that this phenomenon is not altogether new in the West, though perhaps its present intensity is. The traditional learning for its own sake, the intellectual love of God, was less widely practiced in the West generally than in Jewish tradition. This is new. Reading mainly for fun was common before the relatively recent advent of specialization and more recent publication-pressure. I shall discuss all this in some detail in the following pages; here I shall take the opportunity and say a few words on the Jewish intellectual tradition as contrasted with the Western scientific tradition.

The scholastic and narrow character of traditional Jewish studies and the great defects of the traditional Jewish education, make it particularly puzzling that people of Jewish background who were awakened to the tradition of the Enlightenment Movement have been generally so notoriously successful in it. The explanation of this (social) fact in terms of the psychological characteristics of national types must be rejected as irrationalist. My own explanation relates to three excellent characteristics of the tradition of Jewish scholarship that, when retained by Jews who moved towards the tradition of the Enlightenment Movement, gave them such a great advantage that they could do well by western standards in spite of limited knowledge and general background information. (These are so well known that Barbara Streisand's schmaltzy *Yentl,* 1983, has captured them well enough.) They are the love of learning, scholarship, and a critical attitude. The love of learning provides one with a strong desire to improve one's knowledge for its own sake. Scholarly training enables one to survey the literature concerning the problem one has chosen to study, and critical training enables one to appraise that literature sympathetically yet while explicitly observing its defects. One is thus in a better position to receive a synoptic view of the situation in the field than one's gentile colleagues: many gentile intellectuals find it distasteful to state criticisms sharply because they are themselves greatly hurt when the defects of their views are pointed out to them explicitly, sharply, and mercilessly; many intellectuals, especially English ones, are often sufficiently well trained to be sport-like and take criticism bravely; still sharp criticism embarrasses them. They decently try to avoid inflicting similar embarrassment on others. Jewish intellectuals are so trained that to be hurt by criticism would be the last reaction they would be capable of, or at least admit to. They are thus relatively free of embarrassment. This, within the Jewish intellectual tradition, though endearing in itself, usually led nowhere because of the great limitations of traditional Jewish scholarship; but throughout the ages, when Jews applied their traditional training of sympathetic yet sharp and explicit criticism outside these limitations, quite often the results were impressive indeed.

Obviously, all three characteristics I have mentioned — the intellectual love of God, genuine synoptic scholarship, and engagement in sympathetic, sharp criticism — were usually valued and present in the western tradition. Indeed, westernized Jewish intellectuals were often valued by peers because they possessed these qualities — first and foremost by those who shared these qualities with them. But these qualities were much more strongly and deeply present in the Jewish tradition, and its graduates were able to find compensation for their lack of western education and their understandable deficiency in mastery of western traditions.

This is a well-known (social) phenomenon — the clash of cultures, sociologists call it — though it is not usually presented just in this fashion, and so it is not usually explained why Jews, in particular, exhibit it quite in this fashion and to this extent. It has indeed been noticed that aliens and even their children who grow up in the new environment are at a disadvantage because they are not steeped in local traditions. It has also been noticed that this disadvantage may have the advantageous aspect of forcing aliens to study the local traditions more consciously, and even of thus enabling them to improve upon local traditions, especially by importing from their old environment some improvements of the local environment. It has also been noticed, albeit too seldom, that traditional Jewish intellectual style plays a significant role here. (See, e.g. Einstein's "Why Do They Hate the Jews?") Following Popper on this point, let me observe the element of criticism involved sometimes in the process of the clash of cultures; also, let me observe the advantage, in this respect, of those aliens amongst all aliens, who come from a tradition of explicit criticism, even when this criticism was limited in its permitted applicability in the old traditions.

The western scientific tradition is the most critical one that humanity ever knew, as Popper so indefatigably and explicitly emphasized. Yet, when one looks at the history of rational criticism within the history of western science, one finds incredible attempts to offer the criticism indirectly, or mask it or water it down — or else confuse criticism with derision. Critics and their targets share this confusion. This indirectness was a luxury that one could afford, but not without expenses, such as the sacrifice of some clarity and the lowering of the ability to develop synoptic views. This cost nowadays becomes so great as to become unaffordable. Without the greatest attempts at synoptic views each branch of knowledge is left to specialists, and the fruits of their labor are forbidden to outsiders. The specialists soon lose their ability to relate their work to a broader manifold or to that of their predecessors: with the increased intensity of criticism almost every page written in another specialty is obscure to them and almost every page in their own branch on which the printer's ink has dried they dismiss as obsolete. They thus feel that the obsolescence of their most advanced efforts and they feel driven, vexed, and frustrated. Thus, today the desire to water-down criticism leads to a loss of general orientation and thus the general public cannot follow the expert; it also leads the experts to dismiss criticized views as worthless, and thus to a lack of scholarship; it also leads students to an ever increasing fear of criticism, and thus to constant frustration and vexation. These trends are all modern; their roots are all in the traditional western attitude that, though highly critical, indeed the most critical in all human history — is not explicitly critical, is often mingled with a traditional wish of its adherents to remain unaware of its critical nature. This wish spoiled the fun of study in varying degrees.

Since the scientific tradition is critical, as Popper states, it is impossible to appraise it cleanly and synoptically while leaving out its critical dimension — that is precisely what is done by historians of science as well as by those who popularize science — including those researchers who are extremely critical in their daily scientific research. It is this concealment, and the resultant lack of scholarship and love of learning, that concerns me in the present preliminary essays as well as the development of surrogate motives for intellectual work, such as institutionalized publication-pressure. I have, however, found it useful to contrast here the western scientific tradition — that is highly though implicitly critical — with the Jewish religious tradition of learning — that is critical to a sadly limited extent, but is explicit. It is regrettable that too many people take for granted the characteristics of western scientific traditions as if they were universal and even necessary. Although western scientific tradition is in a sense the best, and in any case it is much superior to Jewish religious tradition, one might adopt consciously certain characteristics from the other (cross-fertilization is the fancy word for this) — leading to general improvement, as well as to the ability to solve certain concrete problems that are now becoming acute (like the ones created by publication-pressure). Finally, let me acknowledge my partial indebtedness to both of these traditions and my greater alienation from them. This is neither a complaint nor a boast, but merely information that may help placing my views against more general background. It has become significant in my studies ever since I have tried to pursue them along Popperian lines.

Let me conclude with an anecdote that at the time impressed me. After Popper read his presidential address to the Aristotelian Society in London, called "Back to the Pre-Socratics", in which he claimed that the early period of Greek thought was a kind of a golden age of critical thinking, A. J. Ayer participated in the discussion and said his piece in a surprisingly angry tone.

We do not need Popper or anyone else, he said, to come and tell us to try to criticize ourselves: this is anyway what we do all the time!

Ayer was right (by and large, of course, since there are always irrationalists in our midst; here we should ignore them). Yet at the time he missed a point, and a rather significant one. My friend William Bartley has meanwhile showed that point: Ayer wished to be as critical as possible; yet he was still characterizing western scientific tradition not as critical but as chiefly empirical in the sense that its theories rest on solid foundations of observed facts. Ayer considered this view compatible with the view of science as critical. He considered his view as imposing the conclusion that science is a critical activity. Yet the opposite still is the case: the traditional empiricist view of science describes it as uncritical, as the uncritical tradition of worshipping facts.

Although the Western scientific tradition was always critical, the traditional assessment of science was (almost) always uncritical. It presented science as having little to do with criticism. Attempts, however feeble, at examining the traditional assessment of the scientific tradition, have often met with supreme indifference, sometimes with unusual hostility — at least until recent decades. This is the thesis of the present preliminary part: western scientific tradition is highly critical, but only *de-facto*, not *de-jure*.

FIRST PRELIMINARY ESSAY: ON THE DESIRABLE STANDARD OF PUBLICATION

> Before leaving this account of recent experimental researches, it may be as well to state, that they are felt to be imperfect, and may perhaps even be overturned; but that as such a result is not greatly anticipated, it was thought well to present them to … the scientific world, if peradventure they might excite criticism and experimental examination, and so aid in advancing the cause of physical science.
>
> (Faraday)

My sharp criticism of a number of distinguished and esteemed historians of science, and my claim that their works are below any tolerable standard, place me in a very uncomfortable position. That I invite counter-attacks matters little, as this comes with the territory. Indeed, to my surprise I won more praise than complaints, and this my first volume is what my reputation rests on, such as it is. What I fear is that readers may read me as a purist, a perfectionist, a zealous advocate of high standards. I am not. Commentators have expressed surprise that after my severe criticism I opt for low standards. This is because in my view the main cause for the application of low standards is the use of unrealistic high ones. Of course, I appreciate well-written works, I enjoyed tremendously reading Galileo's beautiful *Dialogue on the Two Great World Systems* and Ørsted's *The Soul in Nature*, the I loved supreme elegance and precision of Einstein's *The Meaning of relativity*, and Fraenkel's *Abstract Set Theory*, the simplicity and directness of Kant's *Metaphysics of Morals* and the dexterity of Collingwood's *Autobiography*, not to mention the urbanity and style of Russell's various works. But when setting general standards it is dangerous to have such works in mind, even as mere unattainable ideals; standards of what works are acceptable should be set as low as possible, so that if reason can be given to justify a publication of a work, its publication should not be blocked. Individual editors may set standards as high as they wish for works to be acceptable to their periodicals, series, or collections; authors may set even higher standards for themselves (the complaint that so-and-so does not publish interesting ideas due to the adoption of standards of publication that are too high is understandable but illiberal at heart: no one is obliged to publish). But speaking of standards generally we ought to be as tolerant as possible.

1. Writing for publication is not a matter of expert knowledge.

My advocacy of tolerance in matters of standards is also a plea for tolerance towards the present volume. It is not up to the standard that I would like to set for myself; I could improve its presentation and eliminate some of its errors by more rewriting and further checking. But this would take me another few years that I am more inclined to invest in different work, because though I am very interested in the history of science I am not a historian of science and my other studies — say, in the philosophy of science — call me back. Thus, I face the choice of publishing this volume more-or-less as it stands or abandoning the thing altogether. I hope that what I wish to say may be of some use and interest, even when said rather poorly, and as no one else seems to be saying it at the moment I feel I ought to say it.

Admittedly, by and large, works not up to standard are better left unpublished. By the orthodox view one should not publish on a topic concerning which one is no expert, concerning which one is not sufficiently well informed. And by that view I am in the wrong to publish on a topic on that I am no expert, and about which I am not sufficiently well informed. I am no historian of science: my knowledge of the history of science is very fragmentary and inaccurate. But the orthodox view that by-and-large works not up to standard are better left unpublished strikes me as rather misleading, since it may imply that I need not apologize for my previous publications, as they concern topics on which I could be called an expert and about which I am sufficiently well informed. To call me such would not be precisely the case: I make no claim to expertise of any kind, and I do not consider myself sufficiently well informed about any topic whatsoever, including those on that I have published before. As I admire some published works that were evidently written by people who were not sufficiently well informed, I do not think being sufficiently well informed a necessary condition for publication and what I do not demand of others I need not demand from myself. Whether expertise and sufficient knowledge should be prerequisites of publication or not largely depends on one's criterion of sufficiency; and from pride or from humility, or perhaps from philosophical considerations, my criterion of sufficiency is really too high for anyone to meet except perhaps a very few people of genius. So I have long ago given up the idea that expertise and sufficient knowledge are prerequisites for publication, and replaced it by a simple and more workable criterion: a work is suitable for publication if it is more useful to publish it than not, and this will in turn depend on the aim publishing it and its intended public.

2. Writing for publication is largely a matter of traditional attitudes.

When is a work ripe for publication? When should an author decide to submit it to an editor or a publisher? This is a very down to-earth problem, as any academic knows: many colleagues constantly worry about it. Yet my wording of it prevents a down-to-earth answer to it: so far I have presented the question in such a wide fashion that it loses its concreteness. A work may be ripe for publication for the popular market but not for the academic one; one editor of a scientific periodical may decide that a piece is not sufficiently developed to be published and a competing editor may gladly accept it, perhaps because their views differ, or perhaps because the one has a higher standard than the other and perhaps because the show must go on. How, then, can one answer in practical down-to-earth terms such a broad question? Is it not simpler, as far as the concrete aspect is concerned, at least, to discuss each given work on its own merit, and, if necessary, to consult an editor of a scientific periodical or series of monographs that the work is intended to seem suitable? Or should one consult peers, contributors of similar works?

I very much favor this answer as a practical solution to the problem, is a given written work ready for publication or not? Some academics include, as a matter of course, the training of their students for publication as a part of the training for research work; sometimes they even write works with their students before sending them off to publish on their own. This way their students receive their standard of publication and their techniques of writing. Many of the less fortunately trained academics waste their time, as well as their intellectual and emotional energy, because they cannot bring themselves to be trained by others, to consult publishers, editors, or colleagues, close or remote; some academics are worse off: they accept only discouraging comments from colleagues; some academics are in a still worse condition: they are ready to accept only encouraging comments, and very readily so, but they will not hear any criticism, except perhaps in order to feel very bad for a while. By-and-large, the question, whether a given work should be published as it is or be further polished and improved upon, is to be answered by tradition and commonsense, by consulting in a reasonable manner appropriate teachers, colleagues, and editors, and by having the editors themselves consult colleagues and referees. But the intellectual benefits of such a mode of conduct will be positive only if the intellectual current with which one is thus swimming is progressive; only if the intellectual society in which one finds oneself has more-or-less properly instituted standards of publication aimed at helping the advancement of knowledge. This is how intellectual standards were forged, but this is not how things are these days, when publication is lucrative. You may expect it to be easy to publish in the trade market, where there is no pretense of innovation and where the appeal to a text for the popular market is easier to test than the pretensions of academic works. This is not so. The academic world is these days well organized and so when a publisher consults an authoritative academic as to the suitability of a popular text written by one judged not deserving the limelight, the opinion that the publisher receives is negative regardless of the merit or demerit of the text under consideration and regardless of its value as a trade item. In such cases referees do not consider the interest of the publishers that they are invited to serve, but the greater glory of the commonwealth of learning; that is to say, their own interests. And however inviting wise publishers will judge a text, having no wish to antagonize the community of experts they will accept their judgment and overriding their own. You might think that academic referees are more prone to judge a text by its merits. This is generally so in very advanced fields like mathematics, but even in the very advanced field of physics heresy does not find its way to the leading periodicals except on rare occasions. In fields like the history of logic or the philosophy of education things invite serious reform. And entry to the intellectual trade press today is harder than to learned periodicals. Publishing a school textbook is a major operation that requires a tremendous investment and subtle orchestration.

All this is the case only more-or-less, because the ideal institution of an ideal standard — right or wrong — is unattainable, of publication as of anything else. This is fortunate. Thomas Young, who revived the wave theory of light in the early nineteenth century, had ideas that a reviewer in an important journal judged — misjudged, of course — unfit for publication and the general educated public followed suit. He tried to defend his ideas by publishing privately, and failed miserably (he sold one copy); he then tried private communications and was very lucky to find cooperation that soon led to public recognition and finally to glorious success. I do not think that too much censure of the reviewer, the editors, or the general public is justifiable here: they were mistaken, but they had strong reasons to support their error; they were corrected, and when corrected they soon relented.

Historians of science who quote this incident usually do so with disdain, implicit or explicit. They ascribe narrow-mindedness to those who impatiently refused to give Young a proper hearing, thus mistakenly display impatience towards the people they censure as inpatient. They take it for granted that standards of publication are obvious, that by these standards Young's works should have been published, etc. These assumptions are far from obvious: at the very least the historians of science who write as if the standards are obvious should state their case fully before passing judgment: we should not accuse too lightly all of Young's opponents — the great majority of the British learned world at the time. The strange facts of the matter are that the reviewer, whose attack on Young secured him an unenviable place in our histories of science, belonged to a group whose aim was to raise publication standards, that in his review he fully explained his grounds for his negative view of Young's paper and that his rationale is very clear. It is easier to call him a villain or a fool than to show his error, especially since most of those who call him a fool accept his publication standard. But I need not elaborate on this point.

What happened to Young is not just a sad event of the distant past of two centuries ago, one that could not have happened later. Some more recent cases come to mind. About a century and a half ago, a classical paper by Helmholtz on the conservation of force was rejected, as later was Faraday's last paper on the conversion of electric forces into gravitational forces. Helmholtz's paper was published in the trade press, with the result that the author received an unexpected small honorarium (that, of course, he would not have received from a learned journal); Faraday's paper is still unpublished, I think. In the eighteen-sixties, a paper by Newlands that anticipated some of Mendeleev's ideas on the chemical periodical tables was rejected by the London Chemical Society, and he too took recourse to the trade press. I do not know much about recent unpublished material, but I can mention two interesting works that were only commercially published, and that have not yet gained recognition though they are of great intellectual merit. The one is Alfred O'Rahilly, *Electromagnetism* of 1938 that is mainly a criticism of Einstein's relativity. Although the general opinion (that I share) is that all such criticism is invalid (at least thus far), the book contains a wealth of important material, and the little notice that this work has so far gained is very regrettable: many recent textbooks on the topic contain errors that O'Rahilly validly criticized over half a century ago. This has changed, but not sufficiently, after the book appeared in the prestigious Dover house. My second example is R. Eisler's *Jesus the King* (1931). I like it because it is a wonderfully inspiring, badly written book, marred by many serious errors, and devoted to a seemingly untenable thesis that rests on a seemingly untestable reading of a document, yet a reading that is strangely refuted (by Shlomo Pines, 1971). It is full of much useful material and interesting ideas. O'Rahilly and Eisler failed to win much recognition of the academic world and had better luck with the trade press; I need not defend them. I mention them here as ones who did not write well. I, for one, would not complain about their poor writing, nor about the present organization of the learned publication-system, merely on account of its failure to give then better place.

This is not to say that I have no complaint about the learned publication system; on the contrary, in my view, if its present guidelines continue, the system of intellectual scholarship as we know it may perish, not because the system sometimes fails to acknowledge possibly value material, but because too often it acknowledges obviously worthless material and thus dilutes the valuable with a deluge of the valueless without offering the young tools for developing their own filters. This is not the fault of colleagues, editors, or referees, who are involved in the publication of material that probably no one will read; it is a defect in our system, a factor only too well known as publication-pressure, summed-up in the slogan bitterly pronounced by so many academics: publish or perish! Publish, that is, even if you know that intellectually your output has no prospective readership.

3. Publication-pressure justifies the printing of much junk.

When is a work ripe? Any interested student of any intellectual problem may benefit from writing, but not all written material will benefit the public. This is not a taboo-ridden approach, but a practical attitude towards the printing press. Novels are usually published in the hope that enough people would wish to read them, but this is not the case with logarithmic tables; whether statistical tables are printed in order to be read, checked, or impress readers, I do not know. Often, academics use the printing press chiefly in order to impress — especially those who happen to sit on academic appointment committees. Publication is one of their best known means for impressing appointments committees except for some unusual signs of academic distinction, such as a Nobel Prize or a fellowship of the Royal Society, or possibly some social distinction, or notable ability as organizers — of choirs, football-teams or intrigues. As long as publication is an important means of livelihood, the question is settled: undistinguished academics must write and publish as much and as quickly as they can, even if they have to lose money in the process. Now when I complain that much too much of the material published in the field of the history of science is not fit for publication, I am not ignorant of the fact that the case is similar in varying degrees in all fields of study as a result of publication being too often a necessary condition for advancing in the academic world, a condition that holds even for academic teachers who have the desire and the ability to teach but not to publish, who may also loathe publication. The history of science suffers more than its share from these ills — as long as publication is a rather necessary condition of appointment and promotion in all fields of academic activity, and as long as the ability to publish original material, especially in the field of the sciences, is still limited. For, it is not unlikely that the existing institutional arrangements of publication-pressure and the difficulty of producing new stuff for the learned press force many good academics to rehash old stuff to publish as the history of their branches of learning, or even to give up being competent teachers of science and become incompetent historians of science. There are other alternatives, to be sure: one can do educational physics, chemistry, etc., instead of physics, chemistry, etc.; one can do popular science, or philosophy of science, or other kinds of rehash, pointless or exciting as the case may be. James Bryant Conant, the individual who single-handedly established publication-pressure during the Cold War, was a burnt-out researcher. He instituted the most powerful history of science research team that covered all aspects of science except scientific research — history, education, popularization and all. He instituted the celebrated *Harvard Case Histories*, and he sponsored Thomas S. Kuhn, arguably the most famous historian of science of that period. Conant raised the standards of research in this field, but in a limited way. He wrote under the influence of the great philosopher and historian of science Pierre Duhem, but at first he somehow failed to acknowledge this fact. Kuhn told me it was a mistake on my part to declare him a Duhemian as he had not read Duhem. Harvard historian I. Bernard Cohen responded saying that this is impossible, as Conant made all his students read Duhem. Odd.

As long as the purpose of publishing is getting along in the academic world, the answer to the question is, then, publish as much and as often as you can. But abiding by the slogan publish-or-perish need not exclude all rational consideration of standard. For example, since having to publish more than to originate imposes rehash, it is perhaps advisable to advocate the method of inserting long quotations into articles, preferably of interesting and well-written passages. This may help raise the standards of writing as a result of a discussion of the question, what a kind of quotation is it desirable to present to the public. Or, priority may be set to surveys and review articles.

Also, the least an editor can do is insist on good style, or at the very least clear style (on the assumption that this demand will not endanger an academic livelihood, as a friend or a relative who writes tolerably well may help out). Perhaps the most important point is to realize that we must discriminate between works published in order to be read with benefit and works published for other reasons, whatever these may be. For, although it is right to publish in order to advance in the academic world, it is a mistake to read material that has been published in order to impress members of appointments committees. After all, even these committee-members have no time to read that kind of material.

Over-publication has not ruined academic standards in all fields; methods of discrimination between the good and the poor do exist and readers apply them regularly. Whatever the purpose of publishing may be, there is always the editor of the better periodical or publishing house to sift the grain from the chaff; there is always the public opinion of the general standard of a periodical or a university press. Consequently, ambitions authors try and publish good material in the acknowledged better academic press, or books in the trade presses that get reviewed in academic periodicals of high standing. Moreover, meticulous scholars would still rather work in small provincial colleges than rush to print and their works, when published, do sometimes win acclaim, even if it takes time for them to earn proper public awareness of their very existence. Yet the general discrimination between the better and the poorer works, periodicals, publishing houses, etc., is not always a sufficient safeguard for the maintenance of the high standards of publication in a given field of intellectual activity: this can be seen from the generally low standard in the field of the history of science: in this field, at least the editors and the reading public do not discriminate sufficiently, nor do they attempt to prevent the publication of the poorest material that seems to pour out in ever increasing streams. Now the average standard of works on the history of science is much lower than that in philosophy or in some other fields. And the safeguards I have mentioned are not sufficient, because standards are being lowered, and even under the pretext that they are raised by the institution of some pedantic rule or another. Although there are particular reasons for the streams of indifferent publications, publication-pressure is the main cause of the general deterioration. Publication-pressure being a novelty, its full ill effects are yet to be seen.

4. It is necessary to discuss publicly the problem of desirable standards.

Until World War I, almost all scholarly publications came from the pens of amateurs. Even when these were academics — members of universities, appointed as teachers or scholars or even researchers (these were until then scarce to the point of non-existence) — their publications were hardly adequate means for furthering their careers: if any material benefit accrued from scholarly or scientific publication, it was the *honorarium* accrued from the selling of a few thousands of copies of an occasional secondary-school textbook; which is scarcely a sufficient reason for viewing as professional any scholarly or scientific publication. And then came the terrific (and most welcome) swelling both of university education and of paid research conducted for commercial purposes and in order to increase national prestige, perhaps also to advance human knowledge and welfare. The problem of selecting the right people for the multitude of a few jobs has thus arisen on an ever-increasing scale. Some people then had the bright idea that, as editors accepted works for publication only when they judged them good, the amount of publications of candidates may serve as a. quick and easy measure of the qualifications of the hundreds and thousands of new candidates for the hundreds and thousands of academic and research jobs created all over the modern world in the decades between the two world wars. This criterion, though meant at first merely as a touchstone, soon expanded to become part of the qualification for a job of a professional academic: its wholesale application forced most academics to publish or perish. James Conant made it a must, and he cared for reputation first. Of course, he meant reputation as a forceful intellectual, assuming that such reputation was due to intellectual achievement; to some extent it still is. But when it becomes a means of livelihood, people who can purchase it do. They may achieve it by foul means like intrigue, or by fair means, like becoming editors or organizers of all sorts. The development of publication-pressure was very rapid and bewildering. On the whole, however, in the early days the results were fairly advantageous: standards having been quite high, publication-pressure at first led to a wonderful and rapid growth; and those who would not write were not necessarily disqualified, as they were often able to become editors by establishing new periodicals in which to publish the flood of new writings, or they were able to move to the administrative side of the ever expanding academic and research world. But deterioration soon set in. Administrative jobs became scarce yet, due to competition, the pressure constantly increased. And the difficulty of publishing while maintaining high standards naturally increased: the well of knowledge that must be replenished at leisure dried up rapidly under pressure; this is particularly obvious in the case of methods of research: the older ones have been flogged to death, but few new ones have been conceived — partly because everybody was so busy publishing. The exaggerated complaint is, everybody is busy publishing and nobody has time to read. Works that offer new methods or new topics of research are different, and so they become fashionable. Learning the gossip became necessary for survival — or so the gossip has it. This way, publication-pressure leads to the deterioration of standards.

All this is no sensational revelation but well-known platitudes — at least in the better academic circles, where lamentations on the deterioration of standards and of reading-habits have become as common in faculty clubs as discussion of the weather in drawing rooms. Yet this seldom goes beyond laments on the deterioration of our culture; public and systematic discussions on question of academic standards are harder to come by, of course. There is some valid and welcome criticism of the peer review systems that has added to the lowering of academic standards contrary to the rationale of its institution. To be fruitful, the discussion should include a historical component, as the standards of the old institutional situation, the standards of seventeenth to nineteenth century learned publications, are now partly forgotten and largely inadequate yet not obliterated. The old standards are bound to be forgotten if nothing is done to keep them alive, because the general standard is deteriorating at such a rate that editors find it ever harder to be choosy, especially as they are under pressure to publish regularly and sometimes even to publish inferior material for external reasons. (Some editors publish mainly works of their friends and their crowds.) Thus, it is quite possible that our present ability to distinguish between the good and the poor, such as it is, consists of the end of a dying tradition; it may be but a survival of some old, inadequate standards. If so, the effect of publication pressure on standards is still working itself out, and ought to be prevented from going further before it is too late.

If the process is still working itself out, perhaps publication pressure ought to be alleviated at once and standards raised. This proposal is inadvisable. The pressure would not be harmful lf we had high standards of discrimination. So, before abolishing publication pressure we have to discuss thoroughly what standard of publication we want. For, our aim is not to stop all publication, nor to reduce quantity without raising quality, nor to increase pedantry in lieu of standards. If we know the standards we want and insist on, this in itself may lead to the easing of the pressure to publish. It would do so, for instance, by enabling people who have published little work of high quality to resist the pressure to publish indiscriminately — thus creating an atmosphere of increased intellectual freedom — or by reducing the publication of useless material by rendering such practices unprofitable. Whether the stream of worthless publication goes on or not, both arguments suggest that we should first concentrate on the question of what our standards should be. But my main and strongest reason relates to the new and unforeseen innovation: the internet system. Its magnificent abundance makes inevitable its inclusion of much poor and even downright objectionable material. This forces users of the internet to learn to discriminate. There is no way to use the internet with any degree of profit without the use of some filters. The simplest filter is the one that excludes every item except the propaganda of my denomination. Even the use of this, poorest filter forces one occasionally to bump into new ideas and some fresh air. Discussions of the standards of the internet started almost at once, heralded by the incredible demand of *sci fi* author Michael Crichton to stop using it as it is full of error. It continued with the contest between the very low-class free *Wikipedia* encyclopedia and the high-class mighty *Encyclopedia Britannica.* It may be only bias on my part that I rejoiced in the victory of the *Wikipedia*: that contest had to be limited to factual errors only, and to conspicuous ones at that, and so it hardly signifies: although it is essential to get one's facts right, this is the least of it. Indeed, the internet has raised the standard of writing of the history of science since it is getting ever harder to replicate the *data* that are easily available as they are on display there, and for free. This forces academic teachers to learn to discriminate, as it is becoming ever easier to copy from the internet while evading the charge of plagiarism and while making it increasingly risky to accuse a student of plagiarism. This raises the hopes that student academic teachers will have to learn what they want students to do and see to it that their essay writing skills will improve. That will be the day.

5. The traditional standards were set by Robert Boyle.

When, then, is a work ripe for publication? This question depends on the goal of the publication, its intended public and more. Hence, it is futile to discuss it in the abstract. It is better discussed, I suggest, in the course of reviving the old standards in the light of newer experiences or some criticism or some change in the setting of the publication system. The old standards of publication were created by the Royal Society of London in the middle of the seventeenth century, as is well known. But the standards were already inadequate then, and they certainly are not adequate today. This is of some historiographic interest.

Most historians of science see no problem here; they take it for granted that a science prescribes its own standards and always has since its inception. They know that styles change, since they translate rather than transcribe old manuscripts. But they take the variance here notational or literary, not scientific. This may be contested. A glance at the literature on physics in the eighteenth and the nineteenth centuries will easily show that forthright reference to problems appears there in the mid-nineteenth century. Historians of science find this quite marginal. They take it for granted that, naturally, if a field of science exists and is developing, then, among other things, there exist standard textbooks in that field of science, as well as essays (or articles or papers) reporting further advances in the field; there may or may not be periodicals devoted to it, but it is natural to expect such periodicals to come into existence sooner or later. Since Thomas Kuhn has made a whole philosophy and historiography of science out of these tacit assumptions historians of science, amateur and professional alike, endorse it rather universally. Yet he added to it all the assertion the thesis that all translations from the language of one period to another is erroneous (the incommensurability thesis, he called it), and he even declared that periods are not matters of convenience for the historian but sets of researches dominated by one paradigm (whatever a paradigm is). This helps keep questions of standards and modes of publication out of the field of the history of science despite the ever increasing interest in it social background. These matters should engage these historians of science, yet they hardly ever do. I am not sure about this point, as I have not conducted a public-opinion poll amongst historians of science. I can only repeat that discussions concerning even well known historical cases of publication of poor papers and of refusal to publish good ones are very rare in histories of science. The view that I declare universal, namely that certain given publication standards are part and parcel of science, has been presented and discussed, to my knowledge only on one occasion, in Thomas Kuhn's *The structure of Scientific Revolutions*. To repeat, despite his thesis of incommensurability, he endorsed this view; he takes the standards of a very specific scientific tradition to be of the scientific tradition as such.

Science textbooks and scientific periodicals reporting new advances, usually taken to be the outcome of the mere application of scientific method, are more specific than that. Scientific method long preceded the standard practice of regularly publishing scientific essays, while science textbooks as we know them today are even later inventions, less than two hundred years old. Experimenters of the period preceding the foundation of the Royal Society of London (like Paracelsus or Van Helmont) never published essays or papers or articles, nor did they publish textbooks; what they did publish were a multitude of monographs — long or short, but monographs. They never dreamt of writing papers, or of organizing periodicals, or of organizing diverse material into textbooks; they did not even think of what we now call symposia, namely books containing chapters, or essays, or papers, or articles, written by different authors on the same theme or idea or question or product. They did not even think of collecting essays by single authors. Such institutions were invented, and not by scientific researchers. One of the earliest essayists, Sir Francis Bacon, began the literature on how to write scientific works. Then Robert Boyle decided, even before the foundation of the Royal Society or its periodical, that for experimental philosophy essays are preferable to books, and that some standard directives for authors ought to be instituted. In 1661 he published a volume called *Certain Physiological Essays* (most of them written long before) to which he added a (very influential) Proëmial Essay on this. His aim was to invite people to be the amateur researcher and to publish brief reports. The professionals were on the whole very poor, especially in chemistry (the alchemists), and the universities were opposing the new philosophy and the experimental method. (They endorsed this method to some extent in the middle of the nineteenth century, and fully after World War II.) So style demanded that a philosophical publication should include reports of experiments, that the reported facts should be separated from, and kept prior to, their authors' theories, and that the whole thing kept short. Boyle's Proëmial Essay is on how to write an experimental essay — probably intended to be a humble beginning. Yet he exercised an enormous influence on the Royal Society — personal, as well as through legislation and through his influence on the first editor of the *Philosophical Transactions of the Royal Society*. His voluminous writings served as a model for the wider world of science. Already in 1667 a symposium published, as a kind of appendix to Bishop Spratt's *History of the Royal Society*, to prove the great value of the work of that Society. Soon after, Spratt's friend Joseph Glenville rewrote a polemical book of his to make it look as if it were reporting a series of observations, in effort to fit his style as much as possible to that of Boyle. This was a condition for admission to the Royal Society. Sir Thomas Browne, by contrast, was never admitted because he failed to fulfill its requirements of presentation and style. (All this is discussed at length in R. F. Jones's admirable *Ancients and Moderns*. These days style is very much under scrutiny, including the contributions of Bacon and Boyle. These are largely obfuscations. The marvelous development that we owe to Galileo and Descartes, as well as to Bacon and Boyle, is the clarity of diverse styles. Boyle had the last word: florid language he said, is like decorations on a telescope: very nice on its sides but deadly on its lens.) We find a century later a self-taught researcher, Dr. Joseph Priestley (a doctor of theology, a preacher and an educator), teaching himself how to write scientific essays by consciously imitating Boyle. He became an eminent researcher who won medals and other marks of recognition; his great scientific opponent, Lavoisier, paid his style of writing (of books of essays) the greatest compliment, by describing it as a string of factual information uninterrupted by thought. The great poet and not-so-great researcher Johann Wolfgang Goethe repeated that compliment verbatim. This is the chief point of Boyle's that had become a standard: publish your new facts as crisply and briefly as a witness on the stand; and describe as many details as is necessary to enable your intended readers to repeat them; as to your ideas, if you must publish them, put then in the brief end of your paper.

6. The traditional standards are not suitable for our time.

When applicable, Boyle's rule is very useful to follow. Yet it seldom is. When one finds an important new fact one need not worry much about style; all one has to comply with in such cases is the demand that the presentation should enable intended readers to repeat the experiment or observation presented. Apart from this, any presentation will do when one is reporting the discovery of a new elementary particle, or of a new chemical element, or of the first compound of an inert gas, or of a new vaccination against a malignant tumors. But these reports, or, in general, reports whose significance is obvious to all concerned, are relatively rare; if one reports a new experiment that is not so obviously important, presentation may make the difference between publication and a rejection-slip, between public notice and neglect. We can go back to history and see what happened, to make us realize how seldom Boyle's rule is applicable. Some discoveries, we know, had the special status of being splendidly isolated, puzzling, and puzzling in their splendid isolation — and for quite a long time. The electric nature of lightning may perhaps serve as an example, since almost no experiments on it were made between the days of Ben Franklin and the advent of the airplane, and it for long remained mystery; the mystery is still not fully and satisfactorily solved. Piezoelectricity, volcanic eruptions, Brownian motion, and Mendel's rule of heredity, may serve as other examples. One need not examine the literature in order to guess that while no new experimental discoveries were made concerning these phenomena, papers on them were published. These papers could not conform to Boyle's standards, since they contained no report on new observations. Some of them, the minority, were presented in flagrant violation of Boyle's rule; they were frankly theoretical discussions of known facts. Most of them, however, seem to conform to the rule: they contain reports on their authors' observations, but these observations were neither new nor interesting; they were included merely because of Boyle's rule. There is a beautiful paper by Faraday, two and a half pages long, that declares all apparently different forms of electricity identical in principle. This takes him but a few lines to explain; the rest of the paper is a report on his observations of lightning. The theory was very important; the report is beautiful but scientifically of very little value. Faraday also wrote a similar paper on electrostatics; but whether because his theory on this matter was more difficult to comprehend or more revolutionary, he was not understood. This is understandable, for even one who knows and understands this ideas may find puzzling the way he reports well known experiments instead of expressing his views more explicitly and at greater length. My explanation of this puzzle is that Faraday was here conforming to Boyle's rules of presentation: he was pretending to speak as an observer while he was speaking as a highly speculative theoretician. He knew this, and showed it in his unusual choice of periodical: whereas he usually published in *The Philosophical Transaction of the Royal Society of London*, these two papers he published in the less rigorous *Philosophical Magazine*.

Obviously "experimental" papers of this theoretical sort were published not because they were experimental, as they were accepted by editors who knew that the experiments they contained were not new. So one may ask how editors decided whether to publish or reject them. I present this question in order to show that the problem of standards is not new, that editors constantly faced it.

What standards of publication should we institute? Attempt to discuss the question should start with the criticism of extant traditional standards. Boyle's standard is inadequate although it was most successful. It stood behind the publication of quite a lot of tolerably good papers, tolerably clearly written, by people who had neither the training nor any profit motive such as the ones that presumably stand behind most writers in scientific periodicals today. Boyle's aim in setting his standard and in writing his voluminous works was explicitly to coax people into becoming amateur empirical researchers and reporting their experiments clearly and without delay. It was admirably successful also where it was not literally applicable, for instance with theoretical work. This success rested on Boyle's sound general principle that was more universally employed (at least intuitively) by writers and editors — a principle that should be retained even though Boyle's standard cannot be. His sound principle is obvious. It is this: publication, like sending a letter (though unlike writing it), is a technical matter, and should be technically considered. When is a work suitable for publication? This question much depends on who are the subscribers or the potential buyers. It is no accident that Boyle repeatedly published essays in the form of private letters: this way he stressed that publications are open letters: he viewed them roughly as we view circulars. We find a century later the common practice of relating new discoveries not merely in the form of letters, but in genuine letters, written, however, in the hope that the recipients of the letters would submit them for publication. This was the way Ben Franklin wrote his discoveries. He resented the tardiness of the recipient of his private communications in submitting them for publication: he showed this resentment many years later in his *Autobiography*. Even in the mid-nineteenth century we find genuinely private communications, like Schönbein's letters to Faraday, submitted by their recipients to editors of learned periodicals. This was a common practice once, though today only a relic of it survives in some of the older British periodicals where a paper written by one person is submitted to the editors by another.

One reason for the death of this beautiful tradition of publishing private communications is perhaps the decrease in the significance of scientific periodicals as means of communication. This, in its turn, is an inevitable consequence of the (welcome) improvements of means of communication in the modern world. The following example may make this obvious. Michael Ventriss circulated privately progress-reports concerning his studies of Linear B. His successful decipherment of it was one such letter. The announcement by Murray Gell-Mann of his elementary particles theory (the eightfold way) was a pre-publication. Such practices come closer to Boyle's idea of scientific publications than that behind *Physical Review Letters.* It comes closer to his specific technical prescription but it is the outcome of publication pressure (and possibly also of over-dramatized pursuit of priority).

7. Current publication traditions evolved with hardly any public discussion.

The method of publication in the form of private letters comes to present scientific publications as intended to communicate information to the interested. It also represents a tradition of allowing authors to choose their referees. Due to publication pressure, refereeing is nowadays a complex large-scale operation. I have not come across any public discussion of this very important institution; my own paper ("Revising the Referee System", in my *Science and Society*, 1981) on the matter did make an impression: editors told me that disgruntled authors would refer to it. But it raised no public discussion. I consider this a great pity. My "Peer Review: A Personal Report" (*Methodology and Science*, 2, 1990, 171-180) was more general and won less notice. The nearest to a public discussion that I have come across is the action taken by my friend J. O. Wisdom, the first editor of *The British Journal for the Philosophy of Science* (in the nineteen-fifties): he composed a questionnaire to referees designed as a safeguard against the most widespread errors in refereeing, and he circulated it first among members of The British Society for the Philosophy of Science that publishes that journal with a request for critical comments and further suggestions before putting it into use. This action is at least partly responsible for the relatively high standard of that journal at the time. Under Wisdom's influence Ian Jarvie keeps this tradition alive for his *Philosophy of the Social Sciences*. Wisdom did not entirely solve the problem of bad refereeing, even for his own journal. Frequently, when annoyed by a referee's stupid report on my work, I toyed with the idea of returning to the old tradition of authors electing their own referees. But I am not in favor of this because publication pressure makes it hardly workable. The right to choose one's referees in a system that practices publication pressure would cause distinguished people great embarrassment: it would direct to them streams of communications accompanied by pleas for favorable reports so as not to impede their authors' careers.

It would be nice if some other institution would raise awareness to publication as a form of communication of material to be read. Readers may think this obvious unless they happen to be academic themselves; for, it is known that most academics sincerely regret being too busy writing to have enough leisure to read for fun. Surveys of readership are scarce, but the little there is shows that most academic publications are not read at all. It is perhaps the worst effect of publication pressure that it is not so easy to publish even tolerably good stuff today as it was a century or two ago. It is extremely easy to put it on the internet, but this brings no kudos and is hardly a guarantee for finding readers. Founders of internet periodicals hope to overcome these flaws. Researchers who find it difficult to get their papers published, perhaps because the significance of their finds is not obvious, perhaps because often the mere report of a new measurement can be stated in half a page or less whereas it is felt that since it is the outcome of months and months of hard work it must be presented in a paper a few pages long. Editors of *Science* magazine, *Physical Review Letters* and similar publications have suggested that brevity be compensated by quick publication, but their chief reason for instituting this journal is that they — especially pioneer Samuel Goudsmit — accept an incorrect methodology that takes information as primary, which is false when its significance needs elucidation. All this invites public discussion.

I once heard a biologist complain about a strict editorial ruling, accepted by one journal, that statistical results should be presented either in the form of graphs or in the form of tables but not in both forms; those who made the ruling did not know that sometimes there are good reasons for this duplication. These samples show that even with the publication of unquestionably new results, the pressure is high enough to raise the problem of standards and of methods of writing, and that public discussion of this point may be useful. How much more is this so in the field of theoretical novelties. I sometimes think that had Einstein developed his first ideas now, publication pressure might have kept his papers on the editor's waiting list for quite a while, and Malinowski's papers would have had to wait much longer (or make his information sexier). The case is similar with the publication of monographs as there is great flow of monographs to publishing houses; it takes at least twice as much time to publish a scholarly work in today than it was to publish it a century ago (although techniques make the printing process very much faster); and it takes longer to have a book accepted for publication — except for celebrated authors or fashionable items, of course. But what should undistinguished authors do? How are their books judged? By their novelty, perhaps. But what makes a book novel? When Max Planck was relatively undistinguished, he wrote some magnificent books that summed up the situation in physics. Were these books novel? Were they publishable? Would they have been received for publication immediately were they submitted now? I think not — if he were lucky he would have to wait a few years only. Of course, the person who benefited most from these works was their author, since they were his means of reappraising the general situation in the field (as he tells us in his *Scientific Autobiography*), so that it did not matter much whether he published them or not. (And his academic job had been secured not on the basis of his ability to produce good work — this happened before the advent of publication pressure — but by plain old-fashioned favoritism.) Imitations of Planck's books updated appear regularly. What they offer young students may be good thing or bad; in my private opinion they are inferior to Planck's works, but what I suggest is that such questions should undergo investigation, whose results should be instituted and repeatedly put to test.

The problems concerning editorial rules, are well known, and supply regular topics of conversation and complaint in university faculty clubs. They are occasionally discussed in meetings of editors of learned periodicals. (I have participated on one such meeting and in an international conference on it that enterprising Miriam Balaban has organized in Jerusalem decades ago. Nothing came of these.) The need for a public discussion of these matter increases, the need to make publication more of a means of communication of new results — theoretical as well as experimental — to those who may be interested in them. This idea stands behind much of present-day publication techniques, but it being so obvious and commonsense has perhaps led people to the erroneous conclusion that it does not merit special discussion. It does: we are far from having an obvious and commonsense answer to the question, how is this obvious commonsense idea best applicable to specific circumstances — for instance under the circumstances of increasing publication pressure.

The idea that publication is a technical mode of communication contrasts sharply with another, equally commonsense idea: that discoveries ought to be published in order to be put on record once and for all, in order to secure priority, or in some other way to increase the scope of human knowledge.

It is easy to imagine situations in which these two commonsense ideas clash. My firm proposal is to use both in inclusive disjunction: publication is recommended whenever at least one of these ideas recommends it.

8. The problem of desirable techniques of writing is less easily soluble.

What institutions are we to create or modify in order to rationalize the method of publishing written material? This question, important though it is, still leaves unanswered the question closely related to it: how are we to write such communications? On this Francis Bacon and Robert Boyle had something important to say that is unsatisfactory: they said, write down clearly and briefly the new results, for they are what matters most. Obviously this will not do, as the situation is often more problematic. I suggest the following alternative to begin with. Let us start with the assumption that some authors wish to communicate some information to some interested public. I propose that when they write articles or books they should state their purposes, determine explicitly the type and the level of their prospective publics, write, check their works as much as they can, show their works to fair samples of prospective readers, rewrite in the light of their criticisms, check again, and so on. When the pressure of critics diminishes, and the subsequent improvements of the works diminish accordingly, it seems quite reasonable to present the work to the public at large in the hope that some unknown readers will take it up and improve upon it. And in case of priority claims the format of patent applications is the most adequate. And that format is much more rational that is repeatedly updated in accord with (hopefully improved) legal requirements.

Although I have not found this stated anywhere before, this is no claim for novelty. It is very often employed, and its employment is traditionally indicated in the acknowledgements that authors often make to some of those who had read their works prior to publication. The kind of public and their level is usually indicated by the nature of the periodical in which an essay appears and in the preface to a book, especially in the sciences, and best applied in mathematics. As usual, my emphasis is not on ideas but on the assertion that the problems they come to solve merit public critical discussion (and that this is badly wanting). Plato reported (*Parmenides*) that Zeno had confessed he had not approved of early publication of his (by now lost) book. Boyle confessed (Proëmial Essay) that he had to force himself to stop improving and publish. Collingwood made a similar confession (*Autobiography)*: he suffered from the same conflict very intensely, and rid himself of some of the pain it caused by realizing that the conflict is common. This indicates that no criterion is automatically workable, though (academic) authors may face the problem more technically than usual: they may judge at each stage — with the help of colleagues, perhaps, or even of editor and referees — whether a given work will better serve its intellectual purpose (I ignore the fact that the author may also be an artist) by further improvement or by imperfect publication. (There are also idiosyncratic troubles caused by editors, but this changes only the practical aspect of the problem by raising the question of compromise.) Such considerations are useless unless one has a purpose, and many a paper shows no sign of its author's purpose. My concern here is with problems that persists when the author's declared purpose is to get an idea or a piece of information publicly noticed and debated. Even then, the technical question is simply whether the work in question is sufficiently improved to impress the public with its significance: the less perfect the work, the less people are prone to notice it; yet possibly some people may take it up in spite of its imperfection and improve it faster than the author can do alone; this is risky: the paper may also find no response just because its mode of expression is somewhat obscure, or its style rude, or its discussion under-developed or over-elaborate. This kind of consideration may solve another problem, and one that many students of historical source-material have puzzled about, though to my knowledge it has not yet been openly discussed.

Every collection of works by one author, or even by members of one school, includes a striking amount of repetition. This was quite normal even before the advent of publication pressure; the reason for it is different. When one becomes familiar with the collection of works seemingly relating one idea, one notices nuance. The nuances sometimes contain new ideas that may or may not justify repetition; the nuance may be just a change of presentation — the author has tried to catch his readers' eyes in different ways, experimenting because readers of printed material are anonymous. Sometimes the experimentation concerns nothing more than changing the length of the exposition of an idea. This shows that due to the anonymity of the readers a certain amount of redundancy is essential, namely that we cannot have ideal standards. (Strangely, even the rudimentary knowledge of the desirable kinds of redundancy, acquired by information theorists, has not yet been applied to learned publication, least of all in philosophy and the history of science.)

We tend to forget that standards cannot be perfect, that at the very least we cannot know whether they are. This is dangerous, as we try to promote high standards, and as our effort is laudable, we may fall into the error of putting them beyond criticism. This is the lesson to be drawn from the standards of the classical period. I should stress this because it is far from my intention to censure the fathers of the Royal Society who instituted them or those who stuck to them for centuries, at least nominally. It is particularly understandable that in the classical era people were rather reluctant to criticize existing standards: these standards operated surprisingly well in spite of their deficiency: people conducted research of relatively high standard and published their results without delay in a tolerably clear and brief manner though they had no training and did not expect personal gain. It is only when existing standards are in great need of repair that people agree to take the risk of overhauling their whole tradition and its institutions; I do not myself endorse this caution, but as it is quite understandable I do not wish to combat it. For, even while holding this cautious attitude one may view the present situation as sufficiently problematic to call for an overhaul of our traditions of learned publication. And if due to this caution one or two valuable works are ignored, this is no cause for alarm, especially as long as writers can try to catch their readers' eyes by experimenting with various ways of presentation.

9. My Own part in the problem

This brings me to my own experiment at catching readers' eyes and to my *apologia*. My first book, *Towards an Historiography of Science,* has a brief and simple thesis. It is, however, one for which I must try to catch my readers' eyes, for which I must present a few arguments and historical examples. And here I become more inaccurate than I should allow myself to be. The work did catch the public eye, and even helped me in my career more than my other scores of books and hundreds of papers. But I failed to convey its message: my profound admiration for Bacon and Duhem was left totally unnoticed, simply because they are my constant targets of criticism. Also, my choice of historical examples was misconstrued. I tried to cut down their number, and I felt that this would not do, for reasons that I shall explain in the next paragraph. I tried to improve my historical examples, and I soon noticed that this would lead me to years of further detailed historical studies. I concluded that a work such as my *Towards an Historiography of Science* can hardly be adequately written except by an elderly historian of science, and certainly not by a youngish philosopher. But I did not know of any elderly historians of science publishing such ideas, and I therefore decided to say my piece, no matter how badly. It is true that anything worth saying is worth saying well, but by the same token it is also true that alternatively anything worth saying is worth saying badly. My hope was that my thesis would find its way to those who might like to read it and that later on some young students (perhaps ones who may become historians science) will find my work, in spite of its errors, repetitions, and simple manifestations of ignorance, of sufficient interest to be taken as a starting point and improve upon it. Otherwise, I may say to myself that had I worked another few years on my project it would have better success. Nevertheless, I will not reproach myself: I have invested as much work in the project as I found reasonable.

As it happens, the outcome was very surprising. Although a first and by an unknown, and although very nasty (much against my wish), my book was very well reviewed and so it was very good for my career even though reviewers usually misread it. I particularly cherished compliments about this work delivered in person by leaders like Bernard Cohen, Abraham Halevi Fraenkel, Max Jammer, Alexandre Koyré, Robert K. Merton, Arnaldo Momigliano, Karl Popper, and Owsei Temkin, not to mention passing but wonderful praise in works of Joseph Needham, Sir Peter Medawar, and others, and the active support of celebrities like Doris Hellman, Robert Cohen, Russell Norwood Hanson, Derek J. de Solla Price, Mary B. Hesse, Paul Feyerabend, Ernan McMullin and Paul Durbin. I was full with gratitude. There also were dismissals; a good and honest historian of science I knew, dismissed it as small fry without taking the trouble to explain; a few complained that it is out-of-date, for good reasons and for not so good ones; and there were serious disagreements with my views. I took them all very lightly, with the exception of the criticisms (cited in full below) of Edward Rosen, the outstanding inductivist and dean of Copernicus scholars and gentleman. His comments were in accord with his custom: the merest correction of factual errors, in the almost strictest of the inductivist style (almost, since Baconians prefer not to criticize but to ignore; Baconians usually avoided all opinions and stuck to just the facts). As Edward Grant says in his obituary of him, he "avoided surveys and broad themes. Almost invariably he chose to research and resolve well-defined, highly specific problems that often involved widespread misconceptions in the history of science." (The worst error that he depicted is popular.) Rosen was the ideal inductivist historian of science as depicted in my *Towards an Historiography of Science*. He was good as he knew his limitation. To quote Grant again, "Rosen chose to leave the superstructure to others and to concentrate on the foundation."

To return to the reception of that work, I was gratified by reviews by grand-old-man of the history of psychology Edwin G. Boring, by Thomas Kuhn, Charles Gillispie, and Nicolas Rescher, who were already then leading people in their fields. Famous Gerd Buchdahl spoke of Kuhn and me as having created a trend. A trend it was, but of course Kuhn put everyone else in the shade. He called my book "brilliant" but complained about my disregard for the good of members of the profession. This complaint he repeated all his life with increasing intensity. He also complained about my neglect of a famous yet quite worthless book that he was using in his courses. He was still using it when Gillispie and he kindly invited me to their seminar and my talk there included a detailed report on that book. On the whole the responses to my work surprised me: even the most caustic review was friendlier than I had expected. Nevertheless, the repeated criticism that my selection of texts to examine was too small was trivially correct; the repeated criticism that it was too biased was the opposite: there was no notice of my history of the inductive style and my admiration for it all my qualifications notwithstanding, and the claims that my demand for low standards was surprising in view of my exposure of so much of the literature as not up to standard. This seems to me to be a refusal to see that my aim was to present inductivist historiography today as a poor recipe of writing a history of science, and even that not necessarily so, as the number of very interesting inductivist histories of science is on the increase. It is simply because the Baconian view of science as absolutely true that forces historians to take the up-to-date standard science test-book as utterly veridical. The worst of the comments was the view that my target was the Whig interpretation of history. But this is another story.

Rather than comment on Whig views of progress as inevitable, let me comment on my discussion of the poverty of the inductivist historiography of science of my time. This way, also, rather than explain why I could not survey the whole field of the history of the natural sciences; I will explain why I could not write without presenting some examples: my thesis is that historians of science often write on the supposition that science is always right. It is hardly possible that many historians of science acted consciously on so naive an assumption; it is equally hardly possible that we can make this assumption without noticing it; but my concern is not with the state of mind of historians. So I had no alternative but to discuss some of their output to present this assumption in action.

Two philosophical schools of thought support the thesis that science is always right, and they gave rise to two schools of historians of science. The majority (Baconian) school is the inductivist or *a posteriorist*: science is always right as its ideas are firmly based upon experience. The minority (Duhemian) school is the conventionalist; scientific ideas are mathematical conventions. Although my sympathy, if forced to choose, is unquestionably with the minority against the majority, I belong to neither schools. Rather, I find much more congenial the view of Karl Popper of science not as a body of solid knowledge but as a succession of ideas and of the attempts to criticize them, with no end in sight. In science, then, the best criticism consists largely of new experiments (that comprise discoveries). My reservations regarding this philosophy aside, my aim was to boost it as a wonderfully useful tool in the hand of historians. How much any of the good works in the field are in agreement with Popper's philosophy? I hesitate to judge this question (His direct influence on good historians has so far been negligible; the exception seems to me to be Bernard Cohen. Already Koyré was in agreement with Popper about the value of some scientific errors, as he learned this lesson from the lovely Gaston Bachelard.) I must stress again, however, that excellent histories of science, few as they regrettably are, prove that a historian of science may do excellent work while endorsing a poor philosophy of science or without endorsing any. This I have illustrated in my *Historiography*, but to no avail. Bad philosophy of science may be harmful for the writing of the history of science and, worse, an excuse for the publication of incompetent ones; a good philosophy of science is not quite necessary but it may help.

Here is one example. A wealth of literature concerns the question, is the philosophy of science that Einstein advocated inductivism or conventionalism? Advocates of one answer cite him against the other and *vice versa*: they share the supposition that only these two options are available. That he explicitly expressed agreement with Popper means nothing to them.

10. Present-day double-standard of publication is justifiable.

Let me conclude this part with a friendly word to prospective historians of science who might try to improve upon my work. They may find it difficult to publish even if their output is of a high quality. This they should not resent. Editors have developed a double standard (consciously or not): a very low one for conventional unreadable works and a very high one for unusual works, especially such that might be read or quoted. There are a few good explanations for their behavior. The implications this behavior of editors has for the careers of the unusual authors are in line with a (very nasty) tradition that requires of deviants proof of sincerity before granting them recognition (as a substitute for genuine examination of their output, rooted in the leadership's want of ability to judge). Yet these days it is possibly less dangerous than it used to be. The demand for a large quantity of publications is claimed to be universal, but it does not always apply to authors whose work is of a quality somewhat higher than the average, or even if it merely promises to be so. Names of such people can be supplied by most leading members of the profession. So, although I see the situation as pretty grim, there are a few bright spots in it that keep me very hopeful.

SECOND PRELIMINARY ESSAY: ON THE DESIRABLE STANDARD OF CRITICISM

> … and they get into a passion and begin to quarrel, both parties conceiving that their opponents are arguing from personal feeling only and jealousy of themselves, not from any interest in the question at issue… why do I say this? Why, because I cannot help feeling that what you are saying is inconsistent … and I am afraid to point this out to you … Now if you are one of my sort, I should like to cross-examine you, but if not I will let you alone. And what is my sort? you will ask. I am one of those who are very willing to be refuted if I say anything that is not true, and quite as ready to be refuted as to refute; for I hold that this is the greater gain of the two …
>
> (Plato)

> One can really quarrel only with brothers or close friends; others are too alien.
>
> (Einstein)

My discussions of some works in order to explain why I view them as worthless or as below standard displays a procedure known as debunking. I particularly dislike it. Let me explain then why at times I have (reluctantly) adopted it. The situation is quite simple: we ought to be tolerant of disagreement, and hence to be reasonably respectful towards others or else to ignore their output entirely. Our inability to ignore opinions that we cannot respect is a weakness. This weakness is present, in a democratic state or in the commonwealth of learning. Possibly the field of the history of science is so poor that some poor authors invite debunking rather than disregard. I suppose it was indeed that poor when I wrote about it half-a-century ago; but this was not my reason for taking up the cudgels; I was forced by circumstances to do so, perhaps because the field was so very poor, perhaps because I was unable to solve my problems more satisfactorily. So I do not think my work violated any reasonable rule.

Objectionable practices sufficiently widely and frequently employed to be harmful invite open criticism. The parry that the objectionable practices are hardly ever employed is a challenge to cite examples. When consequently examples are cited, this inevitably brings in an element of debunking. In such cases reasonably complete surveys would probably serve better than sporadic examples: a survey has an air of scientific detachment, and may even be more useful than examples as enlightening those who wish to know precisely how widely and frequently the objectionable practices are employed. But scientific detachment is a mere ideal; preparing surveys is cumbersome, and my examples should suffice as a basis for a preliminary discussion. My effort was never a pretense to be more than that. It is bad enough to thus to debunk a few examples; I had no wish to survey them all. Earlier drafts of my study had no examples: they roused only incredulity in friendly readers. Later drafts had only two or three typical examples; some competent, informed readers then claimed that the examples were untypical. (An interesting example that sticks in memory is a remark of a very learned friend who wrote to me then that the orthodox adherence to Newton that Whittaker's classical history had expressed is limited to the first edition of his book. This error is very nice. It showed me the importance of documenting my debunking in detail.) The friendly critics of my manuscript suggested I must have chosen particularly stupid authors in an attempt to exemplify my thesis. This, I confess, is not too pleasant, because the authors I had selected were justly celebrated: I did not — and still do not — know how to impress upon my readers that I criticize the best authors I could find, even when unfortunately my criticism is a form of debunking. I admit that in my opinion it is a folly to think that all error is the same as folly, and I regret that so many historians of science still write as if it is. We are all prone to fall into this folly, even such an admirable thinker as James B. Conant, whose work I think highly of.

1. The Demand for constructive criticism is dangerous

I owe an explanation to my readers, just because whenever possible folly is better left ignored. Let me put this in an orthodox manner, although this is not to my taste. It is too easy and useless to debunk; it is much more valuable to study the worthy, to draw attention to them, and to add to their stock if possible. My aim was to dissuade others from wasting their time writing inductivist histories of science old-style, while advocating doing so in better ways. Now although it is proper to try to dissuade people who intend to spend time on worthless projects (such as studying worthless books), this is a simple matter not requiring much attention, much less public attention. Moreover, it is most easily done by diverting attention to better works. (The better should oust the worse, to coin a phrase.) Youngish academics, as I was when I wrote my *Historiography* about half a century ago, should not waste their and others' time by systematically revealing all the errors and follies present in this or that history of science, much less is it worthwhile to analyze them carefully; it is better to begin the positive job by writing as good a history book as one is capable of. At the very least one should write mainly about good history books. This would be either a positive contribution, or a discussion of an example worthy of being followed; either being a much better way of raising standards than drawing attention to, and thus publicizing, the follies of other historians.

So much for the orthodox version, that is not too much to my liking, of the thesis that debunking is always better avoided. (This thesis is the point of a letter that Koyré wrote to me shortly before he died.) I endorse the thesis.

What I particularly object to in the orthodox version of the view expressed in the previous paragraph is the bogus contrast between debunking and positive work. The proper contrast here is between debunking and respectful criticism. This latter contrast is well-known. Most people regrettably view the contrast between debunking and positive work as genuine, not noticing that they thereby confuse debunking with respectful criticism. (To my surprise even Popper fell prey to it: when Bar-Hillel accused him of having spent more time in the negative than on the positive he denied the charge, not the positivist assumption behind it that he was combating all his life. See *The Philosophy of Karl Popper*, 1974.) They assume that it is easy to criticize but difficult to construct. That is a corollary from the contrast between negative and positive work that is equally erroneous. Let me, then, discuss the contrast between debunking and positive work, the conflation of debunking with respectful criticism, the contrast between debunking and respectful criticism, and the contrast between these two contrasts. My concern is with the little noticed practical consequences of all this.

2. The contrast between debunking and positive work is a mistake.

The contrast between debunking and positive work is one of the most pernicious of the many impediments to progress, particularly in politics, but also in other fields of human endeavor. The advocates of the contrast wish to dissuade people from debunking each other; they achieve the opposite, as they reinforce the practice of debunking, particularly the (unjust) debunking of honest critics. This happens under two conditions that are prevalent: the widespread resort to the practice of debunking and the immense difficulty to produce positive work. Let me elaborate.

It is easy to say to a critic, and it is often said to ones whose strictures are not easily answered, “if your strictures are correct so that my view is false, then there must be an alternative to it; what, then, is your alternative?” This kind of approach is well-known as the demand for positive criticism, well-known in particular to anyone used to arguing with communists. They are particularly prone to use this mode of argument, since they view theories as chiefly plans for action and their critics’ destructive strictures as chiefly excuses for inaction — for the desire to prevent social change. They are prone to use this argument for a simple reason that looks very clever. Marxists debunk critics as reactionaries, just as the Freudian debunk critics as victims of denial, and Roman Catholics debunk doubts about Catholicism as the work of the devil. Popper called this attitude reinforced dogmatism. When one points out to advocates of such a view that it is a reinforced dogmatism, they can answer that they are quite willing to examine their own view from an alternative point of view, so that the charge of reinforced dogmatism is unjust. In the wish to avoid falling into the same pitfall and thus become reinforced dogmatists, one may take this reply seriously and briefly present an alternative point of view and the criticism that it suggests. This, however, may be of no avail: when one tries to criticize a reinforced dogma from an alternative, the dogmatist may first demand to know more about the alternative and understand the criticism well; and before long the target of criticism shifts from the dogma to the alternative. Then perhaps there is no way to criticize the victim of reinforced dogmatism. One cannot help them to avoid their error. Religious people of all sorts are prone to argue on this or similar lines, and to view their opponents' inability to correct them as evidence that they are right. Moreover, they have a poor reason to support them: they start with the premise that of necessity everyone believes in something, and conclude immediately that therefore even if a doctrine is successfully refuted, its adherents will not give it up unless they are presented with a better alternative. Consequently they feel justified in asking that their critics provide a satisfactory alternative as a condition of their listening to criticism. Were this argument valid, then at the very least it would lead them to the choice of the best religion; but their argument works all too well, regardless of what religion they defend. Hence, it is the irrationality or the dogmatism of their argument, the irrationality of the demand for constructive criticism that is the root of the trouble. This seems rather obvious, yet it is not: the philosopher Carl Hempel and researcher Sir Harold Jeffreys explicitly advocated the rule, do not give up a refuted doctrine before you have a better alternative to it.

The demand for constructive criticism is popular, at least in some mild version. Critics are therefore often willing to stop pressing their destructive criticisms in order to seek a better alternative. This is the moment for the target of the criticism to become critic and retaliate. The unpleasant demand from oneself to admit error becomes the pleasant demand from someone else. The communists even have a slogan for this, first used by Lenin: the best defense is attack. He was right on this point, as is easy to see in retrospect: he was a powerful critic who succeeded demolishing his opponents, first intellectually and then physically; he then stuck to his errors with integrity. For that his beloved country paid dearly.

The case of Lenin's demand for constructive criticism is a good example since it is blatant: most people hardly notice that they share this view with him. I wish my readers to resist his idea. In my *Towards an Historiography of Science* I present both criticisms of the most widespread methods of writing a history of science and an alternative to them. I do not know how good my alternative is, and how good my readers may find it. So I hope that they will not appraise my criticisms methods in the light of their appraisal of my alternative.

The demand for constructive criticism may be used cynically, as a method of winning debates, with no concern for the truth. In such cases defender-turned-critic-of-the-alternative often uses arguments based on the theory that has been criticized to begin with, having escaped acknowledging the validity of the criticisms by demanding an alternative and switching to discussion of it. Let me ignore here such willful malpractices. Even if the demand for constructive criticism is made in good faith, it is harmful because too often the alternative is just not good enough. Such cases are the majority because creating new ideas is difficult. The demand that criticism must be advanced along with an alternative amounts to justifying the rejection of the criticism — however valid it may be — and it tends to lead to the disregard for valid criticism. As long as existing views are not considered insufficient, there is little drive or incentive for a search for an alternative. The result is stagnation. This is particularly the case in view of the psychological fact that some people are good at criticism but not at invention of theories and some are good the other way (Fewer excel in both, of course.) The demand for constructive criticism prevents the coordination between them and renders them both idle, by keeping the critic silent and the creative mind in false contentment with the existing views as their criticism is suppressed.

The fault is not in the method of constructive criticism, however. It is in the demand to avoid destructive criticism. Undoubtedly, constructive criticism is preferable even if the alternative is not good. That alternative may be most welcome, and a critic may be advised to state it: it facilitates the understanding of the destructive criticism and its structure. This, however, rests on the supposition that readers are not deterred by the disagreement they have with critics and are willing to consider the possibility that a critic may hold a false view and yet offer valid criticism. It is important not to use the weakness of the critic's alternative view as evidence that the criticism is invalid or unjust: just criticism is oftener coupled with weak or no alternative. The fault, then, is in the demand that all criticism be constructive, the demand, that is, those critics who have no alternative views to offer, or merely questionable ones, should keep silent. This is censorship.

The history of science offers a few striking examples. Destructive criticism is, as a rule, dismissed by the multitude, until someone offers an alternative to the criticized theory that proves to be invulnerable to that criticism. Usually this is no accident, but the result of the fact that, appearances to the contrary notwithstanding, by luck or otherwise, some individual learned about that criticism and took it seriously. Such practices impede progress, since the criticism is sometimes lost and then re-discovered and fewer people make effort to meet it. The most obvious example for all this is the criticism of Newton's doctrine of absolute space that Berkeley and Leibniz launched and that Mach and Poincaré revived and Einstein took up.

To put it bluntly, the validity or invalidity of criticism does not depend on whether anyone offers an alternative to idea under fire, although negative criticism creates a vacuum. The vacuum is in some cases the only way to shake people out of dogmatic slumber and spur them to seek an alternative.

3. Confusing criticism with debunking lowers standards.

This brings me to my second point, to the (traditional) conflation of debunking with criticism that results from the demand for constructive criticism and the prevalence of debunking. The tendency to ridicule opponents, to call them barbarians or uninformed, to analyze their backgrounds and psychology, is all-too-common a method of dismissal. It is common even in contemporary science: the criticism that David Bohm has launched against the orthodox view on quantum theory was dismissed with a reference to his Marxism, as Einstein's was with a reference to his determinism. The most harmful consequence of this behavior is that it leads to the conclusion that critics are debunkers, and hence, that retaliation in the form of debunking the critics is just. Argument is thus rendered a contest rather than an examination of opinions in the search for truth. In a contest it is only fair to encourage each party to attack and defend. If argument becomes a contest, then it would be fairer to insist that criticism should be leveled against each side, namely that each party should defend a thesis.

The chief difference between arguments and contests is that contest are usually symmetrical. (Not always: they are symmetrical in duels if when the signal is given, both parties turn and fire at each other, not if one party shoots first and the other is allowed to retaliate afterwards if they still can.) Not so in arguments, as there each party can attack and both parties may be shown to be mistaken. Being shown mistaken is not quite losing: the exposure of error is more like a cure than a loss, as Socrates claimed millennia ago; to criticize is to show the view criticized (and its supporters) the courtesy of considering it worthy of critical examination. This is why debunking is particularly sad: to render debates contests is to lower intellectual standards. Martin Gardner, a popular American mathematics and science writer, ridiculed Popper's Socratic praise of refutations by ascribing to him the view that loss in a horserace is more fun than winning (*Skeptical Inquirer, 2001)*.

Criticism being more helpful than contests, critics should select opponents more carefully. Excellent critics may do injustice to their own critical ability by criticizing worthless opponents; this lowers their critical standards by diluting criticism with debunking. This has happened to many important thinkers. It is an art to choose opponents carefully and to criticize only the most interesting and important opinions concerning given question. It is an honor to targets of attacks of such artists, and this is publicly recognized. It is akin to censorship when governments ban only influential novels and dramas. Censors then pay reluctant tribute to authors by the very act of banning their works. To have been publicly criticized by a leading researcher is an honor that very few people share, especially if these leaders are artists in criticism. (No one outdid Einstein this way.) Thus, one way of maintaining our high standard is to make it clear, by institutional means perhaps, that we select opponents for the purpose of criticizing them just because we suppose that they have great merit.

Members of a party defeated in an intellectual contest can quickly join the winning party and proceed to debunk views that they had previously advocated. They are thus unjust to the old and uncritical towards the new — forgetting that both parties to a debate may err. They thus lower critical standard: shamefaced defectors forget why they had advocated erroneous views, thus losing the opportunity to learn from mistakes; they develop resentment toward criticism of their new views. Shamefaced defectors are known as zealous converts. Their enthusiasm does not win them the respect that they seek. People who judiciously change parties and keep their appreciation of their older views are better respected, and as people of balanced judgment. Many researchers have noted this, but they fail to see that this does not tally with received views about science as solidly based on solid facts but with the view of science as the poor best that our betters come up with and that leaves much room for improvement.

To conflate debates and contests lowers standards by not permitting one to select opponents carefully, and by allowing one to be unjust to others and uncritical towards oneself — by further conflating criticism with debunking. This is why I merits stress that arguments are not contests and criticism is not debunking. The less we engage in the practice of debunking the better.

Debunking is not always avoidable; attacking intellectually worthless works is at times right: regrettably, it may even be necessary, as when worthless ideas have become popular and powerful and are genuine obstacles to progress. Yet all the same in a way this is a tribute in disguise, an admission that some ideas do have some kind of merit that makes them deserve critical notice, although possibly not intellectual merit. Ideas invite debunking that are not good enough to be properly subject to criticism and not bad enough to dismiss off hand. The best examples, perhaps, are ideas developed after long casuistic or Talmudic critical discourses that should but would not lead to their abandonment. Scholasticism excelled in western culture in the high Middle Ages; in the second half of the twentieth century most philosophy was. Such literature does not merit critical notice on intellectual grounds; nevertheless it is at times too influential to be ignored. It is too big and too intricate to criticize in detail: if any extant body of learning merits debunking rather than either criticism or disregard, it is this kind of literature. Francis Bacon rightly said so, and wrongly legitimated debunking for centuries.

4. Traditionally, criticism is confused with debunking.

It is not surprising that Bacon the great debunker is so important a thinker; he was the arch-debunker of all learning up to his own day that was chiefly mediaeval learning. He also debunked the ancients and Copernicus, whom he deemed an impostor; but this was merely the over-exuberance of the debunker who equated debunking with criticism. In the preface to his collected works he said, you cannot criticize an author and admire him at the same time.

I do not know how much Bacon's influence, how much the religious wars, and how much the gentlemanly tradition, led to the distaste for argument and disputation in England, a distaste that became a necessary condition of being a gentleman, and that soon spread to other Western countries. It may have been Robert Boyle who started this, since he introduced and explicitly defended the method of implicit criticism, the method of criticizing a doctrine without stating it, a very gentlemanly procedure. He recommended laying the arguments against any doctrine on the table, merely letting the facts that refute that unstated doctrine speak for themselves. He proposed this as a concession, regrettable but justifiable, to the popular confusion of criticism and debunking: he feared that the advocates of a theory openly criticized would give up research, when his effort was to institute it as an amateur gentlemanly pursuit. More likely it was the great Newton who was the greatest researcher ever, but not much of a gentleman.

Of all the factors that led to the conflation of criticism with debunking and that led to a climate of distaste for criticism, only religious persecution was soon forgotten. The distaste remained. It was at least partly due to Bacon's equation of criticism with debunking that rested on his equation of error with pseudo-science, prejudice, and superstition. (This equation is still very popular as the Vienna Circle made it worse — more extremist — when it equated all kinds of non-science, especially theology, with sheer gibberish.) It was also partly due to Boyle's gentlemanly dislike for dispute. It was adopted by the Royal Society of London whose fellows were learned gentlemen Newton, its most illustrious and most neurotic president, ruthlessly imposed it. Boyle partook in public disputes, however reluctantly; the great Newton preferred not to publish his ideas rather then get entangled in dispute. He was touchy and a formidable destroyer of all explicit critics who had ever crossed his path. The dislike of dispute was soon deeply entrenched, to become universal in the West in the early eighteenth century. The method of implicit criticism remained standard. It is not easy to implement, as critical debates sometimes become so involved that explicit statement and careful analyses are necessary for comprehension. When this happened, explicit criticism was voiced; but then even great thinkers confused that criticism with debunking. The chief critic in eighteenth century science, Antoine Lavoisier, allowed if not actively encouraged his wife to burn ceremonially the chief work of his chief adversary. The pretext was that the book advocated a false doctrine. The books merits were totally ignored. Almost all people involved in the dispute — between the phlogistonists and the antiphlogistonists — called each other names, debunked each other, viewed each other as superstitious and prejudiced.

This is not quite true: Joseph Priestley, the stubborn phlogistonist, noticed that Richard Kirwan had won great respect because he was a famous defector and so, Priestley observed, blaming his stubbornness on the wish to be popular cannot be right. No one took this criticism seriously.

The view he refuted is still popular, petrified in histories of science that still denigrate eighteenth-century phlogistonism. It was an error, but as the ability to distinguish between criticism and debunking was developed in the meanwhile, we may expect modern historians of science to have benefited from this progress. (To be fair, I should mention that philosophers of science find it even more difficult to rehabilitate phlogistonism — or any other refuted theory; see the tortured book by well-intentioned Peter Smith, *Realism and the Progress of Science*, 1981. In efforts to rehabilitate refuted doctrines many leading thinkers, up to Imre Lakatos, went so far as to deny that scientific refutations are at all possible. Who will find it possible to defend the idea that some errors are admirable and other errors are not?) After all, in the eighteenth century the conflation of criticism with debunking, or of disagreement with disregard, was common in all fields of thought, yet modern historians of fields of thought other than science do not allow this conflation even when discussion the eighteenth century. In his obituary on his friend David Hume, Adam Smith says he would rather refrain from discussing Hume's philosophy because those who endorse it admire it and those who do not despise it. I need hardly say that today historians of philosophy would not dream of adopting this frightfully naïve view on Hume's philosophy: practically all serious philosophers today admire Hume's ideas in disagreement with them. Most historians of science still fail to imagine the possibility of disagreement combined with admiration towards phlogistonism, unless they take refuge in instrumentalism or in some other casuist philosophy: the small minority of historians of science who consider it admirable find reasons to assert that no scientific doctrine is erroneous. It is astonishing what a degree of sophistication philosophers and historians of science are capable of when they try to maintain the frightfully naive positions according to which no false idea is admirable. They find it impossible to conclude that scientific disagreement s possible and that in a disagreement at least one party is in error. What then was the historical dispute over phlogiston about? If Priestly was not in error than Lavoisier and his crowd were in error when they set the dogs against him.

Historians of science must recognize the traditional equation of debunking and criticism and they must let it go. The failure of many historians of science to notice the difference between debunking and criticism is traditional but too confusing. Evidently both debunking and criticism are attempts to spot errors in given theories, attempts to expose theories as erroneous. Let me now try to provide a criterion for distinguishing between errors that should be criticized and those that should be ignored or at worst debunked.

5. Critics are respectful towards the views they criticize.

A simple criterion may serve as an approximation. A theory advocated despite its having often been criticized successfully may be debunked; more generally, a theory whose falsehood could easily be detected, may be debunked. In contrast, a theory that has not yet been criticized, and that is not easy to criticize, must be criticized with respect. For instance, when Faraday was confronted with the erroneous identification of criticism and debunking he had to criticize it. But when the same error was committed by historians of science a century later, at best it was advisable to ignore it and at worst to debunk it. In view of the widespread of this error amongst historians of science, possibly it had to be debunked. Repeating an old error, or more generally, committing error unnecessarily or committing an easily avoidable error, is different in character from committing an error in spite of caution. This idea is ascribed to Marx, who debunked those who ignore criticism, declaring that they were doomed to relive their error that was first a tragedy and is then a farce. Very cruel but possibly not unjust.

"Scientific error" or "reasonable error in science" still sound like oxymoron to most people. Commonsense recognizes reasonable error. The law recognizes reasonable error. Mathematics and computer science recognize reasonable error in approximation theory and its application. Not official philosophy and history of science. Many a practitioner of these disciplines has not yet noticed that some error is reasonable. On the contrary, although unwittingly, a few historians of science have implied that a researcher *qua* researcher should not err, does not err, and even cannot err, since science must coincide with reason and error cannot be reasonable. One may say that in each scientific Dr. Jekyll hides an erring, anti-scientific Mr. Hyde who is .the author of the errors of Dr. Jekyll. Thus, Dr. Lyman H. Butterfield, editor of the celebrated 1951 *Letters of Benjamin Rush* declared that as his hero's treatment of patients is not scientific, it was performed in his capacity as a private citizen. This is absurd; private citizens who practice medicine break the law, and as it happened, Rush was even exonerated in court.

The divorce between science and error is objectionable on the technical ground that it blocks the differentiation between science and superstition. Suppose that we trust the judgment of editors of respectable periodicals that publish only *bona fide* scientific papers. Let us say, whatever appears in *The Philosophical Transactions of the Royal Society of London* is scientific. Then, obviously, science contains some errors that are easy to spot. One need not be expert for that: even an amateur who leafs through old copies of that august periodical will find there errors with ease. Tell that to a researcher, and you will meet with an angry response to the effect that these errors are corrected. For such researchers, if they are not quite up-to-date on their research, then there is an escape route to save their careers: they can write histories of science. These histories will be tuned with the latest science textbook. This technique is still rampant, but, according to my unscientific impression, it is much less popular now than it was when my *Towards an Historiography of Science* appeared, and when in a conceited mood I tend to flatter myself that this is to some small measure thanks to that book of mine.

Back to the idea that science is error-free. If the science textbook is no infallible judge about science, how shall we distinguish between the scientific and the unscientific parts of the old *Transactions*? A leading philosopher and historian of science, Larry Laudan by name, has published a famous paper that debunks Popper's ideas on what is science ("The Demise of the Demarcation Problem" 1983). Not only does Popper offer the wrong answer, says Laudan; he even poses the wrong question. Now this may be true, yet it is a violation of etiquette anyway: Popper has not invented the question, since it is traditional, and when rejecting a traditional item one has to reject it as traditional and not as one proposed by some of its latest advocates. This is not the worst, however. The question is not what theory is scientific, says Laudan, but rather, why should I believe the scientist down my corridor when he advocates this or that theory? Now I cannot contradict Laudan, as I do not understand his question; or perhaps I just have a difficulty that needs sorting out before I can agree or disagree with him. The ignorant of a given theory cannot believe and they cannot disbelieve it, or anyone who advocates it, be that one a scientist or a pretender. Can Professor Laudan distinguish between a genuine scientist and a pretender? If yes, can he say how? If not, should we believe Laudan? If yes, then he falls back on the question he so emphatically rejects, be it one that tradition raises or that Popper does, all his protests notwithstanding. If not, then how does he decide that the scientist down his corridor is genuine? Because he is a member of Laudan's august university, of course. In other words, he believes the system of accredited universities, or the commonwealth of learning or something. So I have a question for him. Does he believe everything that he hears in the system? If not, then he falls back on the question he so emphatically rejects, be it one that tradition raises or that Popper does.

Science as free of all errors renders researchers' errors superstitious and evil; for if error is avoidable, then erring is inexcusable. Only after we agree that no individual and no part of any individual's activities is exempt from error, and that some errors are less obvious than others, only then can commonsense step in. Thus, if we admit that error is unavoidable, even in science, then we may be able to distinguish between various errors made in the past, and declare some of them reasonable and even ingenious. The claim that error in science is avoidable implies that in scarce all errors are obviously erroneous, so that a researcher who errs in is at fault.

There was, for instance, a famous debate between Einstein and Bohr. Evidently, at least one of the two had erred. (It is now clear that both were: Einstein and his colleagues discovered that quantum mechanics yields the effect now known as quantum entanglement; on its strength they declared the theory incomplete; insisting that the theory complete, Bohr declared this effect impossible to observe. Unbelievably, both parties were in error.) Yet it is difficult to compare such an error with the error of a schoolchild. For the error of the schoolchild can easily be criticized and corrected. The schoolchild is to be criticized patiently and at times even complimented, while an academic who makes the very same error may be belittled. The difference between the school-child and the scholar who commit the same error, one tends too easily to assume, is psychological or educational. And this is the standard background assumption in most studies regarding children's errors. (The exception is the study of children's grammatical errors that under the influence of Chomsky receive the respect that they deserve.) The judgment of children's errors as exceptional is erroneous: the difference is a matter of intellectual level, not of age or mentality, as the following obvious argument illustrates. The schoolchild who commits the same errors as Adam Smith deserves not only criticism but also — and more so and primarily so — high praise and encouragement; although a professor of economics doing the same is hardly praiseworthy. Respect for Smith and scorn for that professor show that we find certain errors sometimes respectable, sometimes not, depending on the intellectual level given as the background against that the error is appraised. This indicates that before an idea is deemed worthy of criticism it is found respectable; even if it may later turn out to be an error; so we have no right to debunk its proponent. Having heard it and the criticism leveled against it we are all better off. Consider a view that cannot be criticized here and now although it may be open to criticism due to some new ideas about it. It is valuable, Popper says, insofar as it provides as incentive for the search for the criticism that refutes it.

So the contrast between debunking and criticizing as well as the distinction between ideas the invite debunking and those that invite respectful civilized criticism are most important in history, particularly in intellectual history, and more so in the history of science. Perhaps the crux of my distaste for many of the works that historians of science publish is the fact that it is very hard to find an error praised in them. Koyré, to repeat, was the pioneer who studied an example of it in great and patient and respectful detail. Many historians of science admit, though rather implicitly, that Lavoisier erred; yet they are concerned, as historians of science, *qua* constructive historians of science to be precise, with his correct results, not with his errors. Particularly, they sometimes argue, we should not censure him for his errors since in his time they were not as unjustifiable as they would be today; and, moreover, since we should not censure him for his errors we should also not discuss them. Here, obviously, the identification of censure and criticism has become a complete muddle: the historian who so argues uses the idea that criticism is no censure as an argument in favor of not criticizing Lavoisier, because criticizing him would amount to censure! It is surprising that serious historians of science think in this fashion or have a good word others who do; so it is proper to cite instances. This I did. But I derived no pleasure from discussing such follies in detail. I enjoyed much more discussing Lavoisier’s ingenious theory of matter that survived Davy’s refutation of his idea that all combustion and acidulation involve oxygen until it was deposed by thermodynamics.

Some writers have boldly asserted — in respect for the truth — that Lavoisier has erred. The great Émile Meyerson admired three heroes in the history of science most, one of whom was Lavoisier; yet he was too honest to conceal Lavoisier’s errors. No writer, however, has thus far stated the rather obvious fact that Lavoisier’s greatness was in his very error, that his greatest ideas were erroneous. Even Meyerson, who did not conceal Lavoisier’s errors, values what he took to be his positive contribution. Though he did not debunk him, he showed insufficient sensitivity to the immense difference between admirable and other errors.

6. Debunkers are disrespectful towards the views they debunk.

Let me conclude this discussion with the contrast between the two contrasts (between debunking and positive work and between debunking and criticism) and the little noticed consequence of their conflation. Some people accept both contrasts, the first (between debunking and positive work) as major and the second (between debunking and criticism) as minor: they deem positive work the real task, whereas both criticism and debunking they deem tasks to avoid if possible. They admit that sometimes criticism or debunking is unavoidable, and indicate, in these cases, a preference for criticism over debunking. This amounts to the view that the difference between criticism and debunking is largely a matter of civility, a question of style. This view seems to me prevalent; it stands behind much of the erroneous tradition of conflation of criticism with debunking. The point was forcefully brought to my notice by friends who have strongly advised me to tone down my discussion — especially its debunking part — as did the great Koyré in his last letter to me. I regret I could not take the advice.

Worse than debunking is benevolent debunking. It is condescension concentrate. Admittedly, debunking is usually satirical or even sarcastic, and criticism is usually expressed with civility. Yet these are points of style and so they are of minor significance and open to ingenious variation: debunking of an authoritarian government can be clothed in the most civil of styles. (When forced to praise the Gestapo as a condition for his escape from their claws, Freud wrote, he recommended them to everybody.) Likewise, genuine good-spirited criticism, especially of esteemed friends, may be done in fun, irony, and sarcasm. The difference between debunking and criticism, then, is a matter of content rather than of manner; it rests on the honest appraisal of the errors under discussion: it is the appraisal of an error as folly that makes for its debunking, and the appraisal of an error as reasonable that makes for respectful criticism of it. And though the question of style in itself does not signify, it signifies inasmuch as it adds to clarity or to confusion. Some people are better than others in calling a silly error silly in an inoffensive way; I sincerely admire this ability and regret my limitation in this respect (especially when writing in a foreign tongue), but I still think it better to call a spade a spade in an offensive way than to call it an artwork. Perhaps Robert Boyle thought otherwise, but this is unlikely. True, he suggested that we do not call a silly error silly; he suggested even that we do not call an error an error; but he never stooped so low as to condescendingly pretend that a silly error was clever. Moreover, his own proposal he viewed as rather regrettable a compromise, and on this technical point that for all I know he judged correctly. He repeatedly drew authors' attention to their readers' sensitivity when advocating the method of implicit criticism; he advocated this method as a compromise, as a sacrifice of some of the clarity of the criticism for the benefit of those who would not accept it, or who would even refuse to read it, if it were sharply put. Whether Boyle's judgment of the sensitivity of the seventeenth century readers was correct or not is a difficult question that I am not competent to discuss. I would contend, however, that passing the same judgment on modern readers is a mistake: Boyle's compromise is no longer necessary. Nowadays we are much less sensitive than our forefathers, and we are the better for it. This is fortunate because most of current criticism is too intricate for it to be implicit yet effective.

7. Choice between criticizing and debunking may be misguided.

The view of a certain errors silly may be an error, and even a silly one. This often happened, especially in the eighteenth century, when all scientific error was dismissed as sheer folly. James Bryant Conant admirably describes this in his *Science and Commonsense*. As he has pointed out, much scientific polemics of previous ages was marred by abuse and by the dismissal of worthy ideas. Yet in the last resort the valuable part of this eighteenth-century discussion, the rational criticism it included, did come across. Both parties in the phlogiston dispute, in particular, were validly criticized in the guise of debunking and abuse and counter-debunking and counter-abuse; the criticism was sifted out from the mass of abuse and irrelevancy. In the nineteenth century things were worse: controversy was suppressed and controversial ideas were rejected by editors of respectable journals. To repeat, a short period before Mendeleev published his periodical table, Newlands had his paper on it rejected because of its controversial character. Yet somehow the controversy continued and criticism went on flourishing. That criticism is suppressed and debunking encouraged is a pity, but fortunately neither is done very successfully. That debunking and criticism are confused so often is also a pity, but fortunately the confusion is often sorted out. And we may benefit from clarifying the difference between debunking and criticism, but we should remember that we cannot avoid all confusion, and we cannot avoid erroneously debunking a theory we should respectfully criticize, and *vice versa*. And when we do thus err, our error need not be beyond repair. There is, of course, always the risk that our errors might lead to irreparable damage, private o public, but in itself erroneous debunking is hardly dangerous. If it were, then the enlightened thinkers of the eighteenth century would have killed the critical tradition by their excessive debunking of each other. One clear-cut example should suffice to show how wide of the mark they sometimes were and how far their error was from being fatal.

Perhaps no eighteenth century thinker was so isolated, ridiculed, and disliked, as Bishop George Berkeley; and for a mixed bag of reasons, just, half-just, and totally unjust. One of the most unjust of these reasons was the fact that Berkeley dared to criticize Newton. (Strangely, and very much out of character in many respects, this is a rare case of very respectful and very strong criticism. Berkeley was one of Newton's most earnest admirers. Also, as John O. Wisdom has shown in great detail, it was very important and very fruitful criticism: amongst the ideas of Newton that Berkeley criticized was the calculus, or more precisely, what we nowadays call the method of differentiation.

His criticism rested on a rejection of Newton's idea that differentiation was the division of one vanishing quantity by another (not to be confused with the much later idea of limits) that, he contended, was meaningless. This criticism has a strange case-history. The upshot of it is that behind the mask of ridicule and offhand dismissal of Berkeley's criticism some of the greatest mathematical minds, amongst them MacLaurin, Lagrange, Cauchy, and Weierstrass, tried hard to meet Berkeley's criticism by a variety of ingenious ideas and methods invented for that very purpose (including the theory of the limits). It is a pity that present day historians of mathematics do not treat Berkeley as well as he deserves, but clearly, the erroneous debunking of his criticism in the past did not put a stop to progress. It only put a stop to the development of histories of mathematics, but this damage can be repaired too, and fairly easily. The early part of the story is told, for instance, in Dr. Thomas Thomson's *History of the Royal Society* of 1812, but though written in a rather civil style it is evidently debunking. That is perhaps the reason why the story has been omitted by later (constructively minded) historians of mathematics. The right thing to do is to rewrite the story, replacing Thomson's debunking with a more critical attitude.

We often say that the study of history is useful particularly because it helps us develop a sense of perspective. Historical perspective does have an important corrective function, and different kinds of historical studies may have different corrective effects on their readers' perspectives. There is nothing better than the study of political history, for example, for those who are swept along by exaggerated boasting, by false political grandeur. But political historians have to study false grandeur in political history not merely in order to serve such people: certain chapters in political history cannot be written satisfactorily without their authors describing historical cases of false grandeur, and as cases of false grandeur: they must comment on their documents, and sometimes sharply dissent from their records. When we have the record that the Egyptian president says that his troops have completely destroyed the Yemenite monarchist forces, and a record from the same source dated a few weeks later about the courage that the Egyptian forces show in fighting the very same enemy, it is simply impossible to take these records at face value. Historians can dismiss or interpret them. Although any contributor to science is greater than the greatest or most boastful military dictator, there is a similarity or an analogy here between political history and the history of science. It is impossible to take at face value the records that contend that Lavoisier's theory is verified, and a record that it is refuted by Davy. This invites interpretation. Historians of science who do not notice that some documents are erroneous misunderstand them. Documents on Lavoisier's theory in different histories of science are surprisingly confused. It matters little that most historians of science are still confused about his theory, except in that they are writing poor histories. Nor does it matter overmuch that these historians accept the story of the alleged total victory of

Lavoisier over the superstitious phlogistonists, that over-estimating Lavoisier they underestimate the phlogistonists and disapprove of them and censure them excessively. After all, the phlogistonists worked not in order to be immortalized and glorified in our histories of science. Yet, again, historians of science do err here, and to their own loss and, incidentally, also to the loss to their prospective readers who need and may gain some perspective from reading the history of science. For, proper perspective reveals the high frequency of the over-estimate and the under-estimate of scientific ideas by the scientific public and its leadership, and the low frequency of the appearance of respectful criticism.

To recapitulate, the contrast between debunking and respectful criticism invites a simple criterion for distinguishing between them. The confusion between the two is not fatal to science, but it is better avoided, and the proper mode of writing the history of science that is all too rare should provide a proper sense of perspective and help in some measure to avoid this confusion. Since historians of science are the most ready victims of this confusion, let me outline, however briefly and superficially, a view on the roots of this confusion. It rests partly on errors of a psychological nature, partly on errors of a philosophical nature, and partly on inadequate traditions and institutions; and these three factors reinforce each other.

8. The confusion between criticism and debunking has diverse causes.

Why is respectful criticism often confused with debunking? This is partly due to similarity of our emotional reaction to these two different activities, especially where emotional reaction is rather predominant. Debunking an idea is calling it a silly error. No one likes to be caught in a silly position, whether it is a silly action or the advocacy of a silly idea. Even clever and competent people, who will readily admit in the abstract that (like everybody else) they are unable to avoid all folly, may nonetheless vehemently and agitatedly defend any specific action or opinion of theirs, past or present. It is inconsistent to deny of any particular action of mine (since my adolescence) that it was a folly yet admit in general that I have committed some folly (since my adolescence). To be consistent, if there is no shame in admitting having committed a folly, say, during the past year, then, also, there is no shame in admitting having committed a folly in a particular action. Yet most people feel very embarrassed when they are shown that they have committed a folly on a specific occasion. The result is that when one's folly is brutally pointed out, one tends to stick to it in a sort of denial. One may take recourse to more drastic methods of falsely demonstrating that one has committed no folly, perhaps by challenging critics to a duel, or by sending them to Siberia — or by merely poking savage fun at them, ridiculing the length of their noses and such. This kind of practice, it may be remembered, was not uncommon in the West amongst officers and gentlemen not very long ego. Admittedly, the same officer who would challenge a critic of his manners to a duel, would gladly accept serious criticism, and from anybody, when planning a battle upon the successful outcome of which his life and honor might depend. This shows that he would accept criticism when he could not avoid it, and that he never carefully considered whether his mode of emotional reaction to criticism was the most sensible. It was not the officer or the gentleman but the general educated public — with the aid of modern moral reformers, authors, and psychologists — that today forces officers to increase their openness to criticism; but the emotional reaction to criticism remains largely unchanged: embarrassed in the face of any criticism, an officer still feels that a critic is making a fool of him and putting him in an inferior position every time.

To put this more generally, and perhaps a little psychologically, to be shown to have committed a silly error is embarrassing. Consequently, pointing out silly errors to friends and acquaintances often occurs incidentally, often with no mention of the error, much less its being silly. Consequently, there is no knowing whether the critic deems the error in question silly or not. Room is always left for the suggestion concerning any error that it is silly. The confusion of respectful and silly error thus persists. Emotional confusion between criticism and debunking is, in its turn, sustained by the intellectual confusion between them. This is quite contrary to commonsense: the officer on the eve of a battle does not respond this way, being much more interested in truth than in honor, even if the interest in the truth is due to the wish to win the battle for the utterly personal desire for honors.

This is not the place to enlarge on the psychology of emotion, and the stupidity of some modes of emotional reactions and of bowing to them. So let me conclude with the following two observations. First, the emotional reaction discussed here is often deemed natural. This is an error: no reaction is entirely natural and unconditioned in any except in the youngest. We can condition it differently by creating different institutions. For instance, the institution of dueling conditions one to be embarrassed and annoyed by criticism and even by a beastly competition for the hand of a belle. It also conditions one to be prone to feel that criticism is humiliating, since one may always face criticism from the better swordsman (or gunman), being thus unable to react honorably. If, to take a hypothetical instance, it were to become a rule of the Royal Society of London that everyone subject to public criticism from a Fellow of that Society automatically receives an invitation to become an associate member with certain specified privileges, the position of having drawn the critical attention of a Fellow might become highly coveted. Moreover, it will make the Fellows think their criticism expresses not a sentiment but a well considered appreciation. No Society of that sort will accept any such rule, I suppose. Those institutions ceased long ago to adjust in response to the new needs of the scientific world. Hypothetically, however, the rule might have been established, and its outcome prove that the reluctance to be criticized is not purely psychological.

My last observation on the psychology of emotion is that the dislike of criticism is rooted in an error in a kind of sense-illusion. Assume that the fear of criticism rests on the fear of being debunked, namely being made to look a fool, and that the dislike of looking a fool rests on the fear of rejection. The first error (all criticism is debunking) is intellectual; the second (folly leads to rejection) is a distorted psychological observation. It is an error that makes adolescence often so exceedingly and needlessly painful, and that many a practicing psychotherapist has to fight again and again. Now take the case of Shakespeare's *Lear*. He makes a fool of himself straightway, and gets no sympathy from the public. He later makes a bigger fool of himself by refusing to admit error, yet, strangely, he slowly wins the public sympathy nonetheless — perhaps because he acts in desperation rather than within reason. Shakespeare could have made him an even bigger fool than he does; he could have made him blind to his error to the very last (as Strindberg and Ibsen would); alternatively he could have made him utter the truth and add either that he always knew the truth or that when he had erred it was not he himself but the devil who had made him do it (as many twentieth-century playwrights did). These practices are quite common in the real world, and the purpose of employing them is to gain acceptance and avoid rejection. But Shakespeare knew better: wanting end of the play to have Lear to gain the greatest degree of sympathy from the public (to make them feel the tragedy), he makes Lear do the most endearing thing: admit error bluntly. Let me add this. The emotional reaction — of approval rather than of rejection of one who admits having committed a blunder — is not confined to the theatre. There is no class of people more anxious to avoid admitting having committed a blunder than politicians. Yet they are harboring the same emotional sense-illusion. When John F. Kennedy confessed his blunder over allowing the abortive invasion to Cuba popularity polls recoded that his popularity increased, not decreased, as a consequence of his honest admission of having committed the blunder.

Why then is criticism so often intellectually confused with debunking? Debunking is often intended as censure. Holding standards of correctness that are too high (due to the demand to avoid error under all circumstances) renders all criticism debunking and censure. As we cannot adhere to these very high standards, ambiguity must be introduced. Strangely, the word "false", often used by logicians, is very seldom used by researchers, and then in quite a different sense. (This is even more conspicuous in the case of practicing physicians.) In the sense in which logicians use the word "false", most theories presented in the physics textbook are plainly false. Yet telling physicists that is asking for trouble, perhaps because most of them hear in this an indication of disagreement and contempt. This does not really matter as long as they can express the disagreement-without-contempt that they must sometimes express, simply because the high standard of avoiding all error is unattainable. The expression for disagreement-without-contempt in the vocabulary of physics is very peculiar; one that I often heard in scientific meetings is, "I entirely agree, but". The trouble starts, however, when ordinary mortals fail to distinguish between this “I entirely agree, but” that is an expression of respectful disagreement and “we entirely agree, but” that is meant to be taken literally (with the “but” referring “but I am disappointed that you did not mention” or to anything else except for criticism of the content of the lecture). This confusion reinforces the vulgar identification of disagreement with contempt. For that confusion science pays heavily by becoming esoteric and thus losing its public character

9. The confusion mutes controversy and renders science esoteric.

The confusion between respectful criticism and debunking plus the respect for science reinforces the view that science is above criticism, that its ideas are perfect. Whatever ignorant advocates of science say, researchers are painfully aware of the shortcomings of its current ideas, much more so of its older ideas. This brings about a refusal to admit that some scientific theories are false, and this leads to the view of the refuted theories as mathematical rather than as empirical. The difference between theories of mathematics and of science then imposes itself, and the ready answer to it is that mathematical theories in the service of science are applied to empirical material: the allegedly scientific theories are mathematical frameworks to store empirical information and to use in diverse ways, including technology. Thus the respectful view of science turns in the hands of its defensive advocates into the contemptuous view of it that the irrationalist schools of philosophy advocate: science is a mere instrument in the hands of technicians, a glorified system of engineering; cultured people may safely ignore it.

Science came to replace religion or at least a part of religion: even the very religious Galileo openly intended to exclude judgment concerning natural phenomena from the authority of the Church and transfer it to enlightened individuals. Descartes and others tended to overestimate the power of enlightenment and thus they viewed the authority of the minds of any educated intellectuals a substitute for the authority of tradition, including those of Aristotle, the Bible, and the Pope. They (rightly) debunked Aristotle. Galileo tried to say gentle words about him, but respectful criticism is not a matter of kind words but of high appraisal, and Galileo evidently considered Aristotle’s errors rather unbalanced. He deemed him rather muddled, though less so than his Renaissance disciples. Galileo and Bacon, each in his own way, contrasted the debunked views of Aristotle with the power of enlightenment to lead to the ultimate goal, to the finding of the truth about nature. The tradition of wanting science to be strong, and to maintain its position of strength, developed in the seventeenth century, during that struggle for a legitimate place for science in Christendom.

Both Bacon and Galileo fought for the intellectual independence of the researcher and against the authority of the religious leadership. They also fought for the social and economic independence of the researcher. Thinkers of the late Middle Ages were intellectually subject to the spiritual authority of the Church, and socially they belonged to the Church organizations (including most universities) or to a lay court (as physician, alchemist, or astrologer). In the days of Bacon and Galileo, the greatest scientific names were those of Copernicus, who belonged to the Church, Kepler, Harvey, and Gilbert, all courtiers, and Galileo, half academic half courtier. Prestigious amateur scientific societies like the Royal Society of London did not exist, as those that did exist were feeble and ephemeral. Bacon's aspirations as expressed in his *New Atlantis* went further: this slim, unfinished work offered the vision of a state that owed its assets to a scientific monastery that controlled though not governed it. The monastery could decide what invention to publicize, what to keep as a state secret, and what to keep secret even from the state. Real scientists cannot control even the bomb. As soon as they had it, the military decided to control them.

Bacon's scheme is alleged to have engendered the Royal Society of London. The evidence is one letter, written to Robert Boyle, proposing to found the lay monastery. Boyle accepted the proposal. The meeting took place and was the first step in the foundation of the Society. This evidence is misread. The Royal Society, contrary to the intention expressed in the letter, never was a lay monastery *à la* Bacon; it was a lay society, also *à la* Bacon. He had two visions, one inspired, grandiose, and never implemented, and one prosaic that had tremendous success: the idea of amateur, gentlemanly, experimental philosophy. His first disciple was a dilettante called Henry Wotton, sometime diplomat, sometime provost of Eton. A letter addressed to Boyle by a school-mate of his in Eton, John Beale by name, expresses an effort to enhance Boyle's sense of gratitude to Bacon and Wotton, reminding him that while in school they learned from Wotton about Bacon. Boyle's vision of the Royal Society was not of a lay monastery but of a gentlemanly, group of amateur, seemingly dilettante researchers, loosely organized in a club. He legislated the idea of science as publicly available knowledge. He also unwittingly contributed to the esoteric tendencies of the Society by his idea of implicit criticism and muted controversy. This already opens the door wide for the introduction of esotericism, since in effect it was the proposal that one researcher may criticize a fellow researcher only under the condition that the strictures would be incomprehensible to the non-initiate, to the lay public. It would be an error to criticize Boyle on this count, because his proposal may have been justifiable at the time.

In his entire writing career Boyle criticized explicitly four writers, all of them outsiders, and he did it in a debunking fashion. One of these four was a monk, Father Franciscus Linus, who defended Aristotle against Boyle's claim that a vacuum is possible. Another was Thomas Hobbes who criticized Boyle's vacuism from his own viewpoint that much resembles that of Descartes. Hobbes quarreled with some of the leading members of the Royal Society; this may explain Boyle's hostility. His critique included unpleasant and irrelevant remarks such as that he would not criticize Hobbes' political writing though they were objectionable on religious grounds. The third was an attack on Henry Stubbe, perhaps the greatest and bitterest opponent of the Royal Society, because Stubbe published without Boyle's permission an open letter to Boyle as a preface to an insignificant volume. Now, obviously all these three attacks of Boyle were justifiable in matter, though not in manner or in appraisal of the parties under attack. They were disrespectful attacks on people who did not belong to the esoteric elite. Boyle was rather appreciative of some of Stubbe's points, but he refrained from defending him publicly, and allowed the esoteric elite to publish official slurs on his person. The contributions of Fellows of the Royal Society were known then as much more significant than those of Stubbe; so there was hardly any need to debunk him and of his views. He was nevertheless right on a few important points. Historians of science still endorse the debunking of Stubbe and still have nothing to say in his favor, though they cannot answer Isaac Disraeli's defense of him; they prefer to ignore it. Incidentally, Disraeli's beautiful work, *Calamities and Quarrels in the Royal Society*, is the only discussion I know of the history of the dislike of even the best forms of criticism. Though it is a work that is well known and often referred to, it had no impact on the writing of the history of science or of its institutions.

Boyle attacked Henry More, the famous Cambridge Platonist. His explanation for this was that he had previously warned More (they were friends of sorts) that he would attack him publicly if he went on calling Descartes an atheist. This explanation is too poor: it made criticism identical with censure and also (at least when it came from eminent people) a penalty. More complained (in a letter to a friend) about Boyle's conduct that he found uncritical and immature. The episode was noticed by the greatest scholar and admirer of Boyle, John F. Fulton. But its consequence has not yet been studied, and not sufficiently corrected.

Among themselves, gentlemen often still partake in it only in muted forms, often still viewing controversy as embarrassing. Since for centuries researchers were gentlemen of leisure, muted controversy was the rule in the world of science. This fortified the confusion and the esotericism of science, while the confusion and the esotericism reinforced each other.

10. Historians of science can try to reconstruct past muted controversies.

It is usually difficult to reconstruct old scientific controversies due to their having been muted by their participants. Historians of science parade excerpts from available documents as pictures of the past. Their proper task is to use these documents in order to reconstruct the past. This is well-known to social and political historians, to historians of art and of philosophy. It has not yet been heard of in most of the circles of historians of science, partly at least because they have no desire to wash the old dirty linen in public; because, that is, they still think that their task is to be defensive.

There is ample evidence for this. One might imagine that historians of science do not report muted controversy because in their credulity they have not noticed any. But, to repeat, controversy can get too involved to be understandable muted and then it bursts out as the opposite extreme of aggressive debunking. Evidence for this is either accepted by historians of science as true and just, as is the case of the debunking phlogistonism, or else they find it embarrassing and then they ridicule or suppress it. Let me give examples.

The paradigm case is the dispute between the action-at-a-distance electrodynamics school from Ampère to Ritz and the electromagnetic field school from Ørsted to Einstein. Historians of science fail to notice it, the way a visitor in a zoo would fail to see the elephants there. The more intricate examples are easier to overlook. Laplace claimed that Young's deduction of the laws of diffraction from the wave theory of light is ingenious but not valid. Laplace said he could not make that deduction to his own satisfaction. Even Young's latest biographer jeers at Laplace, stating that the deduction is elementary. It is barely credible that a great mathematician could make a simple mathematical blunder. Admittedly, he applied an unusually severe standard when examining Young's deduction, so that his manner was unfair, simply because he was apologetic for Newton, whom he wished to rescue from all criticism, seeing it as debunking. But there is no need to take it for granted that Laplace either faked a difficulty that was not there or deceived himself into seeing one that was not there. Even if this happens to be true, it should not be taken as the default option and allowed only after proper investigation that may prove interesting and that has not been performed so far because it is known that Young was right and this leads to the excessive readiness to debunk any of Young's opponents — even Laplace, even at mathematical deduction that was his forte.

When Davy validly criticized Lavoisier, no amount of detachment and expression of respect that he showed the target of his criticism succeeded in misleading his leading French contemporaries: they knew only too well that all criticism is necessarily debunking. They therefore felt amply justified in suppressing the criticism by any means, including the threat to call in the police. The document revealing this is easily accessible to historians of science. It has been published only well over a century later — by myself. I need not say, I hope, that I published it not with the intention to debunk these people who so confused criticism and debunking that they were willing to call in the police. But I will not accept the idea that such sad incidents should be suppressed by historians of science, for the suppression of evidence is the action most alien to the spirit of science. I was censured for my having published the document in question. I dismissed this censure. My paper received two strong criticisms (Maurice Crosland, "Humphry Davy — an Alleged Case of Suppressed Publication", *Brit. J. Hist. Sci.*, 6, 1973, 304-10 and C. W. P. Mac Arthur, "Davy's Differences with Gay-Lussac and Thenard: New Light on Events in Paris and on the Transmission and Translation of Davy's Papers in 1810, *Notes and Records of the Royal Society of London*, 39, 1985, 207-228. This is more to the point; I am ready to be corrected; I confess I find it hard to do so when my critics totally overrule my evidence without explaining how come it at all exists. Is this a demand for constructive criticism, Heaven forbid? I do not know. My concern here is with the suppression of unpleasant evidence, not with taking it at its face value. Suppression is justified by the claim that historians of science should promote science, not undermine it; this justification amounts to the claim that historians of science are (self-appointed) propagandists for the cause of science. Propaganda for any successful cause, particularly for the cause of science, is questionable. There is a deeply inherent inconsistency in the idea of propaganda for science, or for the sake of the spreading of science. Propaganda is the opposite of providing readers with a scientific approach and of helping them acquire a historical perspective. When the item in question is science, a scientific approach to it become s particularly important, and substituting propaganda for it is particularly irksome: as is well known, the worship of power is a dangerous superstition, and science is nowadays very powerful. Hence propaganda for it, the advocacy of the worship of science, is an inducement of a superstition, and a dangerous one at that. Finally, since a license to make propaganda is a license to debunk opponents, real or imaginary, propaganda is rightly a form of debunking and thus historians should not criticize it although on occasion they may have to debunk it. But the practice of writing propaganda under the veil of writing history (or philosophy or popular exposition) of science, is a menace too big to be entirely ignored.

THIRD PRELIMINARY ESSAY: ON THE DESIRABLE STANDARD OF POPULAR SCIENCE

> If a science has to be supported by fraudulent means, let it perish.
>
> (Johannes Kepler)

> In the progress of the division of labour, the employment … of the great body of the people, comes to be confined to a few very simple operations … The man whose whole life is spent on performing a few simple operations … has no occasion to exert his understanding … He naturally loses, therefore, the habits of such exertions, and generally becomes as stupid and ignorant as it is possible for the human creature to become.
>
> (Adam Smith)

The historians of science who belong to the majority school and who have read the previous pages nay be angry with me; at least I would be if I were m their shoes. I intend now to try to present their reply to me as well as I can. Briefly, I suppose their reply would be that they are performing a very important and urgent task ─ that of popularizing science. My rejoinder would be that far from fighting ignorance they (unintentionally) exploit it. I shall begin with a general comment that reviewers have made against my *Towards an Historiography of Science*, including the complaint that when criticizing the standard historian's black and white pictures, I fall prey to the same fault, that sharing the faults of the standard historians of science I show that my criticism need not be taken seriously.

1. The standard historian of science hits back

Agassi states that our works are sub-standard and that we are too often prone to commend or condemn, especially condemn, so that our works is not serious. He would, nonetheless, discuss them ─ merely because we are well established as serious scholars. Without trying to defend these charges he goes on to admit that his own works open to the same charges; that almost all works are: because the standard of all academic publication is at present falling and because throughout the tradition of science people commended and condemned justly or unjustly this or that person or idea. If his charges are true, then even by his account we are in good company.

It is thus easy to dismiss all of Agassi's charges, or to attack him the way he attacks us; he admits that he is open to the same charges. But we do not want to quarrel with him, especially since he says certain things that we gladly endorse anyway, even things that we have been repeatedly saying. We would not want to adopt his unnecessarily and excessively belligerent or at least bellicose tone. So let us start by defining the area of our agreement with him. It will soon transpire that, nuance apart, (almost) nothing will remain in the area of disagreement, and this will show how misplaced is his hostility towards us.

We are indeed well established, as Agassi says, even though our discipline, as an academic discipline, is relatively new. The reason for this is that apart from being an academic discipline proper, the history of science has a very significant and broad public function to perform. Since our academic discipline is so new and narrow and its public function so broad, we must write works specifically intended for the general public. The public is quite eager to learn more and more about science and its history, and there are very few who can supply the necessary material. Agassi notes that intended to serve different publics should abide by different standards. Yet he ignores this when he discusses our works, and presents us as poor academics merely because on top of being academics we serve the general public.

We face a very difficult situation of having no definite public: we have to write mainly for the popular public and in one way or another we manage to slip in material for the specialist historian of science. True, we have our own learned societies and learned journals, but their function is chiefly to provide us with platforms for specialized studies, because their small size and experimental character, as well as the vastness of the task, allow for little room for more as yet. The sheer size of the field to be studied and of the amount of work that needs to be done in teaching and in advocating our achievements, then, place us in a very difficult situation. Trying to cover so much with so few means, and while using relatively new methods of presenting science as a part of culture (the honor for this must go chiefly to George Sarton), we must be superficial from time to time; we must coordinate our meager research facilities and resources so as to do the most urgent studies and teaching first.

So much for our general situation. Agassi mentions many problems and aspects of the field that are still unstudied. He puts this as strictures against us. Inasmuch as they are justifiable, these strictures are expressions of impatience, very understandable in view of the great deal that is still left to be done, but extremely unfair to blame us for it, especially as long as we both work very hard and constantly clamor for more help and do our best to recruit it. To a large extent his strictures are plainly beside the point, however, because he is mistaken about our aims and intended public: he treats our popular works as if they were intended for professional historians of science, and he totally ignores our works that are intended specifically for the professional publics because, trying to debunk us, he chooses to discuss our professionally inferior material — professionally inferior largely because it is not intended for the professional — and he ignores, or is perhaps ignorant of, the best material published in our professional journals and books.

Agassi complains that we publish in order to advance our careers; that we succumb to publication-pressure. This is neither censure nor the truth. He observes that pressure to publish is put on academics in all fields; that the reasonable thing to do when yielding to such pressure is to write surveys of developments in various fields of studies: his heart is exactly in the kind of work we are doing. So he ought to know that our field was first developed just before publication pressure became embracing, so that many of our pioneering works were published before the pressure was inescapable. Thus, this allegation of Agassi dissolves upon first scrutiny, and can be ascribed to nothing else but his youthful exuberance. We may excuse his getting excited because all academics dislike publication-pressure, especially historians of science like ourselves, who need much freedom and leisure in order to study the vast amounts of material scattered in large archives and innumerable private collections — many of them almost untouched treasures — and in order to process the accumulated *data* in peace. So we forgive him his first allegation and move on.

The only other concrete allegation against us is that we do not discuss the history of academic standards, that we take certain standards for granted, and that we are therefore scornful — implicitly or explicitly, he says — of those who did not appreciate people like Young or Helmholtz at first glance. Yes; the history of academic standards is worth studying and there is a vast amount of material concerning it that still demands a lot of work; such a history could help researchers and others develop a proper perspective and would increase their appreciation and willingness to make efforts to cherish and preserve our current standards. Agassi is not the first to observe this. Sarton made this point very eloquently. Agassi refers to a couple of excellent works on it, and though he does not refer to others, they do exist; we agree with him that little has been done; his failure to appreciate what has been done and his allegation that we should have done more is as unjust as his previous one. As the history of publication standards has regrettably not yet been fully studied, there is little that a historian of science can do in the meanwhile but state in passing that regrettably at a certain date important scientific material found no appreciative editor. This is what Agassi calls implicit scorn; but he neither suggests replacing this by implicit approval, nor does he tell us how to say explicitly more on the subject that is still unstudied while we are so few and all of us are busy with one urgent project or another. At best Agassi's stricture here may be taken as a plea to someone more able than he is, and more ready to do hard work and study this interesting field. To this, we assure him, we have no objection whatever. [Kuhn, let me report, said this in almost the same words; he also repeatedly said that rather than complain about historians of science I should have joined their ranks.]

There is publicly well-known evidence that suffices, perhaps with the addition of a pinch of commonsense, to refute Agassi. His chief complaint is that we present science as all white and debunk everyone who does not conform to our ideals of science. His attack on the black-and-white picture of the world is enthusiastic; unfortunately it has kept him too busy to notice that he has presented us as all-black. Of course, he will respond in self-defense saying that his work is merely a superficial introduction, that he invites historians to sort out things more carefully, to distinguish the various shades of gray, and even say a kind word about one or two of us on some rare occasion. Precisely the same kind of defense holds for some of our introductions to the history of science that, on account of their unavoidable superficiality, possibly look more black-and-white than we would like them to look; and if he is permitted superficiality, surely we are also allowed it in our introductions to something somewhat more vast and significant, such as the history of science. If he cared to look not merely at an introduction to the history of science, but also at a history of science proper, such as Thorndike' monumental eight volumes on the history of science and magic, he would find that the author has carefully sorted out there the brighter and the darker sides of the work of researchers like Paracelsus or Kepler, like Boyle or Newton.

Agassi complains that the history of criticism has not yet been studied; this is again an expression of understandable youthful impatience, unjustly leveled against us. He complains that we do not analyze — that we suppress, to use his words — certain errors of the great researchers of the past, and so on. All these allegations are answerable on the same lines: some of his complaints rest on his ignorance (which we do not blame him for, as the literature is vast; but we do blame him for his rash baseless accusations); some of them are just expressions of impatience unjustly worded as complaints. [This is the verdict of the friendly review of Charles Gillispie.] Moreover, unlike Agassi, we find little use in analyzing Newton's errors, especially when writing for a general public: the general public must be informed about Newton's great contributions before any detail can be discussed critically, let alone the analysis of his errors of detail. Moreover, it is much more intriguing to analyze great achievements than small failures. (Agassi will agree, we hope, that the errors of the great Newton were of detail, that by and large he was right. Otherwise he is not the friend of science that he claims to be.) Our learned journals are full of detailed analysis and in our more scholarly works, and we invite him to read and enjoy them.

2. The standard historian of science as a popularizer of science

Consider the role of history of science as popularization in a culture split into small, isolated groups of specialized experts, roughly classed as two larger groups, christened by C. P. Snow "the two cultures". Specialization as such is ancient; in its present form it poses new problems.

The most telling ancient comment on specialization is possibly in *The Apology of Socrates* by Plato. It is a record of speech that Socrates made in his trial in the Athenian court, a speech against his accusers that the law of Athens allowed the accused to make. This self-defense begins with the declaration that the accusation was chimerical and rested on the resentment that many citizens felt towards him. The source of this resentment, he added was this. The Oracle pronounced him the wisest. He then was determined to refute the Oracle by finding someone wiser than himself. He studied leading politicians and found them stupider than common citizens. He then studied poets and dramatists and found them inspired but not wise: even their publics understood their creations better than they did. He then studied experts. On the matters of their expertise he found them much wiser than himself; he found them stupid otherwise, as they considered themselves universal experts. The stupidity of experts outweighed their wisdom. Having discovered this, Socrates could regularly beat any expert in a debate, thereby giving the impression that he was a still better expert, and thus the best universal expert; in truth, he explained, his wisdom lay in his knowledge of his own ignorance. It was this erroneous reputation of being a universal expert, Socrates concluded, that had created unjust resentment against him.

In this respect we are better off today. Had Socrates lived today, he would not be sentenced to death but ignored or ridiculed at worst. Experts are not as bad today as his contemporaries were. Today experts know their limitations and speak only on matters within their fields of accomplishments. Great as these advantages are, two great disadvantages outweigh them. According to the record, ancient Greek experts knew about other fields of human activity much less than they thought they did, but at least they took interest in them; modern experts, knowing their limitations, gave up almost all outside interest; ancient Greek experts did not over-estimate their expert knowledge as much as the modern ones. Scarcely have there existed a society of such incredibly learned, dexterous, and clever experts in the various natural sciences as the modern ones. Yet, the cost of this specialization is a lack of perspective and of general knowledge. Experts may over-estimate their own field of study, and their own expertise, and live in small and closed intellectual environments. This is the complaint launched in Snow's *The Two Cultures*. He said, the London art circle has more of a common language with the New York art circle than with the circle of British nuclear physics that, in its turn, is closer to the circle of American nuclear physics than to circles of British art. Obviously, this is a disadvantage to experts who are closed in their own shells as well as to the culture at large. Snow rightly says that the beauty of the second law of thermodynamics is great enough to justify a general interest in it, so that its being limited to experts is a general loss, just as is the general ignorance of the novels of Dickens also left to experts.

Historians of science can — and do — make contributions here. When telling readers interested in biography about the stormy life of Galileo, the varied life of Franklin, the adventurous life of Count Rumford, the struggling life of Faraday, or the noble life of Madame Curie, they serve by building tiny bridges, or tiny parts of a bridge, between the two cultures, by informing readers, *en passant*, of these people's scientific accomplishments. They do the same when they narrate the development of Renaissance science as an integral part of Renaissance culture. And they do the same as they describe the details of the development of classical chemistry that many lay people know and understand, or can learn with ease, as it elucidates for them the proper processes of scientific development and provides them with a sense of historical perspective, thus preparing them for further historical studies that increase their stock of knowledge of both science and its history.

All this is rather obvious, and would hardly need mention but for the sad fact that the history of science has not attained the degree of recognition it deserves in this our modern world. This explains why popular introductions to the history of science, contrary to Agassi's proposal, ought to be simple — superficial, if you will — and mainly expository. An exposition of the history of science easily accessible to lay readers is no mean achievement, because in the immense complexity of natural phenomena fell into pattern only very slowly, and during arduous and sometimes tortuous processes; it therefore takes a great amount of judgment before one can write an exposition that has simplicity without oversimplification, yet this is essential for helping lay readers, enabling them to get a glimpse of the complexity of the scientific work as it took place in history. This kind of historical study is of necessity a survey of the various sciences relative to their socio-cultural backgrounds — because the purpose behind it all is integration. It may lead to the wrong charge that was leveled against Socrates, namely that we are self-appointed universal experts. Our historical introductions to science must rest on more specialized surveys performed by specialists, but with the aim of integration. Our introductions (as Sarton has observed), though somewhat superficial, give readers a taste of the spirit or the outline of important work done in the various specialized fields over the ages. Our general introductions to the history of science are sometimes more superficial than we would like them to be, and often they contain serious s *lacunae* and even errors. This is why we publish more and more improved and up-to-date general introductions, in the hope of improving the efficacy of our dissemination of major advances of the specialized fields, past and present. This effort Agassi pokes fun at, calling it up-to-date-science-textbook-worship. Does he want us to stay behind?

3. The encyclopedic approach of standard historians of science is improper.

So much for the attack on myself. The gist of it is that my claims so far rest on my ignorance, that they are requests for desirable improvements and studies put in an exaggerated form, and that I forget that much of the work that I chose to criticize is directed to the general public. In response to this I should explain my disdain of most of the literature in the field: most of what I criticized long ago (it is still going strong), is of no use to the general public because the received methods of writing popular works, popular science and popular histories of science alike, is faulty.

All the undesirable aspects of this literature are immediate corollaries to the fundamental idea of the (Baconian) tradition that most historians of science embrace, much more so half a century ago than now. It is the idea that the task is vast because science rests on vast collections of scientific *data* (and the history of: science on vast collections of historical *data*). To take one example, this idea is behind the popular conviction that Newton (the researcher, as distinct from Newton the superstitious) was essentially right and his errors a mere matter of detail. Contrary to it, with all due respect and admiration, we should know that Newton's theories are mistaken: his ideas are superseded. (The notable exceptions are the identity of inertial and heavy mass, the equality of action and reaction, the law of inertia in empty space, and so on.) The reason for the reluctance to admit this is the disdain of all errors, which is a serious error indeed. Most historians of science explain their conviction that Newton was essentially right by the hypothesis that his theory rests on vast collections of *data*; they themselves state, however, that Newton had at his disposal nothing more than Kepler's and Galileo's theories, plus an observations concerning the moon and the tides. This seeming inconsistency they try to resolve by pointing out that Kepler and Galileo, in their turn, had based their theories on vast collections of *data*. They have to admit, however, that Galileo had no store of *data* except rather common or garden observations with the exception of one, rather problematic experiment concerning inclined planes and that Kepler's *data* were, at most, a collection made by one single individual — Tycho Brahe. Strangely, the majority of historians of science, who spend their lives in the frantic search for more and more *data*, ignore one very important historical *datum*, namely the fact that certain astronomical *data* were available to Newton, that Newton declared rather crucial to his theory of universal gravity, and this he declares in his *magnum opus*, no less (the end of Book II of his *Principia*): it is a set of astronomical *data*, compiled in his own days, that deviate from Kepler's laws. It is now clear why the majority of historians of science ignore these *data* or at least their significance: they must stick to the theory that Kepler's theory was right even at the cost of throwing certain facts overboard or at least ignoring their relation to Kepler's theory. In this fashion they also miss one of the chief urgent problems that Newton tried to solve, namely, how to explain these newly found deviations from Kepler's ellipses. The greatest Newton scholars, Alexandre Koyré and I. Bernard Cohen, were of a different ilk, yet most historians and philosophers of science still claim that Newton based his theory on those of Galileo and Kepler, and even that he logically deduced it this way. This story is absurd, but it has a tremendous appeal to true believers in Bacon's theory of induction.

An interesting new variant appears in 1962 and was subject to a wide controversy that engaged historians and philosophers of science. It was the idea of Kuhn, a historian of science turned philosopher and sociologist of science. He rightly disliked the dismissal of Aristotle's physics on obviously anachronistic grounds. He therefore decided to rescue it from the neglect that it suffered ever since Galileo refuted it. But he stuck to the Baconian idea that falsehood is to be ignored. So he ignored Galileo's refutation of Aristotle as did Pierre Duhem and Henri Poincaré before him. Did Kuhn agree with their instrumentalist philosophy? This question was discussed endlessly.

Nobody knows the true answer. Finally what happens is what usually happens when ambiguities come to fore and stay: it bored discussants and they moved on. Kuhn's views were immensely popular, especially among historians of science, but it is now on the decline for now. This is no progress.

Historians of science who take science to rest on vast quantities of (empirical) *data*, and who consequently stuff their works with vast quantities of (historical) *data*, view science as always right or at the very least nearly always nearly right because it rests on many empirical *data*. They therefore commend people who discovered *data* and people whose have the consent of researchers today, and when they encounter different views, they choose arbitrarily between a few alternatives. They ignore the discrepancy between the older and the newer views; or they commend the one as a forerunner to the other (Kepler as a forerunner of Newton, or Newton as a forerunner of Einstein); or they denounce it as superstitious. There is no criterion by which to judge how big a discrepancy should be before it should be impermissible to ignore it, and there is no criterion by which to judge similarity, and thus to judge one idea a forerunner of the other. Consequently, any idea can be classed by our (Baconian) historian, according to tradition or according to personal taste to be true, approximate (a forerunner), or superstitious. (Ørsted judged phlogistonism approximate, most historians judge it superstitious.)

The weakness of Baconian historiography is thus its arbitrariness: when Baconian historians want to commend an idea, they show the resemblance between it and one that has gained respectability; if they want to condemn it, they point out bluntly the errors that follow from it. Paracelsus had his three principles, of sculpture, mercury, and salt, at which ample fun has been poked; yet within the accepted techniques of these historians one can praise these principles as the forerunners of the theory, still taught as gospel-true in all secondary-school chemistry textbooks, of the composition of acids and bases into salts.

Baconian histories of science are stuffed with historical *data*, and therefore, on the conviction of their authors, they should count as scientific. But the amount of arbitrariness that they tolerate obviously qualifies them as pseudo-scientific. Often judgments diverge greatly, but critical discussions between their advocates are wanting. Henry Guerlac told me he had criticized Douglas McKie (they were both leading Lavoisier experts) thereby acquiring his hostility. I looked up his review and found none of it.

Baconian writers agree about one thing: they praise unconditionally all discoveries of new facts. This makes discussion of the discovery of facts completely superfluous. All that remains for the historian of science to do is to mention the date of that discovery and perhaps add incidental information about it.

This is regrettable, since not all discoveries are equally important. Historians are unable to record all facts; they select discoveries to record. How? What makes a discovery important? My example is the discovery of Ørsted, whose significance is hailed in all histories of science and explained in none. I tried to explain why it was an important and difficult breakthrough. I also discussed the relation between that great discovery and one that followed it within a few weeks — Ampère's discovery: surprising as it may sound, historians of science do not explain the relevance of the one discovery to its immediate successor. Histories of science often consist of presentations of heaps of details with hardly any discussion of problems; consequently they are boring and unreadable. The public has learned to accept this boredom as a matter of course and consequently barely notices the few exciting exceptions.

Here I bump again into the insoluble problem that every writer of a critical survey has to face. There are too many examples to choose from, so that citing any one is too arbitrary, and citing none is unacceptable, since readers may want to see at least one example. I have chosen a most recent work that is charming in its way, since its author does not bombard readers with innumerable *data*. Although charming, it is still unreadable, because its author is defensive. Here is my example, then. It is Bernard d'Espanat, *On Physics and Philosophy*, 2006, first chapter, an overview. Speaking of the physics of Descartes, of Pascal, and of Newton as students of matter and motion, the author comments: "Were they right? Yes, of course, in a sense. Pioneers they were, and, as such, their most urgent task was to explore the ins and outs of such a natural idea. Moreover, the idea in question proved spectacularly successful. Still today …" Isn't it simpler to say, no, their theories were superseded long ago, and by more powerful studies, but …

Having to handle false scientific theories is one major difficulty of (Baconian) historians of science. The other is that their task of is vast, and so they agree that very much remains to be done. My complaint, however, is not that the task is as yet far from completion but that much of it is useless: most of the existing studies are dull, and increasing their numbers will lead to no improvement. They rest on ideas that prevent interesting studies: those interesting studies that are carried out by the few good historians of science largely in protest against the theory of the vastness of the task. This theory leads to ever increasing compilations of historical *data*. Attempts to explain the significance of one fact as compared with others are poor but already much to be preferred to vast collections: such a task sometimes proves useful and interesting. It can be completed in a short time and its usefulness judged. (See Robert Kargon's amusing Review of the life of Boyle by R. E. W. Maddison, *Isis*, 62, 1971, 258-259.)

This counter-attack of mine is launched against most professional and most popular works, against Thorndike's professional eight volumes as well as against Dampier-Whetham's popular one volume, both terribly celebrated. The difference between professional and popular works is general, and its use in the history of science should not be more of an excuse than generally. It is time to put a stop to the phony idea of a bridge between two cultures; it is an idea that survives on the strength of our bad conscience concerning specialization that makes us tolerate poor performances.

4. The view of the history of science as a bridge is bogus.

The educated lay public comprises intelligent but rather ignorant readers who are at least mildly interested. The simplest staple diet for them is an elementary standard textbook with a large portion devoted to interesting highlights, and a small portion devoted to essential technicalities, abbreviated and patiently worked out for them in detail with many amusing examples. Such introductory textbooks in almost any topic of general interest are at times excellent. More often they are unsatisfactory or out-of-date because more people neglect writing them in preference of popular-science and histories of science. This is a pity. Also, the writing of a popular introduction to an exact science may present great technical difficulties, rooted in the inability of the educated public to read mathematics. This problem, however, is not much different from the one that the writing of a popular introduction to, say, music presents due to the inability of the educated public to read scores, or to recognize chords and the sound of instruments. There is no general formula for how to handle each of these problems, but the *desiderata* are clear: the writer has to try to convey the most information with the least technicality and to introduce the technicality gradually, without causing indigestion or boredom.

If the educated lay public is not interested at all in a given field, it is safest for writers in that field to leave them alone; mildly interested prospective readers present enough challenge, as the best introductory works in each field repeatedly illustrate. Come to think of it, totally uninterested publics hardly exist: it is easier to find hostile publics. For example, it is practically impossible not to interest people in mathematics unless they are hostile to it. The same holds for political history. In both cases the hostility stems from first-hand experiences in schools, as there can be no natural hostility to such artifacts. To catch the interest of people who are hostile to some studies on the basis of firsthand experience, it is necessary to start with an honest discussion of that hostility. Probably the first to publish a competent, savage attack on current methods of raising hatred for mathematics or for history will be quite an achievement.

As to bridge literature, interested people tend to read introductions to the sciences that interest them — preferably rather light or popular introductions. Successful popular writers leave hostile prospective readers alone or give them empty sermons as (ineffective) antidotes to hatred. Popular literature on science and on its history may be meant to raise the interest of uninterested people. It is intended for lay readers who care about one intellectual field but do not know or care enough about another, and their interest in the one field can be used in order to increase their interest in the other. For instance, one may be interested in the life of a researcher, and a proper biography may whet one's appetite for science. Unfortunately, such prospective publics hardly exist. In principle they may, but they are scarce. People who spend much time listening to popular music and are deaf to classical music may be trained to develop their tastes by listening to bridge-music, and records of bridge-music of diverse sorts are on sale. The same goes for all high arts. There is no reliable estimate of the success of bridges. Few people develop good tastes past adolescence, and these few are seldom connected with bridges. As to adolescents, they develop their taste by simultaneous exposure to diverse means of improving their tastes. The appraisal of the success of bridges is not very important, since it is anyway harmless. And some bridge items are valuable in their own way.

Can we compare the history of science to other bridges? Successful bridges are chiefly used by young people and bridges to science for them obviously exist and may be histories. My own earliest knowledge of trigonometry was derived from Jules Verne's *Mysterious Island*. I had no scruple about skipping the trigonometry in that book, but the author cleverly prevented me from doing so. I do not know how much my love of mathematics is related to such incidents, but surely it would not have an effect other than positive. I suppose I am not the only one who has gained something from a bridge-work intended for the young, but I have not met or heard of any adult who has so benefited. Even historians who are interested in cultural history and who have therefore tried to read terrific works that include chapters on science (like those of Sir Leslie Stephen, Basil Wiley, R. F. Jones, and Paul Hazard) have not thereby been allured to science or even to its history: usually they try to understand the significance of science to culture in a given period while absorbing hardly any science proper.

Unreadable histories of science are still on sale. They are bought, I assume, to be read (usually not by the buyers themselves). Even researchers, historians or historians of science, can barely read them. They may, at most, use them for occasional reference. Otherwise, the success of sales of popular histories of science should by now show some effect: after all, the history of the writing of the history of science in order to bridge the gap between the two cultures is possibly even older than the two cultures themselves.

5. The alleged bridge cannot prevent the decline of the popularity of science.

Historians of science often promulgate the idea that they find in old books or in histories of science written by others: once upon a time darkness prevailed; in the Renaissance a few brave individuals introduced a little light, but had a small following; the age of the Enlightenment then followed. Since then and until today increasing numbers of people are enlightened but still many either ignore science or are ignorant of it. This story presents popular science writers, including historians of science, as the throw the first beams of light, however weak, into dark corners, and thus successfully push the boundary between the light and the dark in the right direction.

This is a myth. The success of popular science, including popular histories of science and other bridges, is great when it is on the level *of Reader's Digest* and the Sunday paper, but hardly on the paper-back level. The story of the spread of enlightenment is not of progress from superstition to science.

We are much more enlightened than our great-grandparents in the sense that illiteracy was normal everywhere but a couple of centuries ago and is rare now in many countries. We are more enlightened also in that the use of literacy in daily work is common. Yet in another sense we are also behind our great-grandparents: our intellectual elite is bigger, relatively and absolutely, than theirs, yet its uncultured portion is definitely larger and on the increase. We are training skilled-workers (including research-workers) instead of educating cultured individuals.

This comparison between us and our great-grandparents shows clearly that the recent changes are chiefly due to experiments in educational reforms. The main interest of C. P. Snow was to promote more educational reforms. This has little to do with bridge-subjects, the history of science or any other, except insofar as these may play some role in our reformed *curricula*. It is an ironic situation, perhaps, that the history of science has been introduced as a bridge-subject concurrently with educational reforms; this coincidence explains why the reform has failed — even in Harvard: it had no chance of competing with specialization. The old successful reforms dealt with much more urgent problems than of building bridges.

Bridge-builders appeared on the stage a century ago. The people for whom the bridge was intended were not ignorant workers but educated people who were indifferent and even hostile to science. This led to a certain condescending attitude on the part of bridge-builders towards their public, and as a consequence they met with little success. Not having asked since that time what publics they address, these bridge-builders have not yet come across the idea that perhaps they should change their attitude.

In the seventeenth century researchers were amateur aristocrats; *Curiosi*, as they styled themselves. They helped the advancement of learning and the improvement of the lot of humanity. In the eighteenth century they were still amateurs and universal dabblers of sorts, but they were then largely middle-class, especially on the Continent. They displayed a little greater zeal in their quest for knowledge: they developed an ideal of self-education that partly supplemented partly replaced the older idea of the *Curiosi*. This ideal of self-education was rather new; it came together with the aspiration for social justice rooted in the idea of science as self-improvement that was also rather new. It led to attempts to disseminate the idea of self-education to the lower classes.

In England, the ideologist of the seventeenth century *Curiosi* was Robert Boyle, the aristocratic author of *Seraphick Love* (that advocates research as the intellectual love of God, to use Spinoza's expression); the ideologist of eighteenth century self-education was Dr. Isaac Watts, the puritanical author of *The Improvement the Mind*; the ideologist of the self-made man of the nineteenth century was Sam Smiles, the energetic author of *Self Help*, the low-class edition of Watts' work. They all resemble Dale Carnegie's *How to Win Friends and Influence People*, yet are much superior to it in many ways.

The rise of working-class intellectuals, including such giants as Michael Faraday and George Jacob Holyoake, scared the British aristocrats off science and rendered them devotees of the arts .instead — especially in view of the following few developments.

The rise of professional science and technology rapidly followed the foundation of the *Ecole Polytechnique* and other technical universities and the Franco-Prussian rivalry. C. P. Snow describes (*The Two Masters*: Appendix) evidence for the theory that (though the industrial revolution first took place in Britain) the Continental technological growth made nineteenth-century Britain almost an under-developed country. Professional science developed in Britain too; but its practitioners led a very hard life, depending on odd jobs and on public lecturing — that remained popular for a while as a survival from an earlier days. With the wane of this fashion, a new public for popular lectures appeared: working-class publics. (These were initially the target publics of Count Rumford and of Hans Christian Ørsted.) Thus, with science spreading as a profession, often rather shabby; it made aristocratic and even middle-class researchers feel a little awkward. In England they tried to create a kind of class-barrier for entry into the Royal Society of London. Charles Babbage, Cambridge educated yet rather low-class, fought this with a special zeal (*The Decline of Science in England*, 1830) and succeeded in transforming the Society of gentlemen into the society of experts that we know today. The gentlemen who preferred to appear as rather dilettantish (even when, as in the case of Boyle, they were top rank experts) gave way to professionals who preferred to appear as experts (even when they were ignorant). The attempt to develop expert scientific traditions appeared in England as the foundation of the British Association for the Advancement of Science ("The British Ass") that was divided into specialized sections (and behind which stood the same Charles Babbage). A multitude of Royal Societies and Institutes for the various specialized subjects appeared in the nineteenth century.

Other factors have also contributed to the decline and fall of the society of amateur researchers. As utilitarian philosophers identified science with technology, their view discouraged research. The distasteful quarrel between the two low-class inventors, Sir Humphry Davy and George Stephenson, concerning priority over the discovery of safety lamps (they fought for honor, not for a patent), may be yet another such factor. Another contributing factor was the desire to render Oxbridge decent centers of learning (perhaps in competition with Continental ones), that led to the introduction of specialized education in these places (beginning with the abolition in Oxford in 1839 of mathematics as a compulsory subject for all students in the hope of raising the standard of the teaching of mathematics there). The debate raged for decades and the last echo of it, perhaps, is the ultra-Tory *England as It Is* of W. E. Johnston of 1851. He advocated (to his gentleman readers) the desertion of natural science in favor of the new field of moral (= social) science, including economics. The debate points at another factor: the development of the arts as a more gentlemanly pursuit than the sciences. The split between the two cultures can be roughly dated to that period as far as the upper classes were concerned, though some members of the upper classes, notably Darwin and Humboldt, kept old-style allegiance to science.

On the Continent the two cultures were split on an ideological basis: the Romantic reaction to the French Revolution was a reaction to the Enlightenment that spread an atmosphere of contempt for the piddling natural sciences and boosted historical studies of sorts. Some advocates of science often used the Romantic school as an excuse for accelerating the process of the specialization in the sciences — at times in the name of national prestige in agreement with the Romantic spirit that is alien to the spirit of science.

The idea of self-help, now taken up zealously working class social and political organizations, kept alive universal education (chiefly for adults) until World War I. Snow describes his own grandfather as a member of this movement: a worker who got his education in adult socialist circles. Working-class scientists began to emerge, and some of them even as leaders; but on the whole the working class made its real debut only after the War, and even then with a struggle. Snow's autobiographical novels *Strangers and Brothers*, relate his own struggle from a small-town low-class *milieu* to Cambridge *via* the socialist self-education movement. The ease with which nowadays working-class youngsters can arrive at the best seats of learning with medium ability and effort sharply contrasts with the conditions of but a few generations ago. (Until World War II, entry of Jews to universities was limited everywhere; this impeded the improvement of academic education in general more than the education of Jews.)

The amateurs' abdication of research constituted a danger to scientific progress: some amateurs of relatively higher classes went on, and some other researcher, usually professionals, joined in; but a great gap developed. It was fortunately filled in by the academics, who won increasingly prominent place in research. They performed the task for love — there still was no publication pressure — and they harnessed themselves to research as well as to popular science. They developed a new tradition of popular lectures, pamphlets, books, and even periodicals, in popular science and the history of science — directed chiefly to the working class. Each of these traditions, developed in that period of the second half of the nineteenth century, still claims nowadays the status of a bridge. For nearly one century these two traditions gained force, yet the bridge is not in sight as yet: the gulf is widening at an ever increasing rate. This is so not because of lack of enthusiasm but chiefly due to the wonderful emancipation of the masses through lovely vocational education and the inadequacy of the means for bridge-building.

The trend towards vocational education was accentuated by a further trend: overnight academics became leaders of scientific research and its chief carriers as professionals — almost to the extent to which they are today. The beginning of this trend is the foundation of the *Ecole Polytechnique* and of later rival institutions in Germany and in the United States of America. But the idea of vocational education came a little later: the early members of the *Ecole* respected eighteenth-century traditions of amateur research; the advocacy of specialized skills became popular after the war, when the view of it as vital (Johann Heinrich Pestalozzi) was accepted and taken for granted with no opposition. The idea that universities are chiefly institutions of vocational training prevailed with no one ever having sponsored it: it looked so natural. This forced the members of the Faculty of Arts into a retreat; the Two Cultures became institutionalized and the science wars were on. The semblance of amateur science vanished.

There was no popular science of the eighteenth century: then it was identified with popular science. Isaac Disraeli describes with a rare sense of fun and human understanding the disposition of eighteenth-century philosophers to pretend to be more dilettante than they were. This was the case with the non-academics as well as with the academics who happened to be researchers, such as Joseph Black and Adam Smith, whose works barely differed from those of Priestley the priest, Hume the diplomat, or Lavoisier the aristocrat. Laplace and Davy inaugurated the age of professional science, yet they tried to qualify as amateurs. Davy began one of his celebrated series of lectures with a revolutionary deviation from custom: he would not describe an electric pile, he said, as his intended public knew what it was. Laplace went much further. He wrote two books, on astronomy and on probability, and he wrote each of them twice: once technically and once popularly — or philosophically, to use the language of the day. Isaac Todhunter, the great Cambridge scholar of the second half of the nineteenth century, had already lost all touch with the world of the amateur philosopher. He wrote a biography of William Whewell, one of the last universal scholars, whom he greatly admired, and in it he records Whewell's failure in his last years to keep his knowledge of the progress made in the various specialized fields of human endeavor up-to-date. Todhunter also compared in his history of the theories of probability two versions of Laplace of one idea — a mathematical version in his analytic book and an ordinary-language version in his philosophical book — and he claimed that he found the mathematical version unproblematic and the ordinary-language version incomprehensible. This is a symptom: first a gulf existed yet authors like Laplace constantly tried hard to bridge it or at least to underestimate or even conceal it; later the gulf widened and authors like Todhunter gave up hope, and delegated the job to others. The job was not merely a matter of culture; authors ceased to explain items unknown to the general public but well-known to peers; the literature became increasingly compressed and technical and closed to inexpert. The job of translation was thus created. It was taken over by two classes of people — popular lecturers and writers on science and on its history. Sir Oliver Lodge starts his lecture-course on the history of astronomy of the eighteen eighties by a censorious reference to the student of the arts who has no interest in science. His work belongs to the new tradition of writing histories of science with the aim of presenting easy versions of up-to-date science to people with some historical interests. The experiment (admirable, of course) is now old enough to demand that we pronounce it a failure and seek a better tool.

The chief reason for the failure is that this literature talks down to its readers, though under the sugarcoating of an appeal to them as showing historical and scientific interest: it assumed that these readers are discriminating only about history, not about science. In a sense, popular science writers are more captivating than historians of science in their frankness about their talking down to their public, perhaps because they write for genuinely interested, frankly ignorant readers, mainly working-class. Perhaps also Faraday's influence has something to do with the matter. Of all the masses of the charming nineteenth-century popular science, none has weathered the crisis of science except Faraday's two masterpieces delivered to children in the Royal Institution, perhaps also some of Maxwell's lectures there, and a few popular lectures of T. H. Huxley and those of Helmholtz and Boltzmann. (The study of the history of popular science — as science rather than as applied art or as period pieces — is a very recent and most welcome addition to the literature on the history of science.) Faraday designed his lectures with immense care and devotion, partly because he was painfully childless, partly because he was an ostracized thinker. Yet there is something rather disquieting about them: they present some of his most revolutionary ideas on nature as if these were obvious and manifest in thousands of everyday phenomena. His success was tremendous: one generation did not know of his ideas and the next generation had sucked them with their mothers' milk, never having learnt that they were revolutionary. Thus, as a revolutionary thinker Faraday has only recently been discovered, and the disposition still is to tone down the revolutionary character of his ideas by declaring them an elaboration of those of Roger Joseph Boscovitch, and in disregard for the revolutionary character of his ideas (in the eighteenth century, not in the nineteenth). Faraday unintentionally created a tradition, one that is yet to receive notice, a tradition that rests on the idea that popular science should illustrate difficult ideas in obvious examples rather than explain them in detail. That tradition hardly refers to the problems that they came to solve. Perhaps the doyen of popular-science writers still is Arthur Stanley Eddington. His greatness is at least partly in his having developed some very interesting philosophical ideas, not only scientific ones. Although he hardly ever presented scientific problems, at least he presented some philosophical ones. Yet his condescension often makes him almost unbearable.

Condescension is unjust and harmful. It is unjust because we all want to know more about more but have little leisure and often spend it on bad popularizations with frustrating results. It is useless because popular writers depend on the good will of their readers. Snow, who presented himself as a person with a foot in each camp and as an impartial critic, suffered from this traditional bias, with detrimental effects to the cause he was advocating. He did jeer occasionally at the science-culture whose members often have no use for books, but he said they often love music, photography (he declared it a natural bridge-subject), etc. He was harsher to the arts-culture for its hostility towards professional science; he called them reactionaries and natural Luddites. This is untrue, unjust, and unwise. Members of the arts-culture can help a science-culture develop their tastes for art easier than members of the science-culture can reciprocate. Members of the arts-culture consequently feel frustrated. There are successful bridges towards the arts but none towards science. There are many good expositions of the various arts, and few of the various sciences. Members of the arts-culture are puzzled and frustrated. They naturally despair of learning about science. Snow says that this regrettable condition is alterable only by a large-scale reform of the education system. Rather than discuss this rationally he proposed that the Soviet system is a model to emulate. It is hardly surprising that the discussion that this should have raised was aborted.

The general failure of histories and of popularizations of science is due to the tremendous difficulty of the task and to the pretence: their authors appeal to the respect for knowledge, to the appreciation in the abstract of the need for the bridge, and to the sense of inadequacy. Some histories and popularizations of science are interesting, and even very much so; they would be more successful if publics were more discriminating, if they were more critical of boring works and purchased only interesting ones. The ability to discriminate does not come easy. The lack of discrimination of untrained guilt-ridden readers of histories of science and popularizations of science is very understandable and very regrettable.

6. Biographies of scientists should link the personal and scientific.

There is perhaps no better ground for the meeting of the two cultures, no better bridge between them, than biographies of researchers; and for obvious reasons. And yet biographies of researchers are, with a few exceptions, drab and boring for an almost inevitable reason. That the reason is almost inevitable can be seen at a glance by contrasting Franklin's autobiography or Flora Masson's life of Boyle, whose hero hardly appears as a researcher, with Einstein's or Planck's scientific autobiographies, whose author has no kin and scarcely a friend. Biography is, perhaps, essentially private; and contributions to science are essentially public. Even the motives leading to scientific research seem to be purely private. One may have worked in one's laboratory or at one's desk from one morning to the next in an effort to find the secret of the universe or in an attempt to forget a disappointed love; if the difference between these makes a mark on one's result, then that result is not quite objective, and thus, most commentators regrettably agree, these results are unscientific; science, they claim, is objective, so that the sublimation into research of disappointed love (Boyle) or of happy love (Einstein), being definitely private, is irrelevant. Take the two lives written by L. T. More, one of the calm, placid, and worldly Boyle, one of the neurotic, tense, and rather class-conscious Newton, and change the scientific parts of the two. Is there any reason in either book to prevent Boyle from having written Newton's *Principia* and Newton *The Sceptical Chymist*? There are intellectual differences, to be sure; Boyle, for instance, had almost no mathematics, whereas Newton was unquestionably one of the greatest mathematicians of all time. How is this part of their intellectual makeup link with their social makeup? Each of them had a scientific personality and a social personality, and their matching seems a merely given fact; if you wedded Boyle's social personality with Newton's scientific one, and *vice versa*, inexpert readers of More's two biographies would hardly notice the difference. This creates distaste for biographies of researchers as they are written at present.

This is not to imply that all existing biographical studies of researchers are boring: on the contrary, some of them are genuinely thrilling. Until recently Newton's social personality was only slightly criticized — by Augustus DeMorgan — and even that criticism did not take root immediately; yet since then more has been discovered. Until recently, one of the riddles of the history of science concerned an early eighteenth century person called Stephen Gray, who made superb contributions to our knowledge of electricity, and of whom practically nothing was known. Robert A. Chipman reports (*Isis*, 49, 1958, 414-53) that he had been quite well known, and a friend of a person whom Newton disliked; that upon Newton's assumption of the presidency of the Royal Society Gray was cut off from the scientific world, to reappear only after Newton's death. This is surely a breathtaking story, and considerable achievement. But, to return to my point, the story would have been the same had Newton been the author of different scientific work or Gray a biologist rather than a physicist.

This seems almost inevitable. The question remains, can one write a biography of a researcher that will have in it a bridge between the personal biography and the story of that person's contribution to science? Perhaps the question has never been discussed because the answer to it is obvious: as science is absolutely objective, the difficulty I describe is entirely insurmountable. And perhaps the question has not yet been asked because lip-service must be paid to writers such as the one who managed to bridge the life of an eminent early Victorian and the explanation of how electric motors work, even though the bridge consists of no more than the incidental fact that the electric motor rather than the steam engine was discovered by that early Victorian. But I think the possibility of a bridge can be discussed, and the discussion of it, if it is interesting, may perhaps draw the attention of all sorts of people. It would then be a bridge, if a bridge is at all possible and desirable, somewhat more honest than the one of selling an early Victorian biography to those interested in electric motors, or *vice versa*.

How interdependent are a person's private and public life? Sir Francis Bacon answered in with his uncanny sharpness and with his impossibly demanding attitude: to be a researcher proper one has to be maximally humble and open, and possess unlimited good will; this will insure that all other characteristics will not interfere with research, so that its outcome will be objective, true, scientific. Since his answer is impossible, the absence of a discussion of the question is puzzling. It is the kind of question typical for twentieth century biography. Perhaps the reason why biographers of researchers have not yet discussed this question or instances of it lies in the difficulty it involves in general and in the field of biographies of researchers in particular. To attempt to answer it here would be to underestimate its difficulty. It has intrigued me for a long time and I spent some time studying the life of Faraday in an attempt to see whether a biography of him could be written in that his scientific personality and his social personality would not be separable quite as easily as they are in all the biographies of him that I had read. When I ultimately settled down to write the biography I felt the need to clarify my own thoughts on my aims in writing that biography, and in order to do that I had to clarify my thoughts about the aims and methods of writing the history of science; the outcome was my *Towards an Historiography of Science* (1963), that serves as the core of the present volume. Thus, after years of effort my study in this direction has not yet begun. I finally published my study of Faraday (1971), saying in the preface that my aim was to unite the two persons of Faraday, the private and the public, with no assessment of how successful I was. Here I wish to make a new superficial observation all the same. I have already mentioned the isolation that Faraday suffered, and the ceaseless efforts he put into his advocacy of his theory. The stubbornness and perseverance that characterize his behavior here marked all his scientific work; his discoveries are almost invariably immediately related to this supreme task that he undertook of breaking the wall of silence and bridging about the recognition of his ideas (not assent to them: this he left to his readers to decide about). It was the same stubbornness that pulled him out of the poverty, misery, and dullness of the life or an early nineteenth-century book-binder, to become a paid research-worker in a period when there was almost no such position anywhere. Faraday's achievements are public, and deserve the appraisal of their value without reference to his stubbornness; but the mode of generation of his ideas and discoveries is evidently rooted in this supreme stubbornness. One cannot say that all scientific achievement is a result of stubbornness; Boyle's achievements were the result of a different scientific temperament, a result of his having the character of a person deeply religious and of "a catholic taste", to use his words; many of his results were obtained from experiments designed as examples or what an amateur gentleman could do with very little effort and just enough money, curiosity, and good will. (Go to a chemist and ask for x; if he does not know what x is, ask for y, and so on. Now you have to purify it, etc. Now you have in hand a tolerably pure sample of x and we can start the experiment.) I often wonder what would have happened if, instead of asking others to repeat the experiments with pressures and volumes of gases though under varying temperatures, he had performed that experiment himself. As it happened, no one took up this suggestions, much to his chagrin — until over a century later (John

Dalton). Science may have branched out differently had Boyle been half as stubborn and intent as Faraday, rather than a self-appointed model of the dabbling amateur; but then, had he not been that successful model, the scientific world as we know it might not have come into being at all.

The proper assessment of the results of any piece of research should be independent of its origin. The initial interest in science, the primary — psychological or otherwise — causes of any individual becoming a researcher, belongs to the history of the social background of science, not to the history of science proper: we can easily exchange (in a thought experiment) the places of two contemporaries who became researchers, one in order to escape poverty and the other in order to escape boredom. Nevertheless, personal characteristics that may be related to the researcher's initial interest in science and to accidental circumstances of the environment are often strongly related to the choice of how to push the growth of science. A researcher may invest efforts in acoustics due to the love of both physics and music, as was the case of Helmholtz, thus opening new and undreamt avenues; a researcher may force a whole generation of anthropologists to go and live with savages for two or three years each, quite contrary to the anthropological tradition, because of some accidents that unearthed the fascination of such an experience. This happened to Bronislaw Malinowski, who as an alien citizen was placed on a remote island during World War I and being very clever he gave excellent but questionable justifications for his demand that others should emulate him. He developed a scientific technique and a school of thought for which he had to invent a whole new set of ideas and myths to justify his technique and his school of thought. This was very lucky for us, as it has led o researches that have enriched us all. Thus, purely private motives may leave a deep mark on the public growth of science. (I. C. Jarvie, *The revolution in Anthropology.*)

The direction of the growth of science at any time is thus less objective an affair than its validity. It is not entirely objective. Nor is it entirely subjective either, since the scientific public may refuse to follow the interests of one of its members; at times they do so because the odd move happen to succeed sufficiently to draw attention.

This view conflicts with received opinion, to wit, Bacon's theory that takes the proper path of any science as strictly prescribed. By Bacon's theory the only personal aspect left open to individual researchers is the choice of a subject, not the choice of the direction in which to try to develop it. Most biographers of separate the temperaments of their heroes from those of their contributions to science, perhaps because they agree with Bacon that the growth of science has nothing arbitrary about it. It is rather odd that biographers of researchers sing the praise of their heroes, lauding their devotion, and then assure us that the chief contributions of their heroes to science were made by accident. Bacon's theory of accidental discovery is the extreme form of the idea that the advancement of science is so objective that it has nothing whatever to do with individual concerns: all that an individual has to do is show readiness to push science forward; the rest is up to science; it progresses on a prescribed course that depends on nothing personal: its motive force comes from individual researchers; its course does not.

This makes very poor biography, since the only choice that it allows the heroes of biographies is whether to harness their ability to science, and to which branch. It forms a good excuse for the biographer to present those branches of science and the parts of them that are due to the devotion of their heroes, but the devotion in itself is indistinguishable.

7. A bridge-subject properly developed becomes a new specialty.

The history of science may pose independent and interesting problems, and historians of science may suggest some new ideas as possible solutions to them. As long as these problems and their solutions are not officially recognized as the property of the history of science (despite the works of Koyré and Cohen and their likes), they belong to the no-man's-land. Hence, with luck this no-man's-land may be situated between the two cultures. Even then, they will not serve as bridges without some further investment in them. On the contrary, like any other new kind of studies, they may develop into a new specialty rather than a bridge. The attempt to cater for an interest that depends on different specialties or on different disciplines, usually interest of people with one foot in the arts and one in the sciences rather than with both feet in an independent field of study with its own problems and techniques. The attempt to make any field a bridge rather than a specialty is alluring, since the problem at hand is how to avoid complete specialization, how to avoid the situation in which intellectuals confine their interests each to a single specialty. Yet this allure leads to failure. Every bridge-discipline must be either incompetently managed, or else well managed by a small class of super-specialists, by people clever and talented enough to master two or more specialties or disciplines. In either case, the bridge-discipline will create the opposite effect. Mismanaged, its failure provides yet another argument in favor of specialization. Well-managed, it accelerates the process of the split of our cultures into specialties in the following manner. It is well-known that the easiest way to create a niche for oneself is to create a new specialty and the easiest way of doing this is crossbreeding: we have philosophy, and we have history, so we can create the history of philosophy as well as the philosophy of history (historiography). We now have also the history of the philosophy of science. Similarly social history and the history of science are now respectable and increasingly successful specialties or disciplines. So, now we have a cross-breeding between social history and the history of science; indeed some people are already working furiously in this direction, and with some fascinating results (mainly because they are less burdened than other historians of science with the need to justify past errors). Similarly, we have a crossbreeding between historiography and the history of science, as well as between historiography and the social history of science.

There is no end to this and the further one goes in that direction, the safer one is and the more respectable one's output may sound.

Nor is this confined to the present problem. In the life sciences and in medicine the situation is more worrying because it makes good sense. Let me leave that matter, important though it obviously is.

These arguments are not conclusive, of course. They are not meant to be. Their purpose is to echo the glib talk of some historians of science who claim legitimacy for their specialty as a response to current excesses. Against these my arguments are complete. Those who still wish to build bridge-subjects or bridge-studies have to find ways not vulnerable to these arguments. To show that this is possible, let me mention one special field that is definitely dependent on others and is neither parasitic nor just another field: it is the translation into simple and accessible language of parts of specialized knowledge that are unnecessarily obscure. This idea is partly behind popular science and popular histories of science. Popularizers have not clearly discussed the question what precisely is their role and the scope of their work. The first and foremost question, then, is not how to bridge the various specialties but rather, what is undesirable about them and why is their number on the increase, how can we contain it and how can we reform the system to prevent the undesirable aspect of the situation without excessive cost? For, clearly, we do not wish to stop the advancement of knowledge in order to stop excess specialization, and we cannot expect that all the newest developments will be accessible to all. Hence there must be a constant struggle for the popularization of new results — for colleagues near and far, as well as for others. This struggle is essential both for the further progress of science and for the improvement of our culture in general. We need institutions that should take constant care of this.

8. Institutional reforms of education should fight the evils of specialization.

What, if anything, is undesirable about the increasing number of specialties? We may consider this from diverse viewpoints: of society at large, of science, and of education, among other.

What is the general effect of specialization on society? It is obviously disintegration, we are told, and disintegration is the first sign of decay. Not so. Specialization forces nothing to disintegrate. The problem of specialization and of the two or two-hundred cultures is largely misty talk. Snow, at least, puts the problem situation concretely. We are witnessing the transformation of the society of cultured and civilized people into a society of clever but narrow skilled intellectuals who are ignorant and even contemptuous of everything outside their expertise much to their loss. We do not like this. What then is to be done about it? People have the right to close themselves within narrow compasses and forego the pleasure of the arts and the sciences outside these narrow compasses of their choice, including the beauties of the achievements of other specialists. Those who like to live in a well-rounded, cultured society may wish that voluntary specialization should be rare, but they must tolerate it. This is but one instance of a much broader spectrum of paradoxes of liberalism. All that liberal philosophy can suggest is the idea that education should enable individuals to be or not to be narrow specialists, and so we may wish to reform the educational system that forces many an individual into the mold of a narrow training, offering insufficient choice.

Why, then, is our educational system geared towards specialization? The answer must be found in the university system and even in the leading university departments that train specialists under extreme pressure. They do so in the expectation that some of their graduates will become leading experts and bring them fame and funds. They care much less for their students who show less than exceptional promise. All universities improve their chances for financial support if their graduates may go to leading schools for further studies. Secondary schools that send more students to universities likewise enjoy privileged positions. Educationists and educational administrators involved wish their graduates success as presumably it is good for everybody. Except that it is not. Too little critical thinking and planning for these things has ever taken place. The pursuit of excellence is a curse because that pursuit is of excellence not by one's own considered standards but by publicly recognized ones. People who have failed to go to Harvard tend to view Harvard as heaven on earth. They often are vociferous opinion-makers. The wholesale endorsement of received values is thus often pathetic.

The current trend started on the right foot. From time immemorial education was the privilege of the clergy and the leisured classes. Intellectual professions, inasmuch as they existed, had to make do one way or another, partly by adopting the artisan system of apprenticeship, often depending on the good will of the clerical educational system. The mediaeval universities, usually clerical, with minors designated for priesthood constituting the body of their student population, were the centers of learning. In present day western countries, practically all skilled work rests on some vocational training and almost the whole of labor is skilled; the university system may claim some share in the credit for this stupendous progress. The fact remains, however, that the need to train for skilled labor is now less pressing in the West, at least by comparison to the training of the masses to learn how to use their leisure adequately, as Russell has observed. Industrial psychologists and their likes are familiar with this fact, but they can do almost nothing about it within the present system of incentives — as some of them have pointed out.

The right thing to do, then, is to alter our system of incentives. Regrettably, educationists and researchers follow it all the way. All sorts of researchers in the social sciences and the humanities and the fine arts too want recognition as specialists akin to the researchers in the natural sciences, so as to become more distinguished and receive more privileges. To that end they wish to acquire the worst marks of the specialist in the hope that this way they will achieve it: they want the people at large to be ignorant of their specialization and to be painfully and helplessly aware of it. They say something like this: philosophers, imaginative writers, historians, and biographers, they all bring forward their own particular hypotheses — all more or less plausible and all equally untrustworthy. There is an evident lack of common foundations. And it is for that reason too that my field of expertise commands no respect and has no authority. When a question in physics or in chemistry comes up, the inexpert will hold their tongues but if in my field experts must be prepared to encounter judgments from every quarter and general readiness to contradict experts despite ignorance. People take for granted that my field has no expert knowledge. It is therefore important to show them wrong.

This is not true of all researchers, of course; some of them genuinely wish to popularize their knowledge, to acquaint the public at large with their special studies. They have as their model popular physics, which is very difficult to write for critically-minded readers. Einstein and Schrödinger are the giants of this art, but most popularizers of physics are too condescending to serve as healthy models. The absence in the social sciences of the equivalent of Einstein or Schrödinger came home to me with a jolt. Having formulated in an earlier draft of this section in my own way, I looked for a popularizer in the social sciences equivalent to Einstein or Schrödinger in physics. And the name that first struck my mind was Sigmund Freud. It occurred to me that at least he, the indefatigable and in many ways excellent popularizer of his new science, would be such a model, especially since in his youth specialization was not half as much idolized as nowadays. To my regret I found his complaint that popular readers do not sufficiently respect the expert. He said this in his popular "The Question of Lay Analysts" (the lay analysts being, of course, expert in psychoanalysis though not in medicine).

To conclude, the evil of our specialized educational system is that the system of incentives on which it rests is outdated, yet people forget that incentives are artifacts, that as social institutions they are open to reform. We need not follow them blindly or preach to others not to follow them, as all too often we do, instead of opening a public debate concerning the question of how to reform them.

This is where popular science, including the history of science, may help convert the specialized educational system in the desired direction by offering a historical perspective on the trouble at hand. Received opinion is different. It is that popular science ought to be the remedy to the defects of our specialized educational system, so that the educated public ought to invest some effort in studying the popular literature on scientific topics. This is moralizing; it is useless; it is an excuse for writing boring or condescending works that are allegedly popular; still worse, it diverts public attention away from the needed public discussion of the problem, how should we reform our educational system? The public discussion of the question should replace the pressure on educated readers to feel guilty and purchase boring books.

9. Popularizers should appeal to the intelligence of readers.

The mark of experts, as Freud has characterized it, is that the public bows to their judgment without having judgment of their own. The evil of much of present-day popular science literature, including even excellent works like Freud's and Eddington's, is that it chiefly comprises of attempts to make expert judgments only partly more intelligible to the public and in order to increase the authority of such judgments including their unintelligible parts, but not to discuss anything critically. This is condescension. It is rooted in the conviction that expert judgments are intellectually superior, that the lay public cannot be expected to understand everything, so that some points have simply to be imparted to them *ex cathedra*. Popularization is often poor because it is condescension.

Popularizers, thus, do not try to remedy the defects current educational system, merely to close a gap that it creates. The chief aim of the educational system today is reflected in this attitude: to help the better students become experts. Its main incentives are geared towards this aim. Certain recent educational reforms brought about for political reasons, in Britain, the United States, and elsewhere, were criticized on the ground that they do not offer the best facilities; educationists demand more differentiation so that a child talented in a given direction shall receive all possible help in developing in that direction. The only exceptions are children with learning disorders, and on humanitarian grounds. So much for secondary education; higher education is likewise geared to the very talented and the problematic, to the expert and the inexpert, with excellent specialist schooling for the best and very bad general courses for the poorest; the latter on humanitarian grounds; little popular or semi-popular literature is directed towards the middle group of inexpert, and few decent university courses are directed to non-specialists who are not complete ignoramuses. This is rooted in our educational incentive system and the servile submission to them, as exemplified by the above quotation from Freud. The incentive system may undergo reforms, and such reforms will improve matters only if the system will allow for popular yet dignified discussion of science. If this is impossible, then no reform is called for. In order to create a new and effective incentive system we must know what kind of popular science we deem desirable. Thus, a discussion of incentives should follow a discussion of the aims open to popular science. To inaugurate this discussion we may begin with a criticism of the views of Freud, one of the greatest popularizers ever.

By the expert consensus, inexpert readers should express no judgment on specialized subjects like physics, but simply listen to experts; they may however express views on any question of psychology, politics, etc. This raises the problem, that we should acknowledge Larry Laudan as its inventor: why should we listen to the expert? Not whether, but why. How sad. The obvious truth is that everyone in our vicinity has opinions on everything; people regularly speak about society and about human nature, about the sun and the stars, about the winds and the rain, about motor-cars and bicycles, tables and chairs, sticks and stones. And about weapons for mass destruction. Most people in our vicinity err in what they say, and even to the extent that any genuine expert can correct them on the spot. People err more frequently when they express opinions about things than about people, though (unless it comes to serious accidents). Not everybody is a painter, though almost everybody was once; but we all are physicists, biologists, and a medico, just as much as we all are psychologists, sociologists and politicians. Moreover, we all are philosophers and theologians — vulgar denials from fashionable positivists and tough-and-no-nonsense physicists notwithstanding. We all have ideas about the universe and about whether there is purpose in it or intelligent power behind it.

This is easy to put to experimental test. As many people have heard that sound is a wave-like motion of the air, it may be difficult to find what they really think sound is. But people who have not heard of this wave theory of sound seem to have no theory of sound. A simple examination will reveal that they do. They have theories about sound and its nature, about its mode of action, and about what it can and what it cannot do, and why. Even those who have heard that sound is a wave-like motion may be tested. They may have heard about the properties of wave motion, about reflection, deflection, etc., and they may be asked if, in their opinion, sound can be reflected and deflected and refracted, if we can have sound-lenses akin to eyeglasses, and why sound but not light can go round walls, etc. Even those who have not heard of the wave theory of sound have some theory about the echo; even those who know of the wave theory do not naturally know that echo is to sound as a mirror reflection is to light.

It is also easy to find out that everybody has a theory about the universe, however vague. One might expect this theory to be the sum-total of a person's views about sound and light, and about cabbages and kings. But this is not so. Fate and determinism, the universal pattern and what are things made of, the cycles of the seasons and progress, logic and knowledge; these occupy almost everybody's metaphysics. One might expect, perhaps, some connection between common views about sound and about the nature of things; but this will be asking too much. Few people in the whole history of ideas have ever attempted to have such an integrated picture of the universe, and it is doubtful that anyone has ever come any close to success.

Colleagues in the London School of Economics in the late 50's told me an interesting story. They used to teach freshmen abstract economic theory from the very start. It turned out that students received the theory without feeling the need to revise their initial views, known as folk economics, only to encounter a crisis after graduation, when facing what is known as the real world. My colleagues altered their techniques then and started teaching freshmen by discussing with them folk economics and moving towards the received view as an alternative to it. They found the experience engaging and valuable. Historians of science should do likewise. There is one history of physics written in that way, I. Bernard Cohen's magnificent *The Birth of a New Physics* that opens with folk physics and that teaches more abstract physics than many a popular book on physics.

Thus, in broad outline, almost every individual in our society, thinking or unthinking, has views on almost any question that we may appear in daily life, whether or not the expert has a different view on it, and almost all of these *prima facie* questions that we all answer in our own ways are important to one field of intellectual inquiry or another. The specialist answers are usually the best extant, of course, but only usually.

Consider the view of the inexpert about mechanics. Cohen's *The Birth of a New Physics* begins with the declaration that although readers fancies themselves Copernicans, their views on mechanics are often pre-Copernican and clash with Copernicanism. Cohen argues this historically. It matters little whether Cohen chose his thesis in order to impart his historical knowledge or *vice versa*; what matters is that in criticizing his readers he talks to them as to equals. It may sound paradoxical that authors critical of opinion of their readers are respectful while those who ignore their errors and give them the scientific alternatives are condescending. The air of paradox, however, stems from ambivalence about criticism. Obviously, in respectful discussion of popular errors, the historical method is best.

Waves. They go around obstacles, as we see and hear. Why does sound but not light go round obstacles? The great Newton denial that light is wave-motion rested chiefly on the fact that it does not. He erred, and some take this as evidence that throws doubt on his proficiency as a researcher (Roger H. Stuewer, "A Critical Analysis of Newton's Work on Diffraction", *Isis*, 61, 1970, 188-205, final words). This is the view of research as black-or-white although not of researchers. It allows them to be gray like the human mane that is a mix of black and white hair. (Stuewer refutes the traditional, idealized view of Newton, presenting him as having had clay feet.) When historians of science will be free of this, they will be able to interest readers even if they are unfamiliar with physics, especially since the physics is too difficult even for top-experts. (Helmut Nieke, "*Die Folgen der Nichtbeachtung von Newtons Beugungsexperimenten*" [The consequences of the ignorance of Newton's diffraction experiments] *Sudhoffs Archiv*, 2001, 85, 1-17: "the disregard of Newton's diffraction experiments from 1850 and again from 1900 is … a false direction taken by textbooks".) They will then be able to explain — to themselves and to their readers — how was Newton's objection disregarded. This is simple, easy, and interesting on the popular and on the expert level — for different reasons, perhaps, but interesting all the same. Most histories of science do not discuss such questions; few allude to them.

This is but one example of a problem, or a set or problems, in the history of science that may interest the expert and popular public alike. Interesting expositions — expert or popular — are easiest to follow if they begin with interesting problems, commence with answers to them and critical discussions of them — usually beginning with the easier answers first. The expert may be familiar with the problem, or know that it derives its significance from some other problem or from the general situation in the field of study; the inexpert may have to be told this in detail. Often even the expert does not quite know the problem-situation but is carried along by a vague idea and the tradition that Boyle inaugurated of letting the facts speak for themselves; in this case experts too would benefit from writing or reading general expositions or surveys. There is not much difference between the critical introduction to any field of study, or survey of it, designed for prospective experts, and that designed for the inexpert. The difference is only that the expert can take more technical and detailed instruction than the inexpert, and thus further their knowledge to a greater extent. Some technicalities are essential for the understanding of some problems, so that these problems remain for experts: those who cannot understand the problems cannot understand the solutions either; and thus the inexpert cannot ordinarily hope to become expert without the mastery of these technicalities. But technicalities are scarcely matters that make for expertise: the lack of mathematics bars one equally from reading some material in economics and in geology. Once the technicality is no obstacle, what the inexpert need is a survey of the field and particularly of the problem-situation in it. When both technicality and problems are clear, the road is open to anyone who cares to make the effort and study. Otherwise, readers, expert or not, specialized or not, cannot see the solution. They may understand some results of a solution to a problem that they do not understand, but not the solution itself. For instance, they may know something of explosives and even of nuclear weapons without any knowledge of energy or binding energy. This is equally obvious with experts and inexpert. As to the inexpert, they can easier follow the *Reader's Digest* kind of popular science than the Eddington kind: it does not present solutions to problems that its readers are not familiar with or cannot comprehend (though it may present corollaries to them that solve understood problems).

Presenting a problem before presenting a solution to it disposes of much dead wood in writings for experts and in condescending popular writings. Repeated attempts are advisable to minimize the technical part of a discussion — both on the expert level (Einstein's *The Meaning of Relativity*) and on the popular level (Einstein and Leopold Infeld, *The Evolution of Physics)*. Popular surveys and other works of this kind should help experts to orientate themselves better in their own specialties, at least as long as there are not enough professional or expert surveys.

Specialists and experts are not likely to accept this proposal and write extensive surveys. Apart from their dislike for the studies of errors that surveys are, they condescend towards the inexpert. They often feel that they know what their problems are and where they stem from, and they do not wish to explain this to the public. Logicians asked what Gödel's theorem is would say it is difficult to explain to the inexpert. Asked what problem it comes to solve, they will mention Hilbert's program and say it is the attempt to show that all true mathematical theorems are provable. If this meets with further questions, the answer will again be that it is above the head of the inexpert. A famous book on Gödel's theorem by Nagel and Newman explains it with much patience and few technicalities. Experts have sniffed at it, criticizing it from their Olympian standard of expert precision, as they previously did with Lancelot Hogbin's *Mathematics for the Millions*. Popular literature on mathematics is still very scarce as few experts will accept a compromise or try to improve upon previous efforts. The paucity of popular elementary mathematical works, and the harm of mathematical teaching in secondary schools, block the understanding of mathematics — and of all studies that depend on it. Ignorance of philosophy, especially among science specialists, strengthens the faith that science is technology. During the Cold War governments presented the technological race as scientific and urged universities in the name of survival to give up academic aspiration and be technical schools proper. The prestige of physicist, engineer, and computer maven (at times a brokenhearted ex-mathematician), makes the social researcher sniff at the popular social writer. Everybody follows the system of incentives for research and no one explains them. The few who break away from the system by trying to provide liberal programs unwittingly reinforce the existing specialist-orientated incentive-system by creating a garbage heap on which to dump the debris of specialized education, the students proven unfit for high-powered training. Expert educationists who try to salvage the dignity of liberal education must struggle for recognition. This way they accept the existing system of incentives as it stands. Thus, the less the experts know where they are going, the more they strengthen the extant system of incentives that they follow uncritically. They strengthen the expert-mystique and keep the inexpert ignorant while seeking prestige in order to be able to educate the inexpert.

Fortunately, expert-mystique has not become all-pervasive despite this trend: a few who do not succumb to it are still heard. Perhaps this is the result of the survival of some old traditions: we owe much to those who have fought the expert-mystique in the past, from Boyle onwards, and that we can still expect to live for long on dividends on investments made centuries ago while destroying regularly small parts of our heritage. For, that heritage is of a tradition without expert-mystique and without experts, or with as few of them as possible, and this tradition is dying out. One of Boyle's greatest achievements was killing the prestige of the expert and establishing in its stead the idea that the language of science is public, but he did so while destroying the society of experts. The specialists are nowadays too busy in to read even the preface to his *Sceptical Chymist*, where he explained his behavior. He was particularly anxious to explain himself on this point, because he had bad conscience about it. Professional science depended on expert-mystique, and the mystique depends on specialization; so he was hurting some people's means of livelihood by killing the mystique. He even compromised on this issue and kept one discovery secret in order to help some alchemist make a living; and this was against his principles of the public character of science. He violated these principles three times: in that case, in the case of a military patent that he bought in the (vain) hope of preventing its use, and when he thought he discovered the philosopher's stone (that transmutes base metals into noble ones) but feared that publicizing it may lead to runaway inflation. If after all his self-search and deliberation he still thought that professional science is impossible, because professionalism breeds expert-mystique, one must consider his view seriously. Hopefully present-day professionalism is not as dangerous as he feared. If he was mistaken nonetheless, then we may find new means of reviving popular science as he and his successors knew it without damaging professional science overmuch, as well as new means of preventing expert-mystique degrading popular science again and turning it into expert condescending tidbits for the lay public.

The prestige of science rests on two kinds of achievement: worldly success and enlightenment. *Qua* enlightenment it cannot be the possession of small class of experts; *qua* enlightenment it must be publicly open to critical examination, to developments with no consideration for the interest of any class, not even the class of experts; *qua* enlightenment it is identical with popular science. The public owes much to specialist experts — but only for what they give the public. If they give the public only technological progress, then the public is less indebted to them. What the public can give specialist experts in return for enlightenment is public interest that may be converted into the incentive to keep science identical with popular science and thus to prevent it from becoming once more alchemy and astrology instead of enlightenment proper.

The means of livelihood of intellectuals depend on their being experts and professionals. They should then show little interest in other fields of expertise, especially ones remote from their own. Yet the hope is repeatedly expressed that general and interdisciplinary studies will gain popularity. It is therefore generally agreed that propaganda and philosophy and history and of science and popular science should be available to further this end. This is very discomforting. Public interest should be encouraged not by propaganda but by means of educational (and other) institutional reforms; and the proper literature is less history and popularization and more elementary introductions and critical surveys; and that public interest is not merely in the interest of the educated public but of the scientific society itself whose public character should be better preserved. This incidentally is on the increase and invites intelligent surveys. This is a source of hope.

10. Bridging between different specialized fields is creative.

Two trends characterize the growth of science — unification and diversification. Somehow, diversification is totally ignored in discussion about its methodological aspects, and unification is totally ignored in discussion about its social aspects and the problem of the two cultures. This is an error, but it has a rationale that is very strong and obvious. Take an instance of unification, and you will see at once that it does not destroy the specialties that it unifies, but tends to create a new specialty instead and a less accessible one at that. Admittedly special relativity has unified aspects of mechanics with electrodynamics with hardly any new experts; but this unification did not destroy or unite the two specialties it has unified, just as general relativity has not destroyed them. It only made physics less accessible to the public. General relativity has unified, at the very least, gravitation with geometry, created at least one new specialty, if' not two or three; and destroyed none. Even in mathematics, where unification is an intense activity at least since Descartes, what once was a field of a few branches is now branching to hundreds of specialties. (Einstein reported in his scientific autobiography that he had wished to be a mathematician but as he could not develop a synoptic view of it he took up physics instead.) The process goes on: each act of unification breeds new and more esoteric specialties, and mathematics as a whole becomes increasingly esoteric.

This explains why the problem of specialization hardly ever relates to the unifications that regularly take place in science as it progresses. But this is an error: specialization may lead to the loss of the generality of science, of its tendency towards unification. This may be dismissed as based on a short-sighted observation: if we look at the situation over a slightly longer range, we may observe two phenomena in the opposite direction: some specialties disappear; and some specialists find it increasingly harder to remain isolated. Euclidean geometry, electricity, magnetism, and other items of expertise, became background knowledge of almost all the natural sciences. Chemistry was notorious for narrowness, but this changed due to the advent of physical chemistry, including quantum chemistry

Those who wish to bridge between the two cultures may find little consolation here, just as they would find no pleasure in the current infiltration of some elementary psychology and sociology into all specialties in the arts — and more so into the history of science. These people are over-ambitious, however, often driven by the desire to integrate the whole commonwealth of learning into one family. This desire is anyhow tribalist and objectionable. Their over-ambitious tendencies make them ignore such trite cases as the possible theological relevance of general relativity and cosmology, of the interaction of animal and human ecology, and of other developments or possible developments of similar unifying characteristics.

It is a bit hard to listen to people who say that they humbly contribute a tiny bridge when they preach to members of the faculty of arts to broaden their horizons by self-education, and yet notice how easily they may overlook other tiny bridges, built on the desire to expand our understanding. But one must not dismiss educationists so easily: they are not concerned with what science does well, but with the problems it leaves behind — partly because these problems are their job to handle, and the problems internal to science are not: they too are specialists, and we must allow for their specialization. Nevertheless, the problems of specialization should concern no single group of specialists; it should be free for all. At least the problem of specialization and other problems concerning the self-government of the commonwealth of learning remain everybody's business (to the extent that they are interested).

The philosophy of science offers one kind of possible solution to the problem within the rules of the game of science, namely, the use of the tendency of science towards ever-increasing generality and inter-relatedness. Such progress is a scientific creative act, not something to devise, much less to institutionalize. Nevertheless, simple institutional means to facilitate it can be devised and implemented, and the results may serve as preliminary bridges of sorts. For example, the training of students for the writing of critical surveys of problems in their fields of interest — especially surveys aimed at showing the wider significance of their own special interests, which is the relating of their narrow interests to wider contexts. This would save students from losing sight of their situations, from losing too much time on obviously unpromising detailed studies. This would also enable others to share their interests by studying their surveys. Karl Popper has suggested that this is the proper direction for a serious solution of the problem — by instituting the encouragement to include such surveys in publications, even if very briefly. This appealed to him particularly because it is not a drastic or radical solution, but rather a piecemeal method of solving the problem as often as it arises; and it arises repeatedly since the method of science is not only that of unification but also that of splitting — of (unifying) conjectures and of (shattering) refutations. L. L. Whyte has observed that the significance of surveys is very great, in the past and hopefully also in the future: he observed that every major act of unification in the past was preceded by a critical survey of a highly philosophical character. It is hard to judge how much truth there is in this observation, although it is not difficult to provide intriguing instances conforming to it.

Assuming that critical surveys are possible means towards genuine unification, we may try to solve the following painful problem. We demand that Ph. D. dissertations show some degree of originality, but we can hardly guarantee that a novice will, in two or three years, be original to any degree. How then can we undertake the supervision of such work? A number or make-shift solutions to this problem exist, each having its own merit but none seems satisfactory to its practitioners. It is a familiar and common topic of gloomy conversations. The solution to it should be that students should write surveys of problem-situations whenever possible; they may then make original contributions in the very surveys or as further outcomes at the end. This proposal is obviously not in the least original; the paucity of its application, however, suggests that there is room for public discussion of it.

A multitude of topics might profitably be open to public discussion but are not, due to the rapid rise if these problems in situations that develop too quickly. The process of specialization within modern science is but a century old, two at most, yet it has achieved its completion. The gulf between the two or three or many cultures is as wide as can be; the process of atomization within each of them has its limits within the accepted traditions, and in the following way. In order to become recognized, academics must gain support from a few rather distinguished referees each, not all immediate professional associates. So referees must be, in some sense or another, experts not quite in the same field as the candidate is. Thus, a minimal link between the different specializations, at least in neighboring areas, must remain.

What is and what is not an adjacent field is popularly taken to be God-given. One needs either lively imagination or historical knowledge to realize the naiveté of this. That two fields are adjacent is a theory, explicit (a unifying theory) or implicit. The implicit theory is part of a survey. The survey may be a written text, an oral tradition, or a mere lore transmitted in university corridors during breaks between lectures or during conferences, national and international. It is advisable to write down these surveys, criticize them, and improve upon them. This is not too difficult, possibly useful, and hopefully leading to unification. It is perhaps a meager hope, but it suffices to keep optimism alive.

11. My own part in the story.

Occupying the unusual position of a culture-dabbler with a reputation as an expert and a specialist, I prefer to avid maligning genuine specialists, not even to censure the condescending and the dull popularizers. But I have no wish to conceal my distaste for the defense of specialization as more than a necessary evil and for boring histories of science and condescending works on current science. Perhaps the ease with which I have acquired the position of a specialist of sorts makes me unjust towards those who have earned their specialization the hard way. Many intellectual achievements of no mean caliber have been attained by the immense single-mindedness of individuals who devoted all their time to limited projects to the exclusion of all recreation; for example Faraday. It takes time for these important achievements to reach the public at large. This delay is neither due to the selfishness of experts nor due to incompetence or condescending attitudes, but at worst due to inadequate social institutions, namely an inadequate system of incentives, the lack of planning due to the rapid growth of our present traditions, the immense success of these institutions over the last few generations, etc. The evils of specialization are relatively new and so we need not be unduly pessimistic. The ills of specialization may be altered fairly soon, and with unpredictable beneficial outcome.

These pages aim neither at the established experts who is busy pursuing their expertise, nor at the passive inexpert who just want to hear a little more about more expert achievements. To that inexpert I have regrettably nothing to report except that there some studies in the field of the history of science are well worth reading, informative, thought provoking, and utterly delightful. To the established experts I have regrettably nothing to say except perhaps to some whose training was not very useful — that they may try not to impose the same training on their students but help them be intellectually flexible. I am writing to those who are interested in, and willing to think about, such problems as, how and why one writes a book on the history of science? how and why did people start, all of a sudden, to write the present flood of histories of science? can a historian of science write without having an axe to grind? what benefit can I expect from the study of the history of science? Now, are there people who would study these problems? Are they the specialists (or would-be-specialists) on these problems? If so, regrettably I have little to say to them. But I hope that some — experts or not — may be interested in these problems, and I hope they will enjoy reading my output. If some of these would be historians who wish to try to improve the state of the art of the history of science, I do not know if I can help them much. But at least I hope that not only experts or would-be-experts, but also my own kind of culture-dabblers may contribute to the development of the field, refuse to be brow-beaten and voice independent views on what kinds of histories of science they would like to read.

To conclude, nothing resembling a message here makes reading it of such special value. Yet I for one shall regret it if those who might enjoy reading it will be prevented access to it by the excess bridge-literature to be read and by the pressure to study much unmemorable professional work. If I have any message, it is that compelling ourselves and our having the sense of compulsion to read this or to study that has done much harm. Intellectual activity is enjoyable, and should remain so or else it may be debased. Admittedly, mastering of techniques is not all pie, and technique is prerequisite for some intellectual activity. Yet, as things are, cultivated individuals, professional expert or inexpert amateur, naturally wish to participate in more intellectual and cultural activities than they can. Hence, they must be selective, and their selections will depend on available amenities. For instance, they may prefer reading a book on the history of science to reading a book on social history, but find it easier to get a hold of a readable book on social history and so read that rather than the other. Otherwise they may be barred from reading some scientific literature because of ignorance of some technicalities. It is therefore all to the good to bring to those who are willing to read some kind of literature the elementary technicalities of that literature. This is often found in introductory works, and seldom in surveys, much less in the popular literature. Hence, mine is more of an introduction to the study of the history of science and a survey of its history and its methods, rather than a history of science or a popular work. Let me mention a charming example. The great Oliver Sacks wrote a few essays in the history of science and there he expresses his profound faith in Bacon’s old-fashioned theory of the method of science as inductive. This is most unusual and thought-provoking fact. It is impossible not to be impressed with it. Historians of science defensively dismiss such information on the ground that with all due respect for Sacks, he is no expert. It is time they pay more respectful attention to such facts.

FOURTH PRELIMINARY ESSAY: ON THE MERIT OF FLOGGING DEAD HORSES

> … to try its strength … he drew his sword, and giving it two strokes, undid in an instant what he had been a week in doing. But not altogether approving of his having broken it to pieces with so much ease, to secure himself from the like danger for the future, he made it over again … without daring to make a fresh experiment on it, he approved and looked upon it as a most excellent helmet.
>
> (Cervantes)

To flog a dead horse is to criticize yet again an idea that has already been publicly satisfactorily criticized. Some dead horses well deserve fresh flogging every now and then. I will flog here one particular dead horse, namely the idea that the flogging of dead horses is always useless. Since some flogging of a dead horse is advisable, to dismiss criticism on this ground is too hasty. As some colleagues do so, I should explain.

1. the prevalent attitude is haughty

Surprisingly, there is no detailed discussion on the advantage or disadvantage of flogging dead horses. The reason for this may be that since it is too easy to do so, no one bothers; but as books and articles are dismissed at frequent intervals on no stronger account than that their authors are flogging dead horses, opening a detailed discussion on this may be useful. Whatever the case may be and however obvious, it invites discussion as some serious writers engage in this practice and some serious readers look down at it.

The present situation is even more dramatic: flogging of dead horses is encouraged if done frivolously but discouraged if done seriously. Serious flogging of dead horses is usually dismissed as frivolous. The situation is sometimes, not often, even still more dramatic: a critical writer who flogs a live horse may be dismissed as flogging a dead one and by many a potential reader. After a glance at a critical work, one may claim with impunity that it is worthless: one can easily get away with dismissing most unjustly a critical original work by claiming that its author is flogging dead horses. Only a few readers would bother to check the correctness of such claims: the academic public on the whole swallows them as soon as they are made. Moreover, along with the unjust dismissal of some repetition of old criticism, most of the criticisms one finds, say, in the philosophical or in the psychological literature that might be justly dismissed as worthless cases of flogging dead horses, has not brought about such comments.

The matter at hand is that of the repetition of just criticism. Repetition may also be of unjust criticism. The sweeping criticism of all metaphysics that was repeatedly made in the middle of the twentieth century is an obvious example. Valid or not, it is redundant repetition. It is also possible to repeat assertions of familiar principles. Some repetitions serve obvious functions. The paradigm case is that of a textbook: it is not supposed to convey new information or ideas. Another paradigm case that is more interesting is the treatise that is often just a compilation. Euclid's *Elements* is the paradigm here. And, of course, the best repetitions are new surveys. Hence, some repetition is important, some useless, and some sheer nuisance. Information theory says, some amount of redundancy is necessary to avoid excess miscommunication. We often make redundant assertions because of the love to hear one's own voice and because of publication pressure. But criticism is different: it is viewed as censure and rubbing it in is impolite. So it is hardly surprising that the targets of criticism are the ones annoyed by its repetition, particularly when it is just and has not met with a straight and unambiguous retraction. This is all neither here nor there.

The safest thing to do in order to yield to publication pressure with the least sacrifice (and, incidentally, with the least intellectual consequence), is to flog a horse that the consensus says is dead, one that has been effectively and thoroughly criticized to everybody's satisfaction, one that no one advocates any longer. A paper that flogs a horse already as dead as a doornail is obviously uncontroversial and sound. Hence, publishing it will hardly invite trouble. Hence, mediocre editors, biased in favor of the uncontroversial and the sound, are naturally biased in favor of such papers. Since publication pressure encourages all forms of mediocrity, the result is that such papers are often published. They invite other authors to make marginal comments on them. (The arts do not have the luxury of reporting a boring variant of a known experiment.) The increasing flood of largely worthless scholastic articles is evidence that a discussion between upholders of uncontroversial, sound opinions is quite admissible. This is why much unnecessary flogging of dead horses is tolerated, not to say invited, by common editorial policies. Highly interesting material is too easily dismissed as flogging of dead horses; one might expect then that as mediocre flogging of dead horses is tolerated, its competent portion should be welcome. But this reasoning is open to criticism. Since most of the flat material is scarcely read, there is little reason to discuss its merits or defects, and tolerating it may be harmless. The case would be quite different with a work that one is told one must read and that one makes some effort to read. In that case one might rightly dislike finding that one can learn almost nothing from the work in question, if for instance one finds that it presents familiar criticism as new. This is understandable, and so is the outcome of it, namely developing more cautious reading and tending to refrain from reading works that colleagues say is devoted to the flogging of dead horses. Yet, understandable as this is, it leads to the current decrease of the disposition of academics to read and increase of their disposition to write for decreasing publics. This may soon lead to the stage when academics will read papers only in order to write other papers of comments on them. It is practically impossible to write anything without flogging dead horses to some degree. Hence, if having heard that some work includes flogging a dead horse is a sufficient excuse for not reading it (unless for the purpose of writing something about it), then an academic need not read anything. Indeed, there is a great incentive for not read when publication pressure is on the increase, and when the confusion between debunking and criticism makes the reading of criticism quite a disturbing experience; so there is a strong incentive to accept this kind of excuse as always admissible.

The problem is real, though: the plethora of publications has worried a few serious philosophers, including Michael Polanyi and Karl Popper, who recommended a kind of self-censorship. This will not do, even were publication pressure reduced, since we cannot possibly have general criteria for quality as these two philosophers said. Fortunately, today, thanks to the wonderful world wide web we encounter so much redundancy, that publication pressure hardly makes a difference; we all have to learn to find our way in the wealth of available material and improve our ability to choose well, to separate the grain from the chaff. The only known alternative is to follow the fashion. This most academics do quite traditionally, and the internet only makes life easier for those who want to break the fetters of fashion.

Saying of authors that they flog dead horses is a form of debunking, of saying that they pretend that they have something new to say, that they cannot criticize any opinion that any reasonable fellow is entertaining; that, in particular, they cannot criticize clever me. Oddly, authors often honestly but falsely dismiss criticism as flogging of dead horses, and then proceed to parade that same horse, large as life; authors first accept the criticism of an idea as valid, though dismissing the critic for writing down this criticism as if it were new, and then proceeding to present the idea as true, thus showing how far they are from accepting criticism that they declare already accepted.

A famous doctrine says, necessarily, history develops along a fixed pattern (more-or-less); history may, according to that doctrine, develop slowly or rapidly, with more or less variations, with more or less deviations, but its course is prescribed along a pattern all the same. The most popular version of this doctrine is due to Karl Marx. This doctrine has been labeled by Karl Popper "historicism" and by Isaiah Berlin "the doctrine of historical inevitability". If I may make a terminological aside, let me admit that I find Berlin's term preferable. The German term is "Historismus", except that in German the term names a whole package deal. It was unpacked into two English terms, "historicism" and "historism", the latter possibly denoting the view of the social sciences as historical, possibly denoting the demand to present political history in its socio-cultural background, and possibly denoting historical relativism. This aside reports the contents of quite a few learned papers on the subject. When the famous historian of science Derek J. de Solla Price advocated historicism he was not interested in historism, and many advocates of historism, within the context of the historiography of science or without, were not historicists.

Popper criticized the doctrine of historical inevitability at some length. Berlin joined him. Many a reviewer viewed their criticism as conclusive, but as flogging of a dead horse. The most famous commentator this way is the Cambridge Marxist historian E. H. Carr, who delivered a series of BBC Third Program lectures that he then assembled into a book called *What is History*? In this book he proves that Popper and Berlin flog a dead horse by quoting a few nineteenth century critics. He then cheerfully proceeds to propound his own view of what is history that is but a version of historicism.

2. The distinction between sketchy theories and their specific versions.

Carr need not be inconsistent here: maybe the old authors whom he cites criticized some versions of the doctrine of historical inevitability and Popper and Berlin have criticized the same versions, so that other versions of the doctrine are possibly true and Carr has the right to expound one. If this is so, however, then Carr's criticism of Popper and Berlin, namely that they were flogging a dead horse, is as yet unjust, as he did not show that the old and new critics attack one and the same version of the doctrine. On the contrary, he made it quite clear that the old critics attacked the doctrine of historical inevitability as such; and on this he is right. Hence, Popper and Berlin were not the first critics of the doctrine, but it is no dead horse, as Carr himself shows by his advocacy of it.

Maybe the source of his flagrant inconsistency is a confusion between historicism as such and a version of historicism: between the sketchy version of a theory and a possible detailed version of it: between "history develops along a preordained pattern" and "the preordained pattern along which history develops is …" This seems a simple and obvious distinction, yet one that we sometimes regrettably forget. Let us take another example for it, since unless we keep it clear we cannot decide appropriately that the horse dead.

Example. Historians of physics and physicists speak freely about "the" wave theory of light, about "the" wave theory of sound, etc. although there is a variety of wave theories of light and of sound; this raises the question of the identity of a theory under discussion. When physicists speak of the wave theory, the one they usually refer to is pretty obvious from the context; it is usually the latest and best available. When a standard historian of physics speaks, it is very difficult to identify the theory in question, and it is easy to refute any conjecture about it that comes to mind. In short, they are often inconsistent. Their inconsistency may pass a superficial reading but a little knowledge makes the reading of most of what they say puzzling, not to say embarrassing.

There is a variety of wave-motions. The most well-known distinction is between longitudinal and transversal wave motions. When a drum's surface vibrates it pushes the air near it; each particle moves away from the drum and back, as its motion passes on to its immediate neighbor, so that the motion of the wave is in the direction away from the drum: the wave and the particles move on the same line; the wave is longitudinal. It is the simplest. Waves on the surface of a pond are transversal: the waves move horizontally and consist in vertical movement of the water on the surface of the pond; the two motions, of the wave and of the water-particles on the surface are perpendicular to each other; hence the word "transversal", deriving from the word "versus". There are other kinds of waves, such as the mix of transversal and longitudinal, standing waves (of a string, for example, or of water in a tub), and more. Therefore, the sketchy version of the wave theory, the theory that is no more than the assertion that a given motion is periodical, says too little. Yet, the sketchy version of the wave theory of light, or of sound, does tell us something quite interesting, namely that light or sound is not a substance but periodical motion.

Newton's said that sound consists of elastic waves. The modern theory of elasticity says, in accord with Newton's idea, that elastic gases such as air can produce longitudinal waves but not transversal ones. This is not to say that there is one and only one wave theory of sound, *the* wave theory of sound: Laplace, for example, presented a wave theory of sound in air that markedly differs from Newton's, though the two shared the view that waves in elastic gases can be only longitudinal, not transversal. As to sound in solids and liquids, they present a fascinating range of problems. Solids possess elastic waves both longitudinal and transversal; crystals possess varieties of transversal waves, since their elastic properties may be different along their different axes; how then does sound travel in solids? The problem becomes even more fascinating for incompressible liquids, as they cannot carry longitudinal waves at all: water was considered incompressible (we are still taught so today in our secondary-school courses) yet it transmits sound considered longitudinal waves! How is this possible? This problem was attacked by Ørsted in the early nineteenth century, but almost no history of science discusses it. "The" wave theory of sound is attributed to Newton, the correction of Laplace is mentioned in a number of books, as modifications or improvements of the same theory, and the rest is ignored, or covered under the ambiguous blanket "the wave theory of sound".

The sketchy version of the wave-theory of light ("light is a periodical motion") was invented by Hobbes if not by earlier thinkers. But this enemy of the Royal Society of London cannot, of course, be given priority for anything scientific. Priority is usually attributed to Hooke or to Huygens. Huygens, to be sure, had more than the sketchy version of the theory: he presented many more specific details, saying that light was longitudinal periodical motion of a very light elastic gas called the ether. But on this score he was mistaken, and his mistake is ignored by most historians who thus do injustice to Young, the inventor of the transversal wave theory (of light), and an injustice to the development of the ether from being a gas (that possesses no transversal elastic waves) to being an elastic solid. (It is harder to envisage transversal wave motion in three dimensions than in two.) E. T. Whittaker, the most reputed writer on the history of "the" wave theory of light, went the other way. He said Huygens developed a wave theory of light in analogy with the wave theory of sound; Huygens developed one elastic theory of light-waves to which Newton's objections apply, he added, and Young developed another, to which the same objections do not apply; until 1819, he says, "wave-theorists were still misled by the analogy of light with sound" and Young conceived the idea of transversal waves in 1816. Incredible as it sounds, Whittaker manages to avoid stating that until 1816 wave-theorists, including Huygens and Young, deemed light waves longitudinal. This makes the reading of his text difficult, and his constant reference to *the* wave theory of light quite confusing. I found reading him very difficult and I had to correct this paragraph a few times while checking his text.

The theory of light as transversal-wave meant that the ether is not a gas. This led to a general study of elasticity. It soon transpired that waves in elastic solids are both transversal and longitudinal; this raised the problem. why are there no longitudinal light waves? A number of attempts to get round this difficulty were more-or-less failures. The classical theory of elasticity ultimately led to the discovery of a possibility of elastic solids with only transversal waves; but the earth could not possibly move so freely, if at all, in such an elastic body. Incidentally, Roentgen first thought that X rays might be longitudinal; he refuted this idea.

Standard historians of science ignore all this, at least when speaking of "the" wave theory of light, because the term indicates at times the sketchy version of the theory and at times also a specific and much more detailed version of the theory: it stands for Einstein's wave theory of light of 1905 (his first theory of relativity), namely of waves that are not elastic in any way. But almost no one speaks of Einstein's wave theory of light; the only exception I came across is the already mentioned unpopular O'Rahilly. Usually, they attribute to Maxwell Einstein's electromagnetic theory that includes his theory of light; this is defensible because usually they beautify the theories that they discuss, and Maxwell's theory beautified is indeed an achievement of Einstein.

This comes to illustrate the confusion that the oversight of the distinction is forgotten between a sketchy and a detailed version of a theory — the sketchy wave theory of light that states that light is wave motion — with any special version of the wave theory. It also comes to illustrate the uncritical nature of this confusion that allows us to pass glibly over criticisms of different versions. However many special or detailed versions of a theory may be open to empirical criticism, the sketchy version of the theory hardly ever is.

The technique by which criticism is muted can be applied to render it debunking. As the technique by which a criticized (detailed.) version of a theory is confused with a version not open to criticism is used to ignore the criticism, it can also be used to debunk a new alternative not yet put to test. Also, new criticism of a new alternative may be confused with old criticism of old alternatives: new criticism can then be dismissed unjustly as flogging a dead horse. It is therefore significant that a specific theory may be criticized while its sketchy version stands, and the sketchy version may be developed later on into a new specific one. Thus, Huygens' specific wave theory of light was criticized by Newton and others, but the sketchy wave theory of light was not criticized by anyone; Young could thus use it in order to develop a new specific wave theory of light. The new specific theory was later subject to new critical scrutiny.

The same holds for the bare doctrine of historical inevitability that is similarly not open to empirical criticism or to any criticism for that matter. Popper was repeatedly ascribed the refutations of that irrefutable theory. Popper and Berlin, then, far from flogging a dead horse, have only killed some versions, or flogged some dead versions, of a doctrine that has versions not open to criticism. Even on the assumption that we need concern ourselves with the fine logical point that I am making because the sketchy version of any doctrine is uninteresting ("meaningless", to use the "logical" positivist jargon), my point still stands: assuming that all existing detailed versions of a doctrine have been effectively criticized, some newer detailed version of it may turn up later on.

Often people are overwhelmed by the detailed version of a theory, declare it true, and when it is refuted they have the choice to change their minds or cling defensively to the version that was not refuted, often for fear of debunking, as Bacon has observed. Bacon's observation is a version of a more general one, known as the Duhem-Quine argument, although it is ancient and not an argument at all.

3. Dead horses may refuse to lie down for good reasons

Often the public — including the learned public — sticks to a specific theory in spite of its having been effectively criticized, and possibly for good reasons. Of course, the Baconian tendency is to dismiss such behavior as superstitious; but it is too easy and somewhat suspect to dismiss the whole scholarly world as superstitious. Of course, the majority, even the majority of the wisest, is not always right. But the majority of the wisest is not always unreasonable when rejecting some valid criticism. Possibly (though not necessarily) they do so without being superstitious; possibly, for example, the validity of some criticism may not be transparent and can be questioned.

Consider inductivism, the view that science rests on masses of observations and experiments, so that scientific theory rests on solid empirical foundations of incontestable *data*. Inductivism has been effectively criticized by Galileo, Hume, Kant, and Whewell, by Einstein and Popper, and by many others. Following the Baconian tradition of condemning all error one would have to condemn inductivism. As it happens, inductivism is the semi-official doctrine of science from the days of the foundation of the Royal Society of London to date. Condemnation is out of question.

One may declare the criticism of inductivism invalid, on the force of the majority's rejection of it, by discovering *lacunae* in it, or while hoping to discover one. Appeal to the majority is unacceptable; if it were, science could never have started. The mere hope to discover *lacunae* in a given criticism of a doctrine is reasonable for a while, but not for centuries. So how can we avoid condemning the whole commonwealth of learning?

Answer: The commonwealth of learning had very good reasons — though incorrect ones — to reject Hume's criticism as invalid. He criticized inductivism together with an untenable theory of causality and the theory of knowledge as such, namely, the view that theoretical knowledge is possible. Thus, in a sense, taking it literally and *in toto*, we may even say that Hume's criticism is invalid, as it overshot his target. One cannot deny that science exists, said Kant in response to Hume. (This is his famous transcendental proof, so-called.) We may say that its validity has come to light only by narrowing it down; that though largely invalid, it contains valid parts.

This idea comes very close to the objectionable theory of constructive criticism. One might claim that here criticism was correctly rejected on the ground that it is not constructive; and for Kant's reasons: we do possess knowledge. Hume tried to answer this objection: he said that what we possess is not knowledge but a semblance of it, mere habits that rest on experience. Were this reply of Hume's satisfactory, his criticism would have been taken seriously; since it is not — for it is a paradoxical theory that states that we have no theories (but merely habits) — his criticism could be distrusted. His criticism could be questioned as he had no good alternative to the inductivist theory of knowledge that he had criticized. So only with the advent of better theories of knowledge his criticism became more obvious. These are new theories of knowledge not hit by his criticism. They comprise proofs (in the strict logical sense) that his criticism is valid. But he validly criticized not the theory that knowledge is possible, not even the theory that learning from experience is possible, I but (at most) the theory that learning from experience by basing theories on masses of *data* is possible. Hence, it looks as if the defect of Hume's criticism is that it was not constructive.

No so. There is a great difference between criticism whose validity is doubtful though it may be later substantiated by constructing possible alternatives to the criticized doctrine and criticism whose validity is acknowledged while clinging to the criticized theory because the critic has not posed a viable alternative to it. Those who demand constructive criticism dismiss the destructive criticism as useless without discussing its validity. Worse, they often question the criticism and only when they find that they cannot throw doubts on its validity do they dismiss it as destructive. Not so the case with Hume's criticism. Its validity was sincerely doubted; critically-minded inductivists deemed their inability to dismiss it or to answer it a debt to Hume; they made serious, repeated efforts to answer him.

Because the validity of any given criticism may be doubted, constructive criticism is preferable to destructive criticism, even when the alternative is not viable. The acceptability of the criticism does not depend on the alternative, much less on its acceptability. The merit of an alternative if it exists is that it may elucidate the criticism and help us decide its validity or otherwise.

Often criticism hits a thesis but we do not quite know what the thesis is that was hit (this is again the publicized, purely logical Duhem-Quine thesis); trying to construct alternatives may help decide which thesis has been hit, which horse is dead, even if the alternative is definitely unacceptable, though for different reasons. As long as this is not clear, an unclear attitude towards the criticism is understandable, at least to some degree; yet as soon as the criticism does not do its job because it comes with no viable alternative, we may declare the party whose view is under debate not very interested in criticism and wonder whether it is wise to spend time on them.

4. Historicism is still alive among historians of science.

The situation is similar in the case of historicism. Some scholars cling to it because they reject all known alternative theories of history, while allowing for constructive criticism only. Yet many serious and critically minded scholars endorse it for better reasons. Consider the following case. Einstein declared that of all his friends Max Planck was one of the most civilized and critically minded, even in his nationalism. (It was historicist German chauvinism.) We should not dismiss lightly such a testimony from so staunch an anti-nationalist. So the situation calls for explanation. Although a specialized researcher, Planck knew enough about Nazi nationalism to reject it wholeheartedly, yet he remained a nationalist all his life. It is quite possible that he endorsed it as a part of his historicism that he endorsed as almost a corollary to his philosophy of science. This philosophy is, indeed, a very strong argument in favor of historicism, one that is very important for the historian of science, and one that is far from easy to criticize effectively.

The received view of science is historicist. It takes science as manifesting a rational order in its structure: its structure then is that of an ever-increasing generality (or universality) and an increasingly penetrating comprehension of increasingly minute and recondite details. This structure of science is a fixed pattern that depends on three immutable structures: of the world, of the mind that comprehends it, and of the place of the mind in the universe. On a large scale, so the received view goes, the history of science is the history of the emergence of this fixed structure of science. On a small scale there is room for variation. For instance, the discovery of Kepler's laws of planetary motions might have preceded that of Galileo's law of gravity, or *vice versa*; but on a larger scale Newton's theory of gravity must succeed both. This logic of science shows that on a large scale its history proceeds on a fixed pattern. This received view is a historicist view of the history of science, and is the same as, or but a slight variant of, Bacon's view of the ladder of axioms, the view that science must necessarily proceed from one level of generality to the while next skipping none.

This historicist view of the growth of science still is quite popular. In his contribution to the Maxwell centenary volume Planck asked, what would have happened to science had Maxwell never been born. His answer was, some other researcher would have discovered his theory. And since most theoretical electricians in the nineteenth-century were Germans, the German nationalist Planck comforted himself with the thought that had Maxwell not existed, his theory might have been discovered by Germans. (His historicism led him to disregard the disagreement between Continental action-at-a-distance and field theories.) Much later, Stephen Toulmin and June Goodfield asked the same question concerning Newton. They provided the same hackneyed answer; C. P. Snow endorsed and praised it in his favorable review of their book.

The immense and obvious social significance of science makes it easy to use this rather limited historicist doctrine of the growth of science as a foundation-stone for a more general historicist view of society at large. Take Lewis Henry Morgan's theory of society. He believed in the unity of humanity, and found it necessary to explain why the Native American society was inferior to European society, despite their essential equality. Social reform, he explained, depends on technological invention, and invention is a matter of luck and ingenuity, a gift from God: the difference between Native American and native European societies is not inherent but merely a result from the fact that some Europeans were the most lucky and ingenious at technological invention. Perhaps Morgan was no historicist. But his theory of the dependence of social change on technological development, his assumption that technological development is but a corollary of or a part of scientific development, and. the historicist theory of the growth of science, together leave no choice but to admit that on a large scale history follows a fixed pattern. This historicist doctrine allows for stagnation, and it allows progress only if by and large it follows a fixed route.

Morgan's theory of social reform as dependent on technological innovation is a dead horse. On the basis of examples from the history of science we may still claim that sometimes the opposite is true: certain social conditions are essential for invention. When some ingenious inventions took place in ancient Egypt and in traditional China, they did not intrude on the existing social order and they did not herald newer inventions. Hence, there is much truth in Morgan's thesis: the implementation of an invention, if allowed to take place, may easily lead to social change to the extent that governments may foster them. The same argument also shows that not all inventions always lead to social change — that the social change that brings toleration of new inventions must precede rather than succeed the implementation of invention and the encouragement of newer ones. Egyptian mathematics had a very strange (and fascinating) history because it was allowed to develop only within very narrow traditional boundaries, presumably because it lived in a social and religious straightjacket.

This should suffice now as criticism of one attempt to generalize the Baconian historicist view of the history of science to all history. But it is very important still, especially for those interested in the history of science, to notice other possible attempts to generalize the Baconian historicist theory of the history of science, so that unless we criticize the Baconian historicist theory of the growth of science itself we have not excluded all versions of this historicism. One possible way of generalizing the Baconian historicist theory of science to a historicist theory of society ─ other than Morgan's way ─ is Marxism. To repeat, although some invention leads to social reform, some social reform has to precede it and open the road to it; Marx's economism takes good account of this criticism: it is the claim that both invention and social reform influence each other, both being rooted in the economy, in the economic stage that society occupies. Those who are interested in economism may find a satisfactory criticism of it in Karl Popper's celebrated *The Open Society and Its Enemies*, vol. 2.

In a way Marx's economism entails Bacon's historicist theory of the growth of science, as well as a variant of Morgan's theory of progress. It also renders in a way Bacon's historicist theory of the growth of science into a historicist theory of society at large. Morgan's theory achieves this by claiming that invention is the sole source of social change. We can get this result with the aid of a much weaker theory than Morgan's, and it may well be the one not hit by the standard criticism of Morgan's theory. It would read as follows. Although some social changes, especially in their early stages, are not the results of technological innovation, when technology develops to a high level it becomes the factor that counts most; sooner or later, then, all social reform opens the door for the high level of technological development. Even this weak theory amounts to historicism coupled when with Bacon's historicist theory of the growth of science.

It was this kind of historicism, i.e. a generalized Baconian historicism, presumably, that stimulated many members of the Fabian movement, who consequently kept a much closer allegiance than the Marxists to the radical enlightenment that is at the root of all Western reform movements. H. G. Wells, the Webbs, and others, held a minimum historicist view: they assumed that the growth of natural science and technology (Wells) as well as social science (Webbs) grow in (more or less) preordained in steps of increasing levels of generality. They also assumed that people naturally tend to use existing technological or sociological knowledge to improve their own ways of living and social institutions; hence, Wells and the Webbs concluded, the development of society will lie on a (more or less) preordained path.

This is the most reasonable form of historicism, if any historicism is at all reasonable, because it adds to Bacon's historicism nothing more than that people will act reasonably in implementing the results of science in one and the best way. This very mild hypothesis is already strong enough to be open to criticism, and the root of the error it involves is the unimaginativeness behind it. For instance, it ignores the fact that applied science is imaginative. Thus, the latest consequence of social science is just this: it is often better not to implement the latest results of social science. The Fabian thesis can be modified to meet these criticisms, of course. So it is time to try to get at its very foundation — at Bacon's historicist theory itself.

Many philosophers, historiographers, students of social and political affairs and social historians, have declared historicism a dead horse, from the time Popper's writings became publicly known, and even before. We are now in the middle of a critical discussion of historicism, or of a version of it. This discussion owes a great debt to Popper, but the question is not where credit should go; the question is, is the present discussion legitimate? If it is, then the flogging of a dead horse, or a seemingly dead horse if you like, may be legitimate. Which is the point of this preliminary essay. One could retort that this discussion and its legitimacy prove that the horse is not dead, death-certificates from experts notwithstanding. But then, I should rejoinder, we may flog any seeming dead horse, because we do not know whether it is really dead. So whatever way the argument goes, if the present discussion of historicism is legitimate, then the view that all flogging of dead horses is worthless must be relinquished. Still, is my present discussion of historicism legitimate, and if so why?

It is, and for a few reasons. It is in a new context or for a new public; it is an elaboration of criticism that is to some extent novel and, I hope, interesting. It will come in handy when discussing a certain view of science — inductivism — that is rampant epidemic among historians of science and is thus a topic of the present volume: I claim that certain historicist assumptions are implicit in a widespread philosophy of science. All these reasons may be valid, proving that certain publics need to learn about old criticism. The question remains, are the best publics too in need of criticism of ideas that are out-of-date?

5. Our training for critical thinking is inadequate.

No matter how often a view has been criticized, if it is still popular with a given public, then anyone who can repeat the criticism in that public's ear is praiseworthy. This, however, is an educational matter, not research, not the discovery of new criticism. It is thus no accident that the condemnation of criticism as the flogging of dead horses is typically contemporary academic: it is an allegation made by members of a group of competent, up-to-date, talented scholars who expect themselves to know all the extant theories and criticisms concerning their own subject, who expect others to present them only with novelties; in short, these are genuinely smug people. Let us probe a little into their training and see how well trained they are as critics.

Historians of science unanimously present Archimedes' law as true; science textbooks do a so too. Historians of science have to know rather advanced mathematics and a little hydrodynamics before they can read the formulation of Archimedes' law within the modern theoretical framework, let alone notice the discrepancy here. Archimedes' law, I ask my readers' indulgence, is the statement that a body weighs in a fluid as much as it weighs in air minus the weight of the quantity of fluid that is displaced by its bulk. The floating boat, for instance, is so much immersed in water as to displace the amount of water equal in weight to its own weight in air. Thus, the floating boat is weightless; similarly, any bulk is weightless in water that has the same specific gravity as water; hence, in particular, water is weightless in water. These conclusions are usually not discussed. Rather, we are often told about the immense under-water pressures due to the weight of water (in water) that render Archimedes' law false. It is.

I must quickly pacify irritated readers: I know well enough that in the sense that this book, when resting on the table, has no weight, floating boats have no weight either, and water has no weight in water. This is a very quaint sense of weightlessness; it is neither the one intended by Archimedes, nor the one elucidated to schoolchildren or university students; yet it should be, as it is, after all, the sense in which astronauts in their sputniks are weightless. Of course, they are not: Newton's theory of universal gravity assures us of that. It is not difficult to adjust ideas of weightless so as to avoid inconsistency; physics students asked how Archimedes' law fits into Newton's mechanics can do so. Yet hardly anyone knows how much adjustment of our previous ideas we have to make here. It is clear, however, that students of the history of science who learn this develop a sense of science better than when they read in their history of science textbooks more details about Archimedes.

Where we receive our training for critical thinking is something of a mystery. That we do get it in the Western educational system is obvious to teachers who have worked elsewhere. We get it only partly at school; we get it partly from hearing our parents arguing with their neighbors (especially about politics), partly from reading unscientific critical literature (especially satire), partly from reading in our science textbooks the mock-criticism of the textbooks' mock-Aristotelian physics (Lane Cooper's heroic war against this can be regrettably declared a failure); most of us had a teacher or two who treated us as adults, who criticized us patiently, and who encouraged us to come forth with our critical comments, including the weak ones. I asked a few people whether their favorite teacher was not such a critically-minded person; they were usually surprised to discover that they had to answer me in the affirmative; their surprise surprised me.

The problem is wider. Oddly, we are ignorant of the effect of our critically-minded teachers and of our induction to the critical tradition. When I learned about Popper's philosophy according to which science is the set of explanatory hypotheses and attempts to criticize them, it appealed to me enormously. My earlier interest in the history of science led me at once to wonder how is it that we learn that the history of science is the history of validation if it is largely the history of experimental criticism (to use Faraday's phrase) of scientific hypotheses? How do intelligent people still believe that it is the history of the experimental validation of hypotheses? After all, criticism and validation are pretty much opposites. I have partly answered this question in the previous three preliminary essays: the Western tradition encourages criticism, but only uncritically: it encourages tacit criticism and discourages explicit criticism, not to mention the critical attitude. The historical reasons for this were the desire to have more experimental amateur researchers and the desire to prevent them from quarrelling, as well as the rapid absorption of later traditions of training skilled professional elites — toppled by institutionalized confusion meant to boost these social reasons, especially the confusion of criticism and debunking.

Present educational system is an immense achievement, rooted in the vision of abolishing poverty by providing general education in technology or in skills, the vision of education as the means to destroy along with poverty all degradation (Pestalozzi). The critical education system that never was very strong or popular, has consequently suffered from the first priority, and the subsequent and overwhelming success of education for skills; but not to the point beyond repair. The training for skilled researchers often leads teachers to the proposal that their students should suppress for a while their critical spirit until improves their competence. In the process they are given the best and most up-to-date ideas to absorb and they learn to manipulate them with dexterity. This is the philosophy of Michael Polanyi. It is terrific, but it is erroneous: students are not trained to improve their critical capacity as much as they should were he right. This is a pity; students can better develop views on whatever topic they study by studying some of the less satisfactory ideas to begin with and by subjecting these to severe criticism — with the aid of teachers, if need be (Imre Lakatos, *Proofs and Refutations*). This would make education into the systematic reviving and flogging of dead horses; it would also be a better schooling in the art of criticism and a better schooling in the understanding and appreciation of the up-to-date ideas. This might come as yet. That would be the day.

6. Flogging dead horses may help re-raise problems in new, interesting ways.

Consider psychology. Many psychologists endorse the theory that man has no soul (man-is-machine, epiphenomenalism, reductionism, materialism, mechanism). Some psychologists, sociologists, and philosophers, feel that this soul-less philosophy had led psychology astray. Were any of them to try to unearth the line of thought leading to it, they would easily find it to be rooted in the criticism of Descartes' philosophy, especially of his doctrine of the soul as substance. Regrettably, Descartes was forced to declare the soul immortal, and critics who dismissed the soul agreed with him on this point (namely on his theory of the soul as substance). It is obviously advisable for those who wish to reintroduce the soul into psychology to allow first for the soul's mortality. (This should not trouble religious students: they can keep impartiality for the divine soul and let psychology deal with the earthly soul.) This amounts to a return to Descartes to apply to his doctrine criticism very different from those it was historically subject to. Whether it is advisable to reintroduce the soul is a different and debatable matter; on the assumption that it is advisable, the advisability of flogging the dead horse of Descartes' doctrine of the soul is an almost inevitable corollary.

Consider contemporary atomism. It took L. L. Whyte a number of rather compressed pages to present it. But it is interesting and I wish to draw attention to his remarkable *Essays on Atomism* that is at once historical and topical — because dead horses are dissected in it with a view of finding where and when atomism took the wrong turn. Surely Maxwell's theory has been criticized often enough and thoroughly enough to render just another criticism of it entirely superfluous. But Whyte is not just another critic; he attempted to see in it the rudiments of defects that are still present, in the hope to lead to a construction of a new kind of remedy to that defect (dualism of continuous fields and discrete atoms).

This discussion may solve a puzzling problem: how is it that certain works expounding revolutionary ideas are largely historical in character? For, many a work that leads to a revolutionary change in trends of thought concerning a given topic contains a lengthy history of that topic. (Mach's *Mechanics* is the paradigm here, perhaps because of Einstein's candid acknowledgement of its influence on his early work.) We may study this profitably, if for no other reason than that many an author promises a revolutionary idea in a book containing hardly anything but a history. Sometimes the promise is fulfilled, more often not. So it is a challenge to seek a criterion to distinguish between the two. Matters would be clearer if readers knew what the function of such a history is and which way. In the historical exposition the author of a new idea may parade dead horses, but the whip ought to be substantially new.

We are often unaware of criticisms of old ideas; new criticism of criticized ideas may generate new developments; and they may do so when the purpose of the new criticism is to appraise the present problem-situation in a new way. What is common to these points is a general background discussed in the previous essays: our scientific tradition is critical, but uncritically so. We are unaware of even important criticisms of old ideas, as my example from the present views on Archimedes illustrates. Hence, *a fortiori,* we hardly ever ask whether certain important criticisms relate to dead horses or to live ones. When faced with new ideas presented as criticism of old ideas, some of us are somewhat baffled, until those of us, more at home in explicit criticism, restate the new idea differently for them. New developments often depend heavily on critical surveys of whole fields of study and of problem-situations, though this has not gained the recognition it deserves. One such critical survey, we remember, is Planck's celebrated textbooks of physics that he viewed as his critical reappraisal of the situation in the field — a survey that was crucial to his further researches. These books are hardly referred to by others, except some teachers of science in university elementary physics courses, who view them not as critical surveys but as informative sources.

J. J. Thomson's critical survey of theories of dielectricity of over a century ago may serve as another example. It is hard to judge its contemporary influence, but it greatly influenced its author's research, yet historians of science very seldom refer to it, and then not as to a critical survey. It is just beautiful. Another such a case is that of Ørsted, who was led step by step to his important discovery as a result or his highly critical survey of the theories of matter available in his times. Needless to say, it too has not yet received proper recognition despite my discussion of his work decades ago.

The reasons for the immense importance of some critical surveys of problem-situations may be obvious, at least intuitively. One of them is that it deviates sharply from Bacon's theory of the growth of science. The criticism of an accepted doctrine may lead to efforts to invent an alternative to it not vulnerable to that criticism. Hence, two different criticisms of an accepted doctrine might lead to two different lines of study. Suppose such a doctrine was hit by only one criticism, leading to the less desirable development. One may, then, raise the second criticism in the hope of seeing the second development emerging instead. My examples from psychology and from atomic physics may illustrate this.

7. Flogging dead horses may help present new interesting problems.

Consider again an obviously dead horse, Lenin's myth that monopoly, the highest stage of Capitalism, impedes the marketing of new products that result from new inventions. It is popular. Suppose that one wishes to solve the problem, why is it popular despite its obvious falsehood? (If an error is hard to criticize, then its popularity among reasonable people is less puzzling than if it is easy to criticize.) Presenting the problem forcefully helps present the solution effectively, and to do so it is useful to show that the myth is popular and easy to criticize. One has, in other words, to flog a dead horse, and even to flog it very hard.

Lenin's myth met with strong empirical and theoretical criticism: often an invention is prematurely marketed — perhaps in order to reduce the cost of research, perhaps in order to pump products into an over-saturated market, perhaps in order to exploit people's vanity, credulity, or love of novelty. This evidence against Lenin's myth abounds, yet no avail. This popularity of Lenin's myth rests on some evidence supporting it, on examples that make it hard to resist it. So one must take the detailed examples and show how easily open to criticism these are.

Lenin's examples in support of his myth are forgotten, because they are too obviously false. One concerns a new method of mass-production of bottles; by now much newer methods are massively employed. Another concerns a match that can be used repeatedly an indefinite number of times, but the patent for it has been bought and "pigeon-holed" (this is Lenin's expression) by the matches magnates who feared that its introduction into the market might put them out of business. The repeatable match is a lighter, of course, and lighters are older than matchers. The same holds for unbreakable glass, durable nylons, sturdy cars and other commodities. Almost anybody even mildly interested has a few examples to add. Industries did not fear competition from better products. They adopt them.

Lenin admitted that the rapid economic growth of our age makes the manufacturing of durable goods profitable because the markets cannot be easily saturated while growing rapidly. As to the less durable goods, they are often preferred to their more durable equivalents; the preference for some purposes of breakable glass and ordinary matches over their more durable equivalents may be less obvious than the preference of safety razors over razors and electric shavers, but this is merely the result of mystification. Nevertheless, inbuilt obsolescence does exist. Does it prove Lenin right? This is where uncritical and critical thinking clash: the one seeks instances, perhaps impressive ones, the other seeks refutations.

My aim is not to criticize Lenin's myth, but to show how vulnerable to criticize it and its supporting examples, so as to present the problem, why is it popular nonetheless? As its popularity rests largely on examples, and as all examples of this kind are very easily open to criticism, how is it popular nonetheless? The aim of presenting this problem was to present an example that necessarily involves the flogging of a dead horse — the presentation of criticisms of an already refuted doctrine — though with the stress on the obviousness of the criticism. This obviousness is seldom the point of any critic of Lenin's myth, because the critics are usually not concerned with the popularity of the myth but with its falsity.

The myths within the history of science are mostly of the same ilk. They link great discoveries with everyday events that their discoverers have presumably experienced. Pythagoras passed by a blacksmith hitting an anvil. Archimedes took a bath. Young Galileo looked at the oscillating chandelier in the cathedral. Young James Watt saw the lid of his mother's kettle rise under the impact of boiling water. Signora Galvani ate frog legs by medical prescription. And Newton observed an apple fall down to earth. As these are daily experiences, one can hardly doubt that they happened as narrated. The question is, what is their function in the general scheme of things? By the way, they are fading out; the last effort to grope with it is Stephen Toulmin's 1959 effort to make sense of Newton's apple. Historians of science rightly ignore it, and they wrongly ignore myths altogether. They have a point though: there is a great difference between popular myths, even when associated with science or technology, and the scientific tradition proper. For, as popular myths, they barely differ from the other popular myths, including the myth that our experts do not waste their valuable time flogging dead horses, and the popular myth that as science grows inductively it fosters no myth — the grand myth created by Bacon that is behind so many myths associated with science.

The myth of induction was tamed by the recognition that scientific ideas are invented. This recognition, due to Whewell and Einstein, is known by Hans Reichenbach's stilted expression as the distinction between the context of discovery and the context of justification. It is not justification that is at stake but the tests that a new theory has to undergo; if it has to be justified, then its justification is that it explains known phenomena best. There is no other justification, said Einstein. And as this justification is insufficient, the theory wants a test. Discovery is not of an idea, as Reichenbach's terminology may suggest, but of an observation. The idea that discovery is due to attention to what happens around us is an inductive myth. So is the idea that theories emerge from observations. By contrast, the view that theories are inventions presents discovery as the result of tests.

But is it not commonsense to surmise from the observed to the unobserved? And is this not induction? Yes; twice yes. The myth of science is exactly the idea that science is seeing you wince and surmising that you are unhappy. And as Watt could not surmise anything about steam engines from looking at people, we can conjecture that he saw a kettle's lid. Science is not like that: it is not taking theories for granted and using them to surmise but criticizing old theories and developing new ones and criticizing them too.

A few factors sustain inductive myths for centuries: naive optimism, mock-criticism of popular ideologists, and, most significantly, the uncritical attitude of science public-relations functionaries (including historians of science). These advertisers of science appear as its spokespeople in the public eye and they cultivate public refusal to correct wishful thinking. The naïve optimism and the rise of new and serious threats to survival — from the Proliferation of weapons of mass destruction, Pollution, Poverty and Population explosion (the 4 P's) — fuel the suspicion that a conspiracy is afoot: science can and should bring salvation but dark forces are in the way. The advertisers of science tell the public what technologists are trying achieve but not what the obstacles on their way are. They thus lead the public to expect improvement as a matter of course. This finds its expression in such statements as, if we only got more money put in education and in research, then we would have more spectacular innovations. Such statements are made even by serious scholars such as Robert J. Oppenheimer. These unwittingly reinforce the conspiracy theory as the explanation of the failure of the promised success.

The case of commercial innovations that are expected yet do not appear is similar to the case of non-commercial ones, except that in the latter case the scapegoat is not as readily available. An example is the disappointed hope for controlled nuclear-fusion. As the case remained mystifying, the general public could be expected to blame a scapegoat — if it could only find one. It could not. There is a difference even optimism between the two mythologies, however. There are doctrines designed to solve certain problems, and they can be reinforced by some scapegoat mythologies; and there are scapegoat mythologies whose sole purpose is to reinforce given doctrines. Bacon's doctrine and likewise Marx's, come to solve certain problems, and to that extent they are valuable, even though they fall back on scapegoat myths. Not so Lenin's doctrine: it is but a scapegoat myth and is thus intellectually valueless. Paradoxically, the way to show that one doctrine is more valuable than the other is to criticize both, and contrast the criticism of both.

8. Inductivism flogged again

The classical criticism of inductivism was not generally recognized, as empirical science grows despite it. A new view of empirical science (Popper's) not hit by the classical criticism has removed the chief reason for suspecting that it is invalid. For that the convincing power of the alternative or its absence is irrelevant: if any theory not vulnerable to the criticism of inductivism allows for the possibility of empirical science, then the existence of empirical science ceases to throw doubt on the criticism. The only good reason for the current rejection of the classical criticism of inductivism is gone. This is why nineteenth-century inductivism invites broad-minded criticism and its twentieth-century heir invites debunking: Popper has meanwhile constructed a non-inductivist theory of the growth of knowledge. There are different ways of presenting Popper's theory as such an alternative; my preference is to do so by applying it to the history of science. Admittedly, if we reject inductivism for classical reasons, there is little or no need to accept Bacon's historicist theory of the growth of science — his ladder of axioms. Yet, often the situation calls for a reverse procedure. Popular philosophers of science often dismiss inductivism and proceed with the endorsement of the inductivist ladder of axioms. Naturally, they end up inductivists proper.

Looking back at an old theory that once gained acceptability, we may recognize a variety of criticisms of it with ease. Often the criticism of the new theory hits also the old. To take a very simple example, the criticism of Davy's theory according to which all combustion involves oxygen or chlorine is *a fortiori* a criticism of Lavoisier's theory according to which all combustion involves oxygen. Since any halogen and alkaline metal will burst into flames when put together, both theories are easily refuted.

The ladder of axioms is impossible. The development of a new theory somewhat depends on the criticism of the one it comes to replace, and this is not uniquely determinable. It is largely a matter of luck where an effort to criticize begins. It partly depends on luck; a book containing some criticism may remain unpublished, or its printed copies may be destroyed in a publishing house just prior to distribution, or it may fail to capture the public eye due to its author's careless presentation, crude style, or bitter style. The purpose of the Western publication tradition is to minimize such mishaps but no institution is fool-proof and we have historical examples of such mishaps. Had Leibniz published his logical works, Kant's work as we know it would not have appeared in its familiar form. Had he published his daring ideas, we might have experienced quite a different history of psycho-physics, of non-Euclidean geometry, and of political philosophy. Likewise, had Cavendish published his discovery of dielectricity at least Faraday's researches of the year 1832-36 — and these were of crucial importance — would have been different. As things happened, Cavendish did not publish his a material, and Coulomb's theory consequently remained beyond criticism for another generation or two; it took deep roots, and then even when Faraday quoted Coulomb against dielectricity and showed Coulomb's error, his popularity could not vanish in one go. Thus, history depends on questions of the form, what criticism of a given doctrine came first, and on the answer to it that led to the story of how that particular criticism (rather than alternatives to it) was developed and publicized; and so to the trend of scientific development emerging from it. Conceivably, a researcher would examine the history of a development and decide not to bow to it. The example from the history of science that comes in handy here is the history of optics.

Newton's criticism of Huygens's theory of light was devastating. Why did Young revive it? He was not blind to Newton's criticism; he tended to be just to it (as he sincerely admired Newton). Historical records definitely corroborate this surmise. Nor was he dissatisfied with Newtonian optics. Historical records are very ambiguous on this point. As Sir David Brewster has shown in his life of Newton, Young's hero-worship of Newton (and perhaps, may I add., his sense of guilt towards him) led him into extreme vagueness on this point. But he was very impressed by the high explanatory power of Huygens theory, at least as compared with the explanatory power of Newton's theory. In the face of immense hostility, he received formidable support from one of the most careful researchers of his age — William Hyde Wollaston — who without endorsing any theory corroborated Young's claim concerning the impressive explanatory power of Huygens's theory. Similarly, Young's greatest opponent, Laplace, attacked chiefly his claim to have shown that the wave theory explained diffraction. In modern parlance this shows Young as having demanded an explanation of the success of Huygens's doctrine (as an explanatory doctrine) and suggested that by re-examining it and by criticizing it from a different angle one may shift its strong point from its weak point and thus alter the development of optics. And so he did. And Laplace disagreed with him not on this but on the question explanatory strength of Huygens' theory.

The zigzag history of optics from the seventeenth to the twentieth century, Einstein's publication of two papers in 1905, one belonging to the wave school in optics and one to the particle school, put the ladder of axioms way out. It is no accident that almost every book that endorses the ladder illustrates it by the same example of classical physical astronomy. Whewell, the great philosopher and historian of science of the early nineteenth century, illustrated the ladder with a few instances; they are all gone, with the exception of physical astronomy, and that example too rests on the logical error that Whewell discussed: Newton's theory contradicts both Kepler's and Galileo's , yielding them as approximations.

The zigzag history of optics, or of the theory of heat, for another example, show that there is much truth in Bacon's theory: something like that ladder is the systematic development in science towards ever increasing degrees of generality. Nevertheless, it is not as one dimensional as Bacon and Whewell said. Under the force of Newton's criticism Huygens theory was dismissed for about a century and was later revived in order to be reexamined in a different fashion. This devastates the one-dimensional ladder. Of course, we may now construct a different ladder to replace Bacon's. This was attempted, once by Duhem and once by Popper. So the criticism of Bacon's ladder need not be evaluated in the light of the acceptability or otherwise of alternatives to it. To improve upon it we need a ladder more flexible to variations prescribed by accidental historical circumstances. This will dispose of the historicist character of Bacon's ladder.

Bacon's ladder looks very obvious, convincing, and unproblematic. Now that it is gone, what should historians of science do? Should they neglect historical studies in search for the literature on Duhem's and Popper's alternatives? Is it not better to ignore the discussion that has become so rarified? It is not necessary for historians of science to be philosophers as well, yet they should decidedly give up Bacon's ladder. Its endorsement impoverishes their studies. Consider again the problem, how was wave optics revived? It is interesting. That no one tried to re-examine Huygens's theory of light before Young did is understandable: everyone else took it for granted that Newton's optics was deemed a satisfactory explanation of known optical phenomena. Why then was Young dissatisfied with it? In his earliest publication on this he offered a list of criticisms of Newton's theory. None of these was clear-cut or obvious; presumably, once he found one possible criticism he collected as many more as he could (rather than that he found them all simultaneously). Which one did he find first? What raised his suspicion? And why did he come to think of such a major change rather than try a minor alteration as everyone else in his time would?

These questions were never asked. So they were never answered. Only a remark here or there may be construed as a kind of an answer to one or another of them. In particular, the two extensive lives of Young in the English language contain no reference to, or discussion of, any of them: the zigzag character of the story of optics embarrasses them so much that they were too busy making unnecessary apologetic remarks to be able to raise and discuss interesting questions. The central question here is, how did Young come to be dissatisfied with Newton's theory of light and try to reexamine Huygens's theory of light? The literature on the revival of the wave theory of light put together amounts to thousands of pages. There is less than one page on the questions raised here. Considering how obviously satisfactory Newton's views were generally considered, this is quite intriguing and worthy of study. For it Bacon's ladder of axioms has to be given up. More generally, the rejection of Bacon's ladder may open up for those interested new avenues of historical studies — of the studies of the zigzags of the history of science (as permanent features, not as aberrations).

9. The moral side of an intellectual issue is better avoided.

The charge that critics flog dead horses is largely an innocuous attempt to dismiss them, an attempt by members of the professional elite who do not feel the need for criticism and who anyhow have very little time to read as they are so busy writing. Hence, the charge is meant not as reflections on authors but as excuses of representative members of the clan under criticism who has no time to read. The same holds for the charge that critics knock down straw-men. Strictly, dead horses and straw-men are different: the dead horse was once alive and kicking; not the straw-man. This difference is historical. Though it may be of interest to historians, it is of no interest to those who wish to make excuses for their shunning critical studies. Both kinds of charge imply unintended accusations, and those implicit in the one are different from those implicit in the other. Yet, the confusion of the two is excusable, since the implicit accusations are unintended, and since the accusers suffer from a sense of guilt: otherwise, they would not make excuses to justify their not having read the critical works in question. It is a trivial psychological observation that people who feel guilty are prone to make unintended accusations in a sort of lame self-exoneration. These are better ignored. Inbuilt in our education system is the disposition to inflate people's sense of guilt for educators to use it in order to better control the situation so as to better discharge their sacred duty of enlightening their snotty charges. The sense of guilt thus has an aim, not a cause: ignorance is poverty and one need not feel guilty about the causes of one's poverty. The education system is geared to the use of our sense of guilt about our ignorance, distraction, and laziness. The system is supposed to spur us into working harder for our own good. Even our uneducated schoolteachers know by now that laziness is rare and pathological. What they mean when calling their pupils lazy is merely that they wish them to try harder. The educational system is thus suffering from a disbelief in the idea that people can enjoy receiving enlightenment. For, criticism can be enlightening, and as such enjoyable. It is only confusion that makes people feel displeasure when subject to liberating criticism. Many intellectuals, especially academics, carry the burden of educational pressure from childhood and consequently feel guilt and insecurity when hearing about unfamiliar books; that feeling they try to dismiss by accepting all too readily attempts to put them down. It is futile to discuss accusations that are unintended except when they are quasi-official efforts to wreck the status of critics whom they judge too successful. In any case, the only radical measure against all this is to combat the educational methods that inculcate a sense of guilt and the fear of criticism.

Of course, a critic may be flogging a dead horse in efforts to display sham originality: we academics are all at risk from this pitfall. How can we judge, then, when such a claim is valid and when not? We officially demand of Ph. D. dissertations that they should be original and we often enough claim that academic periodicals and monograph-series contain only original material; writings of such dissertations and of such papers are these days conditions for appointments. We thus sustain the pressure on ourselves to display originality, preferably true, but alternatively sham. And as long as we do not institute, or even discuss, criteria of originality or of novelty, we pressure ourselves to be pretentious. This much we all agree upon: no study can be totally original, and we do not call original a fairly routine work. Thus, we can freely call a work original or routine: it may then depend on our intentions according to our whims. It is hardly surprising, then, that this invites confusion: it is surprising that the situation has not got completely out of hand. The best way of handling the charge that one is flogging a dead horse, then, is rather obvious. It is to disclaim originality when appropriate, and to invite those who make the charge to present their criterion of novelty. The suggestion that we disclaim originality whenever appropriate is, of course, the suggestion to acknowledge indebtedness to predecessors when possible, including the acknowledgement of criticisms. This is an obvious proposal, especially here, in a volume written for the historically-minded and for the ready to appreciate criticism. Yet an already mentioned technical objection wants an answer. If in one acknowledges all of one's ideas to others, then one's output will be rejected as unoriginal. This is very serious indeed, since doctorates and scholarly publications are academics' most important of means of livelihood. It is therefore very comforting that the system is not consistent here: most of the more scholarly academic works contain such disclaimers of originality, even if only implicit, and yet their authors have managed to publish them nonetheless. Perhaps some more pretentious scholars publish more easily and do better regardless of the quality of their output, but this is not the point: if one wants to be well-off, then one is well advised to leave the academic market-place anyhow. The discussion here concerns our need to keep our meager academic jobs. (One rejection slip I received let me report, was accompanied with an editor's reader comment: "Agassi himself admits that his paper is unoriginal". Nevertheless, after a few efforts, the paper was published in an obscure journal, with my explicit admission. My case is atypical, however, as I never depended on editors: I always had too many publications for my own good.)

My proposal to silence the dismissal of unoriginal works by asking for a criterion of novelty is a bit dangerous since no criterion for originality has won general recognition. We can thus all be silenced this way even when we ought to speak up. Yet since the charge of unoriginality implies that we ought to be original, this matters little. It is odd that sometimes the wish to avoid being nasty and making personal charges leads to doing so in excess. As reviewers wish to acknowledge some criticism, they may do so saying that the horse is dead. They thus unintentionally cast aspersion on their critics.

Intellectual honesty and honesty are not always the same. A person may be intellectually dishonest without being a dishonest person and *vice versa*. To attribute to opponents silly arguments that they repudiate and to ignore their stronger arguments is intellectually dishonest. Young students of promise may become Marxists, often driven towards this by both the intellectual dishonesty of their environment towards Marxism and their inability to distinguish between intellectual and moral dishonesty. This inability comes from the underestimation of the difficulty that lies on the way to becoming a good critic, and this underestimation comes from our educators' inability to distinguish between criticism and debunking. When we learn to appreciate our elders' honest inability to notice the strong points of Marxism, much less to criticize it, and their intellectual incompetence that makes them choose weaker parts of Marxism and distort its stronger parts, at that stage we are beyond the adolescent urge to praise Marxism on the ground of our elders' injustice to it.

What our elders do as they debunk mock-Marxism instead of trying to criticize Marxism is known as knocking a straw-man. The charge that critics knock down a straw-man is much more serious than the charge that they are flogging a dead horse, because repeating criticism of an important error is I superior to repeating criticism of a silly error. Yet this kind of charge is often made rather casually. This casualness testifies to the lack of appreciation of its seriousness, but it does make the situation less easy to avoid. Perhaps there is no choice but to speak only to those who know the value of criticism, or to explain the whole situation. But there may be simpler, more readily available technical means: we may quote authoritative statements by classical authorities or by other authorities, and discuss their authority, their impact, their significance, etc. This may be cumbersome and involve irrelevancy. Conant, in a most charming passage, tries to prevent the claim that he is knocking down a straw-man by confessing that he himself used to hold the view he was criticizing. This technique is not open to free use by all. The easiest and most advisable method is, perhaps, to begin with a problem, and to state one's intention to present better and better, or stronger and stronger, solutions to it, and criticize them in turn. In such cases knocking down a weak theory will be a preliminary or preparatory to presenting and knocking down a better one.

There is no foolproof method against accusations. Often one reads a very convincing and smooth criticism of one's opinions. Confusing criticism with debunking one overlooks the ingenuity that went into the criticism and the smooth presentation of it. One concludes that the view under scrutiny is rather stupid since the criticism of it is rather obvious. Being no fool, one concludes that one has never seriously upheld such a folly. While reading the criticism of one's view one readjusts one's views while wondering who on earth has ever held the ideas under scrutiny and why should the clever author be spending so much time scrutinizing them and taking them to bits. Such a fit of mental acrobatics seems almost impossible; its commonsense, however, makes me marvel at the possibilities open to those free or inhibitions that divert our best mental abilities to such useless channels.

Since it is not easy to readjust one's opinions while under criticism, much less to do so unconsciously, the result of such activity is often that the readjustment is ephemeral, that as soon as the pressure of criticism terminates it is forgotten and the old opinion restored. More acute people, however, merely confuse the old and the new views — enough to exempt themselves but not so much as to be unable to teach the newer ideas. Among their students, the bright and clear-minded will write textbooks that do not betray their teachers' slight confusion; everything falls back into place and the stage is ready for a newer criticism and newer adjustments. The process is impeded — one major adjustment per generation, so to speak, but no more — and the history of thought suffers a set of distortions.

The slightly confused do get irritated by critics who seemingly knock down straw-men. This is a measure of honesty, since it gives the show away: watching a straw-man knocked down does not annoy us except, at the utmost, by boredom and waste of time; poor and boring books hardly annoy academics, especially since they learn fast not to read them from cover to cover. Books that get under their skin are different: they report that they find it annoying to see a good mind going to waste by frivolities like knocking down straw-men. This is the experts getting annoyed at themselves for their wasting so much time in the rut.

10. A plea for a critical history of science

My purpose in flogging the dead horse of inductivism is to dissuade historians of science from employing this philosophy and the techniques traditionally associated with it, and to dissuade readers from attempting to read them and from feeling guilty due to the failure of such attempts. My criticism is thus chiefly destructive, and in the full sense of the word. I also wish to contribute within my powers to the destruction of the dangerous popular myth that science is always right. Most extant studies of the history of science still nurture it, be their authors inductivists, conventionalists, or eclectic. Inductivism has a glorious past, but it is gone. Historians of science do not have to formulate a philosophy of science in order to be able to write interesting studies. All their need is free reins.

I also present here the application of the critical philosophy of science to its history. I do so partly from not wanting to conceal my alternative views on science and its history for what they are worth. Partly I do so because, even for those who are ready to take destructive criticism while rejecting my alternative, it may be easier for them to read my criticism with the aid of some familiarity with my alternative. But I do advocate an alternative; I will be gratified if some competent people would try to write histories of science with this alternative in mind and some will criticize it. My reasons are varied. I think the alternative will lead to the better appreciation of criticism and of its place in research. It might render the history of science more interesting, at least in allowing the raising of new and interesting problems and offering new and interesting solutions to them — as well as to some older problems.

Let me also advocate one technical point before concluding the present essay. I suggest that historians who wish to take up my challenge should also try to apply the historical method, that is, the method of presenting and discussing all past solution to given problems before presenting their own solutions, even if these are dead horses. Again, I will not elaborate on this point here except to say that to some extent the method might insure some avoidance of some past errors. Let me remind the readers again that my primary aim here is destructive, and that its constructive part is utterly subsidiary.

CONCLUDING PRELIMINARY ESSAY: ON THE SIFTING OF THE GRAIN FROM THE CHAFF

The tendency to identify the professional with the expert, and hence the amateur and the dilettante, rather than seeing the significance of proficiency as against the accidental way that anyone makes a living, has led to the myth that from a serious intellectual viewpoint the professional and the popular literatures are the grain and the chaff. This is a grave error, especially in view of publication-pressure. The identification of the professional with the expert is partly justified by the current increase of both egalitarianism and standards of living. For, with the reduction of the leisure classes and its significance, proficient amateurs become rare; and with the increase of the level of skill of the productive classes, the professional becomes increasingly proficient. Egalitarianism pushes the more proficient workers to higher positions in the professional world. The situation has thus rendered the identification of the expert and the professional so obvious that we no longer expect others to understand the professional literature, despite efforts to master its language. One of the cleverest contemporary philosophers of science, Michael Polanyi, has used this despair of the amateur as the basis of his philosophy. He said, scientific method is as indescribable as every highly developed skill is; if you want to learn it you must apprentice yourself to a worthy master.

This is untrue: the tradition of science in the eighteenth century had no apprenticeship the way Polanyi had in mind. Amateurs then were able to follow the scientific literature without much prior training, mainly because at the time science was free of mystique. Nevertheless, Polanyi is right about the significance of the traditions and institutions of science. He declared that traditions are personally transmitted from master craftsman to apprentice, although both are unable to articulate it. This renders the mystique the most important element in the tradition. Not so. The traditional institution of the commonwealth of learning was simultaneously a republic and a parliament. It was its own institutional reformer. Institutional reforms were implemented in the commonwealth of learning in the nineteenth century, and on the whole with stupendous success. One unintended consequence of this, however, is the ever-increasing flood of worthless publications; the risk involved in this development is that the good publications may be drowned and lost among the worthless ones. We need a new reform of our traditions to cope with the new problem of how to avert this drowning, though, of course, the new reforms may raise new risks. The invention of the internet gives the problem a new character and makes it exciting to explore.

The question, as always, is whether the risks entailed by the existing traditions are serious enough to justify reform and the risks it entails. At least in education and of training researchers(including the study of the history of science) the risk of the existing institutions has come too close to realization.

Specifically, we must educate our young to be able to distinguish between the grain and the chaff, to be able to find the better works in any field of study that might interest them. For this purpose we should institute new educational devices and new scientific traditions.

As a first step some of us should advocate publicly a more conscious critical attitude. In particular, some of us should fight the readiness to be impressed by highfaluting talk, humbug, and expert mystique. When bamboozled, or when bored, we should all be ready to confess frankly, openly and boldly that we feel being bamboozled or bored — and if necessary take the consequences. It is not easy for members of the public, much less for moderators, to say to renown and honored guest speakers that their performances were on an unacceptable level. Yet such things did tale place, and they can be done amicably or at least with as little embarrassment as possible.

Nevertheless, let me end these preliminaries on the positive. The field of the history of science is parasitic on science, and to be balanced it should also be parasitic on general history — social, political and cultural. As a parasite, it is the loveliest *bouquet*. This volume lavishes praise on quite a few wonderful studies that illustrate this fact. And their number has increased in the last century, and more so since my early adolescence, when I fell in love with the history of science. Those who complain that my work on it is destructive should initiate a constructive work, to a survey of the best in the field, opening with the ancient histories and ending with the latest and best.

II. TOWARDS AN HISTORIOGRAPHY OF SCIENCE

INTRODUCTORY NOTE [1]

The history of science is a most rational and fascinating story; yet the study of the history of science is in a lamentable state: the literature of the field is often pseudo-scholarly and largely unreadable. The faults that have given rise to this situation, I shall argue, stem from the uncritical acceptance, on the part of historians of science, of two incorrect philosophies of science. These are, on the one hand, the inductive philosophy of science, according to which scientific theories emerge from facts, and, on the other hand, the conventionalist philosophy of science, according to which scientific theories are mathematical pigeonholes for classifying facts. The second, although some improvement over the first, remains unsatisfactory. A third, contemporary theory of science, Popper's critical philosophy of science, provides a possible remedy. On this view, scientific theories explain known facts and are refutable by new facts.

The inductivist philosophy of science has been criticized by Galileo, Kant, Einstein, Popper, and many others. And the inductivist method of writing the history of science was superseded by Duhem around 1900. It is not my intention to flog this dead horse, but to show that its carcass is, as it were, still harnessed to the band-wagon on which the majority of present-day historians of science fancy themselves to be riding. Some of the notes assembled at the end of this essay may illustrate the latter point. My target is not the horse but the band-wagon itself; I wish to draw attention to the fact that far from being able to progress, the wagon is being dragged backwards by the dead horse to which it is tied. It is the few historians, especially of Koyré's school, who have departed from it who — almost alone — are truly advancing the study of the history of science.

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CORRECTIONS

This pleasing occasion of reissuing my *Towards an Historiography of Science*, is also a pleasing occasion to acknowledge corrections of some of its errors. This is no survey of the many reviews and other comments that it won, highly complimentary as many of them were and quite dismissive as some others were. (Also, one was unkind despite its generous compliments, and one was caustic.) All of their authors have my gratitude. My task here is to respond to some criticism. I say some with no disrespect. For, most of the comments were reasonably philosophical, and these need no response here, at least not positively so: as a philosopher and a guest in the territory of the historians of science, I can refer interested readers to my philosophical output for elaborations on my responses to philosophical comments. As to the borderline between the philosophy and the history of science, Maurice Finocchiaro has done me the great honor of having discussed (*History of Science as Explanation*, 1973) the philosophical aspect of my historiography with great acumen and much better than I could. Both his assent and his critique are very gratifying. I cannot thank him enough. My own philosophical criticism of my historiographic work appears below, in the first essay after this one, "A Retrospect".

Much of the historiographic criticism is the same: I provide too negative an impression of the field that until recently was non-existent or amateur and that is now professional and much better than I describe it. In brief, my essay is quite out-of-date ("hopelessly" a recent commentator mused). I confess I do not like this kind of comment: it is rather defensive. As an amateur historian of science, I am not particularly charmed by professional yen for the latest. I like old books, and find the efforts of historians today to be up-to-date somewhat funny. For, the whole point of history is that the up-to-date does not drive the dated to oblivion! If we have room in our field of vision for Lavoisier, then surely we also have room there for Meldrum on Lavoisier! So I was incredulous when Bernard Cohen told me that when he first planned to republish the works of admirable inductivist historian of eighteenth-century chemistry Andrew Norman Meldrum, Guerlac opposed the idea: Meldrum was not sufficiently up-to-date. Maurice Dumas reports cryptically his sharing Guerlac's view on Meldrum as rather out-of-date (*Isis*, 48, 1957, 186). Fortunately, Cohen finally did publish *Andrew N. Meldrum: Essays in the History of Chemistry (The Development of Science)*, 1981. I find inductivist historians of science of the eighteenth and the nineteenth centuries and even some twentieth-century ones (like Meldrum) — all amateurs — quite appealing, and I am less enthused by the output of their heirs — mainly professional. I feel no need to document my love of the field and my deep admiration for many up-to-date historians of science, older and younger, independently of my assent to their philosophies or my dissent from them.

Much of the criticism I met is of my choice of errors to criticize: I wrongly chose ones that have already been corrected (I flog dead horses), and ones that no one has propounded (I knock down a straw man). I discuss these at length above, in my *Chroniclers in the Courts of Science*.

Much of the historical criticism that I met is rather philosophical. I choose the most conspicuous: the review by Roger Hahn ("Reflections on the History of Science", *J. Hist. Philos.*, 3, 1965, 235-42). I ascribe to Laplace the Baconian view that the chief obstacle to the progress of science is prejudice and superstition. Hahn corrects me: it is not these but religion. I find this correction surprising. Hahn assumes wrongly that I censure Laplace and other giants of the past, merely because I do not share their philosophies. (None of my commentators notices my admiration for these old giants, especially my profound admiration of Bacon, merely because I often present his views as obviously false and his influence on contemporary historians of science as detrimental.) Presumably as an expression of dislike for my alleged censure of the giants of the past, Hahn says, "To understand their limitations, we must examine all of their prejudices." No: censuring them is wrong regardless of efforts "to understand their limitations". And the demand "to examine their prejudices" is excessive; it rests on Bacon's view that Hahn says he rejects. He does not live up to much less stringent a standard. Had he examined the philosophy that Laplace had advocated even cursorily, he would have realized that Laplace viewed religion as a supreme superstition. There is no difference between my view of Laplace that Hahn rejects and the alternative to it that he advocates.

The most historical comment is of William A. Smeaton (*Annals of Science*, 1962, 18, 125-27). He corrects my complaint that the Alembic Club Reprints are out of print. I am glad they are available and sorry for my error. He also censures me for my praise for Dr. Thomas Thomson in disregard to the easily available information that he plagiarized. To mention this I should have discussed the standards of acknowledgement accepted at the time. This I did not wish to do then. Perhaps this was an error of judgment on my part. I retain my praise of Dr. Thomson nevertheless, despite all failings. As I hardly gave my reasons for my praise, Smeaton is not to blame for not having discussed them. But I do cite from Dr. Thomson enough that is hardly stolen and that is obviously delightful. Smeaton should have no trouble appreciating it, as it is orthodox Baconian all the way.

And then there is an odd item: Kuhn's review (*Brit. J. Phil. Sci.*, 1966, 17, 256-8). He compliments me most. I am sincerely flattered and grateful. He also says, "Agassi often chooses men who would agree almost entirely with his historiographic theses (there are many of them), borrows heavily from their work, and then castigates them for missing points that he has seen there himself." This is less pleasant, but then Kuhn repeatedly declared agreements ("Agassi's historiography, which is very close to my own,") and belittled disagreements (see my chapter on him below). He did not offer an example for my alleged systematic plagiarism. The sentence next to this charge is another: "No inductivist has been more guilty of attributing all mistakes of fact or interpretation to errors of method. The historian who fails to find in his material what Agassi discovers there is quickly labeled an inductivist, conventionalist, or some bastard mixture of the two. Contemporary historians of science can profit from criticism, but Agassi's blunderbuss is aimed at the wrong targets and too often misses even them." I respond to much of this above in my *Chroniclers*.

And so I come to the sharpest criticism of my essay. It is a brief passage tucked among the notes of admirable Edward Rosen to his *Three Copernican Treatises*, 3rd edition (1971). I do not like his oversight of my criticism of the inductivist technique that he excelled in, not to mention his seriously objectionable scientism. But this is beside the point, which is my task to endorse his criticism as much as I can. To that end I cite him in full. I number his seven criticisms in square brackets.

> [1] Copernicus "merely said that Ptolemy's system has too many epicycles" (p. 5). [2] "Tycho Brahe's theory is this: what one chooses as the centre of the universe is entirely arbitrary" (p. 13). [3] "Brahe's disturbing idea that the centre of the universe can be wherever you like is a candidate for the title of precursor to Einstein" (p. 14). In his 195 footnotes on 38 pages (pp. 79-117) Agassi does not tell us where Copernicus "merely said" what he never said, and where Brahe precursed Einstein. [4] "One could not plot ellipses easily without knowing a certain amount of mathematics (Kepler used the newly invented logarithmic techniques)" (p. 53); in 1605 Kepler arrived at the elliptical orbit, which he published in 1609, before he ever heard of logarithms. [5] "Kepler's use of the logarithms was inessential though very helpful for his purposes, [6] as were most if not all of Brahe's records which he made use of" (p. 53); according to Agassi, all of Brahe's records were unessential for Kepler, who said "I build all of astronomy on Copernicus' hypotheses about the universe; secondly, on Tycho Brahe's observations; and finally, on the observations of the science of magnetism of William Gilbert the Englishman." [7] William Cecil Dampier-Whetham "never took his information from the primary source" (Agassi, p. 15). In the case of Copernicus, Dampier (4th ed., pp. 110-111) cites the primary source, the *Revolutions,* in part from his own *Extracts from the Writings of Men of Science to Illustrate the Development of Scientific Thought,* as he and his daughter subtitled their *Cambridge Readings in the Literature of Science,* reprinted as a Harper Torchbook in 1959. The perpetrator of the atrocious inanities mentioned above has the unmitigated impertinence to hurl the epithet "pseudo-scholars" (p. 5) at eminent writers who do not happen to share his muddled methodological predilections, and to proclaim that he is "trying to explain the low standard of work on the history of science" (p. 77) and to improve "the present lamentable state of affairs in the field of the history of science" (p. 78).

This is authoritative and compelling. Rosen presents me as a trigger-happy cheeky youth, which is not far from the truth, I am afraid (it was almost half a century ago). He says I have "muddled methodological predilections". My "methodological predilections", I stubbornly insist, are less muddled than his, or less muddled than his pretense that he has none, as you will. The passage allows readers to surmise that my errors on the period of his expertise are a fair sample of my errors about historical matters in general, which for all I know may be true. Let me take up the details, then.

[1] I misascribe an opinion to Copernicus. Rosen says, Copernicus "never said" what I ascribe to him. Perhaps my reading is too liberal, I cannot judge. But I am not the only one. A debate on this developed largely after I published my *Historiography*, so let me not rely on it. I relied, I confess, on my possibly too free reading of Burtt and of the original (Edward Rosen, "The *Commentariolus* of Copernicus", *Osiris*, 3, 1938, 123-41, 123-4).

Rosen does not mention the context. It is my claim that often historians of science within the Baconian tradition refer to Copernicus vaguely because despite his considering the multitude of epicycles in Ptolemy's system something of a scandal, he employed them too. Derek J. de Solla Price, ("Contra Copernicus", in Marshall Clagett, editor, *Critical Problems in the History of Science*, 1959, 197-218) is a notable exception but still within the Baconian tradition, as he condemned Copernicus. For my part, I deem his epicycles legitimate stop-gaps; those of Ptolemy he deemed scandalous ─ or so I fancy ─ because they are ancient and so they were no longer temporary. Rosen leaves all this out as irrelevant to his condemnation of my text. So be it.

[2] The same goes for Brahe: whether his theory is or is not as I state it I do not know. I was relying here on two great authorities, the already mentioned history of Laplace and J. L. E. Dreyer, *A History of Astronomy* that I deem admirable, my criticism of it notwithstanding. Laplace's criticism of Brahe is just, yet answerable by Einstein's covariant principle. Laplace denied the option of letting light bodies like the earth's moon serve as the center of the entire world system; Einstein made room for this option.

[3] Rosen complains that I do not document my view of Brahe as a precursor to Einstein. No document exists, I am afraid: the assertion that Brahe may be viewed as a precursor of Einstein is my hasty conclusion (to use a charming if antiquated Baconians expression) from their shared permission to view the earth's moon as the center of the world-system. Newton's dynamics allows for a free choice between inertial frameworks even though he refused to allow it (*Principia*, Book III; see reference to Copernicus there). But the principle of inertial systems would not exonerate Brahe's choice, as the moon's path is not inertial; Einstein's theory will.

[4] I said "One could not plot ellipses easily without knowing a certain amount of mathematics (Kepler used the newly invented logarithmic techniques)." Rosen responds saying that Kepler published his first law before he heard of Napier. True. This is a widespread mistake, but it is no excuse for

me. *Mea culpa*. Incidentally, Kepler used logarithms, if at all, in his work on his third law. On this opinions are not decided. (See Kevin Brown, *Reflections on Relativity*, "8.1, Kepler, Napier and the Third law".)

[5] Hence, my supposition "Kepler's use of the logarithms was inessential though very helpful for his purposes" may be false.

[6] I said the same of "most if not all of Brahe's records that he made use of". Rosen comments, "according to Agassi, all of Brahe's records were unessential for Kepler." This is odd: I said "most if not all" not "all". Let this ride. Rosen has not shown that Brahe's records were essential for Kepler, or which of them were. Kepler's lovely testimony that Rosen cites as evidence against me will not do. The interested should start with works of Curtis Wilson.

[7] Rosen castigates me for what I said about Dampier. He ascribes to me an assertion that I did not quite make (see below) that he "never took his information from a primary source". Let this ride. And he shows at great length that Dampier did. Here Rosen is misleading; he offers a wrong poof of my regrettable lack of erudition and he pretends to respond to my discussion of Dampier, but he does not: that discussion concerns Dampier's distortion, not his erudition. I said,

> Acting on the assumption that Dampier-Whetham never took his information from a primary source, I casually tried to find out where he did get it. My present hypothesis is that he got most of it from E. T. Whittaker to whom he is possibly indebted for some other errors and confusions).

Rosen's refers to an anthology of Dampier and his daughter, suggesting that I am unaware of it. Surely, this great meticulous scholar could not miss my criticism of that anthology (note 43 below). Still, my casual observation, made in youthful exuberant fun, is misleading and unkind: I now replace "never took his information" with "did not take this information". *Mea culpa*. This does not affect my criticism of the scholarly technique that historians like Dampier and like Rosen are outstanding examples of and that is a response to some intolerable demands of inductivist historiography.

On a second thought perhaps I should say more. Rosen is picking on me and I am picking on him in return. This is not all that there is to it. We see here a hatchet job. Of course, I have no right to complain, and no wish to: I asked for it. I am not peeved but puzzled: why did he pick on me? It is not my small errors: we can all rectify some of our small errors with a little more attention. Even Rosen's attack on me shows this. These are of little consequence. Nor is it my big errors: Rosen excelled in rectifying some big errors with no expression of contempt. He is contemptuous of my

> "unmitigated impertinence to hurl the epithet 'pseudo-scholars' … at eminent writers who do not happen to share his muddled methodological predilections, and to proclaim that he is 'trying to explain the low standard of work on the history of science' … and to improve 'the present lamentable state of affairs in the field of the history of science'."

Is this the cause of his ire? No; he is not the only one to have dismissed me off hand, and he is not the only one to have castigated me in some unpleasant detail. He is the only one who has done both. Kuhn castigated me at least as harshly, and with the aid of insulting terms that outdo those of Rosen, yet with some sense of balance he complimented my *work* and some of my historical analyses too; Rosen has no good word for me, not even for my comments on the literature that he is expert on. Why? Not because of my insults to "eminent scholars". These are immune to insults; not-so-eminent scholars survive them with relative ease too. Nor is his indignation at my assault on the low standards of his profession heartfelt: he is reputed to have been most critical of these standards. ("Rosen avoided broad themes", says Ed Grant in his obituary. "Almost invariably he chose to research and resolve well-defined, highly specific problems, problems that often involved widespread misconceptions in the history of science." So "widespread misconceptions in the history of science" were no news to him.) I angered him enough to deserve a hatchet job that is quite out of character. The last time I saw him he was defending Copernicus against the charge that he was a mystic ("Was Copernicus a Neoplatoinst?" *J. Hist. Ideas*, 44, 1983, 667-669). For, by the Baconian canon it is a serious charge. He was at his caustic best; also, he was a perfect gentleman; and he was very impressive. Bernard Cohen and I exchanged impressions of his talk immediately afterwards; we were unrepentant but moved. His assault on me is different: I hit a nerve, and I regret it. I now read him thus: we should better stick to professional expertise, leaving "methodological predilections" out of the picture as they are bound to be muddled. This is professionalism. Its advocates know that to discuss it is self-defeating. If you have to do something unpleasant, do it briefly and angrily and forget it. This is a part of the traditional gentlemanly code, and he was a gentleman. But he was in error all the same: traditional gentlemen had the choice of very few high-class professions. It is no accident that after World War I gentlemanly codes altered. It is no longer possible to stay aloof and leave things unexplained. Rosen wanted (Baconian) standards taken for granted. This, to repeat, is no longer possible. Here then is my discussion of standards. It is regrettably still marred with errors, impatience, and other defects. Perhaps it is also out-of-date. Let those who are ready to take up the cudgels try to improve upon it. They will hopefully replace it soon.[1a]

1. The Inductivist Philosophy

Almost every classical or contemporary history of science bears the stamp of Francis Bacon's philosophy of science. Bacon's philosophy divides thinkers into two categories variously characterized as right and wrong, scientific and superstitious, open-minded and dogmatic, observer of facts and speculator. The open-minded person, according to this view, can observe and record facts as they are, as they appear to his eyes accidentally; he does not form any opinion until significant facts lead him to a sound — i.e., scientific judgment. The prejudiced and superstitious person, on the other hand, starts by speculating, by conjuring a hypothesis; he then forces the facts to fit the preconceived scheme of his hypothesis, and so sees the world as in a distorting mirror. As he sees only the distorted image of the facts that refute his theory, he is in no position to correct his views by bringing them into accord with these facts; and since he can never see that he has made a mistake, he will continue to see facts distorted. Thus trapped by a vicious circle inside his distorting view, he will be unable to avoid adopting a dogmatic attitude towards it.[2]

This philosophy leads the historian to attempt to record without bias all the facts as they are; yet once a person, historian or not, accepts a division of mankind into open-minded and closed-minded, he almost invariably finds himself on the right side. And being on the right side, he is assured by Bacon that he can see facts as they are. This is an agreeable doctrine. The recorder of facts-as-they-happened may record facts of social history, of natural history of animals, or of the heavenly bodies; or he may record facts from the *Annals of Science*. The latter recorder is peculiar amongst recorders of facts-as-they-happened: he is the most qualified to approach his historical material as a bad schoolmaster approaches his pupils' work: being open-minded himself, he is able to discern who was open-minded and who was prejudiced. It is even rather easy to do this; he who sees facts correctly or who has a correct theory is an open-minded scientist; he who sees facts incorrectly or has an incorrect theory is a closed-minded dogmatist. It is also very easy to distinguish between correctness and incorrectness, especially for the historian of science whose knowledge of science is limited: whatever the up-to-date textbook says must be correct; for, naturally, it has been scientifically deduced from solid facts. Therefore, the open-minded thinker is the one whose ideas agree with the up-to-date science textbook.[3]

This approach of the up-to-date textbook worshipper paints all events in the history of science as either black or white, correct or incorrect.[4] Thus, Kepler's theory of the elliptic orbits of the planets and Black's (factual) discovery of the fixed air are described as pure white.[5] No inductivist historian both presents Kepler's work and reminds readers that according to Newton's theory planets do not move precisely in ellipses; for this would amount to saying that however near to white Kepler's ideas might be, they would remain a bit grey. Similarly, when the inductivist historians speak of Newton's work they usually avoid any reference to Newton's perturbation theory — in spite of its intellectual and historical significance and its importance as a forerunner to Schrödinger's perturbation theory — because its chief application is to calculate deviations of the planets from their Keplerian ellipses. And just as the inductivist historians glibly avoid mentioning the small difference between the Keplerian and the Newtonian orbits of planets, they avoid mentioning the difference between Black's fixed air and the CO_2 of the up-to-date science textbook. Not only was Black's fixed air an element; it also could be transferred from soda (Na_2CO_3) to slaked lime ($Ca(OH)_2$), causing the latter to become chalk ($CaCO_3$). Thus fixed air can equally be identified with CO_3 and with CO_2; but since CO_3 does not exist as a gas and since CO_2 is a gas, fixed air must be CO_2 in order for Black's discovery of fixed air to accord with the up-to-date chemistry textbook. Therefore, the inductivist historians pretend that the name "fixed air" is only an archaic synonym for the modern name "CO_2". One way or another, all white events in the history of science must be made to accord with the up-to-date science textbook.

The simplest formula for an inductivist history of science is to arrange the up-to-date science textbook in chronological order, to describe some of the circumstances surrounding the occurrence of an important event in the history of science, and say something about the chief actors involved in that event; in short, to provide the human side of the history of science.[6] Max von Laue's history of physics[7] is the best and most scholarly work written by this formula, an unusually readable example of this kind of literature.

There seems to be little, if any, reason why this formula could not be followed by all inductivist historians. And, indeed, von Laue is not the only modern writer who has used it. Yet, on the whole, few writers adhere to it very strictly, and some quite definitely deviate from it. Although most inductivist historians prefer to dwell on the bright side of history, most record at least a few of their excursions into the darker patches of the history of science — if only for the sake of contrast. Others, notably Lynn Thorndike[8], writing histories of science and magic, put into their books more dark patches than bright ones. Some historians of science paint people as well as ideas black or white; they would blacken Descartes and Stahl, and whiten Kepler and Newton.[9] And still others would commend a scientist for one reason and condemn him for another. Florian Cajori, for example, gave Gilbert a big plus for having discovered earth-magnetism and a small minus for having thought that the earth's magnetic and geographical poles coincide.[10]

This marking business can be a little dangerous. In the first edition of his history of physics of 1899 Cajori gave a big minus to all those who believed in electrons.[11] In the second edition, dated 1929, he gave a big plus to the same people. A cryptic explanation for this change of attitude is to be found in the unbelievably naive preface to his second edition, where he expresses his loyalty to the up-to-date textbook of physics.[12] Thus, whenever the textbook alters, the history of science changes accordingly.[13] In the last century Newtonian optics was a prejudice; since the revival of the particle theory of light it is not.[14] And so, the nonsense that creeps into even the best of up-to-date textbooks (and the nonsensicalities included in even the best physics textbooks of the nineteenth century are by now transparent) is to the inductivist historian as sacrosanct as the greatest of human intellectual achievements.

Viewing the situation in this fashion, one may easily understand why inductivist histories of science have to be rewritten from time to time, but the problem remains why inductivist historians keep pouring out books even during periods that see no drastic change of the up-to-date science textbook. After all, most of these history books adhere to the same formula, resemble one another to a large extent, and manifest differences that even their authors think are insignificant. (The differences must be insignificant since all books written in accord with the formula are white.[15]) What, then, is the role of the ever increasing number of similar histories of science that keep pouring into the market? In my opinion, their main function is ritual.

2. The Ritualistic Function of Inductive Histories

A glance at a bibliography of the history of science will show that the number of writers in this field is rapidly increasing. A survey of the teaching of the subject in universities again shows a marked increase in the number of university teachers who teach mainly the history of science.[16] What is the purpose of all this growth?

The answer might be this: in as much as the growth of interest in any field of study, say the field of social history, justifies the growth of the study of that field, so the growth of interest in the history of science justifies the growth of the study of the history of science. It is not my purpose to deny that interest in the history of science is growing, yet I do deny that this growing interest is satisfied by the new literature in any way comparable to the situation in the field of social history. Even people whose interest is more in the history of science than in social history prefer to read social history because most of the works in the history of science are so boring. Moreover, the rapid growth of literature in the history of science provides no measure at all of progress in the field, which has in fact been very slow; for many books on the subject just repeat one another.

Take a history written by no less an author than James Jeans — a brilliant writer and an important figure. His history of physical science[17] is only slightly more readable than average, and his attempt to cover the whole field, including ancient and mediaeval physics, mathematics, and chemistry, within three hundred and fifty octavo pages, makes it practically impossible even for the specialist to read more than a few pages of it at one sitting. And yet in his preface he has nothing more to say for his work than "a vast number of … histories … of … science … are admirable for the scientific reader, but the layman sometimes cannot see the wood for the trees", and that his own book "may prove of interest to the general educated reader, perhaps also to those who are beginning the study of physics".

The facts are different. Little is written "for the scientific reader"; most of the vast literature is expressly presented for "the general educated reader";[18] but readers, expert or not, educated or not, still cannot see the wood for the trees, not even in Jeans' book.

Readers are meant to see the wood for the trees by being shown a few very important landmarks, such as Copernicus. One can hardly ignore a landmark of which the following is said. "The fact that Copernicus wrote out arguments of this kind shows that he thought, and meant to prove, that the earth was actually rotating and moving round space … . With his refutation of Ptolemy's arguments, Copernicus has proved his case, at least to those few who could assess his arguments. Man could no longer claim that Ptolemy was right.[19] I suppose it may sound pedantic to point out that Jeans' phrase "moving round space" is unclear; but there is an interesting point here, especially since Jeans' writing is in general quite clear. By reading the phrase as "moving round a point in space", one gets, I suppose, what Jeans wanted but could not say. According to Copernicus, the sun is in the center of the universe, and the planets, including the earth, move in circles with the sun in their centers. But Kepler, who still believed that the sun was in the center of the universe, had already both destroyed the circles and shifted the centers of the planetary orbits from the sun. Newton viewed the sun as moving close to the center, but Herschel destroyed even this tenet of near-Copernicanism by putting the solar system in a corner of the Milky Way. Thus, Copernicus' doctrines are not white. Nor did he succeed in showing that his system is better than Ptolemy's, let alone in refuting him: he merely said that Ptolemy's system has too many epicycles, and then produced a system of his own that included as many epicycles, or at least sufficiently many to make the advantage of the proposed change doubtful. The non-inductivists know very well that Copernicanism was rejected by many scholars — and often for very good reasons.

Jeans too, one must assume, knew all this; in the passage quoted above he leaves everything open by being vague. In the text preceding the above quotation he briefly mentions the position of the sun in Copernicus' system, and also his epicycles and false estimates of celestial distances. As to his remark about the convincing power of Copernicanism, he retracts some of it, one feels, when he says a page later that over a century and a half after Copernicus' death "the Director of the great Observatory of Paris, and one of the most influential astronomers of his time, expressed himself as a convinced anti-Copernican …" Now the words "expressed himself as" instead of "was" may be due to some caution: the Church of Rome opposed Copernicanism, and the "influential astronomer" may have simply conformed in speech. I do not know;[19] but I wonder if all this helps "the general educated reader to see the wood for the trees". Did Copernicus prove anything? What? How? To whom? At least for myself I can say that I found no simple answer in Jeans' book. It seems to me that the main function of the passage quoted above is as a eulogy to Copernicus. Copernicus has to receive a high mark from the inductivist historian of science; and the eulogy is necessarily vague because not a single idea of Copernicus is pure white. The function of histories is to stress that the field of study is important and that big marks must be given, at least as a token of gratitude, to some past scientists. The inductivist histories of science are, briefly, scientific ancestor-worship in pseudo-scholarly guise.[20]

The pseudo-scholarship of the standard inductivist historians is manifest in their largely uncritical and unacknowledged transcriptions from colleagues. This I shall discuss in more detail later (section 5). Here I wish to speak of another aspect of the inductivists' pseudo-scholarship; namely, their inability to notice criticism of the inductivist outlook, of the formula by which they write their histories.

The last word in all this is the remarkable inaugural lecture of Professor Douglas McKie, of the University of London, called "Science and History" (1958). It deals briefly with the relations between the philosophy and the history of science. A historian of science, says McKie, may be personally interested in the philosophy of science, as he himself is; qua historian, however, he has no use for it;[21] his business is simply to study the rise of scientific ideas as it took place "in fact with the scientific detail of experiment and observation from which these ideas emerged".[22] Professor McKie emphasizes that his is the majority view, and he is right. But the majority view need not be correct; to argue that the philosophy of science is irrelevant to the study of the emergence of scientific ideas from facts is barely feasible because the philosophy of science is largely about whether scientific ideas do emerge from facts. One great historian and philosopher of science, Pierre Duhem, has argued cogently that Aristotle's theory of motion is much easier to deduce from observable facts than is Galileo's .[23] If true, this would be something of a blow to a worshipper of the up-to-date textbook. Quite possibly Duhem's view is mistaken; but had McKie been a historian like other historians,[24] he would have argued against Duhem and explained his disagreement. Being a historian of science, however, he can continue with his self-appointed task of recording facts-as-they-really-happened from the *Annals of Science* undisturbed by Duhem's criticism of these facts.

One defense might be to suggest that a historian of science ought simply to record some factual information and some (scientific) ideas in chronological order and leave it to the philosopher of science to discuss the relations between them. Although McKie aims to contest this suggestion, it may nonetheless fit in better with his tight compartmentalization of the history and the philosophy of science. The suggestion might be adopted; but what purpose it would serve is not apparent.

McKie wishes his study, among other things, to bridge the arts and the sciences, to be a guide to science for lay readers. He does not say why Jeans' efforts in this direction were not good enough, and how he hopes to improve upon them; nor, in particular, does he promise to refrain from boring arts students with a massive and detailed chronology of events in which an arts student might easily feel lost — especially as these are, in the present deplorable state of specialized arts education, quite beyond the comprehension of educated lay readers.[25] Nor would serious arts students be interested in black and white pictures of history. They might show interest in problems that historians have; but in order to raise this interest one has to pose problems and explain their significance — something that inductivist historians of science have yet to do. This is not to say that they have no problems.

3. The Standard Problems of The Inductivist Historian

Since inductivist historians of science are chiefly interested in chronology and in giving marks to past scientists, the range of their problems is rather definitely set for them. The formula they employ is: In year *x* scientist *y* made discovery *z*. Consequently, they have three kinds of problem: (a) chronological problems; (b) priority problems; and (c) authorship problems.

Chronological problems concern the dating of events; priority problems, the awarding of medals for the discovery of a given fact or white idea: to which person goes the honor of having discovered a given fact or white idea? Authorship problems concern the reasons for having given medals to given white persons: What discovery of fact or white idea was made by a particular great person? Chronological problems are rather rare, but they may occasionally be of some interest. George Sarton's inaugural lecture[26] about the aim and method of the writing of the history of science contains a critical discussion of chronological problems and a curious example. The problem of why Newton delayed publication of his *Principia* raises chronological issues. It was, at one time, assumed that the delay was caused by some difficulty; this assumption was, however, later refuted by Cajori,[27] who showed that the solution of the difficulty in question preceded the publication of the book by several years. This criticism is of some importance, since Cajori's own substitute for the criticized doctrine is relevant to the question of Newton's character and attitude towards science.

Priority problems are often rendered insoluble by the black and white approach of the inductivists. For example, the problem of the priority of the discovery of the law of inertia is insoluble for the inductivists. Did Galileo discover it or did Descartes? (Or neither of them, as Duhem claimed?[28]) The impossibility of answering this question stems from the impossibility of identifying the white view of inertia. Galileo's circular inertia is identified with inertia along a straight line because the inductivist historians are reluctant to mention Galileo's circles on account of their being rather black.[29] But the law of inertia does not exist, as Einstein's law of inertia should have made obvious to any historian of science. Einstein's law of inertia (bodies move along geodesics) can, indeed, be viewed as a modification of Newton's (bodies move along straight lines unless forces act upon them), which is a modification of Descartes' (bodies move along straight lines unless they collide with other bodies) which is a modification of Galileo's (bodies move along circles unless they collide) which is a modification of earlier views. But a modification means moving within the gray towards the white — and inductivists will not have gray; there is the law of inertia in the white area and all else is in the black. Since purely white ideas rarely exist, the inductivists simply distort historical facts. Consequently they face the insoluble problem of who invented a particular white idea, when no one held anything remotely similar to it much prior to the writing of those present day textbooks in which it appears.[30] Even Newton's laws are presented in modern textbooks in a quite different fashion from that in which they were presented in textbooks a century ago, not to mention the fashion in which they were presented by Newton.

Problems of authorship are often insoluble for the same reasons. This may be illustrated by the inductivists' attitude to Lavoisier and to Dalton: both are generally regarded as deserving commendation; but inductivist historians have not yet decided which ideas should be credited to whom.

The problem of Lavoisier's contribution, for example, arose fairly soon after his death. Early in the nineteenth century, soon after Davy had refuted Lavoisier's theory, it was alleged that Lavoisier's theoretical contribution was nil. The fourth edition of the *Encyclopedia Britannica* (1810) credited him only with experimental contributions and with the overthrow of some prejudices (namely phlogistonism, a doctrine claimed to be demonstrable by observation in the first edition of the *Britannica* , 1771).[31] Dr. Thomas Thomson, the great historian of chemistry, blamed Lavoisier in 1830 for having tried to introduce new prejudices — to wit, the mistaken theory that all processes of combustion, calcination and acidulation are produced by oxygen — just in order to bolster his own vanity.[32] It did not take long for it to be realized that this would not do. Instead, Lavoisier was credited with having defined the word "element", both because his definition accords with usage current among the modern textbook writers, and because definitions, being verbal, cannot err. But it was soon discovered that Lavoisier had merely been quoting Boyle's definition — and almost *verbatim*.[33] So the credit for the definition had to be transferred to Boyle. Another suggestion was that Lavoisier was the first to state the law of conservation of matter. But, as we know, the law had already been stated in antiquity.[34] So it was suggested that Lavoisier's contribution had been to prove the law. Now if by "proof" is meant corroboration, then many chemists before Lavoisier had amply "proved" it. If by "proof" is meant conclusive verification, then Lavoisier could not have verified the law since, as Meyerson has observed,[35] in his experiments he weighed matter before and after chemical processes, and the weights after the processes were always smaller than before — presumably because in the meantime some matter escaped. Moreover, it is logically impossible that a law has been both conclusively verified and found to be a mere approximation to a better law — in this case Einstein's law of conservation of matter-energy.

Inductivist historians of science are divided into those who ignore these refutations and continue to declare that Lavoisier's doctrine is true,[36] and those who agree that Lavoisier deserves credit for some solid theoretical contribution, even if they cannot say just what this might be.

My other example concerns Dalton. Some say that Dalton revived atomism. "Dalton", says Dumas in his influential Faraday Lecture,[37] "was the son of Leucippus. Between these two points, so far remote, there is no link." It is hard to understand what Dumas meant, since we must assume that this younger contemporary of Dalton, who was himself a brilliant chemist, must have read everything that Dalton wrote, and hence must have known that Dalton often referred to Newton's atomistic explanation of Boyle's law. But perhaps he ignored Newton's atomism because, as Dalton had shown, it was not quite white. Alternatively, wanting to credit Dalton with something, and being unable to quote any specific idea of Dalton's that was unquestionably white (Dumas himself refuted some of Dalton's ideas) Dumas perhaps had to resort to generalities. But I confess that neither of these explanations satisfies me.

What, then, was Dalton's discovery? In their famous book, *A New View of Dalton's Atomism* (1896), based on notes of Dalton discovered in Manchester, Roscoe and Harden contend that Dalton developed his atomism to explain the diffusion of gases in fluids; their view contrasts, they say, with Dr. Thomson's claim that Dalton aimed to explain the chemical law of multiple proportions. Why their evidence and Dr. Thomson's personal testimony cannot both be accepted they do not say. Perhaps he wanted to solve both problems. Anyhow, apart from the question of Dalton's purpose, the problem of what to credit him with seems to have been answered: each theory can be used in order to credit Dalton with some important contribution to theoretical chemistry. It would be natural to assume that Dalton's contribution was to suggest a new explanation of the diffusion of gases.[38] But the matter is not as simple as that: his explanation did not occur in any up-to-date textbook of chemistry, so Roscoe and Harden could not credit him with it. Nor could they credit him with an explanation of the law of multiple proportions, and for the same reason. Obviously, Roscoe and Harden did credit Dalton with some contribution, and even important one; many historians of chemistry refer to their book as the study that establishes this fact; but none of them seem to be able to quote the book on this point,[39] or to say what contribution Roscoe and Harden attributed to Dalton.

In despair, some inductivist historians have suggested that Dalton discovered the chemical law of simple proportions, multiple proportions, and/or reciprocal proportions. But that these three chemical laws were commonplace even before Dalton made his first contribution to chemistry can be easily documented.[40] To be sure, prior to Dalton these laws were not stated in the manner in which they appear in the modern elementary textbook. But then Dalton explicitly and emphatically rejected this modern formulation — or, rather, a similar formulation from the tradition of Gay-Lussac, Avogadro, and Cannizzaro.[41]

Another attempt attributed experimental rather than theoretical discoveries to Dalton. But this, again, was difficult: his results were very inaccurate, and therefore not white, and therefore black. "Dalton was a crude experimenter" is the general verdict. I shall return to this verdict later (section 5);

first we must peruse the problem of what Dalton did contribute if this was neither a new theory nor a new experiment.

It has been suggested that our system of chemical notation is due to Dalton. This suggestion is as untrue (Berzelius started the present tradition of chemical notation) as it is ridiculous. No one dreams of crediting Leibniz with his invention of the notation of the differential calculus, although it is the traditional notation, because it is insulting to credit so great a figure with so minor an invention. The inventor of the current vector-notation is not considered half as great a thinker as Dalton.[42] Although the suggestion was refuted and ridiculed by authorities like Faraday,[43] it is still rather popular.

Another suggestion that has been revived is that Dalton's contribution was minute; that he took his ideas from his predecessor Higgins.[44] Although this view was argued competently and critically, it was never explained why Dr. Thomson said that he had read all that Higgins ever wrote without obtaining from it Dalton's great idea, an idea that he did get hold of at once when he received from Dalton information that could be written on half a page.[45] Dr. Thomson was a great chemist and historian of chemistry; the information to which he was referring was soon published by him, and he claimed to be the first to have published Dalton's ideas. Today, Thomson's passages are less comprehensible than they once were. At any rate, I myself do not understand them, and to my amazement I have been unable to find any history of chemistry that does explain them. Not only Dr. Thomson, but other leading contemporaries of Dalton, amongst them Davy and Wollaston, agreed that Dalton's theory was a novelty.[46] They may all have erred, but the matter needs more investigation before their evidence may be dismissed. The investigation will lead to no firm white or black result; the problem should not be what to credit to Dalton but rather what his new idea was and why it was taken so seriously by his contemporaries.

So much for contemporary routine inductivist problems: chronology, priority, and authorship. But the situation has not always been this way. As long as the history of science was no more than this, it hardly existed at all. At first, there were other reasons for writing history. Priestley's *History of Electricity* was intended as a scientific textbook. Whewell's *History* was meant to illustrate his own philosophy of science; Berthelot tried to combat the popular prejudice that all alchemy is pitch-black. Even Laplace's short history of astronomy (Book V of *The System of the World*), written as a kind of appendix to a popular exposition of Newtonian celestial mechanics, had a serious problem; namely, why the history of astronomy did not develop on Baconian lines. Such works are more interesting than most histories of science that later started pouring out in ever increasing quantities although seemingly without any problems and for no apparent reasons.

In the next section I shall discuss Laplace's problem, as one example of a serious inductivist historical study. But I wish to say a few words first about the black side of the story. If the white side of the story presents chiefly the problems of priority and authorship, the black side hardly presents any problem at all; it is an inductivist historian's paradise. The only problem is, why bother about the black side at all? The answer is, of course, that it is our duty to fight and condemn the black with all our might. This Baconian idea has led to a literature of vulgar errors that began with Sir Thomas Browne's charming *Pseudodoxia Epidemica* of 1646 and that is still very lively. Yet this argument for recording some errors does not justify the recording of errors that are not vulgar or are long forgotten.

Lynn Thorndike, the historian of science and magic, tells us that he records errors so as to prevent their repetition.[47] I doubt this; I cannot believe that informing the public about long forgotten stories about werewolves in any important way averts the risk of a revival of belief in them; rather, I think, it increases this risk somewhat. I very much sympathize with Thorndike's demand that the fact that almost all astronomers before Newton were astrologers should not be concealed; but I see no reason to transcribe all the details of their astrological views. I also sympathize with his view that "magic and experimental science have been connected in their development", even though I disagree with him on this point; one can hardly speak of the development proper of "magic", (i.e., magic, mythology, superstition, etc.) and science has evolved out of "magic", not together with it.[48]

Undoubtedly, Thorndike's greatest fault, and the one that renders his work so useless and infuriating, is that he puts all errors on a par, in a true Baconian fashion. The worst mediaeval superstitions are lumped together with Kepler's animism: all errors are prejudices and superstitions. Even Laplace, writing nearly two centuries ago during the peak period of Bacon's influence, knew better than this.

4. History of Science — as It Is and as It Ought to Be

Bacon's philosophy of science makes the history of a science an essential part of that science. A proper science must have developed properly: it must have started with observations; the "ladder of axioms" must have been climbed slowly without skipping any step; the less general theories must have preceded the more general ones; and the theories or axioms must have consequences that lead to discoveries and inventions.[49] This is all that the history of a proper science can consist in; and it all belongs essentially to that science. This is what Popper calls proof by pedigree"[50] the past history of a science, its genealogy, is the proof of its validity.

In his *System of the World*, a popular exposition of Newton's system Laplace follows the Baconian scheme in as orthodox a way as possible: he begins with facts and presents theories gradually. But, as he admits in the preface, he is describing the history of astronomy as it might have occurred, not as it actually did. He puts this admission more boldly when he presents a sketch of Kepler's work in the last and historical part of his book. Perhaps his sketch of the history of astronomy as it really occurred is merely meant to complement his hypothetical history. Perhaps it is a way of emphasizing the hypothetical character of his hypothetical history. But possibly he was trying to answer a question: Why did the actual history differ from the hypothetical history when, according to Bacon, the two ought to coincide? A few of his remarks might be viewed as part of an interesting answer to this question, an answer that would appear to go somewhat as follows. In actual history people did not altogether cease speculating; they remained partially prejudiced; they grasped the true inductive method of science only gradually. Laplace asserts that Copernicus was still "deceived" by previous prejudices (epicycles), and that even Kepler speculated ─ and thus erred because he did not at first know how to conduct his researches properly. He also exposes Bacons (geocentric) prejudice ─ quite unprecedented behavior for a Baconian[51] ─ and he explicitly condemns Cartesian physics, towards which he himself is known to have had strong leanings)[52] as a set of prejudices. He seems in this manner to suggest that the inductive method was not rigorously followed before Newton's time because people were not fully converted to it until the failure of Descartes' (allegedly) *a priori* (i.e., preconceived) physical theory and the success of Newton's (allegedly) *a posteriori* (i.e., inductive) one.

This suggestion not only solves what might have been Laplace's problem; it is an interesting idea in itself. It explains the spread of inductivism as due to more than mere admiration of Newton's success. For, it amounts to saying that there was a kind of crucial experiment between Bacon's *a posteriori* and Descartes' *a priori* rules of method. Such an experiment would be possible if there could be a criterion for choosing between *a priorism* and inductivism, a criterion that must, therefore, presuppose neither *a priori* knowledge nor *a posteriori* experience! It would seem impossible that such a criterion should exist, but it is not: Laplace's criterion presupposes neither kind of knowledge: it depends on what we expect science to achieve.

Laplace's solution to his problem thus seems to be satisfactory. Admittedly, actual history deviates somewhat from the path inductivism decrees it should have followed, but this deviation is due merely to the fact that inductivism was accepted so slowly.[53] This, however, raises a new difficulty. According to strict Baconian inductivism, science cannot be developed by prejudiced people, but only by pure-minded inductivists. This aspect of inductivism, Laplace was forced, albeit painfully, to reject.[54] Indeed, he seems to have argued that this assumption had to be relinquished in any case. But, as before, his argument is implicit, and presented by way of commentary on historical details: he merely points out that some prejudiced people, notably Tycho Brahe, made great contributions to astronomy, and that even some prejudices have been very fruitful in the history of science: Ptolemy's system served as a means of preserving the ancient factual knowledge throughout the Dark Ages; Descartes' system, being more attractive (or intellectually gratifying) than the ancient ones, helped to destroy them. Although this seems only a minor reform of inductivism, it is an idea with far-reaching consequences. For, if prejudices can be useful, then they are not as black as Bacon and others have painted them. Had Laplace pushed the idea that prejudices can be useful to the limit he might have cast off his Baconianism altogether. At least he might have been more appreciative, or at least not so confidently scornful, of prejudices of which he did not approve, such as the ideas of Tycho Brahe.

Tycho Brahe's theory is this: what we choose as the center of the universe is entirely arbitrary. He confessed freely that his own choice, the earth, was partly based on religious predilection.[55] His theory has never been appreciated, I think, because of the pro-Copernican and anti-religious prejudices of historians. Brahe's basic discovery was logical: he found that the ancient principle of superposition of motions and the Copernican theory of relative motion can be used to defend even the view that the moon is the center of the universe. The argument that Laplace launched against Brahe's lunicentric doctrine stems from a Newtonian root: he declares that the moon is too small to govern the whole system by (Newtonian) force. One must admit, Laplace notwithstanding: on this issue Brahe was right. If there is any weapon with which to confute Brahe's idea that existed in his own time, it is merely the following claim: since Brahe's epicycles are bigger than the cycles on which they were mounted, they would have to intersect each other; but the revolving cycles consisted of crystal spheres, and these cannot intersect. To be precise and to talk in Laplace's language, Brahe's prejudice was an important tool with which to destroy the crystalline heavenly spheres. Indeed, he claimed priority for the destruction of the crystal spheres. And, obviously, Kepler's second law concentrates on the planet while ignoring its orbit; and his first law is even incompatible with crystal bodies that have no epicycles.[56]

Moreover, by the standards of the up-to-date textbook worshipper, Brahe's disturbing idea that the center of the universe can be wherever you like it to be is a candidate for the title of precursor to Einstein. Yet it is historically more interesting and important to note that the Copernican system was modified by introducing ellipses (Kepler), linear inertia (Descartes) and forces (Newton) — that invalidate Brahe's suggestion that the choice of the center of the universe is arbitrary. Had Laplace wanted to find out whether or not Brahe's prejudices were useful, he might have tried to see whether the ideas that he uses as arguments against Brahe's prejudices were not introduced in order to invalidate them. Obviously, it is useful and laudable to present the worst prejudices conceivable, provided that ensuing attempts to dispel them result in Keplerian ellipses and Newtonian forces and other marvelous ideas. One need not blame Laplace for not having thought of how to defend the usefulness of Brahe's views; one may, however, regret that he was not more explicit about why he wrote his short history of astronomy. Had he done so his task might have been taken up by others.

Laplace's history shows that even an inductivist can do interesting work. It does not matter that inductivists have their own prejudices about how history ought to have happened, provided they are willing to ask whether their prejudices fit the facts. And one way to do this would be to take up Laplace's suggestion, make it explicit, and discuss it by means of more critical studies of the history of science. If this were done, the inductivist literature would be less boring than it is. This last point ought to be stressed, because black and white inductivism need not necessarily be boring. It becomes so when it is done uncritically; and it is ordinarily done uncritically because the usual inductivist methods serve the sole purpose of compiling large masses of historical details, leaving no time for critical thinking.

5. The Inductivist Techniques

Inductivist historians of science are so overworked that one can scarcely blame them for inaccuracy. Consider, for example, Dampier-Whetham's history of the natural sciences from antiquity to the present. It was an extremely successful work that ran into several editions, even though five minutes browsing in it should convince almost anybody that it is rather unreadable. Suppose a critic selects any single item from his work — say the claim that "by a series of masterly experiments Faraday reduced the complexity of the phenomena [of electrolysis] to the two simple statements known as Faraday's laws" — and proves by quotations from Faraday's *Diary* and papers that Dampier-Whetham was mistaken[57] that Faraday arrived at his laws by *a priori* reasoning and that he did not reduce the complexity of the phenomena — at least not to his own satisfaction — but only a part of it. This critic will be considered a pedant, and rightly so. Why select one detail out of thousands? I confess to having selected this one maliciously because I do not believe anyone can reduce by series of experiments complex facts into simple laws — not even the Faradays among us (see Section 8 below). But I admit the malicious intent: it is most unreasonable to demand that an author should know the whole history of human knowledge — not even one who has written a book about it. One has to assume that Dampier-Whetham could not check all that he wrote against the original works that contain the details he reports — or even understand all these details, which number thousands and thousands. "One almost feels compassion for so much impotence", to quote Nietzsche.[58]

Since Dampier-Whetham clearly did not acquire this information from a primary source, I casually tried to find out where he did get it from. My present hypothesis is that he got most of it from E. T. Whittaker to whom he is possibly indebted for some other errors and confusions). Now Whittaker was a careful scholar, and not even an orthodox inductivist: his own chief concern was to show the continuity of the history of science, and in describing Faraday's work he emphasized its similarity to that of some of Faraday's predecessors. I shall explain and criticize this doctrine and technique in sections 9 and 10 below; here I propose that Dampier-Whetham, not being interested in Whittaker's non-inductivist continuity theory, and reading Whittaker's passage in haste, found there a perfect model of inductive investigation: "Faraday reverted to" a field of research previously studied by

Davy; he first dispelled some received opinions: "the ground being thus cleared by the demolition, Faraday was now free to construct a theory of his own", and "many of the perplexities which had harassed the older theories were at once removed … ". This is not all inductivist; at least not necessarily so. But as Whittaker records neither the problems leading to Faraday's study, nor the problems ensuing from them, nor their mistakes and deficiencies, and since, in particular, Whittaker leaves the question of what came first, Faraday's theory or Faraday's observations of facts, Dampier-Whetham's conclusion from Whittaker's description — assuming that this is where he obtained his material — would be quite natural.[59] He knew *a priori* that such smoothness as described by Whittaker can be achieved only by an open-minded observer.

One concern of Faraday in his electrolytic studies was to find phenomena explicable by his electrostatic field theory. He had no empirical justification for introducing it, and therefore, still being an inductivist, he withheld his idea from 1832 until 1837, when he discovered the phenomena of dielectricity. This story is rather well-known, partly because his *Diary* is now published, but mainly because in his report of the discovery he entirely breaks from the inductive style of presentation and explicitly states that his experiments leading to his discovery of dielectricity were designed as crucial test between the received action-at-a-distance theories and his own view, in which the action travels through the electrostatic field.[60] Whittaker comments that "the discovery … [of dielectricity] raised the question of whether it could be harmonized with the old idea of electrostatic action." His own answer to the question is negative. "The problem", he continues, "could be solved only [sic] by forming a physical conception of the action of dielectrics; and such a conception Faraday now [sic] put forward". This is quite true except for the word "only", and quite clear except for the word "now"; but this provided sufficient inaccuracy and ambiguity to mislead an inductivist. Reading this passage very hastily, Dampier-Whetham may, I imagine, have got the impression that Whittaker was saying that Faraday first discovered a new fact, that he then thought about it, and that he finally found the only possible explanation for it. That is, he thought that Faraday had arrived at his explanation by the method of induction from the facts. It is hardly surprising, therefore, that after explaining Faraday's theories of the magnetic and of the electrolytic medium, Dampier-Whetham smoothes his transition to Faraday's study of dielectricity with the remark that "Faraday examined this dielectric medium [electrolytes luckily happen to be dielectric] in another way". H. T. Pledge, another successful historian of all the sciences, evidently felt somewhat uneasy about all this. "If we call conducting solutions [i.e., electrolytes] media," he wrote, "we may say that Faraday now began to consider other media in starting his great work on electrolysis. At all events, he went on to consider effects of different insulators in Leyden jars [i.e. dielectricity]." By contrast to Pledge, S.F. Mason exhibits no embarrassment at all. “In 1837 he [Faraday] discovered [dielectricity] … “, he states, adding unhesitatingly: “In order to explain this discovery Faraday supposed …”. This is, however, explicitly denied by Faraday, but can easily be read in Dampier-Whetham, who evidently missed the subtlety of Whittaker’s formulation.[61] At last fact and theory appear to have reordered themselves in the perfect inductive chronology.

The chief result of the process of transcribing and re-transcribing is the streamlining of the history of science, namely the rendering of the history of science as it was into the history of science as it should have been.[62] In spite of Laplace’s statement that the two differ in the case of astronomy, many writers still present them as one and the same.[53, 54] I have just given an example of how the chronology of Faraday’s theory of dielectricity was altered to fit inductivism. But this is only the beginning. According to Bacon, a scientist must be totally unprejudiced, and the unprejudiced person does not err. Many historians quote Lavoisier’s plea to be unprejudiced as evidence that he was unprejudiced; but unprejudiced people do not err. Lavoisier’s errors, therefore, have to be ignored. Some authors simply overlook these errors; most authors, however, have the easier job of transcribing from those who have already done the overlooking for them. To take an example, compare a comment in McKie’s *Lavoisier* (1952) with a comment based on it by a recent historian of chemistry, Henry M. Leicester. McKie does not say that Lavoisier’s table of elements is true; he even speaks of Lavoisier’s tentativeness, meaning, I suppose, to apologize for Lavoisier to all those who know that his table of elements is erroneous. But he does not say anywhere that this is what he is doing; he praises Lavoisier for the correct predictions contained in his table, and says nothing about those that were refuted. Consequently, Leicester speaks about Lavoisier’s “great insight into the nature” of chemical elements.[63]

it is clear, again, that the chief vice of the transcribers is to transcribe too many details to be able to check, let alone comprehend them; but it is also obvious that a part of their trouble is due to transcribing from other inductivists like themselves. Leicester is doubtless an inductivist, yet when he transcribes from Hèléne Metzger, who is not an inductivist but a conventionalist, his text is not as objectionable as when he transcribes from the inductivist McKie; and the same holds for Dampier-Whetham: when he transcribes from Einstein he is almost safe, but when he transcribes from Whittaker he renders him even more inductivist than he is.

I contend that there is method in this madness, a method common to the original historian and the transcriber: they try to do their job in accordance with Bacon’s injunctions, with the inevitable result that their heroes look as if they too worked in accordance with Bacon’s method. Bacon demanded that science begin with the writing of true histories of nature, and one part of this job is to sift out, to transcribe from old works, all true facts that they contain, while ignoring any fancies, legends, or errors they may also contain. Bacon demanded that all facts be written into these natural histories without discrimination, insisting that attempts to select observed facts, or even to determine their relative significance, indicate theoretical bias and therefore prejudice. Bacon's inductivism led him to this idea with admirable consistency; less admirable is the inductivists' vacillation between following, through recording all facts, and deviating by recording only important facts, those important by the standard of the up-to-date textbook worshipper. Admittedly, since no historian of science can record all facts, special attention to the textbook will be necessary. But the historians of science should decide whether to follow the textbook alone, like Von Laue, or to follow some additional precept. Instead they may mix up facts and myths, both of which are to be found in textbooks and in other histories, adding to his concoction — so as to make it contain some so-called original research — the little information picked up almost at random from dusty periodicals. And if they collect more than their share from dusty periodicals they may become distinguished. When they select their factual information, very few historians give their readers the feeling that there is a point behind the selection; and fewer still state explicitly their criterion of selection and their reasons for employing it. Before their work may be considered serious scholarship, historians of science ought to tell their readers how they select their details and to defend critically their principles of selection.

Consider the question of whether Dalton was a good experimenter. Most authors, as I have claimed above (Section 3), agree that experimentation was not Dalton's strongest point. The latest edition of the *Britannica* explains: (Art. Dalton): "Dalton was a crude experimenter; a good many of his results have since been disproved." One of the very few who disagree with this verdict is Partington; and as he disagrees with the verdict, he must reject its justification: "Dalton's quantitative experiments were usually accurate and he was an expert gas analyst." A sample of Dalton's "usually accurate" results is given by Partington a few pages before the passage just quoted. It includes the following atomic weights: carbon, 4.3; ammonia, 5.2; oxygen, 5.5; carbonic acid, 15.3. One can hardly describe these results as "usually accurate", quite apart from the fact that Partington never bothers to explain his judgments, let alone defend them.[64] Was Dalton a good experimenter, then, or a crude one after all? If the standard is the up-to-date textbook, he was as "crude" as all chemists prior to the period of the up-to-date textbook in question (provided that science is progressing). But was Dalton a good experimenter compared with his contemporaries and predecessors? Dr. Thomas Thomson answers with an unequivocal yes: though they have since been improved upon, he says (1830), Dalton's measurements were by far the best in their day.[65] The eleventh edition of the *Britannica,* in a hostile article, chooses the evidence of another witness: "As an investigator, Dalton was content with rough and inaccurate instruments, though better ones were readily attainable." Sir Humphry Davy described him as "a very coarse experimenter", who "almost always found the results he required, trusting to his head rather thin to his hands." In the preface to the second part of the first volume of his New System he says he had so often been misled by taking for granted results of others that he "determined to write as little as possible but what I can attest by my own experience", but this independence he carried so far that it sometimes resembled lack of receptivity. Thus he distrusted, and probably never accepted, Gay-Lussac's conclusions as to combining volumes of gases."

We see that the author of the later *Britannica* article on Dalton did not just transcribe his predecessor's work; he changed the word "coarse" into "crude"[66] and omitted the evidence, thus pronouncing the verdict ex cathedra. But should we trust Davy or Thomson? And if so, why? Of course, Thomson was a personal friend, and an advocate of Dalton's doctrine (whatever this may be). But then Davy was an enemy, and an opponent of Dalton's doctrine; we know that he was jealous of Dalton who, humble as he was, had inadvertently challenged Davy's position as the leading chemical philosopher.

The witnesses alone cannot decide the issue: their evidence has to be weighed. And the eleventh edition of the *Britannica* is no guide: it joins Davy in blaming Dalton for judging theoretically rather than experimentally and yet also blames him for relying on his experimental findings that oxygen combines with hydrogen in ratio of one volume to 1.98 volumes, rather than relying on Gay-Lussac's theory according to which the ratio is 1 to 2. Evidently, Dalton had to believe Gay-Lussac's idea rather than his eyes because the up-to-date chemistry textbook confirms the idea; but when Dalton believed not his eyes but his ideas, and rounded his 6.5 into 7 as the atomic weight of oxygen, he was wrong again, of course; and again because he did not arrive at the conclusion of the up-to-date textbook!

There are at least two ways of dealing with this matter. One is to state the verdict ex cathedra; another is to accept Davy's evidence, and to support it by documents. The latter course was taken by J. Kendall, who ridicules Dalton for having got as the atomic weight of oxygen "exactly 7"[67] (with the accent on "exactly").

Dalton's estimates of the atomic weight of oxygen were, respectively, 5.5, 5.66, and 6.5; the last result he rounded into 7. The same "experimental" error was later committed by Davy, who rounded his 7.5 into 7. The correct "experimental" result, namely 8, was achieved by Prout not by observation but by "sagaciously meditating" (as the inductivist phrase goes) over observation reports made by others.

That the up-to-date textbook gives the correct result as 16, incidentally, rather than 8, is a totally different matter. First, it is the direct outcome of replacing the formula HO for water by the formula H_2O. Second, it has been conventionally agreed to keep it at 16 in spite of the vagaries of measurements that are still being improved, making the best research workers of yesterday into "crude experimenters".

So much for the conflicting evidence of Davy and Thomson and for the inductivist historians' singular inability to record both and exercise judgment. They cannot record any piece of evidence without either accepting it or ridiculing it.[68] Take the simple historical case of the widespread recurrence of those famous myths about Newton's apple, Watt's mother's kettle, Signora Galvani's diet of frog-legs, and the like. Today the custom is still either to endorse such a myth ─ especially if it was invented by the discoverers themselves ─ or to ignore them.[69] Very few people criticize these myths (which is not difficult), and none has yet studied their origin and historical role, or explained their persistence. For, if they are true they are not myths; and if they are myths they have nothing at all to do with science. Science and mythology are opposites! (Only Bernard Shaw spoke about the presence of myth in science, adding quite rightly, that as far as science is concerned, myth is of little significance.)[70]

The purpose of the following section is firstly, to show that a historian need neither transcribe nor ignore historical evidence, and secondly, to criticize the Baconian identification of error with wickedness, dogmatism, mythology and superstition. In it I shall make use of historical evidence which cannot be taken at its face value, and tell a story about how Ampère's success was due to his refusal to reject a hypothesis in spite of empirical evidence. I find the story very interesting, and I fervently wish to put it to critical readers because I am not qualified to examine it myself.

6. Ampère's Discovery

Ørsted's discovery in 1820 of the interaction between magnets and electric currents caused a sensation. The French astronomer Arago heard about it during his travels and related it in a meeting of the French Academy upon arrival. A week later his friend Ampère announced his discovery of the interaction between electric currents and his discovery of the electromagnet. As Maxwell has noted,[71] Ampère preferred to relate the Baconian rather than the true history of his discovery. My attempt to reconstruct the true history of the discovery makes use of the following historical material.

In his beautiful obituary notice on Ampère, Arago reports[72] that after Ampère had announced his discovery some people said that it followed from Ørsted's discovery: for, if two currents interact with a magnet, they said, clearly they also interact with each other. This greatly annoyed Arago, who immediately pulled out of his pocket two keys and asked these same people whether they believed that since each of the two keys would interact with a magnet they would also interact with each other. This charming and thought-provoking story has not, to my knowledge, ever been discussed by historians. Inasmuch as the anonymous people to whom Arago refers wished to belittle Ampère's, there is indeed little point in trying to analyze what they have said. Inasmuch as what they have said may be considered as an historical explanation, I find it highly interesting and challenging, supportable by quotations from

Ampère's[73] and perhaps partly true. I cannot understand why historians have failed to take up this challenge. Here we have a situation where a thinker makes a discovery a week after hearing about a similar discovery. Evidently things are as closely connected here as one can ever expect them to be in the field of scientific development. What was the connection? What exactly was Ørsted's influence on Ampère? Evidently Ampère's discovery does not just follow from Ørsted's discovery. But what assumption links them? And was that assumption new or a development out of old ones? And how did Ampère come to that assumption?

I suggest that perhaps Arago's adversaries were right on one point: Ampère did use the assumption that only likes interact (only gross matter acts on gross matter, only magnetic matter acts on magnetic matter, and the same for electricity, heat, etc.). But he used another assumption as well, which they overlooked.

The assumption that only likes interact is highly problematic, and, as Einstein has shown,[74] it leads to essentially insoluble problems. But the amazing unanimity with which it once was held may be explained by showing how many problems it helped to solve. Take, for example, the one to which Arago was referring. As only likes interact, and as iron interacts with magnets, it must contain magnetic atoms. Why, then, does iron act on iron only in the presence of a magnet? This was answered by Coulomb's theory of magnetism:[75] there exist two magnetic fluids, north and south. A drop of each is present in each magnetic atom, and this explains why a piece broken magnet is itself a magnet. In a magnet the magnetic atoms are ordered in one direction; and, by Newton's law of addition of forces, the forces that they exert on another magnetic atom strengthen one another. In an iron bar, magnetic atoms are orientated in all directions and thus, again by Newton's addition law, the forces they exert on another magnetic atom practically cancel each other out. When a magnet is present, the magnetic atoms in the iron bar reorient and the bar becomes a temporary magnet. This theory of Coulomb explained a wide range of phenomena of magnetization and demagnetization.

Ørsted seemingly refuted the law that only likes interact by finding that the magnetic needle of a compass is deflected in the presence of an electric current. He did not try to refute the law that only likes interact; rather (see below, Section 17), he argued that electric forces and magnetic forces, and indeed all other forces, are likes; that essentially there exists only one kind of force. But this idea was too bold — or mad — to be considered even for a moment. Even in the twentieth century, Ørsted's chief biographer and the editor of his *Works* chides him for his silly speculations (which influenced Faraday and Einstein).[76] In any case, there is little doubt that Ampère had nothing to do with Ørsted's speculations. Ignoring Ørsted's idea of the essential identity of all forces, then, one seems to be bound to admit that Ørsted refuted the hypothesis that only likes interact — unless, of course, either currents are really magnets or magnets are really currents. Ampère's brain-storm was marvelously simple: the hypothesis that only likes interact can be reconciled with Ørsted's discovery by assuming that magnets are really currents. Coulomb's atom of magnetism that contains a drop of north and a drop of south magnetic fluid has to be abolished; it should be replaced by small electric currents that run inside the magnet. Magnetic matter does not exist, and magnetic phenomena are all produced by electric currents. Thus, Ampère's hypothesis comes to rescue the hypothesis that only likes interact! Ampère's discovery of the interaction between currents and his discovery of the electromagnet follow logically from Ampère's explanation of Ørsted's discovery, by his hypothesis of currents in iron plus the normal Newtonian assumptions about forces, moments (rotations), etc. As magnets are really currents, we must conclude that Ørsted's discovery was the discovery of interactions between currents, although Ørsted himself did not know it. This conclusion is not mine but Ampère's.

So much about Ampère's factual discovery. He did more than this, of course. Ørsted thought that his own discovery, the deflection of the needle of the compass in the presence of a current, was caused by forces of rotation. Newtonian theory, however, denies the existence of rotational forces; it assumes the existence of forces that either pull or push; and it regards each enforced rotation as the result of a couple of forces acting in opposite directions (as a driver rotates his steering wheel by pulling and pushing it). Ampère's explained the interaction between currents as a result of such forces. Yet even this was not orthodox enough. Newtonianism recognizes forces that depend on particles and their configuration (namely relative positions), but not on their motion. In order to render his theory more orthodox still, Ampère tried to make some hypotheses about the ether (the all-pervading thin elastic fluid) and the friction between the ether and the current. In this way he tried to explain the interaction between currents as forces depending on configurations rather than on motion.

Ampère's, who was an admirer of Newton and an inductivist, thus opposed the method of speculations.[77] The theories he used — that only likes interact and that all forces are pulls or pushes that do not depend on motion were not mere hypotheses but were verified by experience. And what he added, particularly the theory that the atoms of magnetizable matter contain electric currents, he considered to have been verified. At the time, almost everybody shared this view. But eleven years later, Faraday refuted Ampère's theory,[78] and after a further fourteen years Weber claimed[79] that Ampère's error was due to his having speculated; namely, due to his not having followed the proper method of science. The connection between Ampère's errors and his discovery was then ignored: any connection between black and while must be purely accidental.

7. The Broad Outline of the History of Science

I come now to my chief criticism of the inductivist historians of science, which I shall begin by summarizing. The inductivist historians of science tend to ignore, the existence of scientific schools of thought, intellectual climates, trends, and the like. To be sure, in the twentieth century some of them have at last admitted the historical fact that trends do exist in the history of science; but they explain them as follows. Science has, it is said, at different times centered on different fields of inquiry. But such an explanation is hardly satisfactory. For what gave rise to the interest in a given field at a particular time? The inductivist cannot reply that interest in a specific field was based on the hypothesis that this field might prove fruitful; for such hypotheses, since they cannot be based on evidence, must be prejudices. What an inductivist historian may do, however, is to explain this concentration of interest by material or social conditions or needs. Here Marx's theory of the economic infrastructural basis of science is convenient. Thus, seventeenth-century interest in astronomy cannot be rationally explained by the inductivist except as the result either of navigators' needing good nautical almanacs or of the development of technological conditions. Marxism is very influential in the Western intellectual world in a field where it is least applicable — the history of science. This paradoxical situation is explained by the fact that most historians of science, despite their inductivism, still try to account for trends in the history of science.

Let me now turn to a more detailed consideration of this thesis. Inductivism, I have said, blinds historians of science to the chief factors in the history of science — contending schools of scientific thought. Bacon said that science and schools are poles apart, science being based on facts and schools on dogmas. Consequently, the inductivist historian is forced to side with one school, the scientific one, and to pretend that the others never existed, being unscientific. Inductivist historians of science have, for instance, generally agreed that towards the end of the eighteenth century Rumford proved experimentally that heat is motion. The survival until the middle of the nineteenth century of the opposite school, which contends that heat is matter, they view with dismay,[62] and they are quite blind to the highly rational argument in which the two schools were engaged between the second and fifth decades of the nineteenth century. They also ignore the fact that Dalton, Sadi Carnot, Gay-Lussac, and other great contributors to the theory of heat, belonged to the mistaken school. Rumford's alleged empirical proof, it is thought, ought to have vanquished the mistaken school through the observation that friction is an unlimited source of heat. This fact, of course was well known even to preliterate people.[80] That it does not disprove the theory that heat is matter is obvious: friction was also known to be an unlimited source of electricity, yet in Rumford's time the Cartesian view of electricity as motion had been almost universally rejected. But the inductivist historian, thinking that Rumford's view accords better with the up-to-date textbook, is disposed to accept all that Rumford (allegedly) claimed, and to ignore Dalton's just contempt for Rumford's mode of argument.

Davy's experiment[81] of rubbing two pieces of ice in a vacuum until they melted, is often quoted by inductivist historians as further evidence that the calorists were prejudiced. No comment is added to this telling argument. But one need only stop to ask why Davy's experiment was conducted in a vacuum to locate the inductivist error. The calorists' answer to Rumford's claim that he could create unlimited quantities of heat is that he could only transport practically unlimited quantities of heat from the environment to the rubbed body. Unless Davy was thinking of this reply, his repetition of Rumford's experiment in a vacuum was pointless; and indeed one can see from his works that he was thinking of this reply. Unlike Rumford's experiment, Davy's invited some reply: how did caloric pass to the two almost entirely isolated pieces of ice? The answer, or at least the first step towards it, is provided by Sadi Carnot's theory of the relation between work and heat-transfer. His theory was based on the calorist idea that heat-transfer is always limited by the total quantity of heat available in the system. The doctrine of heat as motion was conclusively routed by Carnot. The story that it really lost the battle is a puerile inductivist myth. The heat-as-motion school had to undergo a drastic reform[82] and admit that heat is motion plus something else, a new property called entropy, which took over a great deal from Carnot's caloric.

Although this account strikes me as rather obvious, all my attempts to find it in histories of physics have failed. In saying this, I do not wish to imply that no historian of science has ever studied the history of any controversy: Koyré is perhaps the chief promoter of such studies. But they remain extremely rare. More frequent are historians' studies of past metaphysical and methodological controversies amongst researchers. The inductivists naturally try to ignore such controversies, and with some reason: they are obviously extra scientific. Yet their relevance to science is too close for them to be able to avoid noticing such controversies altogether. That inductivism, determinism, and Newtonianism characterize so-called classical, but not modern, physics is well known. The inductivist historians usually either ignore these opposed views or else they endorse them one at a time. Dampier-Whetham, who was an arch inductivist, rejected[83] inductivism with marvelous ease when discussing a point in modern physics (namely, Bohr's principle of complementarity). Whittaker has entirely ignored the conflict between classical determinism and modern indeterminism in physics. And as to the question whether Newton's theory is entirely true or only approximately so, Whittaker gave one answer in his first volume and another in his second.[84]

In explanation of such practices, we may notice that it is a traditional policy of inductivist historians of science to pretend that in science there was only one revolution, the Renaissance revolution against prejudice and superstition that started the smooth development predicted by Bacon. Inductivist historians seem unable to contemplate the overthrow of a view without concluding that the overthrow proves the pointlessness of ever having held such a view. In a number of histories of chemistry I have read that Faraday overthrew the popular belief that chlorine and carbon do not combine; but nowhere, save in Faraday's own article,[85] have I found the statement that this view originated with Davy (and was accepted by most chemists for most of its life). Now small revolutions and big revolutions must be regarded by the inductivist as similar at least in that they are events that ought to be concealed; the bigger ones, however, are somewhat less easy to conceal.

A different explanation of the behavior of the inductivist historians is also possible. It is that historians of science unconsciously adopt the view of science accepted by a particular scientist while they are writing about the work of that scientist. The revolution in chemistry was announced by the inductivist Lavoisier, who encouraged his wife to burn Stahl's books ceremonially. Consequently, Lavoisier's labeling of Stahl's view as a prejudice is still transcribed by historians of science. The revolutions in modern physics were begun by Einstein and his followers; and Einstein was a staunch anti-inductivist, who respected and admired Newton greatly. Consequently, Einstein's expression of respect for Newton is transcribed by the historians. Most historians of science are transcribers who appear as inductivists simply because most of the physicists (and other scientists) whose opinions they transcribe — having lived prior to Einstein — were inductivists.

This explanation is, however, false: historians of science usually fail to transcribe details that do not fit into Bacon's scheme, and emphasize those which do. Nowhere in the literature on the history of physics have I found discussed the fact that methodological disagreements have been as common in science as in philosophy; on the contrary, most historians of science insinuate that all great scientists, ever since the time of Kepler and Galileo, have, of course, been staunch inductivists.

Some inductivist historians will agree that science and other human activities do interact — perhaps in order to explain the broad outline of the history of science. Yet, since, from an inductivist viewpoint, an extra scientific intellectual influences are bad, they tend to confine their discussion to extra scientific nonintellectual influences. As Marx's theory explains scientific interests as being rooted in social, economic and technological conditions and needs, rather than in preconceived opinions, it often appears to be the best answer to the inductivist historian's needs.

It is my impression that almost the whole of the current literature about the social and technological background of science contains nothing that goes beyond Marxist doctrine, though by no means all of the authors in question accept all of Marx's doctrines. I shall discuss the inductivist-Marxist mythology about the dependence of specific discoveries on technological advances in section 16 below. What inductivist historians say about scientific trends and the broad outline of the history of science is also, as a rule, Marxist. (The exception is the broad outline of the spread of inductivism, already discussed in section 4 and discussed again later in section 10.) To criticize such accounts, one can point out that according to Marxism, taken at its face value, mediaeval science is greater than ancient science, because the mediaeval feudal system is a higher historical stage than the ancient economic systems that were based on slavery. One who can swallow this can swallow almost anything.[86] Moreover, although it is an important fact that social and economic interests may influence scientific interests, Marx's theory according to which social and economic interests always are the ultimate causes of scientific interests is probably the silliest of all his doctrines. For instance, the Marxist suggestion that we owe interest in geometry to the need for land-surveying is important up to a point, beyond which it is absurd. The geometry that fits in with the suggestion is a primitive geometry, and this primitive geometry is very important because without it there would have been no Euclidean geometry. Yet Euclid's semi-axiomatic theory, as well as the intellectual interests behind it, are a different matter altogether. The uniqueness of Greek geometry and the uniqueness of the Greek intellectual climate stand in marked contrast to the commonness of the Greek socioeconomic system. Admittedly, the uniqueness of the Greek intellectual climate may be interestingly related to the uniqueness of the Greek political setting; but to say this is to contradict yet another Marxist doctrine, namely that political systems merely reflect socioeconomic systems.[87]

Another example of the dogmatic character of Marxism, and its wide influence, is the widespread Marxist myth that the development of the steam engine was an outcome of the rise of capitalism. The Marxists explain the development of steam engines as due to the economic interest of the capitalist. They stress the fact — which they can explain — that the Greeks did not use steam engines; they ignore the fact — which refutes their explanation — that the Greeks had steam engines. When Marxists are asked about this latter fact they may brush it aside with the remark that, obviously, Heron of Alexandria was "ahead of his time". They would not even suspect that this amounts to the admission that the sequence of events refuses to fit Marx's rule of ordering, and that Marx's theory of the material causation of intellectual developments is thereby refuted.

In my own view, inasmuch as historical intellectual trends are explicable at all, their intellectual causes are much more significant than their socio economic causes. The spectacular success of Newton's theory is definitely an intellectual matter; its success was judged by certain highly intellectual standards (though it also greatly depended — if Einstein's theory is anywhere near the truth — on accidental circumstances which happen to characterize our solar system). And the success of Newtonianism influenced, and is still influencing, many aspects of our intellectual activities. Newton's influence did not lie solely in the acceptance of his doctrine. For Newton encouraged the view that human knowledge — even certainly and absolutely true human knowledge — is possible. His influence is also to be seen in the acceptance of his program of explaining all physical phenomena as the results of forces that are attached to material particles that pull or push each other at a distance, forces that depend on matter and on distances alone. It seems to me impossible to present a coherent picture of the history of physics in the eighteenth and nineteenth centuries without bearing this in mind; and it is this aspect of the Newtonian school that to a large extent characterized the trends and intellectual climates of these centuries. One of the most remarkable works in the history of physics is *The Evolution of Physics* (1938), by Einstein and Leopold Infeld, which narrates the stories of the rise and fall of the Newtonian school and of Faraday's school.

To sum up, the broad outline of the history of science is the history of the choice of central problems, and of the various schools of thought that attempted to answer these problems. But inductivism tells us that science begins not with the choice of problems but with observations of hard and fast facts. Inductivism cannot attribute intellectual interests to free choices based on preconceived ideas, but only to extra-intellectual factors usually of a socioeconomic character.[88] Thus, inductivism is at its worst when, applied to the broad outline of the history of science.

With all its faults and myths, inductivist history of science had a golden age; I have referred to the pioneering works of Priestley and Laplace and to Dr. Thomas Thomson's excellent history of chemistry; and I must also refer to Whewell's monumental *History of the Inductive Sciences* that succeeded it almost immediately, even though Whewell himself was partly Baconian and partly Kantian. The last remarkable inductivist work was J. Munro's humble and charming *Pioneers of Electricity*, of 1890, written with the fresh naiveté becoming to his naive inductivist philosophy. After that there was the occasional inductivist scholar, like A. N. Meldrum; but even he was on the defense: the glory had gone. The golden age of inductivism came to an end with the rise of the conventionalist philosophy of Duhem and Poincaré, a viewpoint that made it utterly impossible to return to the naive days of innocent inductivism. Yet inductivism is still going strong; and it has borrowed from Duhem new ideas of how to write the history of science while indicating its broad outline. The result of this grafting of an anti-inductivist idea onto inductivism proper is intolerable, even when handled by great scholars like E. T. Whittaker. I shall try to show this (Section 10) after first sketching a general picture of the conventionalist philosophy (Section 8) and the particular idea of Duhem that has had such a great influence on inductivism (Section 9).

8. The Rise of the Conventionalist Philosophy

Although Newtonianism and inductivism reigned together during the eighteenth and nineteenth centuries, neither ever enjoyed unanimous support. Even when Laplace defended Newtonianism-cum-inductivism — when both doctrines were still at the height of their popularity — Kant had already launched a severe attack on the latter. In the most interesting part of his most popular epistemological work, the Prolegomena to Any Future Metaphysics that May Claim Scientific Status, after concluding his discussion of natural science,[89] Kant gives examples to support his dictum "the understanding does not draw its laws (*a priori*) from nature but imposes them on nature". His examples, from physical astronomy, are the theories of Kepler and Newton that he declares *a priori* true. The concepts of circles and conic sections, of force varying with the inverse square of the distance, and of the relation between such a force and conic sections — all are conceived as *a priori* concepts that cannot possibly emerge from the phenomena. Rather — here relying heavily on Cotes' preface to the second edition of Newton's *Principia* — Kant maintains that they all belong to geometry. They are first intuited, and then combined with the phenomena into scientific experience.

No single idea between the publication of Newton's Principia and that of Einstein's papers on relativity and photo-electricity had as great an impact on philosophers and on historians of physics as did Kant's idea of the *a priori* origin and validity of scientific theories. Kant's influence on historians of science was, however, by no means simple. His theory of knowledge is notoriously difficult to understand, and it is even more difficult to decide whether, on any given interpretation, it is useful to historians of science in their daily work. In my own view it is not so, because it allows for only one system of scientific thought. Fortunately, however, other thinkers modified Kant's ideas in various ways, yielding a variety of theories of knowledge and different applications of those theories to the history of science. What is common to all of them is, first, the idea that theories do not emerge from facts, and second, to a lesser extent, that the adoption of one or another attitude to science or its products is an intellectual choice. Whatever one may say about Poincaré's conventionalist philosophy or about its relations to Kantianism, one can barely fail to notice and admire his bold, Kantian (and very influential) assertion that no known fact can dissuade one from adopting, even today, Ptolemy's system of the heavens, and that the current choice of another system depends on the preference for simplicity in theories, a preference that cannot itself be defended empirically.[90]

The central doctrine of conventionalism is that scientific theories are neither true nor false, that their general frameworks are mathematical systems that serve as pigeon-holes within which to store empirical information. Which pigeon-hole system to adopt is a question of choice, for which simplicity provides the criterion. We can rearrange a pigeon-hole system or change it without thereby proving its falsehood, or unscientific character, or badness. Theories can fit facts with greater or smaller degrees of simplicity. Hence, simplicity is a criterion not of absolute, but only of relative, merit; it is a substitute for the absolute criterion of merit of the inductivist.

The conventionalist view that the simple theory is preferable to the less simple one, not a true theory to a false one, has proved a useful tool in the hand of the historian of science — as the physicist, philosopher, and historian of science Pierre Duhem illustrates in his works. The reason for this is not, in my view, the admitted importance of simplicity, but the introduction of a new criterion of graded valuation to replace the old inductivist criterion that divides theories into the good and the bad. This can be seen by contrasting a conventionalist history with, say, Whewell's history that is (in my view) the very best inductivist history. Whewell discarded the myth of induction from fact to theory. But because he maintained that facts verify theories he still allowed no gray in his pictures, only white, demonstrably true theories. He even had a very important grading criterion: the more general or universal a hypothesis, the better. But as all the hypotheses in his ladder of axioms were pure white, his grading criterion did not help. Philosophically, degrees of generality are as important as degrees of simplicity. And yet, historically, degrees of generality did not produce gray shades for Whewell because he stuck to his verification principle, whereas degrees of simplicity did allow Duhem to use gray shades, because simplicity was for him an alternative to verification. And here lies Duhem's major advantage over Whewell: here at last gray shades entered the historian's pictures.

Admittedly, even before Duhem's work there did exist some historical passages that were neither black nor white; but these pictures were not, so to speak, complete. I have already mentioned Laplace's gray. But the existence of even more important gray patches can be seen, for example, in Dr. Thomson's claim that phlogistonism was so widely accepted at one time because of its high degree of explanatory power, and in Ørsted's claim that phlogistonism is a close approximation to Lavoisier's antiphlogistonism that it resembles. Most important, perhaps, is Whewell's wonderful sketch of the gradual emergence of Newtonianism that refers to quite a few half-baked ideas and false starts that occurred between Kepler and Newton. These few cases[91] provide samples of almost all the colors that the historian of science needs. Not a single philosophy of science, however, was able to accommodate them — much less a philosophy of the history of science that is still nonexistent. So these colors were never used systematically prior to the development of conventionalism. And even conventionalism accommodated only one grading criterion — degree of simplicity. This explains why this particular criterion has been the only one historians of science have used systematically.

In this way conventionalism avoids the black-and-white pictures of the inductivist. It throws overboard inductivism together with the claim that theories are empirical. It allows freedom of thought in theoretical science at the price of viewing theoretical science as purely mathematical, and the empirical side of science as the fitting together of theories and facts. This fitting can be done with varying degrees of simplicity, depending on the facts and the theories available.

What, then, is the historians' task according to conventionalism? Their first task is to refute inductivism by historical example; further, they should interpret the history of science as the history of the growth of simple theories.

Where inductivist historians relate the story of a black theory and of a white theory, conventionalist historians will try to show the indebtedness of the allegedly white thinker to the allegedly black one. The inductivists who are consistent in their approach will have either to reject the conventionalist picture or rethink their general philosophy. Regrettably, they are often too busy for this. We should recognize the immense significance of conventionalist studies of this kind, of the connection, on the part of conventionalists, of many allegedly white thinkers with allegedly black thinkers — the description of Newton's debt to Descartes, of Galileo's debt to his semi-mediaeval predecessors, and the like. While admitting the great significance of this method, I wish nevertheless to criticize the theory that justifies its excessive use.

9. The Continuity Theory and the Emergence Technique

A few historians of science who are neither inductivists nor conventionalists accept some ideas from both schools of thought but in an eclectic rather than systematic fashion. E. T. Whittaker's history, for example, is an inductivist history with conventionalist elements. Butterfield's exciting The Origins of Modern Science, to take another example, is a conventionalist history with inductivist elements. Although there exist today a variety of other eclectically based histories, I know of no exposition, let alone a critical examination, of the middle-of-the-road philosophies on which they are based; and significant though these histories undoubtedly are, they lack that consistency of approach that might lead to a critical examination of their historical reasoning.

It is no accident that the most severely criticized historian of science is Pierre Duhem, and that the criticism of his work consists of the most interesting and exciting historical studies. I admire Duhem and his consistency. It is easy to improve upon him by patching up his philosophy so as to cover its greatest defects. But possibly the understanding of these defects and the attempt to present explicitly a better philosophy might prove as fruitful as his. Since, to my knowledge, this has not been done, I shall now try to explain Duhem's philosophy and its defects as explicitly as I can.

The historical examples of how one thinker may be indebted to a previous opponent's idea already constitute refutations of inductivism. And Duhem has shown that such cases are numerous. The question may arise now as to how often things have happened in this way, and to what extent any given thinker has been indebted to a predecessor. Duhem's answer is what I would designate as "the continuity theory": all thinkers are greatly indebted to their predecessors, and all progress is in small steps.[92] This is a substitute, in a way, for Bacon's and Whewell's ladder of axioms no step of which may be skipped. In one way, there is a similarity between Bacon's approach to science as gradually developing and Duhem's approach. Yet there is, in another way, an immense difference between the radicalism of Bacon's approach to science, and the conservatism of Duhem's. Bacon's radicalism let him to dismiss medieval science — lock, stock, and barrel.[93] Duhem's continuity approach made him present even Copernicus as a follower of some immediate predecessors. Although I reject equally strongly both approaches, I think that Duhem unquestionably had the upper hand over Bacon: science never does start afresh. But Duhem's view is much too conservative. Admitting that there is some degree of continuity between the ideas of Faraday and those of his predecessors, as well as between the ideas of Ampère and those of his predecessors, I also think that Faraday was undoubtedly the more revolutionary of the two.

The continuity theory of Duhem justifies the use of a particular technique that he shares with most conventionalist historians. This is the same technique that Whewell used to describe the gradual emergence of Newtonianism from Keplerism that I have already mentioned. The rules of this technique that was Duhem's major tool are very simple: every idea has a predecessor that resembles it more closely than any other predecessor; find it! And find between the two ideas all those events that make the transition between them smoother, especially discoveries of some facts. I would call this "the emergence technique".

In using Duhem's emergence technique one may often be tempted to suggest false or possibly false historical hypotheses that one would not assert explicitly. If I were to present Duhem's emergence technique while using the emergence technique, I should first describe all the applications of this technique prior to Duhem's, including Whewell's passages, and then, after quoting Duhem on it, say that he used it more extensively than Whewell. I would thus leave it to my readers to guess for themselves, if they like, that Whewell influenced Duhem, rather than say explicitly that in my view Whewell did do so.

There are two reasons for this temptation to insinuate ideas rather than to assert and discuss them openly. First, I prefer to conceal the sad fact that I have not read through all the many volumes of Duhem's works in order to check my view. Secondly, Duhem's refusal to acknowledge his debt to Whewell is, in my opinion, explained by the fact that he was a nationalist and an Anglophobe; an opinion that is very unpleasant to report, especially when it is difficult to support it by clear-cut evidence.[94] Both difficulties can be avoided by using the emergence technique that helps one to gloss very elegantly over bothersome details.

But it would be cowardly to employ the emergence technique here; it is better to assert explicitly, despite my ignorance, the hypothesis that Duhem, although indebted to Whewell, did not acknowledge this because he was an Anglophobe. I shall be very glad — being a sincere admirer of Duhem in spite of his admittedly great faults — to be corrected on this point. And I shall be glad if my assertion interests someone more qualified to judge its correctness.

But the problem of the emergence of Duhem's technique, interesting as it is, is less interesting and important than the problem of why Duhem developed the emergence technique. And this problem cannot be answered while using the emergence technique. More generally, the emergence technique is based on the assumption that ideas are developed in order to have simple views of things; but this is not only an insufficient explanation of the history of ideas, but is often just untrue.

Duhem developed his own ideas not simply in order to have a simpler view of science, but also in order to criticize inductivism, rehabilitate the dignity of medieval science, and defend his conservative theory of science and its history. He seriously thought that every intellectual development is but a small variation of some previous idea, and this view obviously justifies his development of the emergence technique.

So far I have merely criticized the application of the emergence technique to Duhem's philosophy. This, it is very important to notice, is no criticism of Duhem's philosophy itself, since Duhem himself never dreamt of the application of his technique outside the field of the history of science. On the contrary, he stressed that it cannot be applied to metaphysics. And he used his idea that the history of science is a history of gradual development as a stick with which to beat those who assert a connection between metaphysics and science.

However, as E. A. Burtt argued in his exciting book *The Metaphysical Foundations of Modern Physical Science* (1925), the history of science is connected with the history of metaphysics and therefore does contain revolutions. Duhem himself, a Roman Catholic, was an Aristotelian who desired to combine Aristotelianism and devotion to science. This may or may not be possible; but undoubtedly the Renaissance thinkers thought not; and their view profoundly influenced the growth of science. Hence metaphysics had a profound influence on physics, and hence Duhem was mistaken. Whatever one may say about the Renaissance thinkers' indebtedness to their medieval predecessors, the great and revolutionary development was their attempt to release themselves from Aristotelian fetters. This is not the place to sum up the splendid modern criticism of Duhem's continuity view of the connection between medieval and modern science. My point is merely that the work of Burtt, Koyré, and other historians of science has severely battered Duhem's continuity theory, with the obvious result that the emergence technique should now be applied more critically and less universally.

10. The Cancerous Growth of Continuity

Examples of continuity exist to refute inductivism; examples of discontinuity exist to refute the continuity theory, though not conventionalism as such, since a conventionalism without the continuity theory is possible. One might, therefore, expect historians of science to abandon inductivism and the continuity theory while accepting either a version of conventionalism or some other alternative. Instead the tragicomedy of the historiography of science culminates with a vast number of historians of science writing histories from an inductive-cum-continuity viewpoint.

One can hardly blame these people, especially since some of them, notably Whittaker, have produced important studies. It is exceedingly difficult to relinquish inductivism, especially since the traditional alternative to it — conventionalism — is defeatist to the extent that it regards scientific theories as uninformative formal systems. And the pressure on historians of science to accept the continuity theory has been hard to resist, especially after Duhem had criticized the radicalism that inductivism embeds.

Bacon's radicalism — his demand that all past opinions be discarded and science started afresh — has led to radicalism both in politics and in the writing of political history.[95] One reaction to such radicalism has been traditionalism, which is a sort of continuity theory of politics and of political history, and whose most famous promulgator was Hegel. Cassirer, who was a Hegelian in a semi-Kantian, semi-conventionalist guise,[96] was also a kind of historian of science, or at least an originator of a philosophy of the history of science; and it was Cassirer who transported Hegel's philosophy into the field of the history of science.

Cassirer's style is somewhat confusing: when he says that Kepler was "the first to formulate exact laws of nature"[97] one may puzzle about why a law involving ellipses is more exact than a law involving circles or epicycles, and one may try to provide a few answers and see that none of them fits the context. It is hazardous to interpret such an author, but as he seems to play an important role he cannot be overlooked. The thesis of his The Problem of Knowledge seems to be that philosophy and science have evolved together[98] and that as philosophy has well-known national traditions — notably English, French, and German — so has science. This is not to deny that the various traditions interact, of course, but their interactions do not deprive them of their individuality.

Quite apart from the traditionalist and nationalist elements of this philosophy, quite apart from Cassirer's having taken over from the inductivist tradition an irrational offhand way of dismissing people whose ideas do not fit his scheme,[99] and quite apart from the fact that in Cassirer's philosophy people must fit not the textbook but an obscure scheme that nobody quite knows — apart from all these defects, Cassirer's philosophy serves up an appalling superabundance of continuities. If a thinker's idea can be only partly traced to a predecessor, try to trace the rest of the idea to another predecessor. There is no limit to this (Hegelian) dialectic, of course, and historians may continue it until they are quite convinced that they have given readers the feeling that the ideas that under analysis are deeply rooted in the tradition — in home ground and in neighboring countries.

It is difficult, if not impossible, to decide whether Cassirer has influenced other thinkers.[100] Nor is this relevant to my aim. I shall merely note a few forms of the continuity theory of our century that — although less harmful than Cassirer's — are still objectionable. The earliest introduction of continuity into the story of the growth of science appears to be Laplace's idea of the gradual spread of inductivism. Laplace introduced this idea — if at all

— very cryptically, we may remember, and only in order to solve a problem. Nowadays the idea is stated explicitly and emphatically, in an attempt both to introduce continuity for its own sake and to connect the history of science with general intellectual history. That the superposition of this idea on orthodox Baconianism is inconsistent, I have already explained (Section 4). 1 shall, therefore, confine myself here merely to mentioning a few examples. The geologist Frank Dawson Adams, puzzled by the fact that in the past so many intelligent thinkers uttered so many absurdities about the history of the earth, tried to study the history of geological science in an attempt to explain this fact. Adams came to the conclusion that people believed such absurdities because they speculated and that geology could only really begin in earnest when the idea that science begins with observations had been widely disseminated, an idea heralded by Roger Bacon but fully understood and put to practice by geologists only yesterday. How this agrees with his excessive employment of the Duhemian continuity technique, I cannot understand; and I only hope that the information in his highly informative book[101] will be put to use by some more critically minded historian of geology.

My second example is *The Western Intellectual Tradition* (1960) by J. Bronowski and B. Mazlish.[102] (I shall not discuss its parts on the social sciences here.) It is a book no less difficult to comment on, though for different reasons. For instance, the part on Copernicanism begins with: "to us the system of Copernicus is coherent and satisfying", and almost immediately following this remark three objections to it are listed, two of which are unanswerable. The ensuing text makes it impossible for me to judge what force the authors attribute to these arguments. Another example from the same book is even more remarkable. "With the discovery", the authors say, of Kepler's laws, "exact, compact, and remarkable, the paths of the planets were mapped once and for all …. Kepler had established the system of Copernicus in these formulas beyond challenge. There was no further step to take until the three laws could be shown themselves to be parts of a single unity, a single law holding each planet to the sun."

With this collector's piece I hope I may omit what the authors say about the details of the history of science, and pass to their comments on the history of science at large. "A dominant trend of the period which this book covers," they state, "is the rise of the scientific method, both in the natural and in the human sciences." They say, and claim to have shown in the book, that the proper method is that of "the interplay of empirical experiment and of rational enquiry." Leonardo, Galileo, and others, they claim, knew the trick of balancing experiment and thought; the Cartesian school tipped the scale in favor of thought, and the Royal Society of London and its derivatives tipped it in favor of experiment, thus leading to both technical achievement and the "smug inhumanity of the early industrial society." Here we have a broad outline of the history of science, its connection with the social background of science, and a methodological continuity theory similar to the one in our earlier example, all in one single idea.

The sad situation in the field may be better appreciated when the work just referred to is seen in its context. For instance, it has been warmly received, and has been reviewed in highly appreciative terms by no less qualified a person than C. P. Snow,[103] who tried to read some Marxian continuity into it, on lines discussed above (Section 7).

The thesis Bronowski and Mazlish champion is similar to that of Butterfield's well-known book, *The Origins of Modern Science*[104] that covers much the same ground in a superior way. Butterfield accepts the continuity theory together with conventionalism. In the first edition of his book Butterfield does not refer to Duhem except in the short book-list, and in the second edition he refers to him in his text only as one who has exaggerated the continuous flow from mediaeval science to modern science. But he expresses indebtedness neither to Duhem himself nor to Koyré and others who previously criticized Duhem on similar lines. Now Koyré's criticism of Duhem's continuity theory raises no problem for Koyré himself since he is anti-conventionalist and opposes the continuity theory; it does, however, create a problem for Butterfield, who does accept the continuity theory.[105] His solution is this: science is the proper mixture of facts and theories, with the open-mindedness towards theories that conventionalism prescribes. The quest for facts and the right open-mindedness were attitudes very difficult to acquire; indeed, their eventual acquisition constitutes the revolution of science. This development Butterfield describes in a masterly fashion. From this point onwards, Butterfield implies, the growth of science was smooth, with the exception of the revolution in chemistry; but this revolution came to abolish a mere prejudice — the phlogiston theory — so that it resembles the Renaissance revolution.[106]

The readers should be able to recognize the Duhemian and the Baconian elements in the mixture; I hope they will also agree that it has novelty and interest. When, however, the same mixture is used by others after being spiced with the traditional inductivist-style eulogies to Copernicus and Kepler, as in the passage I have quoted from Bronowski and Mazlish, it becomes somewhat less palatable.

The talk about the right mixture of facts and thought (originated by Bacon) that is common to be the two books just mentioned, is intolerably vague.[107] The claim that only one methodological revolution took place in the history of science is a flagrant violation of well-known truths. Duhem could never have asserted this, knowing all too well that the Renaissance, including Galileo, was rather *a priorist*, that inductivism did not sweep the whole of Europe until the late nineteenth century, and that the diversity of views amongst scientists concerning method makes it impossible that scientists' method and their views of method should always coincide. Although the claim that there is a continuity in the history of methodology is a myth, even its most recent versions are accepted with acclaim despite their inferiority to earlier versions.

Duhem, we may remember, was aware of the impossibility of applying the continuity theory to the history of metaphysics. A heroic attempt to perform this impossible task was made by Max Jammer in his masterly *Concepts of Space* (1954). One can only say that the book is excellent despite this serious blemish. The continuity aspect of it is most unconvincing, and it may even sound somewhat nationalistic: one can hardly credit the medieval Jewish thinkers about space with the amount of influence that, by applying the emergence technique lavishly, Jammer implicitly attributes to them. Like Adams' history of geology, Jammer's book is rather a source book for a future historian than a proper history of concepts of space. But future historians will have to abandon the idea of compressing the whole history into a few hundred pages. Koyré's *From The Closed World To The Infinite Universe* (1957) deals with a small aspect of the story, not from the dawn of history to the present, and his book is longer — and more readable.

It is interesting to note that although the various continuity theories of methodology and/or metaphysics are philosophically inferior, they have given rise to exciting works like Butterfield's and Jammer's. It looks as if even the strangest approach were preferable to following the well-trodden path.[108] But this is not always true. Some of the new continuity approaches are too trite to be of any use. I shall briefly mention one of the most popular before closing this section.

Duhem's emergence technique consists in presenting in succession a variety of pictures of the world, differing from each other only to a small degree. The inductivist cannot allow this: each element in each picture must be the last word, a quotation from the up-to-date science textbook. What the inductivist can do is to start with the picture as presented by the textbook, and erase parts of it in stages, until it is entirely erased, and fix a date to each stage in this process of erasure, going backwards to the dawn of civilization. In these partly erased pictures not only details known since long ago appear, but also — and this is the novel continuity aspect — a faint sketch of present-day pictures. Thus Max Caspar, Kepler's biographer, is immensely pleased with his hero, who had a glimpse of Newton's theory of force.[109] This is, of course, somewhat dubious: Kepler's idea of force, one can argue, has almost nothing to do with Newton's; Gilbert's force is much more akin to Newton's. It is undeniable that thinkers have glimpses of ideas that later on are rendered more specific and prove to be of importance. But to select only such successful glimpses while ignoring vague ideas that do not turn out to be glimpses of the future, or which turn out to be such glimpses only if drastically modified, and to render a specific idea vague so as to make it look like a glimpse of a future idea, and to ignore the modification of a vague idea that have rendered it a glimpse of a future scientific idea — these techniques, popular as they may be, are hardly worth criticism.

Loewinson-Lessing's *A Historical Survey of Petrology* assumes continuity in the history of the subject based on the history of the development of empirical techniques. "There is one fundamental and leading principle," he writes, "which lies at the base of petrological science. This principle was dimly perceived at the dawn of modern geology and petrology and since the time of its conception has been steadily growing and expanding. The conception is that of development — the evolution or gradual change to which every rock is subject from the very moment of its formation. This conception was expressed as early as 1858 …"[110] This evasive talk is very understandable to the petrologist, who knows that on the basis of false hypotheses about crystallization it was taken for granted that metamorphic rocks would crystallize only when cooling, and hence must be ancient, until Hans Reusch found fossils in a metamorphic rock that show that the rock was relatively young. Reusch's finding is mentioned in every history, and the ensuing problems of how metamorphic rocks were formed, which problems led to studies combining generalizing and historical hypotheses, are referred to in many works; but no historian of geology or petrology, to my knowledge, has reconstructed the history of views on the topic. All that Loewinson-Lessing tells us is that all is in flux, and that now we know of more flux than a century ago. He cannot bring himself to state the hypothesis that implied the antiquity of metamorphic rocks, let alone explain why that hypothesis was once universally accepted.

To conclude, the general points of this section are these. For philosophical reasons the need was felt to describe an internal organic growth of science, and an organic growth of western culture with science as one component in it. This kind of philosophy is irrational, and has led to the uncritical search for connections of all kinds at all costs — national connections, methodological and metaphysical influences, and glimpses into the future.[111]

The continuity theory is historically important in providing a refutation of inductivism and as a source of a set of problems concerning the extent and causes of continuities and disruptions in the different chapters of the history of science. The emergence technique has obscured all this by allowing historians to present views by implication, thus leading to the cancerous growth of uncritically presented, and often silly, continuity theories. I shall ignore the continuity theory from now on as much as possible in order to study some more important, if less popular, aspects of conventionalism. Yet before doing so I should acknowledge that I have perhaps done the majority of historians of science some injustice by classifying them all as inductivists and compromisers between inductivism and conventionalism. As I have noted (Section 5), there do exist historians who exhibit neither philosophy nor method; and these constitute a sufficiently large class to deserve to have in this work at least one section to themselves. In some measure of amendment for this omission, I shall now mention one prominent example of a methodless work.

My example is A. R. Hall's *The Scientific Revolution, 1500-1800* (1954).[112] I was impressed by his question "What Lavoisier did is clear enough: how did he do it?" (337), for I wanted to know this. The "what" covers a few pages that I cannot summarize. The "how" is easier: Lavoisier was not hampered by prejudice, thought properly, coordinating many facts with rigor and precision. (Two years later, the 18th international congress of the profession declared the situation quite unsatisfactory and invited historians of science concerned with eighteenth-century chemistry to come together and straighten things out; *Isis*, 48, 1957, 185.)

Overwhelming readers with facts is not an inductivist prerogative; nor is ancestor-worship, as the following entertaining instance may indicate. "Clearly Newton, like most great men," Hall writes, "was fortunate in the hour of his birth: in 1642, the year of Galileo's death". (247). But the ancestor-worship is tempered: Hall qualifies his remark that Newton strode the peak of the scientific revolution by a few observations (244). One of them is that Newton was not a biologist; that he even had little influence on biology. "Despite his genius," Hall bravely adds to his qualifications, "despite his rapid and sure mathematical invention, despite his experimental precision, science was always for Newton a detached intellectual pursuit, not an activity, a cause, close to the emotional core of his being. Strangely, to modern ways of thinking, alchemy seemed to have given him a greater sense of the ultimate mystery than the unfolding of the celestial system." How beautifully, to the modern ways of thinking, are the various good old inductivist ingredients mixed! The source of this censure, I suppose, is a poetic passage describing Newton sitting late at night enchanted and wrinkle-browed before the fire on which alchemical concoctions were brewing; it was written by another of Newton's idolaters, E. N. da C. Andrade, in the Newton Tercentenary Celebrations Volume. Andrade, however, presents it frankly as a figment of his imagination. I suppose Andrade was trying to hide behind his poetic imagination some measure of inductivist embarrassment at Newton's alchemy; but poetic apology can be turned into a qualification of one's admiration if not into censure.[113]

Yet it would be wrong to accuse Hall of inductivism: in his preface and conclusion he presents himself as a conventionalist. And, indeed, when he transcribes Duhem's works he becomes almost a true conventionalist; when he transcribes Koyré he departs even further from inductivism. Like Koyré he criticizes Galileo for his errors (84, 89); like the conventionalist Metzger he defends phlogistonism as an important stage in the history of chemistry (328-9); and like almost any inductivist he states that "it was possible to be deceived by such analogies" as follow the pattern invented by Lavoisier (334). And he gives as an example the idea that muriatic acid is a compound of an unknown element with oxygen (rather than of chlorine and hydrogen), while skillfully concealing the fact that the absence of oxygen from hydrochlorine refutes Lavoisier's central doctrine of oxygen as the sole agent of combustion, calcination, and acidulation.

But I do not wish to imply that these are the only ingredients that appear in Hall's book. Aristotelianism, for instance, is rare in the annals of the history of science, so I should like to take this opportunity to quote at least one Aristotelian passage. "From the historical point of view," says Hall (237), "instruments may be divided into two classes: those which render qualitative information only, and those which permit of the making of measurements." This passage comes from Chapter 8, called "Technical Factors in the Scientific Revolution" (217-243).

I shall now discuss briefly the great value of conventionalist histories, since they are the only valuable histories in our own century that were written according to an explicit philosophy. I shall then try to criticize this philosophy and present an alternative to it.

11. The Comparative Method

History deviates from the inductivist theory according to which an idea is either black or white: we are able to compare two theories that we think are good and say which is better. The emergence technique seems to be more popular than any comparative method, perhaps because the latter requires the exercise of judgment while the former operates on the more superficial level of appealing to the feeling that one predecessor to a given theory is closer to it than another. In my view, serious studies of the history of science demand comparison and judgment of theories against a given historical background rather than against the standard of the up-to-date textbook. That there are deviations from the black-and-white picture also means that we can compare ideas of different times, and, most important, that we can show their superiority over their immediate predecessors; thus our appreciation of an idea will depend on a historical context. I consider this last statement, which is already a commonplace among modern political and social historians and modern biographers, a general truth of historiography. Apart from a few early exceptions, the historical context was first introduced as a permanent feature of the history of science by the conventionalists. James B. Conant, a scientist as well as a conventionalist philosopher and historian of science, has strongly emphasized this point.[114] He calls the historical approach "dynamic" since it helps appreciate a theory relative to its background, to see it as progress. It may be preferable to call his view "kinematic" rather than "dynamic", since conventionalism misses a main driving force of science, the thinker's problems and sleepless nights. But this does not diminish the significance of the conventionalist's introduction of the historical context and the comparative method. Conant's explicit discussion of the comparative method is very valuable, even though it is open to criticism as insufficiently dynamic. The merit of conventionalism lies in just this: that it assesses a scientific idea by comparing it with its background and predecessors rather than with the up-to-date textbook.

There can be no greater praise of the comparative method than to say it is genuinely historical. The only way to do it full justice is to apply it extensively to detailed case histories, along lines followed by Duhem and Conant. I shall sketch and discuss the most problematic case history in the history of the physical sciences — phlogistonism.

Phlogistonists claimed that there exists one element in common to all combustibles, namely "the combustible", or "phlogiston". The received opinion about the role of phlogiston theory is indescribably naive. It is said that the progress of theoretical chemistry was stopped by phlogistonist prejudices; that theoretical chemistry between Boyle and Lavoisier is wholly black, Lavoisier having started where Boyle had left off. Indeed, the inductivist J. H. White claimed in 1932 that Lavoisier began where Jean Rey, the early seventeenth-century French chemist, had left off.[115] Thus, incredible though it may seem, the liveliest period of chemistry, the time of the greatest chemical discoveries — of the common airs and the decomposition of water — is pictured as its Dark Ages. Inductivism denies the usefulness of thinking in the discovery of facts, and is thus the source of anti-phlogistonist prejudice that is incompatible[116] with this case of splendid experimental progress in the face of widely accepted prejudices and superstitions. The inductivists get into difficulties here because their philosophy forces them to both appreciate and despise the phlogistonist. (The arch-phlogistonist Priestley reaped all the medals and honors of the scientific world of his day.) Not only the factual discoveries of the phlogiston era, but also the theoretical developments during this period, are quite impressive. Stahl's doctrine was universally accepted in the seventeen-thirties; its serious rival was first presented in the seventeen-fifties; the Lavoisierian revolution in chemistry was started in the seventeen-seventies and came to be universally accepted in the seventeen-nineties. That there was constant change in the field of theoretical chemistry, and that a wealth of theories were proffered at the time, is a historical fact that the inductivists do not deny; yet they cannot be impressed by ideas whose only function, they are convinced, was to rescue an ancient and even mystical prejudice.

The inductivists' bias against phlogistonism may derive from a disapproval of theories that do not survive long enough to leave their mark on the up-to-date (chemistry) textbook. Yet pointing out this fact will not suffice: the conventionalists have to criticize the specific reasons that the inductivist historians offer to support their hostility towards phlogistonism. This the conventionalists will have to do, since, after all, they agree with the inductivists about the status of medieval astrology, and thus have to argue in detail each case where they disagrees with the inductivists about the status of a theory. Moreover, the exercise is highly rewarding because the material refuting inductivism will in itself contrast the theory in question favorably with its historical background. What, then, are the inductivists' arguments against phlogistonism?

The first of these arguments is that phlogistonists defended the theory of levity, or of negative weight. On this allegation rests the popular dogma that Stahl was an Aristotelian. Now, first, negative weight need not be a prejudice or an Aristotelianism any more than the equivalent idea of negative energy that the great modem physicist Paul Dirac once entertained. Secondly, as J. H. White has shown, phlogistonists did not assume levity; on the contrary, Stahl explained the small weight of charcoal ashes, as compared with the weight of the charcoal that produces them, by postulating that charcoal consists of almost pure phlogiston. The trouble was that the "ashes" of metals, the calcinated or burnt (these two processes were the same for phlogistonists, as well as for anti-phlogistonists) metals, such as rust, differed from coal ashes in being heavier than the stuff from which they were produced. But since the ashes of metals were considered as exceptional and the ashes of coal as typical, the idea of the levity of phlogiston could not even be proposed before Lavoisier had shown, at a much later stage, that Stahl was mistaken here; Lavoisier's first great discovery was that ashes of sculpture and phosphorus were heavier than the stuff from which they were produced.

Incidentally, that Lavoisier's discovery was intended to show that metals rather than coal represent the rule is evident from his behavior: he submitted the result of his finding to the French Academy to secure priority, but in a sealed note so as to keep to himself the idea of weighing more and more combustibles before and after combustion. (The tradition of sealed notes was created by Boyle; its inductivist character is obvious: preconceived ideas have to be guarded in secret as they may be valuable guides for experiments; but they cannot be published, and priority for them secured, as, officially, preconceived ideas are bad. The honor for the application of the inverse square law to electricity, accordingly, does not go to the thinkers Franklin and Priestley but to the experimenter Coulomb; this is very unlike the case of the honor that goes to Max von Laue for having conceived, though not applied, the idea of X-ray crystallography.)

What annoys the inductivist historians is that Stahl made no mention of the fact that the ashes of metals are heavier than metals. F. Sherwood Taylor is obviously right in his claim that "the artisans who made it their business to convert lead into red-lead, or lead and tin into the ashes used for glazing majolica-ware, knew very well that the weight of their product exceeded the weight of the metal."[117] He doubts, or gives Stahl the benefit of the doubt, whether Stahl knew this fact as well. For myself I do not doubt that Stahl knew the fact: he was well versed in the crafts related to his studies. And though I join the inductivists in disliking Stahl's reticence about the difficulty, I join the conventionalists in thinking that the inductivists are making much fuss about a difficulty that Stahl had every right to consider as small and to leave open; it was a difficulty that could be answered by various ad hoc hypotheses, such as Macquer's, according to which the emission of phlogiston by a metal is combined with the absorption of some other material.[118] True, such hypotheses reduced the simplicity of Stahl's theory, especially after Lavoisier's discovery, mentioned above; and their failure to be corroborated experimentally increased the ad hoc character of phlogistonism even further. This is why phlogistonism was eventually abandoned in favor of Lavoisier's simpler anti-phlogistonism. But this development took time, and depended on *data* discovered long after Stahl's death, as well as on lengthy considerations of which conceptual system accommodates them more simply. Thus, the inductivist historians are too harsh on the phlogistonists' presentation of ad hoc hypotheses, as these were the best available in their time; and until recently the inductivist historians were particularly mistaken in attributing to the phlogistonists the hypothesis of levity, or negative weight, of phlogiston, as the leading phlogistonists, especially by Priestley, explicitly rejected it. Here is an interesting characteristic of the inductivist tradition: Stahl has been accused, from the time of Lavoisier until recently, of having proposed the wrong explanation of an experiment of which he had no knowledge. Although White has pointed out the important historical fact that Lavoisier's experiment was performed after Stahl's death, Stahl is still accused of not having foreseen the correct result of Lavoisier's experiment. Even White, who is by no means hostile to Stahl, dismisses his doctrine as unscientific for this very reason. Similarly, since Priestley's attempt to reconcile Lavoisier's results with phlogistonism ended in failure, we who are wise after the event must condemn it. Even Priestley's modern defender, J. P. Hartog, views these failures with dismay. "Priestley displayed", says Hartog, "what seems to us almost perverse ingenuity in adapting the phlogiston theory to fit every new fact." As Sir Oliver Lodge has put it: "In theory he [Priestley] had no insight for guessing right …; he may almost be said to have held a predilection for the wrong end [of the stick]."[119] Great scientists have to foresee intuitively the future textbook of science in order to gain the approval of their inductivist judges.

The second inductivist excuse for anti-phlogistonist prejudice is also anachronistic. In 1837 Dr. Johnston claimed[120] that Boyle could have made a hundred years' progress in one step had he weighed a certain bottle before and after a certain experiment.[121] As McKie has discovered,[122] Boyle did do what Johnston suggests, as he was challenged to do so; but, perhaps because this action was not as significant for him as it was later for Lavoisier, he got incorrect results. In any case, it is ludicrous to choose one out of many thousands of experiments that Boyle performed merely because it reminds one rather vaguely of Lavoisier's experiments.

A third inductivist argument against phlogistonism focuses on the alleged similarity of the views of Boyle and Lavoisier. They both used the same definition of "element"; and they shared (with Democritus and Rey) the doctrine of the conservation of matter. The fact is, however, that all phlogistonists, especially Stahl, made extensive use of Boyle's atomism. The inductivists rarely bother to read Stahl, since they are sure that he was prejudiced, and since inductivists like Partington and McKie, and like White, who have read the phlogistonist literature, reaffirm that phlogistonism was a prejudice and waste of time. Also, Boyle, Hooke, and Mayow had views that resemble

Lavoisier's more than do Stahl's.[36] Yet this objection has been answered long ago.

In 1831 Dr. Thomson explained the eighteenth-century acceptance of phlogistonism by reference to the simplicity with which it had explained so many of the then known phenomena.[91] His idea was along the lines of thought later developed by conventionalists. Conventionalist historians, since they employ the comparative method regularly, can do much more to impress their readers with the greatness of phlogistonism. They can describe the mechanistic view and its utter simplicity, a simplicity that makes it difficult to see how it can possibly apply to the complex phenomena of chemistry. They can then show how well Stahl's doctrine integrates with the mechanistic idea and how with few assumptions he nevertheless could correlate many chemical phenomena that Boyle and Becher had been unable to correlate. The simplicity and elegance of phlogistonism make one appreciate it quite regardless of the subsequent history of chemistry; in the true historian's fashion conventionalists can revive phlogistonism as an exciting intellectual experience of the discoveries and of attempts to incorporate them in the phlogistonist framework. [123] They can describe the various attempts to modify phlogistonism, and show how Lavoisier's modification proved to provide the simplest and most beautiful way out. And they can thereby at once make one relive a great experience, and show the relative merit of Lavoisier as compared with Stahl and other phlogistonists.

I fear that this sketch is insufficient to illustrate the power of the comparative method. To do this we would have to study a historical problem in detail, and imagine Stahl's or Lavoisier's picture of the world, the integration of their ideas and of the facts they knew. This demands both detailed historical knowledge and active historical imagination. It is because the conventionalist historians are imaginative, and because they choose details judiciously, that I so much prefer them to the inductivists. But even the conventionalist approach is seriously limited. Since simplicity is the sole conventionalist criterion, the conventionalists cannot make much sense of any controversy that took place over a long period: assuming that sooner or later the simplicity of one of the competing doctrines becomes obviously greater than that of the other, the conventionalists have to dismiss one school even before it ceases to be scientifically important and interesting. Let me show this by continuing my sketch of the story of phlogistonism beyond its point of utter defeat. From here on it is chiefly the story of Dr. Joseph Priestley.

12. Priestley's Dissent

How can we explain Priestley's obstinate adherence to phlogistonism long after Lavoisier's theory had gained unanimous acceptance? Stephen Toulmin's attempt[124] to defend Priestley, and his conviction that it is the merest prejudice to view the old master as prejudiced, seems to me impressive and laudable. But his argument is unacceptable. Toulmin's view, in brief, seems to be this: it is not easy to judge, at least immediately, which of two competing theories is simpler. Hence Priestley had a right to stick to phlogistonism for a time. Toulmin's view is true not of Priestley but of his friend Richard Kirwan. Having explicitly stated that simplicity was his criterion of choice, Kirwan quite consistently was able to give up phlogistonism later. Priestley stuck to phlogistonism to the end. The explanation of Kirwan's behavior can be fully stated, using the comparative method, within the conventionalist philosophy, by showing how the further discovery and discussion that brought out the greater simplicity of Lavoisier's anti-phlogistonism led to Kirwan's change of mind. The explanation of Priestley's behavior, since it differed from Kirwan's, must be different; otherwise the comparative method will permit too much arbitrariness, and thus lose its point.

The traditional Baconian explanation of Priestley's behavior is that he stuck to his views dogmatically. Bacon's doctrine of prejudice explains prejudice psychologically[125] in terms of self-interest: a person becomes prejudiced from the motive of "a desire for victory, or of distinction"[126] and from fear of losing face by admitting error. Priestley explicitly refuted this explanation of his behavior. My refutation is extremely simple: self-interest prescribes the denunciation of phlogistonism, as the public renown for Kirwan and public contempt for himself shows; Kirwan did not abandon phlogistonism nor did Priestley himself stick to it in self-interest; both acted in the interest of truth. Priestley's own explanation of his behavior, however, is astonishing: if we assume that phlogistonism is false, he declared, we would have to conclude that all the great thinkers of the recent past had been prejudiced — a conclusion to which he could not bring himself.[127] Priestley thus adhered to Bacon's doctrine of prejudice with respect to his immediate predecessors, while disproving it by showing that it did not apply to his own generation. The result was that he dogmatically stuck to phlogistonism, or became prejudiced, because he was not sufficiently critical of Bacon's doctrine of prejudice. He was thus a quaint mixture of criticism and dogmatism, as well as of amiability and harshness; and he still waits for a master biographer, especially since he led a rather stormy life. On the one hand he had a great facility for changing his views.[128] On the other hand he stuck to his phlogistonism to the end. On one hand, like a truly rational thinker, he despised attempts to compromise between his own phlogistonism and Lavoisier's anti-phlogistonism.[129] On the other hand, he lumped under the single label "phlogistonism" Stahl's and Cavendish's doctrines, though these two differ from each other no less than do Cavendish sand Lavoisier's doctrines. (There is no scientific doctrine that answers the historian's description of phlogistonism. There exists Stahl's phlogistonism, and Black's and Cavendish sand others' scientific versions of phlogistonism. What is common to all these doctrines is only a metaphysical assertion that there exists "the combustible".)

Could one say, then, that from a certain stage of his development onwards Priestley ceased to be a scientific researcher and became a dogmatist or a metaphysician? Although this might appear a concession to the inductivists, in some cases (e.g., Lorentz adherence to a kind of Newtonianism to his dying day)[130] concessions seem reasonable. Yet in Priestley's case such a concession would be mistaken.

It is fortunate that Priestley did stick to phlogistonism. His final masterpiece begins with an ingenious challenge to the anti-phlogistonists to answer his criticisms and so prove their own doctrine. I find his list of serious and clever criticisms impressive.[131] They seem to have been taken up immediately, some leading to important discoveries, and some to the subsequent downfall of anti-phlogistonism. I shall mention only three. First, according to (Cavendish's) phlogistonism, but not according to anti-phlogisto-nism, one can extract inflammable air — hydrogen — out of charcoal. Priestley mentioned an experiment supporting phlogistonism here. A year later Cruickshank showed that the inflammable air obtainable from the experiment with charcoal to which Priestley had referred was not hydrogen, but a previously unknown compound — carbon monoxide (CO).[132] Cruickshank's discovery was both the driving force and the tool of Dalton's early researches (see Section 3 above).[133] Second, when metal dissolves in an acid solution, inflammable air appears — out of the dissolved metal according to phlogistonism, and out of the water according to anti-phlogistonism. But where, asked Priestley, is the oxygen that must be released when hydrogen is extracted from water? As Davy showed later, both parties were mistaken. His subsequent studies of these processes led him to discover the alkaline metals. Third, Priestley's stress on and analysis of the difficulty facing anti-phlogistonism because of past failure to decompose oxy-muriatic acid led Davy to the idea that the acid is an element — chlorine — and thus ultimately to the refutation of Lavoisier's claim that all acidulation, calcination, and combustion involves oxygen.

The relations between the old ostracized Priestley and the young successful Davy are as interesting as they are touching. Modern historians hardly pay attention to Priestley's criticism since it appears in a book called *The Doctrine of Phlogiston Established* (1800), even though its subtitle was "and that of the anti-phlogiston refuted". And to contemporaries the friendly relations between Priestley and Davy merely made the latter suspect. Their suspicion was proved correct after Davy was beyond the wrath of the narrow-minded and vexing Baconian public opinion: his posthumous Fragmentary Remains clearly show that he did indeed flirt with the idea of reviving phlogistonism.[134] I do not consider this of any importance, but suggest that it makes it even more embarrassing for the inductivist historians of science to study the history of phlogistonism and its aftermath: they kindly turn a blind eye to Davy's slight phlogistonist tendencies; it does not occur to them that there is nothing to turn a blind eye to: great scientific researchers need great freedom of thought and they commit no sin even when they investigate ideas that are very convincingly dismissed by everybody else.

It is this intellectual obstinacy, dissatisfaction,[135] and restlessness, so characteristic of scientific research, even when conducted by a calm, well-adjusted, and placid person, that even the best conventionalist literature somehow manages to neglect. Both the emergence and the comparative methods are, *in toto*, post mortem procedures; they miss the very life and living force of science, its problems and difficulties, its struggles and disappointments. The conventionalists have done much to move away from the picture of the history of science as a smooth success story, but their account still leaves much to be desired.

13. The Advantage of Avoiding being Wise after the Event

Although the comparative method is the only widespread method that is at all satisfactory within its limits, these limits keep the method within too narrow a range of application. Obviously, all historical methods must be of limited applicability since it is impossible to record the whole historical development as it really happened; but although we should not pursue a complete method, we can investigate interesting aspects of history that lie outside the domain of a given method, and see whether they can be tackled either without a method or with the help of a new method.

There is much to be said for trimming the edges of the history of science. We may appreciate false starts but find it unimportant to dwell on them at length because, important as they may have been, they led nowhere. Einstein would, admittedly, be seen in better perspective after an attempt to survey briefly the history of attempts to tame the ether prior to his own daring dismissal of it. If we are to be less parochial than the inductivists, we should appreciate Einstein's indebtedness to those who made false starts, since they discouraged him from trying to do what they failed to do, and thus directed his energies to perhaps more fruitful channels. And yet, all this does not justify the effort of studying in detail today the entire history of theory about the ether. Since all these ether theories came to nothing, even the most ingenious of them is of limited interest. E. T. Whittaker's *History of the Theories of Aether and Electricity* is at once an extremely sketchy and quite satisfactory report of the major theories of the ether (though not of the Einsteinian revolution, of course); it would be tedious to go into the various theories of the ether in any further detail; Lorentz's detailed study of the aether[136] is nowadays justly ignored.

To say this is to concede to the conventionalists that in certain respects we are allowed, as historians, to be wise after the event in the interest of arousing our readers' curiosity. Quite possibly, it is a mistake to make this concession; for the time being, however, let us make it without debate. Even when making it and thus allowing historians to use up-to-date knowledge in order to choose their topics, one can claim that the conventionalist historians trim the history of science to a smoother outline than is necessary, that they beautify the topics that they chooses to study. It is *prima facie* obvious that valuable studies could be made of many interesting debates between the various scientific schools without excessive use of hindsight. And yet the conventionalists must ignore such debates. They are not so poorly equipped as the inductivists who must side with one school and ignore or defame the other; the conventionalists may side with the calorists when discussing the contributions of Lavoisier, Dalton, and Carnot, and with the thermodynamists when discussing the views of Clausius, Kelvin, and Maxwell. But the conventionalist method is useless when it comes to reconstructing the struggle between the schools, and weighing the merit of the criticisms leveled by each school against the other.

I myself accept Popper's view that intellectual activity consists in imaginative proposals or solutions to given problems, and in successful criticism of these solutions that, in turn, lead to new problems. Popper considers proposals that are open to experimental criticism and the attempts that have been made to criticize them experimentally to be the body of what is called empirical science. This view obviously clashes with inductivism. According to inductivism, phlogistonism (or calories, or etherize) has been successfully criticized; hence it is essentially, and for all times and places, a prejudice. According to Popper, on the other hand, it is a scientific doctrine for the very same reason; namely, it was empirically criticized and hence it is open to empirical criticism and hence scientific. To sharpen the difference: falsified theories are the one thing the inductivist declares to be obviously nonscientific; according to Popper these very theories are most obviously scientific. The conventionalists simultaneously view phlogistonism as scientific and deny that it is open to criticism. They can admit that it was shown to be more limited in application and thus less simple than its successors; but they can claim that this is no criticism — because we cannot, for instance, blame Stahl for not having known about Lavoisier's later discoveries and ideas. True, the conventionalists might add, Lavoisier thought otherwise; he considered himself a critic of Stahl and even encouraged his wife to burn Stahl's books ceremonially; but this terrible obscurantism was rooted in Lavoisier's extra-scientific shortsightedness: philosophically he erred, because he was an inductivist, scientifically he acted as a good scientist, namely, as a conventionalist. The inductivists' blindness to the fact that we can learn from criticism is essentially unhistorical. It can therefore be dismissed, I think, especially in a study like the present one, written for historically minded readers. But the conventionalist view seems to be strengthened by its historical approach. For example, the conventionalists staunchly refuse to criticize Stahl for his lack of knowledge of subsequent developments that he could not predict. I shall try to argue in the following sections that, though the conventionalists' refusal to condemn Stahl is preferable to the inductivist approach, they are still under the influence of inductivism to the extent that they consider criticism as condemnation, confuse Stahl the individual with Stahl's doctrine, and ignore the fact that, historically, Lavoisier's behavior can be understood as an attempt to examine critically the doctrines of Stahl and Black. I shall try to show that in at least three distinct ways the conventionalists are, to their loss, unnecessarily wise after the event. But first I wish to speak in a more general way about why it is preferable not to be wise after the event. This is a point of which concerns all modem historians and all philosophers of history.

In my view, contemporary historians so much excel their predecessors, and the art of biography has become a twentieth-century specialty, chiefly because one of the great discoveries of our age is that we can do better by not being wise after the event. There are various reasons for this. For one, it makes our history more interesting; it makes us relive old experiences. Trying to see the world through the eyes of our predecessors is en-lightening — both in teaching us to avoid parochialism, and in teaching us to appreciate our heritage better. It tells us the story of the struggle that led to the rise of our present conditions — that we tend to take for granted instead of appreciating, understanding, and cherishing. But the simplest, though seemingly rather abstract, reason, is methodological. The only way we have to explain historical events satisfactorily is by the use of what has been called "situational logic", by reconstructing the situation of historical people and their objectives, and by deducing from our assumptions the conclusion that their actual behavior was the most appropriate. This has been emphasized and well argued by Collingwood in one of his most celebrated passages in his *Autobiography* (1939).137 There, Collingwood claimed that this historical method is applicable only to successful behavior (such as that of Nelson at Trafalgar). He came to this conclusion because he sought certitude, and because he thus thought that only when an actor's aim had been achieved can we know for sure what that aim was. Popper, by contrast, has claimed that situational logic leads to theories that are open to criticism; he has no need to accept the limitation set by Collingwood since he does not aim at certainty. It is the application of situational logic that should prevent us from being wise after the event and encourage us to try to reconstruct the problem situations of past thinkers.

The attempt to avoid being wise after the event need not bar us from using up-to-date knowledge that was unavailable to the historical personages whom we study. But this knowledge may be applicable, within situational logic, only to attempts to construct the historical conditions; it is extremely important to notice, as a part of the situation, the knowledge limitations of the historical figures that we study. We may try to apply Freud's theory of the Oedipus complex to Hamlet's "case" on at least two conditions. First, we must remember Hamlet's ignorance of such things as modern psychology. Second, we must remember, or reconstruct, Hamlet's own views on human nature, that are different in some respects from Freud's. By violating either of these conditions, we are likely to misinterpret his character and fail to grasp his problems and difficulties; by following both of them we shall perhaps be able to explain his difficulties and see how he was unknowingly torn between various desires. Although these two conditions seem obvious, the historians of science are normally wise after the event merely because they violate them. Let us look at some standard difficulties of historians of science and ask whether they might not be overcome with the use of these two conditions (that, clearly, belong to situational logic); namely, that we should remember that our historical figures did not have access to present day knowledge, and could not avoid holding views rather different from our own.

14. The Difficulty of Avoiding being Wise After the Event

It was Bacon who asserted that we cannot help being wise after the event when we are confronted with a new discovery: how is it, we wonder, that we did not see it before?[138] Koestler has raised the same question with respect to theoretical discovery, and he too thinks it is practically impossible to avoid being wise after the event. How could people hold such silly views? Perhaps, suggests Koestler, if we try to remember our own intellectual make-up as children, we may learn to be less wise after the event.[139] How could obvious truths be overlooked for so long? Archimedes' insight was to connect two things that were not connected before, but the connection is so natural and obvious, that those who have heard of it can never eradicate it from their minds. We can never quite reconstruct the pre-Archimedean absence of Archimedes' insight, that is too deeply rooted in our own outlook.[140]

One could easily add many well-known cases to this list. It is difficult to understand why oxygen was discovered so late when the difference between fresh air and foul was known to cave dwellers. It is difficult to understand how the discovery of solar spectral lines could wait for almost two centuries after the discovery of the spectral decomposition of sunlight. It is difficult to understand how the vacuum waited for Toricelli centuries after the limitation on the suction pump was common knowledge. It is almost impossible to believe that it demanded a researcher of the scientific powers of Black to discover that melting and evaporating are processes which absorb immense quantities of heat; every primitive cook knows it from experience, it would seem.[141] The evidence for Mendelism is so abundant that one would expect every gardener and farmer to know at least a few instances of it; yet even the simplest instances had to be observed by Mendel, and people are still surprised each time they see in an offspring characteristics shared by neither of its parents.

These genuine difficulties must have been felt by many people, including historians of science (see below). The fact that they have seldom been discussed can be explained, at least to some extent, by the traditional optimism concerning science that hardly leaves room for difficulties. Bacon, for instance, saw no reason why the whole of science should not be developed within a few generations, once the medieval superstitions had been overthrown.[142] And only sixty years ago many serious physicists thought that the task of the theoretical physicist was rapidly nearing completion.[143] It is still commonly taken for granted that science became plain sailing as soon as the inductive method was comprehended; first Brahe observed, then Kepler generalized, and meanwhile Galileo observed and generalized, and then Newton observed and generalized on a higher level, thus finishing off the job of theoretical mechanics. If science in general progresses so smoothly and rapidly one cannot reasonably complain about a few oversights.

Of course, it is only too obvious today that this picture is naive: even Laplace could not avoid seeing that the history of theoretical mechanics is not so simple. And the question of being wise after the event concerns more than mere oversights: the evidence for the existence of positrons, or against the law of conservation of parity, was never a small matter that could be overlooked because its significance was unclear; and yet it was overlooked and even deliberately ignored for at least one decade.[144] This shows that the problem is more serious than that of explaining an occasional oversight. This also invalidates Koestler's proposal to try to avoid being wise after the event by viewing the world as children do: children know nothing about parity or positrons or solar spectra.

Partial solutions to this problem may be given by reference to technical developments: certain mathematical and experimental techniques must have preceded certain discoveries; certain discoveries must have preceded certain theories. Oxygen could not have been discovered prior to certain technical developments concerning the collection and weighing of gases. Solar spectral lines could not be observed without a good telescopic spectroscope; one could not plot ellipses easily without knowing a certain amount of mathematics (Kepler used the newly invented logarithmic techniques). And so on.

Even though the problem is hardly ever explicitly stated, these solutions are often found in textbooks of the history of science.[145] The solutions are often false, as reconstructions of the experimental and mathematical techniques of the period in question show. Oxygen was discovered with the help of primitive methods known to the ancients. The solar absorption spectrum was technically visible to Newton, not to mention the early nineteenth-century observers who missed it again and again. Kepler's use of the logarithms was inessential though very helpful for his purposes, as were most if not all of Brahe's records that he made use of. Leeuwenhoek's discovery of microbes was done not with his microscope but with a mere magnifying glass — at least a millennium after it could technically have taken place. Roentgen's discovery could technically have taken place half a century earlier, with the invention of the cathode tube (that itself could technically have been invented at least a century earlier).[146] And as Gamow has observed[147] the technique of measuring the order of magnitude of atoms is so primitive that even Democritus could have used it.

The problem ought to be stressed because it is surrounded with bigotry and folly. People who could technically make a discovery but failed to make it are sometimes censured by historians who are wise after the event,[148] and sometimes they are defended with the obviously false claim that techniques necessary for the discovery were unavailable. Almost all historians of science stress the very few cases in which discoveries did rapidly succeed the technical innovations on which they depended — like Galileo's telescopic discoveries — and in which theories rapidly succeeded the discoveries of the facts they were designed to explain — like Bradley's theory of aberration. The fact remains that these are a small minority of all cases.

In most cases it is not only easy to be wise after the event; we can hardly avoid being so. Undoubtedly, the oversight of certain facts and ideas was not due to the technical difficulties alone, but to others of a different character. And if we wish to avoid being wise after the event we must find out what these were. The difficulties, I suggest, are usually the scientific theories accepted at the time. We tend to be wise after the event because we tend to forget how the world looked before the event took place, because we tend to forget the scientific errors that the event corrected. We thereby become unable to appreciate either the great difficulty of having produced the event, or its intellectual value. Our tendency to forget past errors is due to the spell Bacon's theory of errors still exerts on us. His erroneous theory ascribes errors to the sinful faults of superstition and prejudice.

The great merit of the conventionalist method is rooted in its daring revival of old errors, and that the serious shortcomings of this method are rooted in the fear of admitting that the revived errors were indeed errors. The conventionalists too are still under Bacon's spell; they too still cannot boldly admit the existence of errors of which humanity can be proud. By contrast, Popper's doctrine of errors treats most of human greatest intellectual achievements as errors to be proud of, and human greatest discoveries of facts as the refutations of such great errors. According to this doctrine valueless errors are those that the person who holds them can easily criticize, whereas valuable errors quite often yield to criticism only after generations of combined effort of many able people.

Modern European languages as yet possess no verbal distinction between a valueless or a silly or a negligent or an irresponsible error, and one committed responsibly and judiciously.[149] Since, according to the misleading accepted usage of these words, all errors are faults and all who err are wrong it is time that phrases like "valuable error", "stimulating error", "intelligent error", "clever mistake", be used and single words be coined to convey shades of meaning. In law courts the difference between responsible errors and errors due to negligence is well known since the law often treats them differently.[150] But ordinary language[151] is so powerful (especially in the present day, when it has become the idol of a philosophical cult) that it reinforces the confusion[152] between different kinds of error. By using "errs" and "is wrong" as synonyms, we often condemn people and imply that one ought to avoid all error successfully. The phrase "had no idea that" ambiguously covers both the case of a person who asked the right question but (understandably) gave it a false answer, and the case of a person who avoided the same error by (foolishly) failing to ask the right question.

If we allow for respectable errors we can avoid being wise after the event by finding errors that were easy to endorse and difficult to criticize, that people presumed prior to a discovery, and that the discovery refuted. This would explain, after a fashion, both the significance of the discovery and the difficulty of making it, as being one and the same thing. I shall now apply this suggestion, first to theoretical discoveries and then to the discovery of facts.

15. The Obstacles on the Way to a New Idea

New ideas are new solutions to old problems or solutions to new problems. When a new idea is a solution to an old problem it is often not the first solution to that problem. Often an older solution to a given problem was a good one, and hence the new idea was not called for until the old solution had been effectively criticized.[153] To accept this thesis, one must first accept that some solutions that are open to criticism can be good. The history of science reveals a number of such examples. As long as Newton's theory of gravity withstood all tests there was no need for an alternative theory — that explains why no serious attempts were made to replace it much before Einstein. However, as I shall argue below, even when a solution has only appeared to be good — such as the solution to the problem that Archimedes solved — it has often happened that no need has been felt for a new solution.

The situation may often be rather complicated. A good solution to one problem may block the way to a good solution of another — as I shall show with the example of Kepler's ellipse — and not until the solution is criticized will the new idea be able to emerge.

My two examples, the rise of Archimedes' theory, and that of Kepler's, are due to Koestler; he confesses that he cannot avoid being wise after these events, and I shall argue that he ignores the difficulties in the way of the development of these ideas by ignoring the important errors that Archimedes and Kepler had to overcome. But this is not to imply that once the obstacles on the way to a new idea are overcome the new idea must of necessity arise. The rise of an idea is the outcome of a work of genius, an unaccountable development. By recognizing the obstacles on the way to a new idea we may increase our appreciation of it; the absence of these obstacles, however, is not in itself a sufficient condition for progress.

To connect Archimedes' discovery of his law with previous events the conventionalists might appeal to Archimedes' character traits, such as his love of geometry and mechanics, and his tendency to combine them whenever possible. The conventionalists are on the right track here, but their explanation is too thin; and the inductivists, whose attempt to reconstruct the case may go a little further, can accept it. The inductivists may claim that -perhaps because his curiosity was raised by the famous problem of the purity of a golden crown, and perhaps for other reasons — Archimedes observed and measured, among other things, weights and volumes of solid objects in fluids. His famous “Eureka!” may be interpreted as his reaction upon seeing facts suddenly fall into a pattern, discovering the applicability of his results to the practical problem of the golden crown, or receiving a clue from the amount of water spilled when he entered the bath. But the inductivist interpretation runs into a number of difficulties. For example, Archimedes was a deductivist who did not collect facts indiscriminately; he had long known that the water level rises when bodies are immersed in them; and he could not have made an induction since the facts were too varied to fall into a pattern by themselves.

The error that makes it so difficult for us to avoid being wise after the event is that we ignore previous opinions about the problem that Archimedes’ law solves. Too few people realize that centuries before Archimedes it had been well known that bodies diminish in weight when immersed in water. Few people know that Aristotle’s theory relates this loss of weight to the density of the fluid and to that of the body immersed in it. Aristotle’s theory connected force, motion, gravitation, and chemistry, as a part of his theory of change (of generation and corruption); it also gave results somewhat similar to those of Archimedes. His results are so obvious that he leaves it as an exercise to his readers[154] to consider the connection between the various possible chemical combinations of things and the question of whether one would sink in the other. The great semblance of simplicity in Aristotle’s theory is very convincing, though (its muddles apart) it is far from simple since it has too many unknown parameters to be applicable and testable.

The weakest point of Aristotle’s theory of matter is its inability to explain the floating of ships built of materials heavier than water. He himself discusses this problem,[155] and explains all dynamical dependencies on shape the speed with which a feather falls and the floating of a needle on water, as well as of boats — as a result of resistance. (This is an auxiliary hypothesis, to be sure; but one that hardly differs from Galileo’s auxiliary hypothesis as far as the fall of a feather is concerned.)

Interestingly, Archimedes’ book is not on golden crowns or other bodies immersed in water but *On Floating Bodies*. In it he discusses two problems: the first is the problem of the weight of solids in fluids, and the second is the problem of the stability of floating bodies, particularly the problem of what happens to a floating half-egg (paraboloid of revolution) when its deck touches the surface of the water, and the mechanics of its capsizing. Archimedes’ theory contradicts Aristotle (although the latter becomes an approximation to the former for relatively heavy solids); it explains the floating of heavy boats without any additional assumption about resistance, and also the capsizing and sinking of boats under given well-known conditions. Archimedes may perhaps have refuted Aristotle’s views while thinking about the capsizing of boats and about the fact that they tend to sink when filled with water. But another explanation is also possible. Aristotle states that just as air has weight in air (as we know from weighing a bladder when empty and when inflated) so water has weight in water (since a boat sinks when filled with water)[156] Now according to Archimedes' law, water has no weight in water. So Archimedes might have followed up Aristotle's suggested exercise, and by using his own method of continuity in varying gradually the weights of bodies immersed in water while keeping their bulk constant, he found that according to Aristotle's own theory the weight of water in water should be zero. Doing this he might have found that raising the water level by immersing a body in it gives the clue; alternatively this result might have made him wonder why a boat sinks when it is filled with water, or decreases its propensity to capsize when half-filled with water. For my own part, I think that Archimedes, belonging to the Platonic geometrical tradition, tried to refute Aristotle's criticism of Plato's theory — according to which lightness is not negative weight but small positive weight[157] However this may be, the important fact remains that Archimedes' view contradicts Aristotle's view on various points and that the long study of mechanics in the Renaissance was largely the logical exercise of bringing this conflict into the open and of replacing Aristotle's dynamics with a dynamics that fits Archimedes' statics as a limiting case (in accord with Archimedes' principle of continuity).

The most interesting and critical studies in the whole field of the history of science are those of Koyré and his followers, that, I presume, have developed in reaction to Duhem's continuity theory, and that emphasize the immense role played by the logical conflict between Aristotle and Archimedes in the history of Renaissance mechanics. My point here is merely that a study of this conflict can help us to reconstruct not only the history of Renaissance research but also the history of Archimedes' researches. For example, is the story true that Archimedes' theory was intended to solve the problem of the purity of the golden crown? And why does Archimedes not refer to Aristotle?[158] I do not know; but I conjecture that the story of the golden crown is apocryphal. As Koestler has unwittingly proved,[140] this problem can be solved rather easily without the use of Archimedes' law: immersing the crown in water and watching the rise of the water enables one to find the volume of the crown; this and weighing it in air enable one to find its average specific weight in air; and this suffices to decide how pure its gold is. Thus, the problem of the purity of the golden crown is answerable without a study of the problem of specific gravity in water. And it is the latter problem that no one before Archimedes could answer. Without further documents one cannot prove that the legend of the golden crown is false, yet its logical irrelevance to Archimedes' law, as well as the antiquity of the problem that the law does solve, make it possible to contend that the story is a typical inductive myth, similar to the famous Aristotelian myth about the inductive origin of Thales' doctrine "All is water" in observations of the moist in food and in seeds. So much for the first point, concerning the golden crown. The second point, concerning the style employed by (Euclid and) Archimedes, it has hardly been studied as yet.

My second example is that of Kepler's ellipse. Koestler, who tries not to be wise after the event, does not know why it was not thought, prior to Kepler, that Mars' well-known egg-shaped orbit is an ellipse, and why Galileo ignored Kepler's ellipse. His error, that I shared until recently, is to accept the myth that Newton first united terrestrial and celestial mechanics. Many passages, such as Aristotle's criticism of Eudoxus, show, however, that this had been done previously. The crystalline spheres were the tool for combining the laws of the heavens and the earth. Since Kepler's ellipses do not suit revolving solid bodies without epicycles, they could not precede Brahe's abolition of the crystalline spheres. Galileo — who claimed that circular inertia is the chief law of heaven and earth — was a keen defender of a closer unification of heavenly and earthly physics than that achieved by the ancients. Kepler had a different law of inertia for his ellipses, a more abstract law that concerns equal areas rather than equal distances. It is the acceptance of Galileo's terrestrial mechanics and Kepler's celestial model — one should not call it a mechanical model — that raised afresh the problem of unifying terrestrial and celestial theories into one mechanical theory. It was this new problem that Newton solved. The desire to avoid running into such a formidable problem, I contend, impeded the acceptance of Kepler's theory. I. B. Cohen has rightly called attention to certain old-fashioned aspects of Kepler's theory that provided still further obstacles.[159] Now each of these impediments was, naturally, much more of an obstacle for Kepler than for his audience.

If we wish, then, to avoid being wise after the invention of an idea, be it Archimedes', Kepler's, or Newton's, then, it is not enough to merely obliterating the idea from the picture of the world and trying to see how the world looks without it: it is better to insert in its place the theory that it came to replace, and do so as convincingly as possible. Once we have appreciated how convincingly Aristotle presents the situation with respect to the sinking and floating of bodies in fluids we shall be less tempted to ridicule Archimedes' predecessors, and thereby to belittle his idea. ("Thereby", because an idea that only a fool could overlook is hardly very great.) Once the role of crystal spheres in the classical attempts to unify terrestrial and celestial mechanics is better understood, it will be seen at once that celestial orbits must be circular or epicyclical. This may lead us to admire Kepler for being sufficiently determined to solve the problem of a celestial orbit to be willing to create a much bigger problem, a problem that was left for Newton to solve.

My last example concerns the so-called laws of proportions. Who discovered them? When? Why? Is it not true that even ancient brewers of odd mixtures knew how important proportions were? Did not Boyle put much emphasis on definite and reciprocal proportions as evidence for his atomism?

Why do historians of chemistry suggest that these laws belong to the period around 1800?

These questions may be answered as follows. Though innumerable examples of definite proportion had been well known for a very long time, innumerable examples — such as salt in water or alcohol in water — of violations of the law of definite proportion were also known.

But, one might say, water and salt or alcohol do not form a compound but a solution! And why? One who is not a chemist will probably say this is so because they do not obey the laws of proportion when they unite; one who is, might say they do not alter their characteristics as much as compounds do. Both these answers are obviously very weak. It can easily be shown, as follows, that water and salt do combine: salt, being heavier than water, ought to sink in it. The fact that it does not, plus the law of addition of forces, show that (unlike oil and water) salt and water interact. Since the observed fact is that salt-water is homogeneous, these forces between the salt and the water must be between the smaller particles of salt and water — in other words, they must be chemical forces.

Now there is an answer to all this. It is that the forces causing the solution of water and salt are molecular, whereas the forces uniting sodium and chlorine into salt are atomic; salt, therefore, is a compound but salt water is not; the forces binding the particles of sodium and chlorine into salt are atomic and hence genuinely chemical, whereas the forces binding the salt to water are merely molecular. One should not be misled, however, into thinking that sodium is attracted more than chlorine to water; nor that molecular forces are not the business of chemists; the story becomes hopelessly complicated if we try to make it agree with the up-to-date textbook, chiefly because up-to-date chemistry and physical-chemistry textbooks are hopelessly complicated. So let us return to history.

The old view about this matter was that salt-water is a compound and that the law of definite proportion is definitely not universal. It was last defended by Berthollet against Proust who, on *a priori*[160] atomistic grounds declared that the law of definite proportion is universal, so that salt-water and alcohol-water and such like are not compounds but mysterious entities[161] (just as calks were for the phlogistonists!). In other words, if one ignores molecular forces, one is forced either to abandon the universal law of definite proportions or else to view all mixtures that are permanent (to exclude the mixture of oil and water) as sinister violations of the law of gravity.

This helps solve another mystery. In spite of all evidence to the contrary, air had been viewed as an element. Fresh air and foul, clear and cloudy, smoke and marsh gas, these varieties of air had always been known; and yet the law of gravity told people that air is an element (and thus homogeneous) or, when they were daring a (homogeneous) compound. Why? Because of the law of gravity. Once air had been split into oxygen and nitrogen and once Newton had argued that all gases are elastic fluids, how should air be regarded? As a compound? Or as a mysterious entity? Berthollet thought it should be a compound, of course; to Proust it could not be a compound in virtue of the fact that oxygen does not always occupy the same proportion of the atmosphere, so that in his opinion it must remain a mysterious entity. Dalton entered the controversy at this stage. Siding with Proust against Berthollet, he refused to consider air as a compound, but he tried to solve the mystery of what keeps oxygen and nitrogen mixed when gravity should separate them as it separates oil from water. He disagreed with Proust in viewing air not as a solution akin to salt-water. He suggested that air is a genuine mixture. He explained the fact that the nitrogen in it does not rest on top of the oxygen in it (as oil does on water) because gases, unlike liquids, are completely interpenetrative: one kind of gas, he said, behaves towards another as if it were a vacuum. This is an ingenious solution, although it does not accord with the up-to-date textbook. It is also very bold: Dalton stated explicitly that his theory conflicts with Newton's view of gases as (elastic) fluids.

To conclude, the assumptions that all forces between chemically different matter are chemical, that these act in fixed proportions, and that gases are (elastic) fluids, forbid the possibility of the existence of stable mixtures of materials possessing different weights. Dalton's new idea was to reject, out of these assumptions, Newton's assumption of gases as fluids but to stick to the theory of chemical action in fixed proportions.

A reading of Dalton's early memoir, or of excerpts from it in the celebrated *Harvard Case Histories*, reveals that at this time Dalton used "mixtures" and "compounds" as synonyms, though his chief object was to oppose the generally accepted view of the French chemists, according to which the atmospheric mixture of oxygen and nitrogen is a compound. Dalton's memoir also reveals that he thought his modification of Newton's theory of airs as elastic fluids was a better solution of the riddle.[162] He says all this in plain language; yet even the editor and commentator on the section on atomism, L. K. Nash, who notices the force of Berthollet's arguments,[163] does not notice its relation to Dalton's problem; he merely adds to Dalton's criticism of the French chemists some explanations that render the criticism lighter[162] And to Dalton's criticism of Newton he adds a footnote saying that if Dalton is the father of atomism, then Newton is its grandfather,[164] and so on.

So much for being wise after the invention of a new idea. And now to being wise after the discovery of a fact.

16. Obstacles on the Way to a New Fact

Bacon said that we cannot avoid wondering how we failed to observe a fact after having observed it. He thought that once we had learned to avoid prejudice all facts would be equally obvious and of equal significance, so that the discovery of any specific fact would be accidental. It would in any case have to be accidental, he argued, since if it had been predicted it would have been known beforehand, and hence it would not be genuinely novel.

There are strong arguments in favor of these ideas. It is undoubtedly true that prejudice involves thinking that we already know and thus amounts to a dampening of the spirit of enquiry. It is unquestionably true that new events that clash with old prejudices are dismissed by the prejudiced as insignificant or reinterpreted by them as not really new. It is also true that the rating of the significance of facts involves theoretical judgment: one never observes the value of an observation. The Royal Society of London met with scorn because of their interest in minutiae like fleas and lice.[165] Hobbes said,[166] anyone with sufficient money can easily build a telescope or a furnace, but very few can write a book like his own on the nature of things. Yet Boyle, Hooke, and others continued to report on minute and patient observations, with strong faith in the importance of the outcome. It is difficult, in view of this, not to admire Bacon's defense of minute detail. Hertz, it may be remembered, rated his own discovery of the photoelectric effects as insignificant, and his error nearly consigned this discovery to oblivion; it was remembered only because the Baconian tradition encouraged people to report, however briefly, even the minutest of their factual findings.

Hertz's example, and other examples of observations that turned out decades later to be important (such as Brownian motion, spectra, harmonics, the anomalous Zeeman effect) suggest that it is almost impossible to rate the relative importance of facts other than by reference to later scientific development; by reference to the up-to-date science textbook; by being wise after the event. Especially if, as Bacon has argued, all discoveries are accidental, their relative merit must lie not in their origins but in their consequences, in the use science makes of them later. And since a predicted discovery was known before it was found and hence was not a real discovery, it seems to be compelling that discovery must be accidental.

Since Bacon's theory makes the significance of a discovery entirely dependent on its unforeseeable scientific consequences, it is necessary to criticize his theory in order to avoid being wise after the event. One might attempt to criticize it by reference to some discoveries that were predicted — by reference, say, to Rutherford's predictions concerning the release of nuclear energy. One might refer to discoveries that eventually were made by people who had sought them year after year. But such criticisms are not unanswerable. Such predictions were incomplete, it might be argued, and this is why they were not verified at once. Had they been completed they would have been verified earlier; the missing information could, however, be attained only by chance — which explains the time lag between the predictions and their verification.

This answer is difficult to accept because it is often the investigator who has sought a fact for years who ultimately finds it; it is quite understandable that facts are regularly discovered and reported by the few scientific researchers rather than by the majority of unscientific people; but it is too improbable that of all scientific researchers, the particular one who should accidentally stumble upon a fact is the one who has been searching for it for years. The frequency of such cases has led to the famous saying: "Such accidents only meet people who deserve them." This providential explanation of these improbabilities was given by Lagrange in reference to Newton, and repeated by Hansteen in reference to his teacher Ørsted. This shows how great the difficulty of rejecting Bacon's theory of accidental discovery is: it is upheld even at the expense of having to produce providence *ex machina*.[167]

Indeed, the situation is almost Kafkaesque. Kafka's main theme, as will be remembered, concerns the man who stands at a gate and spends his whole life there racking his brains in an effort to secure an entry-permit until, just when he is dying, the sentry tells him that no entry-permit is required, that anyone who wishes to enter is entitled to do so. Like Kafka's hero K., Newton and Ørsted were ultimately allowed to enter not because their efforts secured an entry-permit — this is impossible since entry-permits are not required, as anybody can enter who wishes to enter — but because their efforts constituted mitigating circumstances, as they indicated a high degree of goodwill recognized by the powers that be. One may smile at the simile, but I think it is not accidental. As I argued some time ago in more detail in my doctoral dissertation, Bacon's theory of (accidental) discovery is Cabalistic in origin. And, as is well known, so are Kafka's works. But simile or no simile, one need go no further if one accepts the theory that Newton, of all people, was the one who made his discovery quite by accident, simply because he worked hard at it for many years.

The inductivists, when faced squarely with this problem, but only then, produce a very simple answer, which is also Cabbalistic in origin: for years such and such a scientific researcher worked on a problem, but not humbly and purely enough, motivated by self-interest that triggers prejudice; only after preconceived notions were discarded was the researcher allowed to enter the promised land. This theory is, of course, a preconceived notion, and it allows for the possibility that the researcher may have problems in addition to the business of observing; perhaps for this reason the inductivist historians seldom uses it; but they have little scruple in using a similar one; namely, that those researchers can make discoveries whose minds — on account of their ignorance — had never been polluted by prevalent prejudices. Ignorance, it seems, is bliss indeed. The noble savage who is the hero of Voltaire's novel *Huron* is enabled by his ignorance to master all knowledge in a few weeks.[168] Rousseau's *Émile* is not given any books in order that with an unpolluted mind he might the more readily read the Book of Nature. Priestley's success was due, Dr. Thomson explains, to the fact that he was not a philosopher until shortly before he made his discoveries, and thus knew too little about phlogistonism to be prejudiced by it.[169] Paul de Kruif assures his readers[170] that Leeuwenhoek's great advantage was his ignorance of Latin.

This theory might seem to clash with the theory of accidental discovery, as it makes ignorance or the laying aside of past opinions the cause of the discovery. But here I sincerely wish to defend Bacon: I think that here his theory is at least consistent. It is trivially true that some accidental and some causal factors are involved in every human activity, discovery or not. What Bacon meant, however, by saying a discovery was accidental, is that it was not predicted, unsought for; and this is less trivial, and supported by the strong (because obvious) argument that predicted phenomena are not unexpected, not surprising, whereas genuinely new facts are both unexpected and surprising. And if we are to reject Bacon's theory we must answer this strong argument.

One answer has been provided by William Whewell. In his answer, that is a marvel of simplicity, he tried to reconcile the fact that unexpected facts are unobservable with the fact that discoveries are surprising or unexpected. His reconciliation is this: a thinker invents a new idea that raises an expectation of a fact not expected on the basis of any older idea. Usually, this expectation does not come true; but sometimes it does. The observations resulting from these rare ideas are surprising from the viewpoint of previous knowledge but not from the viewpoint of the new idea. Since the world is unaware of this new idea the world is surprised; since the originator of the idea does not know *a priori* whether the conjecture is true it may even be a surprise to the originator of the conjecture; but the observation must in some sense first be expected, however tentatively, in order for it to be perceivable at all.

Here, just as in the case of Bacon's theory of discovery, the point concerns the logical relation between facts and theory (that is, logical independence). According to Whewell's theory of discovery, a new observation does not follow from old knowledge, but from the new idea (that it verifies). It is expected yet surprising.

Examples for Whewell's theory are abundant: of these, the verifications or corroborations of Einstein's theories are perhaps the most spectacular. Moreover, Whewell's theory explains the diminishing returns of verification; once a theory has been incorporated into the body of science, expectations based on it that come true cease to gain attention.

Yet Whewell's theory is unsatisfactory. It fails to accord with many well-known cases of discoveries that did surprise their own discoverers. Priestley, for instance, refused to believe his eyes when he discovered oxygen, and he was one of many discoverers with such feelings of incredulity towards their own discoveries. Two spectacular cases of such counter-expectations are the Michelson-Morley experiment, and the Hahn-Meitner discovery of nuclear fission.

We seem to have arrived at an impasse. The possible logical relations between an observation and a theory are (a) logical dependence or deducibility or expectation; (b) logical independence or accidentalness; and (c) incompatibility or counter-expectation. If we admit the existence of examples of discoveries of each of these three categories we thereby give up hope of characterizing discovery by its logical relation to theory.[171] I have already mentioned cases of (a) and (c), and histories of science are full of examples of (b), some of them argued from historical documents, some of them mythological (see the end of Section 5 above). Yet it is possible to claim, as Popper does, that all discoveries belong to category (c), that they are refutations of past theories.

Bacon and Whewell agree that a new fact is not deducible from old knowledge. If a new fact happens to contradict an old theory, they would hardly notice the significance of this theory, since both dismiss errors as insignificant.[172] Yet, according to Popper, the very crux of the matter lies here: whether an observation is predicted on the basis of a new idea (Whewell) or not (Bacon), its novelty and surprise value depend on its contradicting a reasonable scientific theory.[173] Moreover, it was this theory that was the main obstacle on the way to observing the new fact, an obstacle removed not by laying aside all theories but by immense piecemeal efforts to criticize theories.

Popper's theory, if true, helps avoid being wise after the event by enjoining us to reconstruct the important and widely accepted theory that preceded the event, and that implied that the event was impossible. Popper's theory, if false, might be criticized by an inability to reconstruct such an obstacle to an important discovery, by finding a case where an important discovery did not conflict with an important idea immediately preceding it. Whether Poppers idea is true or false, I would like to illustrate it with two or three sketchy examples, and one more elaborate example.

If Popper's theory is true, Hertz's error in undervaluing his discovery of the photoelectric effect would have to be a logical one. Hertz did make a logical error; he thought that the effect is explicable by Maxwell's theory as a resonance effect. The significance of the effect, as well as of Planck's formula, was brought to light by Einstein in 1905, when he showed that these conflict with Maxwell's theory.

Van Helmont and Stahl assumed that fresh air becomes foul by absorbing smoke or phlogiston to capacity. If so, presumably air should expand during the process. But, as Van Helmont soon found out, air loses about one fifth of its volume during the process of becoming completely foul. This was puzzling but not fatal to the explanation. According to Stahl's variety of phlogistonism, chalk should be quicklime minus phlogiston; Black suggested that it is quicklime plus fixed air. Black thus presented an alternative to Stahl's theory: in his view "plus fixed air" plays a similar role to Stahl's "minus phlogiston". But it was not clear to what phenomena Black's theory should apply and to what phenomena Stahl's, and a debate raged over this question.[174] Now if Black's theory rather than Stahl's should apply to metals, the problem of weights of calx would be solved. In this case it would follow that heating any calx should produce the original metal plus fixed air. In 1774 Bayen heated the calx of mercury and, indeed, it did turn into mercury!

Soon afterwards, Priestley repeated the experiment and — since fixed air extinguishes flames — he put a candle into the resultant (allegedly fixed) air. Surprisingly, the candle burned more intensely instead of being extinguished. At this point Priestley — thinking the air was not fixed air but some kind of nitrous gas that had the property of supporting a flame — tried to destroy this property by well-known methods. But still the candle refused to be extinguished. He then resigned himself to the idea that this was the best, the freshest possible, air for flames; namely, ordinary fresh air. So he rendered the air "perfectly noxious" by a known method that reduced its volume by about one fifth. Surprisingly, however, the candle still refused to be extinguished.

All this is told by Priestley in detail, especially his last surprise that refuted his error — he calls it his prejudice — that fresh natural air is as fresh as possible (this error evidently follows from the Van Helmont-Stahl hypothesis). He then surprisingly goes on to say:

> I cannot, at this distance of time, recollect what it was that I had ill view in making this experiment; but I know I had no expectation of the real issue of it. Having acquired a considerable degree of readiness in making experiments of this kind, a very slight and evanescent motive would be sufficient to induce me to do it. If, however, I had not happened, for some other purpose, to have had a lighted candle before me, I should probably never have made the trial …

This endearingly absurd report is still swallowed by all and sundry — including the confusion between "had no expectation" and "had quite a different expectation" (see Section 11 above). When Priestley apologizes for having tested a false hypothesis, saying he would experiment on the slightest pretext, he means to say that he is not really committed to the false hypothesis; that since testing a hypothesis is not refuting it but verifying it, testing a hypothesis is rather dangerous; but that he could forget it before the experiment was over and thus could observe the facts well. The candle that was obviously there in order to test — and refute — a hypothesis, all of a sudden stands there by accident, for no known purpose, and certainly, not to test — and verify — the forgotten false hypothesis. Indeed, the hypothesis is so well forgotten that Priestley even forgets he has reported it half a page earlier (natural fresh air is the freshest air possible). The amount of confusion resulting from regarding refuted hypotheses as non-existent is remarkable; but we cannot blame Priestley. Even Conant, a sophisticated twentieth-century conventionalist, ridicules Priestley's predecessor, Bayen, for his erroneous view of the calx of mercury as consisting of mercury and fixed air, a view he evidently accepted from Black after corroborating it. "How he could come to such an erroneous conclusion no one knows", says Conant[175] "but he was obviously not a skilled experimenter with gases." This account is rather disappointing, particularly since Conant himself notices that Priestley also, initially, expected his gas to be fixed air; yet he cannot allow one thinker to start an experiment and another to continue it (even though the continuation and completion took Priestley about one year).

The discovery of oxygen was made in two stages: first the refutation of (the extension of) Black's theory (to metals), and then of the Van Helmont-Stahl theory of fresh and foul air. This explains the connection between Bayen and Priestley, as well as removing the mystery from the fact that the discovery was made independently by two or three people. On the received theory any simultaneous discovery being sheer accident, it is most improbable; on Popper's theory it is explicable.

Galvani has told us — to turn to another example — that while his assistant was in the process of discovering that electricity can cause a frog's leg to jerk he himself was immersed in deep thoughts (though he was not expecting his assistant's "accidental" result). Here, again, it might be interesting to try to reconstruct Galvani's deep thoughts, to show that they led to some disappointed expectation concerning the frog's leg, and that the discovery was the refutation of these deep thoughts whose contents he did not mention.[176]

My last example is so obvious that it need not be mentioned were it not so popular. It is a hearsay report[177] about the way Roentgen made his discovery; indeed, according to this report, it was not Roentgen but his laboratory worker whose eyes first fell on the fluorescent screen. The screen was there, of course, quite by accident — just like Priestley's candle. The hearsay report seems to me to be very doubtful on the ground that the situation it describes is almost impossible: the screen was between the cathode tube and a tool cupboard, yet the rays, reflected from the wall, showed the shadow of the tools on the screen. Unless the beam, which must have been quite weak, was very well directed, and unless the observer was watching the screen very carefully, the reported observation would have been quite impossible. Given these conditions — given, namely, that Roentgen was testing some hypotheses concerning the characteristics of the various emissions of cathode tubes (presumably that the rays are longitudinal) — he would have made the discovery one way or another with his preparations; otherwise neither he nor his assistant would have seen anything.

17. Ørsted's Discovery

Turning from these interesting but sketchy suggestions, I shall now present a fuller account of one of the greatest discoveries in the whole of the history of science — Ørsted's discovery of electromagnetism[178] Although no discovery has gained so much unexplained praise, this most problematic discovery has never, to my knowledge, been satisfactorily explained. A striking example is Duhem's highly mathematical and very dry treatise on electricity in which he suddenly bursts into a eulogy of the importance of Ørsted's discovery, which he describes as "the point of departure of all researches which constitute electrodynamics and electromagnetics".[179] This evaluation is correct; and yet it is puzzling that Duhem should say this,[50] especially as he deals with the "point of departure" only at the end of his third and last volume.

The phenomenon that Ørsted discovered, Ørsted's effect, is easy to repeat. All that is needed are a compass, an electric battery, and a wire. When a direct current passes through a wire that is placed parallel to the needle of the compass, above or below it, the needle ceases to point north. If the wire is not exactly parallel to the needle, the effect will be smaller, but it will vanish only when the wire is very precisely perpendicular to the needle — in the east-west direction. The wire can be replaced by an electric torch, though the resistance of the glowing wire in the bulb will diminish the current and thus the effect.

Great experimenters, from Franklin to Davy, experimented with electricity and magnetism trying to find some connection between them; prizes were offered for the discoverer of this connection; Ørsted himself thought and wrote about it for years before his own discovery. The editor who published the German translation of Ørsted's first report of 1820, a Professor Gilbert, said it was clear that the discovery was purely accidental: what Ørsted had failed for years to find while searching for it, he stumbled on during a public lecture! A popular myth developed according to which Ørsted made the discovery while trying to demonstrate the well-known fact that wires conducting electricity glow.

Phillip Lenard has claimed that the fact that the discovery was made in a lecture can be explained by the fact that Ørsted loved to lecture and did so frequently.[180] It seems to me that the explanation provided by his pupil Hansteen is a better one. Hansteen reports[181] that Ørsted could not manipulate instruments — we know indeed from other sources that he was shortsighted and clumsy — and he used to ask members of his audience to experiment for him. In a recent publication R. C. Staufer takes this as a personal insult to Ørsted;[178] I take it to be a possible explanation of why he loved to lecture and prepare new experiments for his lectures. For my own part, I have a different explanation of the event. It is well known that soon after Ørsted's discovery Ampère announced in his first lecture to the French Academy his discovery of the electromagnetic solenoid, prior to observing it, and that Davy had some years before intended to decompose nitrogen and had planned to make the discovery in a public lecture. These can hardly be coincidences, and I conjecture that experimenters preferred to make discoveries before witnesses after explaining the ideas behind them, so as to forestall any claim that these discoveries were accidental.

The fact that Ørsted's discovery occurred in the lecture room may or may not be significant; we cannot know this without knowing how much Ørsted knew before entering the lecture room. The problem, then, is this: if he predicted his phenomenon, why did he not verify his prediction immediately? What difficulty did he encounter?

A Baconian might say that Ørsted obviously made an essentially correct prediction, but was misled by a prejudice. "Obviously," because a Baconian cannot but identify impediments with prejudices. "We heard it stated by Faraday in one of his lectures at the Royal Institute," writes Bakewell,[182] "that when he was first connected with that institute, Davy and Young were frequently making experiments with the view of establishing the identity of electricity and magnetism, but that being misled by preconceived theories of the action of the force, they adopted nearly every conceivable mode but the simple one" adopted by Ørsted. With this the efforts of such great researchers as Davy and Young are dismissed, and, allegedly, on Faraday's authority! But I want to find out what the actual prejudice was, and how Ørsted overcame it, if he did indeed share it. All too often it is alleged that despite all effort a discovery was sought for but blocked due to a preconceived idea, until that idea was laid aside, whereupon, lo and behold, success was assured. This is too schematic and preconceived; more details are needed when discussing the history of a specific discovery. This theory, therefore, can simply be dismissed.

Another theory, also mentioned in the last section, is that the prediction was incomplete; it was applied to our case by Hansteen, who has suggested[181] that although Ørsted, like everyone else, predicted the discovery in general, he did not predict it in detail; he got the detail by accident rather than by prediction. The important detail is that the electromagnetic force seems to be circular or rotational or transversal (see Section 5, above); from this it follows that the strongest deflection of the needle would occur when the current is placed parallel to it, in the south-north direction. This, Hansteen states, Ørsted did not know, as is shown by the fact that he first placed the wire in the east-west direction.

Hansteen's story is quite satisfying to a historian. That Ørsted did not know the fact that the force is rotational, Hansteen shows by using historical evidence — the fact that Ørsted used to place the wire in the east-west direction. Hansteen describes the accident by which this fact was discovered, and his description is so vivid, detailed, and natural, that we must accept it, if not as evidence by an eye-witness, then at least as a serious hypothesis, or as a story that he had received from the horse's mouth and that was probably common knowledge in Copenhagen scientific circles. "Once, after the end of his lecture, as he had used a strong galvanic battery for other experiments, he said [to his assistant]: "Let us now once, as the battery is in activity, try to place the wire parallel with the needle"; as this was done, he was quite struck with perplexity by seeing the needle making a great oscillation … ." All one needs to remember is that in Ørsted's time an electric battery was operative for a few minutes only, and one will vividly see before one's eyes the eager experimenter snatching the opportunity after a lecture and just trying whatever came to mind first, and luckily hitting upon the discovery.

R. C. Staufer has argued that Hansteen was not an eyewitness by reproducing a personal letter from Ørsted to Hansteen[183] (1820) in which the discovery is discussed. He concludes that Hansteen's evidence is "worthless" as a historical document though his story is "very plausible." Later he claims on the strength of a few reports of the discovery written by Ørsted himself that the discovery was not accidental after all.

For my own part, I do not accept Hansteen's story, yet I do not think that the factual information it contains can be discarded without sufficient reason, especially since he was a close friend of Ørsted, and since Ørsted's own reports are sketchy and extremely difficult to understand. My disagreements with Hansteen are three. First, it is untrue that, like many people, Ørsted had expected some relation between electricity and magnetism; significantly, his theory was very different from other theories, and it led him to introduce the electric current into the investigation. Secondly, the current itself was not there accidentally nor was its role predicted by anyone but Ørsted. Thirdly, as to the direction of the current, it is unlikely that Ørsted would have persistently placed the current in the east-west direction, and then, by accident, in the south-north direction. Hitting on any of these two out of infinitely many directions by sheer accident is most unlikely.

Our starting point is the theory that I have already mentioned (Section 5): only likes interact; and electricity and magnetism are not likes; hence they cannot interact. The idea that only likes interact was universally endorsed until much later. This explains why those who thought that electricity and magnetism do interact spoke of the identity of the two. Those like Ampère who rejected this identity rejected the possibility of interaction as absurd.

This is all highly metaphysical, yet there is nothing else to begin with. Whittaker, ignoring this metaphysical discussion (he was an inductivist) and trying to find some continuity here (he accepted the continuity theory) refers[184] to some reports about occurrences of interaction between electricity and magnetism that were known before 1820. But if these reports were acceptable, Ampère (and many other empiricists) would have been anti-empiricists, and the academies that offered prizes for those who could find the interaction, associations of fools. Indeed, Whittaker himself admits in a way that the reports were unacceptable. All the known cases of interaction between electricity and magnetism — the most important of which was the effect of lightning on compasses — were unrepeatable, and unrepeatable experiments do not count. (We know of many experiments that are entirely ignored, although they would count as great discoveries were they repeatable.) Even Ørsted himself, who drew much encouragement from the unrepeatable experiments of his friend Ritter, was somewhat ashamed of the fact that so great an experimenter as Ritter should have published unrepeatable experiments. We shall return to this later; here we must conclude that there was no proper experimental background to Ørsted's discovery.

The doctrine that only likes interact was finally shaken by Nicholson and Carlisle, who discovered in 1800 the phenomena of electrochemistry, namely, the fact that electric forces can oppose and overcome the forces of chemical affinity. Electrochemistry indicates that electric forces and chemical forces are identical, and this indeed was Davy's and Berzelius' guiding idea. But this idea does not suffice; electric forces belong to electric matter and chemical forces to gross matter, so that they must be different and therefore cannot interact! Davy and Berzelius did try to cope with this problem, but, I think, rather unsuccessfully. Ørsted, on the other hand, tackled the problem-situation from a new and wider angle. Chemical affinity and electricity, he imagined, are neither identical nor essentially different; they are different manifestations of essentially one and the same force; all allegedly different forces are different manifestations of one and the same primordial force. The primordial force can change its manifestations, and the task of the experimenter is to find the conditions under which such changes take place. This is Ørsted's own original idea,[185] and one that guided the whole of his scientific and philosophical career from the time he started thinking about the problems posed by electrochemistry to the end. The greatest objection to his doctrine, he reasoned, was the seeming impossibility of converting electricity into magnetism. Accordingly, he looked for a way around this difficulty. In the book about chemistry he published in 1813, he wrote that perhaps galvanic electricity interacts with magnets more easily than frictional electricity. In the subsidiary reports[186] about his discovery he quoted himself on this point as evidence that his discovery had been predicted.

I find this argument amazingly unconvincing. Other philosophers had made this prediction, and they had even tried, although unsuccessfully, to confirm it; this was public knowledge. The intellectual background to the discovery makes it easier to understand why Ørsted persisted in his attempts after other had abandoned their own. But we have already come a long way from the casual remark of 1813 — which, by the way, is entirely false — to the discovery in 1820.

The next step concerns the introduction of the (galvanic) current. Again Ørsted's own report is cryptic. My attempt to reconstruct his reasoning makes use of his theory of the current. He had noticed long beforehand that since, according to his own view, there is only a variety of manifestations of force and not a variety of matters, he could not accept the orthodox theory of electric currents or discharges as (literally) currents or flows of electric matter (in the fluid state). He wanted to explain this phenomenon. Now in electric currents or discharges, as he knew, electric forces transform into heat and light. So this is what currents are — transformations of forces; their mechanism, he assumed, was a kind of state of disequilibrium of conflicting electric forces along the wire that unites the electric poles.

It is now clear why Ørsted's speculations were not taken seriously; why indeed, they and their originator were viewed with hostility almost to the point of public scandal. Not only was the prevailing (Baconian) scientific climate opposed to speculation; Ørsted's speculations were opposed to orthodox Newtonian speculations and akin to the German Romantic speculations accepted in circles that were inimical to experimental philosophy.[187] Returning to Ørsted's train of thought, we have seen that he had thought that in an electric discharge the electric force is transformed into other kinds of force;

namely, heat and light. And now (in 1813 or perhaps somewhat later) he came to think that if the current is sufficiently strong it might also turn electricity into magnetism. In one subsequent report he writes: "somewhat inconsistently, I expected the predicted effect particularly from the discharge of a large electric battery and moreover only hoped for a weak magnetic effect." I do not understand what "somewhat inconsistently" means here unless it means "erroneously", and I think one can explain his mistaken expectations concerning the magnitude of the effect as well as his mistaken expectation that the effect would take place only while the current generates both heat and light. He might have viewed the current like this: when a large concentration of electric forces in disequilibrium (i.e., a current) occurs, they are transformed into heat; when a larger concentration occurs (the wire is thinner or the current stronger) a small residuum is transformed into light as well. So, by a kind of extrapolation, perhaps when a still larger concentration occurs ─ the transformation of a smaller residuum into magnetism may take place. And the fact that the residuum was so small would explain, incidentally, why people have not noticed it before.

Moreover, Ørsted thought that the glowing wire was a magnet, but he did not know any more about it, and in particular he did not know where its poles lay. We know it has no poles, but has circular rather than central ─ attractive or repulsive ─ forces. Ørsted not only did not believe that the force is circular; he definitely thought that it was central. We have ample evidence of this from his own writings, and we might have expected it in any case since he was at once a kind of Newtonian and a kind of Kantian. Now, if one has a long weak magnet, if one does not know where its poles lie or which is north and which south, and if one wishes it to interact with a compass, some knowledge of Newton's theory of force will tell one to place the magnet in the east-west direction. One does so and sees no result. Hence one appears to have made a mistake. One concludes that either (a) the long weak magnet is weaker than thought, or (b) that it is not a magnet after all, or else (c) that the Newtonian hypothesis concerning forces is false.

Remembering that Ørsted was a kind of Newtonian and *a priorist*, one sees at once that he would have to reject corrections (b) and (c) without much thought and accept correction (a), which entails that he would have to repeat the experiment with a still stronger battery in order to get a visible result.

At least one interesting intermediate step occurred while he was trying to obtain stronger and stronger batteries. The intermediate step was to place the wire not at all above the compass but at its side, perpendicular to the plane in which it lay: Ørsted took a hint from the dubious reports that lightning ─ that Franklin had identified as currents ─ affected compasses. It was this idea that occurred to him just before that lecture in March or April 1820, and that he tried out during the lecture. He did get some results, but they were still irregular; his audience remained totally unimpressed. (This intermediate step shows how reluctant he was to place the wire in the south-north direction, a fact that I have explained by reference to Newton's theory of force). Remembering Ritter's irregular results, he accepted his audience's judgment and refrained from publishing; indeed, he left the whole business unattended for three months. Returning then to his experiments, he constantly increased the size of his battery. Thus, he was still trying to stick to his Newtonianism, or else perhaps he still had not thought of criticizing it. His line of attack was still on traditional lines or at least it can be so explained, as I shall now argue.

Having had an irregular result from using the wire in the place of a lightning flash, he tried to find out where the pole lies in the wire. He tried to find the pole in the wire, like a needle in a haystack, by bending it in various places into a V-shape. The results of this experiment are indeed regular. So, having obtained a regular though intermediate result, he decided, I conjecture, to go on trying to locate the poles in public before an audience of highly distinguished gentlemen, while demonstrating to them that he had got some regular result. Although he did make his point, he could not explain it: he found no poles. The poor lecturer had no option but to declare the lecture finished. With some sense of excitement and with more than some sense of disappointment and embarrassment his distinguished guests rose and made for the door; but they were stopped before they got out. For in the mean time the discovery had been made. From a deep sense of despair Ørsted quickly, but very cruelly, took stock of his activities, and tried to explain his strange failure. No doubt it was useless to spend one's life (in a Kafka-like fashion) building ever bigger batteries. The error lay somewhere else. Was he mistaken in believing that the conducting wire was a magnet? Impossible. What other systematic error did he commit? I doubt that he could there and then have supposed that Newton's theory of force was possibly false. What is clear is that he quickly suggested that his systematic error from the beginning of his series of experiments to the end lay in placing the wire in the wrong place. He may have thought so either without asking why, or while (erroneously) thinking that this early deduction of the mistaken east-west direction of the wire from the correct Newtonian theory of force was invalid, or else while suggesting that the Newtonian theory of force was incorrect. Anyhow, he was in a frantic hurry; it was now or never — before the distinguished guests had reached the door. Quickly he placed the wire in the north-south direction, possibly just to get as far away from the early systematic error as possible. And then he gasped; he saw at once how much more important his discovery was than he had ever hoped.

This concludes my reconstruction except for a brief discussion of a few remaining difficulties.

First, the problem of the magnitude of the effect, and hence also the problem of the erroneous use of a glowing wire, is practically irrelevant. Why, then, does Ørsted so stress these red herrings? I do not know, but suggest that he had to explain the delay from 1813 to 1820 and could not very well blame the real obstacle, the Newtonian hypothesis that all forces are central. So perhaps he blamed the small and irregular character of the effect. But there is more to it; admittedly, the need for a big battery, that he retained to the last, but that entirely vanished at the moment of the discovery of the circularity of the force, is quite eloquent evidence that Newtonianism was an obstacle; yet had he started with a strong battery and a thick wire, and placed the wire (erroneously) in the east-west direction, he would have had a nearly fifty percent chance to observe the needle making a full about-turn! Thus, we see that there were several errors interplaying, and that Ørsted preferred to name his own — the use of thin wires — as the chief culprit.

The next difficulty is this. Ørsted claimed that the discovery was made early in 1820, while I claim that it was made in July 1820, three months later. Here, however, there is no difference about details, only about their evaluation;[188] and we should remember that Ørsted was anxious to refute claims that the discovery was accidental as well as claims to priority on the part of some other person.

There remains one final objection: Hansteen says that the discovery of the law of electromagnetism was found (immediately?) after a lecture[189] while I claim that during the wintertime lecture Ørsted found only irregular results. But I have the whole of Ørsted's story to support me. Possibly Hansteen confused the lecture in winter with the discovery in July. I prefer to assume, however, that he was referring not to a formal University course but to a demonstration before the very distinguished audience. That Ørsted would have made a major step in public twice seems rather unlikely, of course. But Hansteen explains it entirely satisfactorily, I think, when he says that Ørsted usually experimented during his lectures, and asked members of the audience to act as his assistants.

18. Historical Explanations

Finally, after this critical review of the historiography of science I want briefly to state my own positive views on how the history of science should be written. The first maxim of enlightened or broadminded historiography should be this: any interesting or stimulating story is good, and should count as history if it fulfils two conditions: (a) it does not often violate factual information easily accessible to its author, and (b) it does not present historical conjectures as if they were pieces of factual evidence. Many methodologists, from Bacon to Radcliffe-Brown, have written against conjectural history; this inductivist bias leads either to the demand that historians should record facts and nothing but facts, or to the demand that historians should verify their conjectures. A better demand is that historians should try to test their own conjectures. I reject even this demand, because I do not see what is wrong with one historian offering a conjecture and another historian testing and refuting it. A much more sensible demand is that historians should only provide (conceivably) testable conjectures. I reject even this demand as too constraining, especially since one author may present a conjecture, another may render it testable, and still another test it. It is obviously preferable to have any conjecture rather than none, a testable conjecture rather than an untestable one, and to test it sooner rather than later. Testing a story amounts to the search for more historical material or to the attempt to throw new light on existing historical material. And it is a fact, I suggest, that any chapter in history is improved only by being written and rewritten by a few authors who try to criticize each other and provide alternative views. In this way there develop more coherent explanations, more vivid pictures of the activities of historical figures, and more interesting explanations of these activities as necessitated by the actors' aims and circumstances.

In the field of the history of science, diverse efforts to study one and the same topic are not to be found anywhere, to my knowledge, with the exception of the studies of the history of Renaissance mechanics from those of Duhem and Mach to those of Koyré and his followers. I wish to explain this defect by the following hypothesis: although the laws used in historical explanation are usually very simple, where the history of science is concerned they are highly problematic and complicated. Let me elaborate.

I shall first briefly restate Popper's so-called deductive model of explanation, and indicate its application to history.[190] Many philosophers implicitly agree that if a statement or a set of statements *a* explains a statement *b*, then *b* follows from *a*. Popper has boldly suggested that any *a* from which *b* follows is possibly explanatory of *b*. In this case, he added, if *a* and *b* are singular (say, statements of observed facts), then the explanation is circular or *ad hoc*. Now alternatively, *a* may contain universal as well as singular statements, as the singular conclusion cannot follow from a universal premise alone. Moreover, if the premises are testable, the explanation will be scientific. Following practice among physicists, Popper calls the singular statements in the explanation "the initial conditions", and the universal statements "the universal laws". In physics this is a familiar pattern; Popper has claimed that it applies elsewhere too — in historical explanation, for instance. Science concerns the search for, and the testing of, universal laws; history concerns the search for, and the testing of, initial conditions; and technology concerns the search for useful universal laws and useful initial conditions. If our interest is in testing a universal hypothesis and we perform an experiment that becomes problematic, we scrap the whole experiment and start afresh; a historian, on the other hand, may try to find what exactly spoiled that specific experiment by offering a hypothesis about the case and testing it. In brief, while concentrating on initial conditions our interest is in history, and while concentrating on the universal laws it is in generalizing science.

There is a minor problem, that Popper has discussed[191] because, I suppose, it has led to an interesting and useful result. It has, however, also caused so much misunderstanding that I very much doubt if I can dispel it in a brief survey. But I cannot overlook this result because I wish to use it here. The problem is very simple. According to Popper's idea, all explanations except some circular or otherwise unsatisfactory ones contain universal laws.

But in history books universal statements are almost entirely absent. Now this is not a very pressing problem: Popper's doctrine concerns fully stated explanations, whereas normally explanations are not fully stated, but merely sketched. If one wants to criticize Popper's deductive model on this level, one can show that it is almost nowhere applicable because we almost never state premises explicitly. Even in mathematics, the only field in which serious attempts at full statements of all premises to given conclusions have ever been made, explanations are usually not fully stated. This is so obvious that I would not have stated it were it not for the fact that many people seem to disbelieve this. It seems clear that in any presentation of an explanation — in mathematics, in physics, in sociology, or in history — authors assume that readers already know parts of the explanation, and they omit such parts simply from the desire to avoid repetition, platitude, and triviality. What is a platitude or a triviality depends, of course, on authors and their intended audiences; yet assuming (correctly or not) that some statement is trivial enough, an author will not state it but, at most, allude to it. Thus, texts of logic and mathematics that contain almost only explicit explanations contain almost only trivial statements; those who do not know what the task of their authors is, and what problems they have to tackle, find these books of fully axiomatized or formalized systems more boring than telephone directories.

What Popper emphasizes is that since most universals used by historians are trivial, they do not explicitly need to state them. This is not to say that physicists do not omit some trivial universal laws from their own explanations, nor that historians do not sometimes state non-trivial universal laws and omit trivial initial conditions. Still, the general impression remains, and it can easily be explained. The chief characteristic of historical research, after all, is its interest in singular statements. Admittedly, if one explains a singular historical event by the use of a controversial law-statement, then the critical debate on it may naturally tend to center around the universal law-statement rather than around the initial conditions, and thus shift from the domain of history to the domain of generalizing science. There exist cases — like the application of Freud's psychology to historical explanation — which turned out to be more of a critical discussion about the psychological theory than of the historical one that the explanation includes.

All this, I suggest, interesting as it may be, should not be taken too seriously: the question of whether one is studying history or generalizing science is unimportant; historians are not bound by any rules to use only unproblematic universal laws although, very understandably, they prefer them to controversial ones. But they may have little choice in the matter, as is often the case with geologists (historians of the earth), cosmogonists (historians of the solar system), or cosmologists (historians of the physical universe): they cannot avoid using highly problematic laws. (See example in Section 10 above.) Yet, although this need not be taken too seriously, it suffices to explain quite a number of facts in the history of history.

For my own part, I am trying to explain the low standard of works on the history of science. Unlike most other historians, historians of science must, in the main, use laws that are highly problematic from methodology and from epistemology. And unlike most historians who have to use problematic laws, they are seldom conscious of the fact that their laws are indeed problematic. I have already noted (Section 2) the naiveté of McKie, who employs, in his historical explanations, Bacon's law according to which every scientific theory emerges from the facts, without noticing that the philosophical literature that he considers irrelevant for the work of the historian of science chiefly concerns itself with criticism of Bacon's law of scientific growth.

Admittedly, bad or boring histories exist in every field and no elaborate effort to explain them is necessary. Yet the fact is that a mediocre political history is less boring than a mediocre history of science — even for a person like myself who is much more interested in the history of science. Since this fact is rather puzzling, my explanation is perhaps not quite redundant. I would go even further, and try to explain two more puzzles. First, why is Bacon's problematic theory accepted as trivially true by so many historians of science? Second, why are their histories, as well as histories based on obviously inconsistent mixtures of inductivist and conventionalist views (see Section 10) so patiently tolerated by a rather enlightened public?

My answer to the first problem is this: Bacon's theory is the only well-known explanation of the rise of scientific ideas. Since the most obvious task of historians of the growth of science is to explain specific developments of this kind, especially the developments of ideas, they need a general theory of the development of ideas;[192] and Bacon's general theory of the emergence of (scientific) ideas has still no widely known competitor.[193]

The second problem is answered by the following conjecture. Science is good; people accept credulously the view that science is good because scientists are always right, or almost always right, or essentially right.[194] (See Section 14); and the task of historians of science is to extol science, especially for the young and for students of the arts (see Section 2). From this standpoint historians of science must accept inductivism or conventionalism, or a mixture of both. A critical attitude towards both may look dangerously like a criticism of the very principle that science is good.

Popper's theory of explanation also helps to clear up a quite different point. Although this is usually a minor point, it gains special significance in the field of the history of science. That is, there is a certain give-and-take relation between the historical and the generalizing sciences. Historians borrow general laws from scientists and scientists develop general laws in order to explain curious historical events. Often the application of a law to history turns out to be a severe test for this law, a test that may raise the suspicion that the law needs modification. This is of particular interest in methodology, whose application to the history of science may be of high interest and raise methodological problems. I shall not discuss this point here,[195] apart from mentioning the utmost significance of the history of the famous crisis in physics for the development of modem methodology.

To conclude, I wish to remind readers that although my frank intention was to advocate Popper's methodology as a means of improving the present lamentable state of affairs in the field of the history of science, I do not think that conscious methodological efforts are necessary for a historian of science to write an interesting and valuable work. The great variety of such histories of science from Priestley and Laplace to Burtt and Koyré indicates that there is no formula for writing an exciting history of science. Moreover, I do not wish to claim that boring and erroneous histories of science are entirely without value; I only suggest that improvement is possible in the direction that I have indicated. Furthermore, I hope that I have indicated how fascinating a field of study the historiography of science can be. (The study both of the history of the history of science and of the methods it employs may be most interesting.) I have tried to study Laplace's brief but exciting excursion into the field of the history of astronomy, and I hope that mine is not the last word on this topic. And there are many other interesting topics that may be studied in a similar or a better fashion. Likewise, there are philosophical problems specific to the field of the history of science — as, for instance, the problem of what a historian of science can hope to be able to explain, and of what is the proper use of the up-to-date textbook of science in the study of its history. My hope is that my rudimentary and unsatisfactory study in the historiography of science will be considered worthy of future attempts to improve upon it.

NOTES

1. I am greatly indebted to P. L. Button, Gerald Holton, I. C. Jarvie, Giorgio de Santillana, A. I. Sabra, Nancy Sutton Sabra, and L. P. Williams, for many valuable comments and criticisms, and for correction of some of my worst errors, and the editors of *History and Theory*, particularly to W. W. Bartley, III, for much editorial assistance — far beyond the usual. He advised me to annotate this work extensively, suggesting it would otherwise be ignored.

1a. Gerd Buchdahl, "A Revolution in Historiography of Science", *History of Science,* 4, 1965, 55-69, a review of Kuhn's famous *The Structure or Scientific Revolutions* and my *Historiography*, said we have put the historiography of science on the map. In 1969 the Minnesota Centre for the Philosophy of Science organized a conference on the topic that intentionally ignored me. I ignore it. There are at least two newer books on the historiography of science; the less said of them the better. Kuhn's historiography was subject to many debates. Little is left of it that deserve study, yet perhaps it influenced the growth of the historiographic literature on scientific revolutions that Koyré and of Cohen had inaugurated and that deserves more study. Apart from the classic works of Koyré and of Cohen in this vein let me mention one remarkable work, H. Floris Cohen, *The Scientific Revolution: A Historiographic Inquiry*, 1994.

2. Abraham Wolf *A History of Science, Technology, and Philosophy in the 16th and 17th Centuries*, 1959, Vol. 2, 633: "Bacon not only knew what qualities of mind were

a hindrance to science, he also had an excellent insight into the kind of mentality that was best fitted for it." Since Bacon confessed that he had exactly that kind of mentality, Wolf greatly regrets the fact that he was a politician rather than a scientist.

George Sarton, one of the most celebrated historians of science of our century, speaks of "a perverse desire to transcend experience. Even the greatest men of science", he warns us, "are not immune from that weakness …" *Horus, A Guide to the History of Science*, Waltham MA, 1952, 37.

3. J. R. Partington was a great admirer of Lavoisier. The greatest tribute that his could pay to is this: "He completely revolutionized chemistry … and although a book on chemistry written before his time would not be intelligible to a student unacquainted with the history of chemistry, Lavoisier's *Traité Elementaire de Chimie* reads like a rather old edition of a modern textbook." *A Short History of Chemistry*, 1960, 130.

   Partington's comment is not far wide of the mark; yet the compliment sounds like a bad joke. Modern elementary chemistry textbooks are conservative to a degree; they are the worst of all. To credit Lavoisier for his slavish followers when he wrote one of the most exciting speculative works in the history of modern physical science is some achievement.

   Partington, it should be noted, is a leading historian of chemistry; A. R. Hall views the book from which I have just quoted as "an excellent introduction". See *The Scientific Revolution, 1500-1800*, 1954, 379.

4. "Sarton's … view of history", writes L. Pearce Williams in his review of the second volume of his *A History of Science*, "is almost painfully naive. The dramatis personae are divided into 'good guys' and 'bad guys', and customs into 'good practices' (writing plain, rather than effusive, dedications; supporting science) and 'bad practices' (slavery and oriental superstitions). From this clash of black and white, one supposes, comes the dynamics of history." *Brit. J. Phil. Sci.*, 11, 1960, 160.

5. Proposition XII of Book III of Newton's *Principia* asserts that planets move in ellipses; but the discussion contains a clear statement that this is only an approximation, and that Newton's own perturbation theory explains deviations from Keplerian ellipses that had been observed and greatly puzzled astronomers.

   For Black's theory of fixed air see A. N. Meldrum, *The Eighteenth-Century Revolution in Science,* Calcutta, 1930; and J. H. White, *The History of the Phlogiston Theory*, 1932.

   For an amusing example of how an author may be squeezed into the up-to-date textbook. See H. E. Roscoe's treatment of Dalton's atomism in his authoritative *John Dalton and the Rise of Modern Chemistry*, 1895, 134 and notes. He quotes Dalton as saying that atoms of different elements may have different sizes, so that if the same volume can be filled by more atoms of *a* than of *b*, the size of the *a* atom differs from the size of the *b* atom. The idea that different atoms possess different bulks does not, however, tally with the idea of the up-to-date textbook. Consequently, Roscoe adds a footnote to explain that "By 'size' he [Dalton] perhaps includes the idea of weight." This explanation conflicts, however, with the text that Roscoe himself quotes. The idea that atoms of different elements differ in weight is ancient, and permanent in the chemical literature from Boyle to Lavoisier. Roscoe adds a note to Dalton's remark that the idea of different atomic sizes had occurred to him in 1805, saying "Dalton seems here to have mistaken the date, for in the autumn of 1803 he gave a table of [atomic weights]". All historical evidence contrary to attempts to make our heroes up-to-date must, it appears, be mistaken!

Incidentally, Dalton viewed the atom's size as the size of the "hard particle at the centre and the atmosphere of heat taken together". This is Lavoisier's idea of hard matter residing in elastic caloric, discussed in note 164 below. Dalton's theory of the heat-expansion of gases (the increase of bulk of a gas is proportional to the increase of temperature when the pressure is constant) is directly connected with his idea of size and his desire to estimate the number of gas molecules. But this fact relating to the pitch-black caloric theory is still unknown to historians of science. Dalton's ideas are discussed in section 15, and the embarrassment of inductivist historians regarding him, in section 3.

6. See George Sarton, *op. cit.*, 41.

7. *History of Physics*, 1950. Henry Guerlac describes this work as "factual and condensed" in his review, *Science,* 62, 1950, 344.

8. For Thorndike's works see notes 9, 15, 47, 48, 58 and 93.

   To take another instance, consider the thesis of L. W. H. Hull's *History and Philosophy of Science: An Introduction,* 1959. Hull's thesis is correctly stated by Carl B. Boyer in his review of it as follows: "broadly speaking, the triumph of empiricism over metaphysics, of reason over authority, of *a posteriorists* over *a priorists*." And, Boyer adds, "there is a tendency to see stark contrasts, such as between the humility and success of science as over against the arrogance and failure of metaphysics in the conflict of science with orthodoxy", *Isis*, 51, 1960, 347. As to the Cabbalistic origin and character of the idea of successful humility as against barren arrogance see sections 14 and 16 below.

9. The history of the process of blackening Descartes is discussed in my doctoral dissertation: *The Function of Interpretations in Physics*, unpublished, University of London, 1956. See also notes 29 and 84. Concerning Stahl, see section 11; concerning Kepler, see section 4.

   I. B. Cohen, "Some recent books on the history of Science", *J. Hist. Ideas*, 15, 1954, 164, refers to the conspiracy of silence about the Book II of Newton's *Principia*, due to its not being white. Laplace's simple criticism of Newton's theory of the tides — namely, that it does not accord well enough with the facts — is rejected as almost *a priori* impossible even by Isaac Todhunter, who writes, "I do not understand this criticism"; *History of Mathematical Theories of Attraction,* 1873, 33.

   Augustus DeMorgan's *Essays on the Life and Work of Newton*, ed., Phillip E. B. Jordain, Chicago and 1914, were written in protest against Brewster's idolizing biography of Newton; their impact was hardly noticed; see J. M. Keynes' essay on Newton, *Royal Society: Newton Tercentenary Celebrations*, 1947, 28-9, reprinted in his *Essays and Sketches in Biography*, 1956, 281. Keynes exposes the conspiracy of silence about Newton's still largely unpublished theological works — a conspiracy that is still not fully broken; Newton, says Keynes, was not the first of the scientists, but the last of the magicians. Keynes' argument is against the black-and-white theory, of course; yet Stephen Toulmin interprets him as saying that Newton is not white but black. "My main thesis", Toulmin writes, is that "the reaction has been overdone." "Newton on Absolute Space," *Philosophical Review*, 63, 1959, 5. Perhaps Keynes' eloquence is a disadvantage in this case — for its exaggerations may mislead, and appear to have done so with Toulmin.

   Lynn Thorndike concludes his *A History of Magic and Experimental Science,* 1958, Vol. 8, with a discussion of the last of the magicians. But we hardly can blame Keynes for this: Thorndike had to finish somewhere, and Keynes provided a pretext. Thorndike succeeds in viewing even Galileo (Vol. 7, 40), and Boyle (Vol. 8, 180) as "magicians", and he considers the use of the word "arcane" as illegitimate on account of its being current in the magic literature (Vol. 7, 8-10). Thus, everyone before Newton was black; Newton was partly white and thus opened the white era!

10. Florian Cajori, *History of Physics*, first edition, 1899; second edition, 1929.

11. One may be indulgent towards Cajori here, I think. For his condemnation of the electric particles theory is the result of being influenced by S. P. Langley's wonderful attack on all fluids, including caloric and electric. Cajori himself seems uneasy about the idea of electric charge without electric fluid, as may be shown by the fact that he quotes Poincaré, who says that it is very difficult to know what Maxwell meant by charge. Yet he does not express his own opinions or difficulties, and his condemnation of Crookes' theory, that led to the discovery of the electron (see end of next note), is evidence of his lack of understanding. The condemnation of Crookes is deleted in the second edition, but hardly anything else was explicitly changed.

12. In his preface to the revised edition Cajori writes (1st ed., 266; 2nd ed., 271):

> "Since the first appearance of this *History*, many things have happened in the development of physics. To young students of science the discovery of radioactivity and the introduction of the electron are not happenings of their own time, but are events of the past fully as much as are Galileo's experiments on falling bodies and Newton's law of gravitation. For this reason it is desirable to present to the younger students the historic outline which to the older generation is part and parcel of its own intellectual life and experience.
>
> The task of describing the principal achievements of physics in the present century has been a difficult one. Experiments and hypotheses which seem important now may appear insignificant later. On recent events we lack perspective. The historic presentation of recent movements must necessarily be of only transient value. But a near view is better perhaps than no view at all.
>
> The revision of this book does not consist simply in the annexation of new material relating to the researches made in the present century. Many additions and alterations have been made in the earlier part of this History."
>
> The changes are introduced subtly, by slight additions and omissions. See for example, the omission of the last words in the comment on Crooke*s*' theory (of ionized gases in cathode tubes): "has been much criticized … and is not generally accepted".

Incidentally, Cajori's "The Baconian Method of Scientific Research", *Scientific Monthly*, 20. 1925, 85-91, criticizes Bacon, saying that it is beyond the reach of ordinary mortals to keep their minds totally empty of all preconceived notions. This need not puzzle us; for his censure of Gilbert and Galileo rested on the assumption that they were men of genius. The irony is that Bacon's method was appreciated so much in the 17th century because it was meant for ordinary mortals — as the following passages from Hooke help to illustrate: "The intellect is continuously to be assisted by some method or engine which shall be as a guide to regulate its actions, so as that it shall not be able to act amiss. Of this engine no man except the incomparable Verulam [Bacon] hath had any thoughts, and he indeed hath promoted it to a very good pitch." In this way, Hooke adds, "the business of invention will not be so much the effect of acute wit, as of a serious and industrious prosecution." See General Scheme or Idea of the Present State of Natural Philosophy, etc., Posthumous Works (1705) 6-7, quoted by Ellis in his "General Preface to Bacon's Philosophical Works", printed in *The Works of Francis Bacon*, ed. Spedding, Ellis, and Heath 1857, I, 25.

13. A striking, though rather unusual, example is P. G. Hall's review of E. H. Bunbury's *A History of Ancient Geography*, of 1879, 1883, 1959. One would expect the impact of the up-to-date geography textbook on the historian of ancient geography to be minute, and the impact of recent changes in the textbook even smaller — on account of the relative stability of the geography textbook as compared with that of physics. Yet Hall condemns Bunbury because his history does not accord with the principles of geography that were created, we are told, thirty years later. These principles, Hall suggests, should lead to a reappraisal of Aristotle's ideas. See *Brit. J. Phil. Sci.*, 12, 1962, 342-345. See also in this connection section 10.

14. In his introduction to the paperback edition of Newton's *Opticks*, of 1952, Whittaker dates this change from 1927, when both "the" wave and "the" particle theories were proved true (lxii). I. B. Cohen's preface (xiv-xv) is different; his rejection of the attitude of the up-to-date textbook worshipper as irrational is similar to the one advocated here.

15. A controversy over the problem of what portion of a historical study should be given to black events and what portion to white, took place between Thorndike and Sarton. Yet in principle Sarton agrees with Thorndike that the historian of science should be interested in "mistakes…, false tracks…, misunderstandings…", *op. cit.*, 41. F. R. Johnson, in his eulogistic review of the last two volumes of Thorndike's *A History of Magic and Experimental Science*, in *J. Hist. Ideas*, 20, 1959, 282, ecstatically reports this wonderful controversy between Sarton and Thorndike. Yet he admits that the two authors were "writing about the same subject from two different, and frequently complementary points of view". To interpret: both Thorndike and Sarton agree that white is white and black is black; but Thorndike likes more black in his pictures.

16. See I. B. Cohen's editorial, *Science*, 114, no. 2973, December 21, 1951; and Guerlac's *Development and Present Prospects of the History of Science, Report submitted to the Nineteenth International Congress for the History of Science* (Paris, 1950). See also W. Mays' revealing, if somewhat inaccurate, statistics on the teaching of the subject in the British Commonwealth, in "History and Philosophy of Science in British Commonwealth Universities", *Brit. J. Phil. Sci.*, 11, 1960, 192-211.

17. Sir James Jeans, *The Growth of Physical Science*, 2nd ed., 1951.

18. I. B. Cohen made this observation in "Some recent books on the History of Science", *J. Hist. Ideas*, 15, 1954, 163 ff.

19. See D. Stimson, *The Gradual Acceptance of the Copernican Theory of the Universe*, Hanover NH, 1917. E. Rosen argues, "Galileo's Misstatements about Copernicus", *Isis*, 69, 1958, 319-330, that the genuine opposition to Copernicus in the early seventeenth century was still considerable. Laplace has suggested (see section 4) that it was Cartesian philosophy that pushed the traditional Aristotelianism aside by taking its place in the public mind; in other words, Copernicanism became the vogue in the mid-seventeenth century. Laplace's view seems to accord with known facts; see note 29.

20. "The main duty of the historian of science is the defense of tradition. The traditions of science… deserve to be known and religiously kept because they are really the best we have, they are all that makes life worth living …", writes Sarton in his "Science and Tradition", *A Guide to the History of Science*, *op. cit.*, 15. I agree with Sarton, although I would not take the word "duty" too seriously — especially since, in a footnote to the preceding paragraph Sarton reports Dingle's view — in his inaugural lecture of 1947, reprinted in *The Scientific Adventure*, 1952 — that the scientific tradition is that of "internal criticism of science, a criticism largely based on historical

knowledge". Sarton seems to agree with Dingle wholeheartedly; for myself, I think all criticism, internal or external, based on historical knowledge or not, belongs to the tradition that is worth defending. But how does Sarton think we defend it? "We owe gratitude to the benefactors of the past, in particular the great men of science who opened the new path …" he goes on. "While we express our gratitude we feel that we become worthy of them …" Characteristically, Sarton begins with noble sentiments about the critical tradition of science and ends with the platitudes of the uncritical tradition of the history of science. And he even supports the "monumental and iconographic tradition" of scientific ancestor worship (*op. cit.*, 42). I cannot help thinking that we shall be able to see the greatness of Sarton's sentiments only after we cease paying lip service (see note 58 below) to his work. Somehow he could not break away from a bad tradition and all that was good and noble in him called to retain the naive but shining faith in science as a humanist tradition. (Although he usually, though not always, adhered to the division between black and white, he repeatedly asked his readers to be indulgent towards the superstitious, especially since we ourselves are not wholly free of superstition.) Again we see noble sentiment combined with and spoilt by adherence to Baconianism.

21. "The teaching of the history of science should be as concrete and clear as possible rather than philosophical and foggy" says Sarton in *A Guide to the History of Science*, 60. With the *a priori* exclusion of the possibility that something may be philosophical and clear the confusion of the wood with the trees is secured.

The demand that the history of science be separated from the philosophy of science is a modification of Bacon's demand that science and metaphysics — and thus the history of science and the history of metaphysics — be separated. He based this demand on his claim that science begins with observation and metaphysics with speculation. Bacon's demand is still supported by S. Sambursky in his *The Physical World of the Greeks*, 1956, and even in his revised edition (1959). Sambursky notices (224) that Descartes and Leibniz are exceptions; and he erroneously considers Galileo an anti-metaphysician. (See also E. H. Hutten's just criticism in his review of this book, *Brit. J. Phil. Sci.*, 8, 1958, 347-8.) This accords with Bacon's doctrine according to which anyone who starts with a preconceived opinion will not observe some counterexamples to it and will dismiss others as exceptions. Yet Sambursky's change of view concerning Stoic physics refutes Bacon's doctrine. Meanwhile Sambursky has also rejected Bacon's doctrine concerning the benefit of divorcing physics from metaphysics, and I hope that he will express this change in print as explicitly and rationally as he has expressed the change of his views concerning Stoic physics.

The inductivist demand that science be divorced from metaphysics is not, I think as objectionable as the crypto-inductivist demand of Sarton and McKie that the history of science be purged of any trace of philosophy of science. An extreme expression of this demand has been provided by J. Ravetz, *Brit. J. Phil. Sci.*, 12, 1961, 250. Ravetz writes: " … so much of the history of science has been done philosophically. The simple question, 'what was Galileo doing in his inclined plane experiment?' has been answered not once but too many times. Quite different answers have come from historians with a prior commitment to mathematical idealism inductivism, refutationism, or dialectical materialism. At this point the philosopher may ask, 'Is there not a general account of, say, the Scientific Revolution, which is reliable but not pedantic, and neither naive nor axe-grinding in its philosophical commitment?' In short, something he can use …" Ravetz wishes to have watertight compartmentalization between the history and the philosophy of science (including inductivist philosophy of science). History of science based on this demand will not be based on any philosophy hence the demand is baseless.

The diversity of accounts of Galileo's experiments, about which Ravetz complains, is not typical. For my part, I would recommend more diversity about more historical problems — and, in particular, a rational debate between the holders of diverse answers to one question.

The view that philosophy must be relevant to the history of science is advocated by a few contributors to Marshal Clagett's *Critical Problems in the History of Science*, 1959. See also G. Buchdahl's favorable comment in his review of this in *Brit. J. Phil. Sci.*, 12, 1961, 79-82, as well as the reviews by L. Pearce Williams and A. C. Crombie. See note 58.

22. Reporting the contents of young Lavoisier's first memoir — on the alleged strength of which the *Academie* was forced to make him a member — McKie says the following: "In it Lavoisier determined the solubilities in water of the different varieties of gypsum; he explained the setting or binding of plaster by showing … that gypsum lost a quantity of water when it was heated … , which it took up again when it was mixed afresh with water and that this recombination *or rather recrystalization* was the cause of its setting *or solidification* … The memoir was well written; the conclusions were clear and careful, theory was not allowed to pass beyond facts" (my italics); McKie, *Lavoisier*, 1952, 44. Since Lavoisier's theory did not go beyond facts it is true, and since it is true he spoke not of combination and setting but "rather" — rather — of crystallization and solidification. For the historical importance of Lavoisier's erroneous view of the process as chemical (i.e., as caused by forces of chemical affinity rather than by molecular forces) see section 15 below.

23. Pierre Duhem, *The Aim and Structure of Physical Theory*, Princeton, 1954, 263.

24. Cf. Toulmin's apt observation: "Lessons which have long been learnt in other branches of history have, in the history of science, yet to make their full effect felt …" *J. Hist. Ideas*, 17, 1957, 205. It is interesting that A. Koyré and E. A. Burtt came to the history of science from the tradition of the history of philosophy, and R. F. Jones and M. H. Nicolson from the tradition of the history of English literature; all four have made an immense impact on the field in terms of quality, although a negligible one in terms of quantity.

25. C. P. Snow advocates the teaching of the history of science as a possible bridge between the two cultures; Recent Thoughts on the Two Cultures, 1961, 6. Sir Oliver Lodge had advocates a similar view; *Pioneers of Science* 1893, 1960. Lecture I, page. 1. But at least Snow notes that "history of science taught badly is awful".

26. George Sarton, *The Study of the History of Science*, 1936, reprinted, 1957, 12-16' *Guide to the History of Science*, 11-18, especially page 16 and note. See also R. C. Stauffer, "Persistent Errors Regarding Oersted's Discovery of Electromagnetism", *Isis*, 44, 1953, 307 ff.

27. History of Science Society, *Sir Isaac Newton, 1727-1927, A Bicentenary Evaluation of his Work*, Baltimore, 1928.

28. Duhem discusses this topic in most of his chief works; he does so both because he wished to rehabilitate the Middle Ages and because the continuity aspect is more marked in the history of medieval science than it is, say, in the history of non-Euclidean geometry (see section 9). This is so in part because of slow progress and bad communication. Another reason is the strange tug-of-war situation between the traditions that follow Aristotle and Archimedes that continued until the victory of the Archimedean tradition in the person of Galileo (see section 15 and notes to it). In any case, Duhem is correct in attributing "the" (i.e., Galileo's) law of inertia to no one, and in attributing to Buridan a close approximation to it. One may notice, incidentally, that Galileo does not claim priority for "the" law; he makes Simplicio assert it,

although he makes Salviati use it for Copernicanism. Inertia of some sort or another was, no doubt, "in the air" even before Buridan formulated his law of impetus (see note 80). Indeed, the difficulty was not to discover it but to remember it: in his early works Galileo himself appears to formulate it on one page and to forget it on the next. Duhem uses this muddled Aristotelianism of the young Galileo as evidence for continuity. The important point historically was Galileo's putting an end to the medieval muddle. This may warn us against taking continuities too seriously, and against claiming that they exist except when an idea was historically "in the air".

29. L. W. H. Hull, *History and Philosophy of Science*, 1959, 154:

> "The natural idea — No force, no motion — became the basis of Aristotelian mechanics. It was not altogether unchallenged... According to Aristotle, the arrow should stop after the driving force is removed. The reply was that...the rush of air continued to propel the arrow . . . It is now not clear why an arrow should ever stop; and it would seem that the arrow cannot move in a vacuum. This last difficulty did not worry the Aristotelians, who denied the possibility of a vacuum. But they could not justify this denial and it was not easy to maintain — especially after the invention of the air pump in the mid-17th century. But despite such difficulties, the view that motion could not exist without driving force was not easily discredited. the law of Inertia . . . was understood by Galileo. It is called Newton's 1st Law of Motion …"

I have quoted from a work published in 1959, but there are a large number of similar passages in other works. Aristotle's circular inertia, Galileo's circular inertia, and the Aristotelians' reply to criticism are all ignored. Aristotelians are particularly criticized for not having learned from the existence of the vacuum after the mid-17th century! Incidentally, Huyghens' surprised that Boyle had bothered to answer Fr. Linus' "frivolous objections" to his work on the vacuum suggests that Aristotelianism was *passé* in the scientific world of the time. See S. P. Rigaud, *Correspondence: Scientific Men of the Seventeenth Century*, 1841, 92. See also note 19 above.

Quite possibly, inductivist historians are less reluctant to censure Descartes than Galileo, as the following, by no means representative, passage from E. T. Whittaker, *A History of Theories of Aether and Electricity, The Classical Theories*, revised edition, 1951, 8, may indicate. Speaking of Descartes he says, "A further weakness in his system was involved in the assumption that force cannot be communicated except by . . . impact" although, of course, already Galileo advocated this view. Whittaker's formulation leaves open the question of the identity of Descartes' law of inertia with Newton's.

30. The paucity of source books in physics — especially when compared with the ever increasing flood of histories of science — is explicable by the inductivist need to process historical material. It is no accident that the Alembic Club Reprints are not as popular as they should be, and that the *Harvard Case Histories* were designed by the anti-inductivist J. B. Conant. (As my criticisms of some of these case histories — given below in notes 38, 141, 152, and 162 — will show, Conant's collaborators are to a large extent inductivists; but this is a different matter.) The systematic care with which existing anthologies have been chosen so as to fit inductivism, and thereby the up-to-date science textbook, has already been noticed with chagrin by M. H. Nicolson (see quotation in note 54 below). Years ago I designed a different kind of anthology that would include such items as Robert Hare's fascinating refutation of Davy's thermodynamics — published in *Silliman's Magazine*, 1820, and *Philosophical Magazine*, 1822. The publisher had initially shown interest but gave up the project, failing to find a single referee whose view was not totally antagonistic. I had called my intended anthology "rare and forgotten essays", and the referees said that

those essays were rightly forgotten: we have no time for all the mistakes that humanity has made, they argued.

31. *Britannica* , Fourth edition (1810), V, 427, Art. "Chemistry":

> "In the mean time, the French chemists were not idle. The celebrated Lavoisier, in conjunction with some of his philosophical friends confirmed, by the most decisive experiments, the truth of Mr. Cavendish's discovery of the composition of water, which was now received and adopted by almost every chemist. The same unfortunate philosopher, whose bright career was cut short by the horrors of the French revolution, had, previous to the time alluded to, enriched chemical science with many valuable and important facts. He had greatly contributed to overthrow the phlogistic theory, by series of accurate experiments and observations on the calcination of metals. It had now become a question, whether metals, during the process of calcination, gave out any substance; that is, whether they contained any phlogiston; and Lavoisier incontestably proved, that metals cannot be calcined excepting in contact with pure air, [i.e., the compound of oxygen and caloric], and that the calx thus obtained was, in all cases, exactly equal to the weight of the metal, and the quantity of air which had disappeared."

Britannica , First edition (1771), II, 68, Art. "Chemistry":

> "From what hath been said concerning the nature of fire, it is evidently impossible for us to fix and confine it in any body. Yet the phenomena attending the combustion of inflammable bodies shew that they really contain the matter of fire as a constituent principle. By what mechanism then is this fluid, so subtile, so active...? It is no easy matter to give a satisfactory answer to this question. But without pretending to guess the cause of the phenomenon [of combustion], let us rest contented with the certainty of the fact . . . Let us therefore examine the properties of fire thus fixed and become a principle of bodies. To this substance, in order to distinguish it from pure and unfixed fire, the chemists have assigned the peculiar title of the Phlogiston, which is indeed no other than a Greek word for the inflammable matter.... Hitherto chemists have never been able to obtain the phlogiston quite pure, and free from every other substance. … [The way of getting it is by burning the body containing it, in which case] it is entirely dissipated in the decomposition so that no part of it can possibly be secured."

32. Dr. Thomas Thomson, *History of Chemistry*, 1831, 11; *History of the Royal Society,* 1812, 480. Dr. Thomson aimed at belittling Lavoisier. My aim, on the contrary, is to illustrate that (contrary to his opinion) even great thinkers err, and that great ideas may be false. Strangely, Dr. J. A. Paris, in his *Life of Davy*, 1831, Vol. 1, 324, speaks in this vein when discussing Lavoisier's doctrine, yet, soon afterwards conceals Davy's error. Of Lavoisier's error he says (italics mine), "Upon the establishment of the antiphlogistic theory by Lavoisier, it became essential to the generalization which distinguished it, that a body performing the functions of an acid, and above all, supporting the process of combustion, should be regarded as containing oxygen in its composition; and facts were not wanting to sanction such an inference. The substance could not even be produced from muriatic [hydrochloric] acid, without the action of some body known to contain oxygen; while the fact of such a body becoming deoxidated by the process, seemed to demonstrate beyond the possibility of error, that the conversion of the muriatic into the oxymuriatic acid, was nothing more than a simple transference of oxygen from the oxide to the acid: an opinion which was universally adopted, and which for nearly thirty years triumphed without opposition." Paris then turns at once to a discussion of "the body of evidence by which Davy overthrew this

doctrine" of Lavoisier. "It will be impossible for me to follow the author through all the intricacies of the enquiry; but I shall seize upon some of its more prominent points, and give a general outline of its bearings." He silently drops the strongest of Davy's arguments; namely, that carbon and chlorine do not combine, on account of its having been refuted (1821) by Faraday (see note 85).

33. The two definitions of "element" are quoted by G. Holton and D. H. D. Roller, *Foundations of Modern Physical Science*, Reading, MA., 1958, 377.

34. McKie tries to keep some of the credit for his hero: Lavoisier was the first to state explicitly the "law of indestructibility of matter applied to chemical change" (that is admittedly "implicit in the researches of Black and Cavendish"); *Lavoisier*, 214. As his own quotation (215) shows, the claim is false: the passage quoted from Lavoisier is but another formulation of the law of conservation, already stated as a principle by Descartes. For its importance see Oldenburg to Spinoza, 3 April 1663, in Spinoza, *Correspondence*, ed. A. Wolf, 110. É. Meyerson has discussed this importance in Boyle's work; *Identity and Reality*, 1907, 1930, Chapter 4, on the conservation of matter. A. N. Meldrum has discussed the significance of weighing in Black's work, as the employment of the law of conservation of matter; *op. cit.* Holton and Roller claim that the weighing of chemicals became of "crucial significance" only with the work of Lavoisier; (*op. cit.*, 270). They do not ascribe to him the principle of conservation of mass (274).

35. *Loc. cit*. Meldrum observed this on several occasions, but apropos a different view. He attributes to Lavoisier only a sense of thoroughness, which helped him to develop generally known views and perform or repeat related experiments; *op. cit.*, Chapter 4 on "Lavoisier's Superiority".

36. "It has not escaped my notice", writes Ørsted, "that the antiphlogistic theory is often mentioned as if it were unrefuted, and in a certain sense this is correct, so far as the circle of experiences is only alluded to, which it embraced … but it no longer exists as a complete chemical theory as every one will now readily grant"; *The Soul in Nature*, 1852, 301n.

    "Lavoisier … demonstrated beyond cavil that the process of combustion … is nothing more than the uniting of other elements with oxygen", say F. R. Moulton and J. 1. Schiffers, the editors of *The Autobiography of Science*, 2nd ed. 1960, 228.

    Partington contrasts "the path of true discovery opened out by Boyle, Hooke, and Mayow" with "the jungle of the Theory of Phlogiston"; *op. cit.*, 84. He deemed phlogistonism a jungle, since "Stahl inverted the true theory of combustion and calcination; adding phlogiston was really removing oxygen" (88). This implies that Lavoisier's theory is true. Indeed, dismissing Lavoisier's merit as experimenter, Partington declares (122) that "his great merit lay in his capacity … of expounding … true explanation …" The explanation that he true declares (131) should be known to be false even to people less versatile in chemistry than Professor Partington.

37. Chemical Society, *Faraday Lectures*, 1928, 2.

38. Leonard K. Nash states that Dalton's theory was "devised to solve this very problem" of solubility of gases in fluids; "The Origins of Dalton's Chemical Atomic Theory", *Isis*, 47, 1956, 108. He also speaks of Dalton as "the effective architect of the atomic theory as we know it today"; *Harvard Case Histories*, 1957, Vol. 1, 218.

39. *Britannica*, Eleventh edition, 1910. The article on Dalton there quotes Roscoe and Harden and attributes to Dalton the law of multiple proportions. This interpretation of their idea may be true; it is incorrect (see the next note). Moreover, the law of

multiple proportions is either untestable (if the factor of proportion is allowed to be large) or refuted, e.g., by paraffin series, C_nH_{n+2}, with n running up to 90.

40. Partington attributes to Dalton the discovery of the law of multiple proportions in 1803 (*op. cit.*, 158), but agrees (166) that it was "foreshadowed" in 1789 by Higgins who knew and discussed in detail the case of NO, NO_2, NO_3, NO_4, and NO_5. See also Partington, "Richter and the Law of Reciprocal Proportion", *Annals of Science*, 7, 1951, 173, and 9, 1953, 189. For more documentary evidence see Cruickshank's paper of 1801, referred to in notes 132-3, and text.
41. *New System*, Appendix to Pt. 2, 1810, 555-559 — *Alembic Club Reprints*, Vol. 4; also A. N. Meldrum, *Avogadro and Dalton*, 1904.
42. The famous and endearing story of how three Cambridge undergraduates achieved a revolution in English mathematics in the early nineteenth century by fighting for the replacement of Newton's "." with Leibniz's "d" is interesting, but it cannot be taken at its face value: symbols matter very little, and mathematicians hardly care about them. Some historians allude to, or state, the view that the revolution was against the pretence that the calculus is a branch of geometry: Newton's pretence had caused great difficulties, and Lagrange's analytic mechanics could not be translated into traditional or semi-traditional language. But there is quite possibly more to the story: Lagrange's ideas about the foundations of calculus were attempts to improve upon Newton and MacLaurin, and the Cambridge youths provided a good camouflage for Newton's and MacLaurin's errors; how else could victory be so easily attained?
43. J. Scoffern, *The Subject Matter of a Course of Six Lectures on the non-metallic elements by Professor Faraday*, 1853. See also *Britannica* , eleventh edition, 1910, article on Dalton. The passage from Dalton most frequently reproduced today is his terminological part; in the *Harvard Case Histories* this passage is even eulogized. Dalton and Lavoisier have not been the only ones to suffer such a fate; amongst the few pages allotted to Faraday in the Dampiers' anthology of science, two are terminological. Fortunately, however, they contain his remark: "I am fully aware that names are one thing and science another"; *Experimental Researches in Electricity*, Vol. 1, §666.
44. See for example A. N. Meldrum, *Avogadro and Dalton*, *op. cit.*, final chapter.
45. Dr. Thomson, *Annals of Philosophy*, quoted by Roscoe, *op. cit.*, 155.
46. *Hist. Chem.*, Vol. 2, 237: Dalton's theory was "so contrary to opinion previously received that chemists were not disposed to admit it". Concerning Davy and Wollaston see the highly indicative and amusing anecdote, *op. cit.*, Vol. 2, 293.
47. L. Thorndike, *History*, *op. cit.*, Vol. 7, 3-5. Thorndike refers to LeGendre as the historian from whom he received this idea. Thus, undoubtedly, the history of pseudoscience and superstition and the compilations of vulgar errors are historically related; my claim is merely that the inductivist's justification of the vulgar error literature does not justify the records of forgotten errors: their recorders should use them or not publish them, at least not as warnings against error.
48. Op, cit., Vol. 1, 2. See K. R. Popper, "Back to the Pre-Socratics", *Proceedings of the Aristotelian Society*, 1958, and *Conjectures and Refutations*, 1963. Thorndike notes that astrology never was "a law in the modern sense of being mathematically demonstrable"; but Thorndike — a recorder of facts-as-they-really-happened — wants "to emphasize that this belief was generally held by scientists and by mankind at large for centuries, and it should be taken into account by every historian of that period". "The True Place of Astrology in the History of Science", *Isis*, 46, 1955, 278, final footnote. I agree with Thorndike only regarding the concealment of facts: it is wrong.

Yet most of his output is worthless: details need not be recorded unless they prove interesting.

49. J. Herschel's formulation of this, *A Preliminary Discourse on the Study of Natural Philosophy*, 1831,104, § 96. is famous:

> "It is to our immortal countryman Bacon that we owe the broad announcement of this grand and fertile principle; and the developement [*sic*] of the idea, that the whole of natural philosophy consists entirely of a series of inductive generalizations, commencing with the most circumstantially stated particulars, and carried up to universal laws, or axioms, which comprehend in their statements every subordinate degree of generality, and of a corresponding series of inverted reasoning from generals to particulars, by which these axioms are traced back into their remotest consequences, and all particular propositions deduced from them; as well those by whose immediate consideration we rose to their discovery, as those of which we had no previous knowledge [including] ... all those facts on which the arts ... depend ..." .

50. K. R. Popper, "On the Sources of Knowledge and of Ignorance", *Proc. Brit. Acad.*, 1960; reprinted in his *Conjectures and Refutations*, 1963.

51. Hume had already criticized Bacon, but only to the extent of declaring Bacon inferior to Galileo — since the former only pointed the way, while the latter both pointed the way and traveled on it. Hume did not refer to Bacon's errors, and thus treated him more leniently than he treated Boyle, though he must have known that Boyle's errors were negligible by comparison; Hume, *History of England*, Appendix to "Reign of King James I".

52. *Celestial Mechanics*, Bk. X, Chap. vii, 3, 22. See E. T. Whittaker, *A History of Theories of Aether and Electricity*, revised edition, 1951, 207. As usual, Whittaker does not explain, or even mention, the Cartesian character of Laplace's *gravifique*, obviously because science and metaphysics are poles apart. But he does state elsewhere that LeSage's similar theory is Cartesian.

53. Laplace's idea has been interestingly generalized by Dingle — although probably not under his direct influence. He asserts that history seldom follows the logical (i.e., Baconian) line of development. See his interesting "Reflections on the History of Science", *The Scientific Adventure*, 1952, 37: " … every student ought to know that the historical and logical developments of the subject are not the same. This might be thought too obvious to mention, but the appearance of Sir James Jeans' posthumous book, *The Growth of Physical Science*, shows that it is not so."

54. In his *History of the Inductive Sciences*, 1837, 410, Whewell quotes in a footnote the following passages that he criticizes in the text:

> "[1] Laplace, *Precis de l'Hist. d'Ast*. p. 94. 'It is painful for the human spirit to see this great man in his last works, priding himself pleasurably about his chimerical speculations, and to look on them as the soul and life of astronomy". *Hist. of Ast*., L. U. K. [Library of Useful Knowledge], p. 53.
>
> [2] 'This success [of Kepler] may well inspire with dismay those who are accustomed to consider experiment and rigorous induction as the only means to interrogate nature with success.' *Life of Kepler*, L. U. K., p. 14. 'Bad philosophy', p. 15. 'Kepler's miraculous good fortune in seizing truths across the wildest and most absurd theories.' p. 54. 'The danger of attempting to follow his method in the pursuit of truth.'"

Whewell comments in the text:

> "Several persons[1], especially in recent times, who have taken a view of the discoveries of Kepler, appear to have been surprised and somewhat discontented that conjectures, apparently so fanciful and arbitrary as his, should have led to important discoveries. They seem to have been alarmed at the moral that their readers might draw, from this tale of a Quest of Knowledge, in which the Hero, though fantastical and self-willed, and violating in his conduct, as they conceived, all right rule and sound philosophy, is rewarded with the most signal triumphs. Perhaps one or two reflections may in some measure reconcile us to this result."

Philosophers and historians of science did not accept Whewell's "one or two reflections" any more than they accepted his philosophy in general, although he did influence the scientific world profoundly, particularly in Britain. Inductivist historians of science repeatedly face this same problem. Laplace at least admitted the facts, and even stated boldly that "without the speculations of the Greeks… [Kepler's] beautiful laws might have been still unknown"; modern historians of science, however, take an easier route, and simply conceal the embarrassing facts. As M. H. Nicolson put it in *The Breaking of the Circle*, 1950, 129:

> "Modern historians of science usually approach Kepler with some misgiving, unless they belong to the group that reads its early science only in extracts carefully selected by anthologists for their 'scientific' value. Newton disturbed the others somewhat: he should not have been more concerned over his apocalyptical interpretations than he was about the law of gravity; he should not have been 'influenced' by such a hazy thinker as Jacob Bohme — yet he was. But Kepler bewildered them even more. They cannot deny the importance of his laws, but they deplore his superstition, and his mysticism. The *furor poeticus* [poetic excitement] may be all very well in a poet, but a scientist should not have interlarded his serious work with poetry, nor intoxicated himself with words, as Kepler did. Kepler believed that the earth was alive and that its nature 'corresponded' to the nature of the universe; he believed in the sacred mystery of numbers …"

55. J. L. E. Dreyer, *A History of Astronomy*, 1953, 360 and note.

56. Dreyer notes (*op. cit.*, 371) that "in his writings Kepler repeatedly claims for Brahe the merit of having 'destroyed the reality of the orbs'." So he continues counting his hero's medals without stopping to ask whether Kepler was merely commending Tycho for having destroyed some ancient error or whether Kepler was not expressing his indebtedness to Tycho. "The idea of the Tychonic system", Dreyer adds (367), "was so obvious a corollary to the Copernican system that it almost of necessity must have occurred independently to several people". This judgment is quite incorrect: the Copernican system had real spheres. Dreyer also doubts the authenticity of the evidence that Tycho had invented his system in 1575, but does so only in view of Tycho's own evidence that the idea occurred to him in 1583 or perhaps a little earlier (363 and note). This is a strange approach indeed in view of Dreyer's own report (*loc. cit.*): "In the eighth chapter of his book on the comet of 1577 … Tycho describes his own system … the two orbits [of the Sun and of Mars] intersect each other, but as they are only imaginary lines [and not parts of crystalline globes as tradition would have it], there is nothing absurd in this." Clearly Tycho could not have developed his system before 1577: it was the path of the comet that ruined the idea of the spheres by penetrating them. But Dreyer does not notice this, perhaps because he is anxious to defend his hero against inductivist accusations, whose system, he continues, "is in reality absolutely [sic] identical with the system of Copernicus." This

view is, however, anachronistic and false: Tycho saw mechanical objections to Copernicus that were dispelled only by Galileo; moreover, in Tycho's system there is no parallax of the fixed stars, whereas in Copernicus' system there is one — observable or unobservable, but real all the same. Dreyer, who seems to have noticed this last point, continues: "and all computations of the places of planets are the same in the two systems." That is, as Tycho had discovered, the two systems are identical only in this very restricted sense.

Koyré has noticed how Gilbert was influenced by Tycho's destruction of the spheres to dispose of the sphere of the fixed stars; *From the Closed World to the Infinite Universe*, 1957, 56. The significance of Tycho's abolition of the spheres had already been noticed by Boyle, *Works*, first edition, Vol. 3, 444a; second edition, Vol. 4, 60. Perhaps it was easier for Boyle to notice this because he had once entertained Tycho's views, as he admits in a private letter to Hartlib (8 April 1647).

57. W. C. D. Dampier-Whetham, *A History of Science*, 1929, 242-3; fourth edition 1948, 223.

Faraday's *Experimental Researches in Electricity*, 1838, Vol. 1, consists of several memoirs: one on his newly discovered magneto-electric effect and his explanation of it partly in terms of fields, partly in terms of polarization in conductors; one memoir arguing that magnetization always consists of polarizing space, not the conductor; one memoir on the identity of all electricities; a few memoirs on the identity of electricity and chemical affinity as well as of electrolytic currents and ionic movements; and a few on the electrostatic field and on electric current as the collapse of the field, not the flow of a fluid. Three important ideas are implicit here. First, the identity of all forces of nature, as taught by Ørsted (see section 17). Second, there is no electricity but electric force, also as taught by Ørsted: that, indeed, all forces are not actions of a variety of matters but of a variety of polarizations, of propensities to act that reside in space whether space is materially occupied or empty. (This is Faraday's improvement on Ørsted's theory of the "conflict".) The culmination of Faraday's study, incidentally, is the attempt to find interactions between electrostatic and gravitational fields of force! It is not surprising that his last memoir on this topic was politely rejected (and seems to be lost). The third idea is that of conservation of force that he evidently developed (under Ørsted's influence; see note 59) when discovering that to create magnetism one must destroy electricity.

As early as 1832, Faraday had tried to show that equal quantities of electricity create equal quantities of magnetism (§366). This experiment — having been fundamentally mistaken — is never quoted: Faraday was unaware that he was using a ballistic galvanometer that provides only crudely approximate results. Yet his experiment was crucial in his career; Koestler might have called it an instance of "sleepwalking". Having shown, as he erroneously thought, that equal quantities of electricity produce equal quantities of magnetism (§366), he concluded (§377) that equal quantities of electricity decompose by electrolysis equal quantities of matter. He already thought of all phenomena in terms of spatial polarization forces (i.e., fields), and of their relations in terms of constant coefficients of conversion.

This interpretation is not based on my own reading of the text alone. In the beginning of his memoir on the electrochemical coefficients (§783), Faraday refers to this point (§377) as his starting point. And the sleepwalker's sureness and the purposefulness of his work is reflected in these memoirs in the most unbelievable way — as, for example, in his announcing his electrochemical laws (§504) and adding (§505): "I have this investigation in hand, with several others, and shall be prepared to give it in the next series but one of these *Researches*", thus referring in June 18, 1833, at the latest, to researches that in his *Diary* fill the period September 17-20, 1833 (the next series) and the period beginning September 21, 1833 (the next series but one)! Incidentally, a sideline of his electrochemical researches was an attempt to decompose all elements except hydrogen (§449, §451). The unification of all phenomena into a field concept is an idea present in all his memoirs from the third onwards. (See also notes 59 and 60 below.)

58. F. Nietzsche,. *The Case of Wagner*, section 7: *Complete Works*, ed. O. Levy, 1911, Vol. 8, 20. But I must confess that Dampier-Whetham's work is preferable to that of many of his successors, as the following example may illustrate. L. W. H. Hull explains in his *History and Philosophy of Science, op. cit.*, the *reductio ad absurdum* twice in succession (71-72), once confusing it with modus *tollens*, once with disjunctive inference. He also says (165) that Newton did not explain his concept of mass, although Maxwell did (meaning, incidentally, the idea of Thomson and Tait). "We must recognize degrees of elementariness" of substances, he says elsewhere (258), "one suitable for everyday chemistry, the other for the deeper purposes of physics. The issue [of chemistry?] was clouded in Boyle's mind by his inability to make this distinction. In thinking of an element as something absolutely elementary, he was in vague touch with a physical idea which went too deep to be of immediate use to chemistry." He adds, however, that a good chap named Dalton did have this idea. It so happens that the reverse is true of Boyle and Dalton; the evidence for this is not very inaccessible (see note 67). The marks appear to be given by the schoolmaster who does not bother to read his students' work.

Sarton, writing in *The Study of the History of Science*, 1936, 65, praised Dampier-Whetham's work as "the best single volume available today" on the whole history of science; and Sarton definitely thought that the history of all the sciences ought to be written. For the latter point see *Guide*, 51-55. Sarton even condemns those who did not follow this maxim: "Nothing illustrates better the backwardness of our studies," he writes, "than the fact that Whewell's book was still commanding the respect of many thoughtful readers at the beginning of this century" (60). He writes this immediately after admitting (49n) that he had never read Whewell's book. His reason for so strong a condemnation is based on his unexplained assertion that Whewell's work comprises not a history of all science but separate histories of separate sciences.

Sarton also admits bravely that he is unable to check his sources or even to understand them (57), yet he pleads for more historical detail (58&n). Similarly, Thorndike admits in one breath (*op. cit.*, Vol. 1, 3) that he probably tried to cover too much and that he made serious omissions.

I fully agree with L. Pearce Williams when he writes, in the concluding paragraph of his review of Sarton (see note 1): "I cannot help but wish that Sarton had cast his net less widely." A. C. Crombie evidently tried, but without much conviction, to be more charitable towards Sarton: "He did not work with the philosophical and analytical approach to the history of science such as is now, in the hands of younger scholars, throwing so much light on the development and character of scientific thinking"; *Brit. J. Phil. Sci.*, 10, 1959, 164. This, however, is only a gloss over the fact that Duhem started the "analytic approach" before Sarton's time. Crombie continues: "A hard critic might even say that Sarton's approach could easily have killed the study of the history of science, which breathes through ideas, by suffocating it beneath the mountain of uninterpreted and unrelated facts which he spent a lifetime collecting. But such a criticism would be too extreme. Now that what was bad in his influence no longer threatens, unhappily removed by his too-early death, it is possible to see that … he did work that is invaluable and was sometimes inspired …" The criticism is exaggerated, of course, but rather less than Crombie tries to suggest half-heartedly. Inspired as Sarton undoubtedly was, chiefly by the idea that science is a part of a living culture, his technique is taken as a model by too many historians.

I. B. Cohen had earlier excused Sarton and Thorndike on the ground that they were "self-taught", glossing over the fact that their work is so much inferior to that of the "self-taught" Duhem; *J. Hist. Ideas*, 15, 1954, 166.

Being apologetic towards past historians of science is as much a violation of the truth, and as unrewarding, as being apologetic about past scientists; since Cohen outspokenly opposes the latter, I do not see why he should adhere to the former. For my part, I sincerely tried to express my appreciation of Sarton in note 20 above. I invite anyone interested to try to criticize it, and if possible also to improve upon it.

59. E. T. Whittaker, *History of the Theories of Aether and Electricity, The Classical Theories*, revised edition, 1951, 178, presents continuity between Davy's theory of electrochemistry and Faraday's. That some continuity exists already Faraday emphasized; but Faraday also indicated discontinuity. Moreover, there is no more continuity between Davy and Faraday than between Berzelius and Faraday. And, as Whittaker notices (79), Berzelius' view was "afterwards overthrown by Faraday." (Whittaker's comment on Berzelius, and the apologetic attitude towards him, are discussed in note 184 below.) He does not see the clash between Berzelius' and Ørsted's views, nor the similarity between Berzelius' and Davy's, nor between Ørsted's and Faraday's. He cannot see all the existing continuities in the history of science because — relying too much on the continuity theory (discussed in section 9 and 10) — he ignores discontinuity. Briefly, both Berzelius and Davy assumed the existence of small interaction between electric and gross matter, whereas Ørsted (see section 17 below) and Faraday (see note 57 above) viewed both chemical and electric forces as manifestations of one and the same primordial force and hence inter-transformable. (From this Faraday arrived at the law of conservation of force.) But these speculations are metaphysical and the inductivist Whittaker preferred, on the whole, to ignore metaphysics as much as possible. He even quotes (176n) Helmholtz's Faraday Lecture of 1881 (132) approvingly, and says of Faraday that his principal aim was "to express in his new conceptions only facts, with the least possible use of hypothetical substances and forces. This was really a progress in general scientific method, destined to purify science from the last remains of metaphysics." The comment on this remark of Helmholtz should perhaps be "positivism is the last refuge of the dogmatist". Personally, my comment is this: a few years earlier, in his preface to the German edition of Tyndall's *Faraday as a Discoverer*, Helmholtz pooh-poohed Faraday's speculations (the translation of this preface is in *Nature*, 2, 1870, 51), viewing them as a "disadvantage" excusable in view of Faraday's "want of mathematical culture"; he also blamed him for having misunderstood the law of conservation of force. Later, in his Faraday lecture, Helmholtz changes his tune, and also re-labels his "On the Conservation of Force" as "On the Conservation of Energy" (as does Whittaker, *op. cit*, 183n). In his Faraday lecture, Helmholtz obliquely claims priority over Faraday of having advocated the law of conservation of energy (134): "The first motive which guided him seems to have been an instinctive foreboding of the law of conservation of energy, which many attentive observers of nature had entertained before it was brought by Joule to precise scientific definition." By contrast, like Mayer, Grove, and Joule, Helmholtz followed Faraday in advocating the law of conservation of force, not of energy.

60. Faraday's *Diary* (§402) shows that he assumed the existence of electrostatic fields at least as early as March 26,1832, though he did not publish his idea until 1838. It also shows that he was deeply impressed with Porret's effect, which he explained as an electrostatic-field effect on September 18, 1832, June 8, 1833, August 26, 1833, and November 4, 1833; he did not publish about it, however, until 1838 (*Exp. Res.*,

§1646). But few are interested in this effect since Faraday mistook osmosis for electrostatics here. Moreover, his explanation of the electrochemical current as transport due to electrostatic polarization (taken together with the following remark of December 2, 1833) show how far his thoughts had already developed. Speaking of currents in electrolytes, conductors, and air (sparks), he says to himself "Are not these all one?" (Cf. Schönbein to Faraday, *Correspondence*, 1899, 46.) His first map of the electrostatic field's potential is to be found in his *Diary* , December 8, 1835; his experiments with dielectricity began on January 15, 1836. But by March 8, 1832 he already entered the following startling items in his *Diary*: "393. Time for travelling of magnetic impulse. 394. Time for travelling of electric induction. 395. Query application of theory of vibration" as early in life he adopted the idea of the *Britannica* that electricity is vibrations.

There is no need to consult his *Diary* to see the aim of his study of dielectricity, since in 1837 he became fairly explicit — although it was not until 1844 that he first dared to speak of vibrations in public. In his published memoir of 1837 (§1166), he writes: "I searched for an unexceptionable test of my view … in accordance of … facts … which would not be consistent with the theory of action at a distance …" (§1168): "Another ever-present question in my mind … It was in attempts to prove the existence of electricity separate from matter … and the utter failure of all such attempts. … that first drove me to look upon [electrostatic] induction as an action of the particles of matter, each having both forces … It is this circumstance, in connection with others, which makes me desirous of placing the remarks … that electric induction is an action of the … dielectric." (See also §1253 for reference to Coulomb's denial of the possibility of dielectricity.)

Incidentally, H. Hartley "Michael Faraday on Electrolytic Conduction", *Reports Brit. Ass*., 1931, 34, already states generally that "Faraday always had a preconceived idea behind his experiments, and never were advances made with such economy of effort". By this he means, I take it, that as much design as possible was put by Faraday into each experiment of his.

As to Faraday's theory of dielectricity, it is the assumption that only bipolarity is possible, erroneously denying the possibility of unipolarity. He looked hard for asymmetric electric effects, and failed to find clear-cut evidence of their existence, although he rightly suspected that lightning constituted such an effect; *Diary*, September 7, 1832. His penultimate researches were on the cathode tube, but his loss of memory stopped him: perhaps the refutation of his central doctrine was too much for him at that stage.

The inductive style of presentation in accordance with which one records facts faithfully and omits opinions, or perhaps mentions them briefly in the final paragraphs of one's report — was suggested by Bacon and Boyle; and it was instituted by Brouncker, Boyle, and Hooke, who incorporated it in the regulations of the Royal Society of London. (I have discussed these matters at length in my dissertation, cited above.) Faraday's breakaway from the inductive style was a fascinating process that I cannot discuss here. About the present point, however, that concerns the great break with the inductive style that he made in his memoir on dielectricity of November 1837, let me say the following. Faraday tended to be rather outspoken, but he was not always clear. His researches in electrolysis, for instance, begin with a statement (§381) of his reason for studying the conductivity of ice. But I, at least, was unable — despite considerable effort — to understand this until I consulted his *Diary* (Sept. 10, 1832, October 26, 1832, §§169-175, and January 23, 1833, §225). By 1837 he was clearer. And although his opinions on currents began to crystallize in 1832 and indeed gave rise to his study of electrochemistry and electrostatics, he did not express them until 1838. His idea that electric fluids do not exist — which accorded with Ørsted's view, although on Boscovitchian rather than Kantian grounds (see section 17 below) — he hinted at only in 1839. And although his *Diary* shows that he had it long before, he did not publish it explicitly until the 1840's.

The following story appears to be relevant here. Sir Humphry Davy had stated that "there are no fluids known", except for water, that can act as a solvent allowing for the electrochemical decomposition of their solutes. Taken literally, this is undoubtedly true. Faraday, however, who understood this to mean (§473) that Davy assumed that no other such solvent exists, criticized that assumption by discovering just such solvents. When Dr. John Davy took up the cudgels with him in defense of his deceased brother Sir Humphry, Faraday answered: "Why there can be no doubt that it I had proved that water was the only substance that could perform these duties [of a solvent for electrolysis], Dr. Davy would have claimed the discovery for his brother" (*op. cit.*, Vol. 2, 1844, 215.). Thus Faraday rebelled against the inductive technique of hinting at a hypothesis so as to be able to claim priority if it were later corroborated, and to deny having stated it if it were later refuted. Undoubtedly, this mode of behavior is rather cowardly, yet the prohibition on error makes it imperative.

Faraday's reply to Davy's brother is dated 1835; his earlier memoirs on electrolysis are still notoriously cryptic (as is well known, he hints at the atomicity of electricity); his later memoir is on dielectricity, where he is much more outspoken and clear. However, he discovered dielectricity on January 15, 1836 (*Diary*, §1831), he explained it there and then (§1832, §§2878-9), and, most uncharacteristically, he went on working and producing results on the same problems from November 3, 1835 onwards without submitting any result to the public until November 1837, after almost two years of reticence. Let me mention also that although his memoir of 1837 is explicit, it does not yet contain his general thoughts on electrostatic fields. Admittedly, he had spoken earlier of electrostatic fields in electrolytes, and had even created a most beautiful analogue to the iron-filing indicator of magnetostatic fields by suspending silk particles in the electrolyte to exhibit electrostatic curves (§1350). Admittedly, he does speak of the dielectric as a medium; but he does not yet express his view of the electrostatic-medium, namely, the electric lines of force are present even after the removal of the electrolyte or the dielectric. This he had already found in December 1835 when mapping the field of force in the vicinity of a Leyden jar.

W. Oswald noticed — *Grosse Männer*, Leipzig, 1909, 117-118 — the two-year gap in Faraday's publications, and explained it by a conjecture that, it turns out, is refuted by Faraday's *Diary*. (Oswald brings evidence that Faraday went to Switzerland in that period and hence probably was already ill; but L. Pearce Williams assures me that there is an error here about the alleged trip.) I cannot help thinking that Faraday was in an intense moral conflict that, after a considerable time, he was able to resolve by explicitly refuting Coulomb — contrary to the inductive tradition. (As L. Pearce Williams has noticed, Ampère viewed Ørsted's discovery as a refutation of Coulomb's view, but said so only in a private letter.) I further think (here in disagreement with those who have worked on this subject, including Pearce Williams) that Faraday's severe loss of memory and general nervous breakdown of 1840 is connected with a stronger conflict concerning the publication of wild speculations: his last fling of high inductive style was in March 1843, as two short letters to *Phil. Mag*. on electrostatics. He was not understood, and his next publication was nearly a whole year later, in January 1844, as another letter to *Phil. Mag*. that for the first time presents wild speculations.

61. Whittaker, *op. cit.*, 185; Dampier-Whetham, *loc. cit.*; H. T. Pledge, *Science Since 1500*, 1939, 140; S. F. Mason, *A History of Science*, 1953, 389.

62. Dingle stresses, *The Scientific Adventure*, 1953, 33-34, that objections made against calorism in the times of Rumford and later by writers of history of science textbooks are based not on historical research but on preconceived notions: that literature

> "represents not what actually occurred but what the writers of modern textbooks, unaware of the general state of knowledge of the time, consider ought to have occurred. The caloric theory remained dominant for another fifty years, and not because of the reactionary tendencies of the bigoted old fossils who ought to have given place to the bright young sparks with the new ideas, but because, on the whole, it was still the more rational theory to hold ... Indeed, a quarter of a century later the caloric theory was very active in the minds of men not conspicuously stupid or uninformed, who made classic and still vital contributions to the science of heat — men like the ageing Fourier, who did not discard it, and the young Carnot, who specifically maintained it."

63. D. McKie, *Lavoisier*, 1952, 215-18. McKie knew of the errors in the table: he reproduced it from the first edition of Kerr's translation, and he must have seen later editions that criticize the inclusion of lime, magnesia, and baryta. (See for example the edition of 1806, note on p. 239). It is hardly possible that he did not know that Lavoisier deemed hydrochloric acid a compound of the element murium plus oxygen. Incidentally, the prediction that came true — namely, that the alkaline earths are not elements — was what made its discoverer Davy suspect that Lavoisier's theory was false.

The quotation from Henry M. Leicester may be found in his *The Historical Background of Chemistry*, 1956, 146, that has been favorably reviewed in *Isis*, 49, 1958, 88-9. Partington too (*op. cit.*, 185-6, and note 36 above) whitewashes Lavoisier — in his review of McKie's *Lavoisier* in *Annals of Science*, 8, 1952, 401, after praising McKie for his accuracy, Partington gives this explanation of why McKie mentions Lavoisier's erroneous calorism: "it is impossible to understand or appreciate his views on combustion, which were in some respect erroneous, without taking his caloric theory into account." Guerlac considers McKie's chapters on chemistry in his Lavoisier "in the main so reliable". The "in the main" leaves room for some "minor criticisms", *Isis*, 45, 1954, 58-9, none of which is related to those presented here.

Another example of the effect that transcribing has is Partington's *A Short History of Chemistry*. In this a gentle censure of Lavoisier's experimental inaccuracy (written in 1848) is quoted: "Lavoisier, though a great architect in the sciences, laboured little in the quarry." Notice the suggestion here that errors are the result of lack of work. In the latest edition, 1960, 122, the quotation is prefixed by the author's observation that Lavoisier was a minor experimenter but a great theoretician.

Partington's claim is, however, simply ludicrous. Black wrote to Lavoisier in 1791, "The numerous experiments which you have made on large scale, and which you have so well devised, have been pursued with so much care and with such scrupulous attention to details, that nothing can be more satisfactory than the proofs you have obtained." Of course the picture is not as white as Black's picture of it, but this is not enough to conclude that it is as black as Partington's.

Hume's celebrated remark (see note 51 above) that Bacon had pointed the way, whereas Galileo had both pointed to and trodden upon it, provides yet another example. This remark led to the view that Galileo followed Bacon, for example, J. C. Gregory, *Combustion*, 1934, 70.

Another example, this time of the process in the making, is G. H. von Wright's assertion, *Acta Philosophica Fennica*, 3, 1941, that as "a physical theory of light rays" Euclid's geometry "is supposed to have been falsified by the physical discoveries which led to Einstein's theory of relativity". The source of this error may be Sir Harold Jeffreys inductivist remark (*The Theory Of Probability* 1939, 186, second edition, 1961, 346) that in principle the eclipse observation could have been made prior to the announcement of Einstein's theory. (G. H. von Wright's idea is not adopted by any historian of science: the facts are too well known for that.)

64. *Op. cit.*, 179; 172. There are many examples of dismissing contrary opinion without mentioning it. One of the boldest is J. G. Crowther's pronouncement, *The Social Relations of Science*, 1942, 349, that Bacon was a good experimenter. He supports this with several silly examples, making no mention of Ellis' and Liebig's profound and scholarly studies that illustrate quite different views.

65. Thomas Thomson, *History of Chemistry*, 1830, Vol. 2, 292:

> "Let not the reader suppose that this was an easy task. Chemistry at that time did not possess a single analysis which could be considered as even approaching to accuracy. A vast number of facts had been ascertained, and a fine foundation laid for future investigation; but nothing, as far as weight and measure were concerned, deserving the least confidence, existed. We need not be surprised, then, that Mr. Dalton's first numbers were not exact. It required infinite sagacity, and not little labour, to come so near the truth as he did. How could accurate analyses of gases be made when there was not a single gas whose specific gravity was known, with even an approach to accuracy; the preceding investigations of Dalton himself paved the way for accuracy in this indispensable department; but still accurate results had not yet been obtained."

See also *op. cit.*, 295.

66. I have no evidence that the *Britannica* author took such a liberty. He may have carefully transcribed from Dumas, who probably did take that liberty as he said (*op. cit.*, 3): "Dalton was a crude experimenter."

67. *Proc. R. S. Edin.*, 63, 1950, 1. Dalton thought that all atomic weights are whole numbers (Thomson, *op. cit.*, Vol. 2, 295), but he rejected Prout's hypothesis that Kendall eulogizes, asserting that atoms are genuinely indivisible. See also Roscoe and Harden, *op. cit.*, 2, and Roscoe, *op. cit.*, 145.

68. My explanation in terms of inductivism of the fact that historians of science either transcribe or ignore evidence is not universal. In some cases the explanation is rooted in the fact that the historian of science is inept, as may be illustrated by reference to biographical material. Yet even there inductivism stands in the way of able biographers of researchers. No serious biographer can accept in its literal sense Boyle's claim ("Proëmial Essay" to *Certain Physiological Essays*, 1661), that he had not read sufficiently carefully the works of Gassendi, Descartes, and Bacon. After all, we have his letter of 1674 (Works, First edition, Vol. 1, xli), that shows that he had read "especially Gassendus, a great favourite of mine," and Dr. Petty's evidence of 1653, (*ibid.*, vol. 5, 297b), that against doctors' advice (he had weak eyes) he used to "read twelve hours per diem or more"; and he exhibits deep knowledge of the three authors in nearly all his works and in many of his letters. Yet his best biographers, Thomas Birch (Boyle, *ibid.*, vol. 1, xxxv) and L. T. More (*Robert Boyle*, 1949, 235), have taken it literally. (See also Cajori, *op. cit.*, first edition, 70; second edition, 78.) And then the information becomes common knowledge and then even R. F. Jones has repeated this silly remark (*Ancients and Moderns*, Washington University, 1936, 328). Marie Boas notes, *Robert Boyle and 17th Century Chemistry*, 1958, 27, that Boyle's admission that he had not read Bacon, Gassendi and Descartes carefully enough should not be taken at face value, adding that it was "deliberate (and Baconian) propaganda". This passage suggests that Boas did not read sufficiently carefully the

whole essay, or even the whole paragraph she was quoting. For it is propaganda for reading Gassendi and Descartes, not against reading them; and hence it is anti-Baconian. Boyle explains why he "regrets" not having read them carefully enough. As to Bacon, since Bacon had no system, and since in that passage Boyle speaks of systems, he appears to add his name ironically as an afterthought. What he implies is something like this: Bacon advises one not to read lest one become prejudiced; well, then, follow his advice and do not read his own work too much; do not be too influenced by what Bacon himself says! Indeed, although Bacon opposed the use of systems and of hypotheses, Boyle in this very essay explains at length why and on what conditions he thinks both systems and hypotheses are useful. Yet it is widely, if not universally, considered that Boyle's chief merit lies in his propagation of the pure milk of Baconianism. (See, e.g., *D. N. B.*, Art. Boyle.)

Further evidence for Boyle's early familiarity with atomism may be found in R. S. Westfall's "Unpublished Boyle Papers Relating to Scientific Method", *Annals of Science*, 12, 1956, especially page 111. These, as Westfall notes, refute his claim "that he had refrained from studying the atomic or mechanical philosophy in order not to prejudice his mind" (64). For a more detailed discussion of all this, and for Boyle's reasons for not attacking Bacon openly, see my Ph. D. dissertation, *op. cit.*

Quite a few errors persist in the literature of history of science simply because of lack of scholarship in the field, and thus lack even the excuse that they persist because they conform to a given philosophy. Chronological errors provide good examples: Paul Fleury Mottelay had already noted (*Bibliographical History of Electricity & Magnetism*, 1922, 453) a chronological error concerning Ørsted; and it is still being repeated, as Stauffer observes (see reference, note 26 above). On the other hand, the persistent habit of smearing the character of Dr. Henry Stubbe because he opposed the Royal Society of London (see note 93 below) — as well as the effort to conceal his personal friendship with Boyle and Boyle's appreciation of his sincerity behind a just criticism that Boyle once launched against him — and of ignoring Isaac Disraeli's defense of his character ("Calamities and Quarrels in the Royal Society"), is rooted in the inductivist division of writers into the pro science and the anti science. Even a scholar like Harcourt Brown still calls him "a hired pen", thus dismissing a modern effort to say something in his favor. This effort finally succeeded, though while playing down his attitude to science and discussing more other aspects of his public life. The most militant here is James R. Jacob, *Henry Stubbe, Radical Protestants and the Early Enlightenment*, 1983. Steven Shapin says of it (*Isis*, 75, 1984, 421-2, "Jacob aims is to display Stubbe's *oeuvres* as intelligible, important, and, above all, consistent. To accomplish this, Jacob confronts and rejects the standard historical view (that of Herschel Baker and R. F. Jones) that depicted Stubbe as an obscurantist reactionary in natural philosophy and as a chopper and changer in religion and politics." To that end Jacob presents Stubbe as a crypto-radical. This is does not tally with his critique of the Royal Society mainly as radical (see note 93 below). The most balanced image of the relations between Boyle and Stubbe is found in Nicholas H. Steneck, "Greatrakes the Stroker: The Interpretations of Historians", *Isis*, 73, 1982, 160-177, esp. 165-7.

69. An exception is the critical discussion in S. E. Toulmin and J. Goodfield, *The Fabric of the Heavens*, 1961, 232, of the story of Newton's apple: they try to modify the myth in order to make sense of it. Though their explanation fails, it deserves appreciation. "One must not think of the apple falling vertically," they say, "as though nobody knew that heavy bodies gravitate." Instead, they say, the apple must have fallen sideways, being pushed by the wind — as if nobody knew that heavy bodies may fall sideways. But although Toulmin and Goodfield have not advanced the matter, they

have at least noticed the problem; moreover, their approach to it is sound. Whether or not Newton conceived his idea that the moon is accelerated by the earth while sitting under an apple tree is irrelevant; the emphasis of the story is on the point that it did come from Baconian stock. Newton had uneasy Baconian feelings about not having contributed to our stock of factual knowledge about the fabric of the heavens: his having based his views on his own factual findings would sound more inductive than his having had better insight into factual findings known to all.

As Norman Campbell says, *What is Science?* 1952, 102, "Newton's … theory of universal gravitation, suggested to him by the trivial fall of an apple, was a product of his individual mind, just as the Fifth Symphony (said to have been suggested by another trivial incident, the knocking at a door) was a product of Beethoven's. The analogy seems to me exact." True — except that historians of science are more addicted to accidents than historians of music are.

70. Bernard Shaw, *Back to Methuselah*, Preface, "A Lesson from Science to the Churches".

71. *Treatise*, Vol. 2, §528:

> "The method of Ampère … though cast in inductive form, does not allow us to trace the formation of the ideas which guided it. We can scarcely believe that Ampère really discovered the law of action by means of the experiments which he describes. We are led to suspect, what, indeed, he tells us himself, that he discovered the law by some process which he has not shown us, and that when he had afterwards built up a perfect demonstration he removed all traces of the scaffolding by which he had raised it.
>
> "Faraday, on the other hand, shews us his unsuccessful as well as his successful experiments, and his crude ideas as well as his developed ones, and the reader, however inferior to him in inductive power, feels sympathy even more than admiration and is tempted to believe that, if he had the opportunity, he too would be a discoverer. Every student should read Ampère's research as a splendid example of scientific style in the statement of a discovery, but he should also study Faraday for the cultivation of a scientific spirit, by means of action and reactions which will take place between the newly discovered facts as introduced to him by Faraday and the nascent ideas in his own mind,"

that is, trial and error. Much as he desired, Maxwell could not reverse the tide; he could not follow his own advice. As Lorentz has stated, *Collected Papers*, 7, 356, "it is not always easy to comprehend Maxwell's ideas. One feels a lack of [inductivist] unity in his book due to the fact that it records faithfully his gradual transition from old to new ideas"; quoted by A. D. Fokker in *H. A. Lorentz, Impressions of his Life and Work*, ed., G. L. de Haas-Lorentz, Amsterdam, 1957, 55. As to Lorentz' own style, P. Ehrenfest comments about it as follows (*ibid.*, 155): "Read Lorentz' writings, read his text books. A picture of a workshop arises before our eyes … And behold the master at work …" As this passage is from a funeral oration, it cannot be as analytic as the two previously quoted. Nonetheless, it shows powerfully the final triumph of the Faraday style. (For the beginning of Faraday's style see note 60 above.)

72. F. Arago, "Eulogy on Ampère", *Reports of the Smithsonian Institute*, 1872, 141.

73. One such quotation from Ampère is this.

> "When M. Oersted discovered the action which a current exercises on a magnet, one might certainly have suspected the existence of a mutual action between

> two circuits carrying currents; but this was not a necessary consequence; for a bar of soft iron also acts on a magnetized needle, although there is no mutual action between two bars of soft iron."

74. A. Einstein, *Out of My Late Years*, 1950, 75:

> "this special kind of matter, however, appeared to be lacking in the fundamental property of inertia; and the forces acting between these masses and ponderable matter remained obscure."

75. See Whittaker, *op. cit.*, 59 ff. and references there.
76. See Dr. Kirstine Meyer's edition of Ørsted's *Skrifter*, Copenhagen, 1920, Vol. 1, xvii-xviii, and her "Ørsted and Faraday," *Nature*, 128, 1931, 338:

> "In his youth, Ørsted ... accepted too uncritically theories which rested only on speculation and had no sufficient experimental verification."

Ørsted's speculations and his critical attitude will be discussed in section 17 below.

77. Whittaker, *op. cit.*, 83 note, sums up Ampère's view thus:

> "Ampère introduced his work by proclaiming himself a follower of that school which explained all physical phenomena in terms of equal and oppositely directed forces between pairs of particles and he renounced the attempt to seek more speculative, though possibly more fundamental explanations in the terms of motions of ultimate fluids and aethers. Nevertheless he indicated two conceptions of this latter character, on which such explanations might be founded."

78. W. Grove, *The Correlation of Physical Forces*, Introductory Remarks:

> "The relations of electricity and magnetism afford us a very instructive example of the belief in secondary causation. Subsequent to the discovery by Oersted of electro-magnetism. and prior to that by Faraday of magneto-electricity, electricity and magnetism were believed by the highest authorities to stand in the relation of cause and effect – i.e., electricity was regarded as the cause, and magnetism as the effect; and where magnets existed without any apparent electrical currents to cause their magnetism, hypothetical currents were supposed, for the purpose of carrying out the causative view; but magnetism may now be said with equal truth to be the cause of electricity, and electrical currents may be referred to hypothetical magnetic lines: if therefore electricity cause magnetism, and magnetism causes electricity, why then electricity causes electricity, which becomes, so to speak a *reductio ad absurdum* of the doctrine."

Attempts to reduce magnetism to electricity were continued, however, not only in the school of Ampère from Weber to Ritz and Duhem, but also in the Faraday school by J. J. Thomson (Whittaker, *op. cit.*, 316, and references there).

79. W. Weber, "On the Measurement of Electro-dynamic Forces", Poggendorff's *Annalen*, 1848, 73, 193, and R. Taylor's *Scientific* Memoirs, 1852, Vol. 5, 489:

> "A quarter of a century has elapsed since Ampère laid the foundation of electro-dynamics, a science which was to bring the laws of magnetism and electro-magnetism into their true connexion and refer them to a fundamental principle, as has been effected with Kepler's laws by Newton's theory of gravitation. But if we compare the further development which electro-dynamics have received with that of Newton's theory of gravitation, we find a great difference in the fertility of these two fundamental principles. Newton's theory of gravitation has become the source of innumerable new researches in

astronomy, by the splendid results of which all doubt and obscurity regarding the final principle of this science have been removed. Ampère's electrodynamics have not led to any such result; it may rather be considered, that all the advances which have since been really made have been obtained independently of Ampère's theory, — as for instance the discovery of induction and its laws by Faraday. If the fundamental principle of electro-dynamics, like the law of gravitation, be a true law of nature, we might suppose that it would have proved serviceable as a guide to the discovery and investigation of the different classes of natural phenomena which are dependent upon or are connected with it; but if this principle is not a law of nature, we should expect that, considering its great interest and the manifold activity which during the space of the last twenty-five years that peculiar branch of natural philosophy has experienced, it would have long since been disproved. The reason why neither the one nor the other has been effected, depends upon the fact, that in the development of electro-dynamics no such combination of observation with theory has occurred as in that of the general theory of gravitation. Ampère, who was rather a theorist than an experimenter, very ingeniously applied the most trivial experimental results to his system, and refined this to such an extent, that the crude observations immediately concerned no longer appeared to have any direct relation to it. Electro-dynamics, whether for their more secure foundation and extension, or for their refutation, require a more perfect method of observing; and in the comparison of theory with experiment, demand that we should be able accurately to examine the more special points in question, so as to provide a proper organ for what might be termed the spirit of theory in the observations, without the development of which no unfolding of its powers is possible."

I have quoted Weber's preface at length to show that he had no intention of being disrespectful to Ampère: as long as Newton's theory was considered highly testable yet — because it is true — beyond refutation, and as long as the origin of that truth was viewed not as revelation but as the result of applying the proper method, refutations had to be viewed as evidence of non-scientific character of a theory, and of its originators' lack of discipline. Even to suspect falsehood in a theory is to suggest lack of scientific discipline on the part of its author. Thus, before Ampère's theory was refuted, W. Sturgeon — who rejected it — viewed it as "an hypothesis which he derives neither from fact nor from analogy", (*Scientific Researches,* Bury, 1850, 29).

August De la Rive, however, saw the situation quite differently (*Treatise on Electricity*, 1853). Having endorsed Ampère's views (Vol. 1, 239, 251), he claimed (Vol. 2, 2) that Weber had merely filled gaps in Ampère's theory, thus providing it with a "degree of probability which approaches almost certainty"! He answered Weber's criticism (without mentioning him) in his "*Notice sur M. Faraday*" (*Phil. Mag*., 34, 1867, 424). Maxwell, on the other hand, viewed Ampère's theory as an approximation and a special case, as correct for stationary currents (*Scientific Papers*, 1890, Vol. 1, 193, 195). However, in his "Action at a Distance" (*Nature*, 7, 1873, 341, and *op. cit.*, Vol. 2, 317-18), he definitely opposes Ampère and his continental followers.

80. The obviousness of the fact that motion (or, as we would say, frictional motion) causes heat has merely led historians to find forerunners to those thinkers — Bacon, Boyle, Black, Rumford — who said that heat is motion. This sounds rather strange to me: if it is admitted that Rumford neither originated the theory (heat is motion) nor discovered the fact (motion causes heat), what then was his contribution? One might suspect that it was the argument that heat is motion since motion causes heat; but this argument is neither new, nor valid, nor due to Rumford. Rumford's argument is this:

since motion can cause unlimited quantities of heat, heat is not matter. This is a better argument, but it too is neither new nor very strong. Still Rumford's contribution was, I suppose, in drawing it to the foreground and making some examine it and others improve upon it. But on the inductivist code only discoveries of facts and theories are worthy of reward (and only in extreme cases does pro-scientific propaganda deserve such reward), not the engendering of fruitful debates.

The present holder of the title of forerunner to Bacon and Rumford is Levi ben Abram, a thirteenth century figure, who said, according to the accepted translation, that "motion produces heat." (See Rose S. Marx, "A 13th Century Theory of Heat as a Form of Motion," *Isis*, 22, 1934, 19-20; see also *Isis*, 24, 1935-6, 202, about the ascription of the theory to Lucretius. As I have said, this has nothing to do with the motion versus matter controversy and is ancient knowledge. Actually, Levi ben Abram does not even say "motion produces heat" — as the accepted translation would have it — but "motion produces heat and awakens it," i.e., activates it; this is of course an Aristotelian idea.

Levi tries to explain how the sun can produce heat if it has no (earthly) qualities. This problem he solves by the argument that just as the lead arrowhead sometimes melts (when it is stopped) even so do light rays when they are reversed (i.e. stopped or reflected) — indeed, more so, since they are swifter. In other words, he claims that the heat produced is a function of the change of velocity. This, incidentally, is only one step from some sort of law of inertia. I think that Levi was much too clever to be recognized as the mere recorder of the obvious fact that heat produces motion.

81. The experiment has been pronounced a fake by E. N. da C. Andrade (*Nature*, 85, 1935, 359). For, he would argue, either friction will be decreased almost to zero by water between the two pieces of ice, or else the two pieces will stick together. Efforts to increase friction by pressure, however, will only lower the melting temperature. Andrade says that he, for one, would not care to repeat the experiment. Dingle (*loc. cit.*) concludes that Davy's experiment is "mythical", and even D. Roller — who claims that Davy refuted calorism — agrees that the experiment is wrong (*Harvard Case Histories*, I, 194-5).

If the experiment was indeed a fake one must explain, on the one hand, how Davy came to publish it and, on the other, how it escaped criticism. The first question is answered by Andrade by the view that at that time Davy was an inexperienced country lad — an unacceptable hypothesis in view of Davy's standing at the time. The second problem may be answered by the claim that the reticence was due to politeness. This theory is refutable by the publication of Hare's criticisms of Davy (see note 30 above), and by a story to be found in *Britannica* (Seventh edition, 1842), 7, 637, in the article on Davy. This story, incidentally, is suppressed or distorted beyond recognition by Davy's biographers: the version nearest to that of the *Britannica* is to be found in J. Davy's *Fragmentary Remains*. When, the story goes, Davy's cure for corrosion of the metal sheets of boats was proved to be worse than the illness — since his electrification caused the adhesion of sea weeds and the like to the boats — public ridicule and "unjust sarcasm" caused Davy "severe mortification", made him leave the country, and hastened his death.

For my part, I should say two things about Davy's experiment. First, it is possible that sand, or iron filings, between the two pieces of ice might lead to complete success; I do not know. Second, Davy's experiment does not need repeating since it is a very convincing thought-experiment. Once it is pointed out, no one would doubt that the heat created by friction is the same in air as in *vacuo*, that the caloric cannot be transferred from the neighboring atmosphere but from the source of motion, since it is caused by the motion. It is no accident that Davy's argument was answered by a thought-experiment; namely, Carnot's cycle, that Carnot developed on these very lines.

82. See Joule's comment on Davy's experiment, *Phil. Trans*., 140, 1850, 61; quoted by Roller in *Harvard Case Histories*, Vol. 1, 190. Joule's careful restatement of Davy's claim has puzzled Roller (*loc. cit*.) since it contains no quotation from Davy's original paper but only from a later work. I suppose the simple resolution of this puzzle is that Joule knew that Davy had not proven his case and had to find and quote instead a statement of a smaller claim on Davy's part. Roller views Rumford's and Davy's experiments as "the decline of the caloric theory" (*op. cit*., Vol. 1, 117-120); and its overthrow in the mid-century "the final downfall". Carnot and his colleagues, it would seem, belong between the decline and the downfall!

83. *Op. cit*., first edition, 468; fourth edition, 476.

84. Rather strangely, Whittaker, *A History of Theories of Aether and Electricity, The Classical Theories*, revised edition, 1951, is even more inductivist than the first edition, 1910. In the second edition he at the same time refers to Newton's theory as "the correct law of gravitation" (28), and censures Descartes' *a priorism*: "in putting forward an all embracing theory of the universe before he had studied any of its processes in detail, Descartes was continuing the tradition of the ancient Greeks, rather than treading in the new paths struck out by Tycho, Kepler, and Galileo: he never really grasped the principle that true knowledge can only be acquired piecemeal, by the patient interrogation of nature" (8). I need not say that this is Baconian mythology; what is surprising is that Whittaker should speak this way in 1951, three years after his The Modern Approach to Descartes' Problem (Spencer Lecture, 1948), and two years after his publication of *From Euclid to Eddington*, 1949, in which he refers to some detailed studies of Descartes and in which he is quite respectful towards him.

85. *Phil. Trans*., III (1821), 47; see also note 32 above.

    The inability on the part of inductivists to see merit in controversy and in criticized views has been criticized by Popper in "Back to the Pre-Socratics," (see note 48). Bacon, on the other hand, writes: "The very period itself in which inquiries concerning nature flourished, was by controversies and the ambitious display of new opinions corrupted and made useless," (*Novum Organum*, Book I, Aph. 79). It is this "ambitious display of new opinions" which characterizes Greek thought for both Bacon and Popper, although their judgments of this characteristic differ. (There is also, it should be noted, a difference of opinion concerning Thales between the two: Bacon considers him the last inductivist whereas Popper regards him as the founder of the critical tradition.)

86. At one time J. D. Bernal could not accept this. In accordance with Marx's economism he therefore reversed Marx's economic hierarchy to fit the reverse scientific hierarchy: the Middle Ages, he claimed, were inferior to Greek society both economically and scientifically; the break from medievalism, both economically and intellectually, came to pass in fifteenth-century Italy. See his *The Social Function of Science* (1939), end of Chapter II, section 1, "The Middle Ages." Whether he thought later that placing the Middle Ages below Antiquity would not do, or because he tried to be a more orthodox Marxist, or for some other reason, Bernal later, in 1954, changed his view. Then, assuming that the Middle Ages were economically superior to Antiquity, he had no option but to argue that they were intellectually superior as well. "Slowly but irresistibly", he writes of the Dark Ages, "a new civilization, which was soon to

surpass its forerunners, arose on a solid basis of abundant, fertile, and well-worked land." See his *Science in History*, 1954, 209.

It is no use asking Bernal for evidence; he might reply that we have no evidence to the contrary. We call the period the Dark Ages, he says (178), "as if, because little is known of what happened in a very partially civilized western Europe, a great darkness covered the whole earth." Yet little as we do know, we know that all mathematical and physical science between the ages of Archimedes and of Galileo, anywhere on earth, was inferior to that of both. If Bernal is right in claiming that the Middle Ages, not to say the Dark Ages, are technologically superior to antiquity, then he must give up the Marxist identification of science and technology.

"The last thing I want to suggest", to use Herbert Dingle's words (*op. cit.*, 6), "is that the view in question is peculiar to Professor Bernal. It is unfortunately only too common, but not every writer expresses it so clearly and succinctly." I would like, however, to quote an author who does not represent himself as a Marxist, S. F. Mason, *op. cit.*, section 10:

> "The ‚dark ages, ... are traditionally regarded as a somewhat [!] barren period … Such was the case in the field of natural philosophy, but during those centuries appeared a number of fundamental [!] technical innovations which provided the basis for a way of life materially superior to that of classical antiquity for the majority of men ... The Middle Ages saw not only development of new techniques, but also considerable refinement of skills ... Finally with the artist-engineer of the Renaissance we get assimilation of the learning of the scholars by the more skilled elements of the craft tradition … they did develop the empirical side of scientific method … Leonardo da Vinci had an even fuller appreciation of the empirical side of scientific method... It was some time before the men of the scholarly tradition developed the experimental side of scientific method and arrived at a similar conception of the place of empirical procedures in science."

However, he concludes, their conception was more mathematical.

Years ago, in a lecture delivered to the British Society for the Philosophy of Science, London, Léon Rosenfeld described the rise of Renaissance science out of the (Archimedean) tradition of late mediaeval skilled artisans. In the ensuing discussion I expressed my surprise at the fact that during the whole lecture about the rise of Renaissance science Copernicus was mentioned only once, and in passing. To this the speaker replied that Copernicus' significance lies in his claim that the universe is larger than previously assumed, thus signifying the new horizons of the coming capitalist era. This was not meant in jest.

Yet one should point out that Bernal and Rosenfeld are overt Marxists. As Dingle says, however, Marxist themes, such as identification of science with technology and the scientific drive with the economic drive, are all too common. See, e.g., L. W. H. Hull, *op. cit.*, 126.

87. The intellectual background of Euclid's work was first discussed, I suppose, by K. R. Popper in his "The Nature of Philosophical Problems and Their Roots in Science" *Brit. J. Phil. Sci.*, 1, 1952, 121-56, reprinted now in his *Conjectures and Refutations*, 1963. (See also the second edition of his *The Open Society and Its Enemies*, notes to Chapter 6, and the fourth edition of the same work, Volume I, Addendum I.) For the criticism of Marx's economism see Chapter 15 and notes there. For the social theory of science and liberty see his *The Poverty of Historicism*, section 32.

88. On an inductivist view, the passive or receptive mind is regarded as better fitted for scientific inquiry than is the active or imaginative mind. This idea has led to what

Popper calls "the bucket theory of the mind." On such a view the mind — and not only the good mind — is necessarily passive. This doctrine of the passive mind, along with Cartesianism, has led to mechanism; and also, either directly or together with mechanism, it has led to economism, in the highly developed form which Marx gave to it. (The same line of thought, incidentally, plus a dash of Darwinism, led Spencer to similar views on science as rooted in needs.) In 1824 Carnot, for instance, applies economism as if it were a truism: speaking of the steam engine he writes (*Reflexions sur la Puissance Motrice du Feu*, trans. C. A. Pearson, *Motive Power*, Birmingham, 1922): "it is natural that an invention should have its birth, and especially its development, and be perfected in the place where its want is most strongly felt", i.e., in England. Incidentally, Carnot is mistaken: the steam engine was developed not because of economic needs but because the world of science (ever since the days when Hooke had encouraged Newcomen, and up to the fourth decade of the nineteenth century) had felt obliged to prove Bacon's doctrine according to which true science would lead to invention and the improvement of the human lot.

Further interesting evidence for the early popularity of economism is the following passage from Malthus who argues that economism is dangerous even from its own viewpoint: "How many useful inventions, and how much valuable and improving knowledge, would have been lost, if a rational curiosity, and a mere love of information, had not generally been allowed to be a sufficient motive for the search after truth!" See Malthus, *Principles of Political Economy*, 16, quoted by Herschel, *Preliminary Discourse* (*op. cit.*), 13n.

89. §§36-38.

90. See his *The Value of Science*, 1958, "Preparatory Essay to the English Translation" for the need to choose, and Chapter 9, §7 for the choice between Ptolemy and Copernicus, and its basis in our desire to find more internal relations described in the theory we select. For the Kantian aspect of Poincaré's philosophy see Russell's preface to the English edition of his *Science and Method*, and É. Meyerson's *Identity and Reality*.

91. Dr. Thomson, *Hist. Chem.*, Vol. 2, 256, 260. Hans Christian Ørsted, "Observations on the History Or Chemistry", *The Soul in Nature*, 1852, 305-324, especially 309: "we shall not be inclined to reproach Stahl and his successors, because they assumed a common principle to exist in all combustible materials. The antiphlogicians themselves assumed this, while they attributed to all combustible bodies a chemical attraction to oxygen …" This is not so exceptional a judgment. Lavoisier himself, commenting on Richard Kirwan's *Essay on Phlogiston* (translated in the second edition of that *Essay*, 1789), says explicitly (20-21) that he would not mind being viewed as a phlogistonist who considers vital air (i.e., oxygen plus caloric) to be phlogiston. See also J. H. White, *op. cit.*, 144; and W. Whewell, Hist. *Ind.* Sci., Vol. 1, Book 7, Chapter 1, Prelude to the Inductive Epoch of Newton.

92. Pierre Duhem, *The Aim and Structure of Physical Theory*, Princeton, 1954, 220-222:

> "*Hypotheses Are Not the Product of Sudden Creation, but the Result of Progressive Evolution. An Example Drawn from Universal Attraction*
>
> … … …
>
> … Can such unlimited freedom be useful to a man? Is his mind powerful enough to create a physical theory all out of one piece?
>
> "Surely no. Thus history shows us that no physical theory has ever been created out of whole cloth. The formation of any physical theory has always proceeded by a series of retouchings which from almost formless first

> sketches have gradually led the system to more finished states; and in each of these retouchings, the free initiative of the physicist has been counselled, maintained, guided, and sometimes absolutely dictated by the most diverse circumstances, by the opinions of men as well as by what the facts teach. A physical theory is not the sudden product of a creation; it is the slow and progressive result of an evolution.
>
> … … …
>
> "… in order to find the germ of this doctrine of universal gravitation, we must look among the systems of Greek science; … the slow metamorphoses of this germ in the course of its millenary evolution; … the contributions of each century to the work which will receive its viable form from Newton … the doubts and gropings through which Newton himself passed before producing a finished system; and at no moment in the history of universal attraction … any phenomenon resembling a sudden creation; nor one instance in which the human mind, free from the impetus of any motive alien to the appeal of past doctrines and to the contradictions of present experiments, would have used all the freedom which logic grants it in forming hypotheses."

See also my review of Duhem's book in *Brit. J. Phil. Sci.*, 8, 1957, especially 241 and 245; and note 28 above.

93. Spratt's narrow and chauvinist *History of the Royal Society*, 1667, that was unofficially the official promotion of the Society, received an astute review by Henry Stubbe, *Legends, no Histories*, 1670. See its preface:

> "It is manifest now that the Antient Learning (and not only Natural Philosophy) is the Rubbish they would remove: This work they would so diligently pursue as if they had forgot their first and chief Employment, carefully to seek, and faithfully to report how things are de facto."

See L. Pearce Williams, *Brit. J. Phil. Sci.*, 11, 1960, 162,

> "There are still those who insist that mediaeval science was mere philosophical vapourings concerned either with the number of angels who could dance on pin heads, or, as 'applied philosophy', with the magical essence of gems, fabulous beasts, and so on. These are the Baconians, such as Lynn Thorndike. At the other extreme are the followers of Pierre Duhem who view modern science as a rather extended footnote to mediaeval achievement."

Williams refers to only two writers on the Middle Ages who do not belong to either extreme, Clagett and Anneliese Maier, and one could think of a few more. The same is true of the Renaissance, with Burtt and Koyré topping the list; yet on the whole the number of non-extremists is incredibly small.

94. See my "Duhem versus Galileo", *Brit. J. Phil. Sci.*, 8, 1957, 242.

95. See my "Methodological Individualism", *Brit. J. Soc.*, 11, 1960, 252-254. See also C. L. Becker, *The Heavenly City of the 18th-Century Philosophers*, to which I regret that I did not refer in that paper: I first read it after my paper had been published.

96. See my "A Hegelian View of Complementarity", *Brit. J. Phil. Sci.*, 9, 1958, 57-63.

97. E. Cassirer, *The Problem of Knowledge, Philosophy, Science, and History since Hegel,* New Haven CT, 1950, 81. Cassirer also attributes to Kepler "the first really precise definition of the meaning and function of scientific hypotheses" (*loc. cit.*)!

98. "Cassirer's greatest achievement in this work," says his biographer D. Gawronsky, (*The Philosophy of Ernst Cassirer*, ed. P. A. Schilpp, 1949, 50; quoted in Cassirer, *op. cit.*, x, "consists in the creation of the broad general background by connecting

the evolution of knowledge with the totally spiritual culture: mythos and religion, psychology and metaphysics, ethics and aesthetics … “

99. The scheme is, I suppose, presented in the Introduction; but since I do not quite understand it I can say no more. For examples of Cassirer’s offhand way of dismissing thinkers who do not fit his scheme, see 27-8 against Lotze, 87 against DuBois-Reymond, and 114-15 against Kelvin.

100. I doubt whether the work referred to has yet been sufficiently disseminated to be considered influential, but it presents somewhat more succinctly ideas that Cassirer had started to sow in 1910 in Germany.

101. Frank Dawson Adams, *The Birth and Development of the Geological Sciences*, 1938; reprinted 1954. The preface and conclusion present the problem and the solution. The continuity technique, mingled with strong inductivist elements, dominates the later parts of the book.

102. J. Bronowski and B. Mazlish, *The Western Intellectual Tradition from Leonardo to Hegel*, 1960. The passage on Copernicus is on page 112-113 and on Kepler on page 117; the rest is from the book’s Conclusion.

103. *Scientific American*, September 1960.

104. 1950; second edition, 1958.

105. This serious problem faces, of course, not only Butterfield, but all historians who deal with the period and who try to take account of Duhem’s criticism of inductivism, if they are inductivists, or of modern criticisms of Duhem’s continuity theory, if they are conventionalists. Sarton, an inductivist, answered Duhem’s criticism by saying, *A Guide*, 33-4, that although the Renaissance saw the birth of science, the “Open Sesame!”, the Middle Ages constitute the period of pregnancy. A. R. Hall, who presents himself as a conventionalist, says, *The Scientific Revolution, 1500-1800*, 1954, 59, in a true Duhemian fashion, that “One might think that the famous cosmological debate of the seventeenth century had been rehearsed in the fourteenth!” He hastens, however, to add a qualification in a footnote: “As was long ago pointed out, with too great emphasis and some misunderstanding, by Duhem in *Revue Generale des Sciences Pures et Appliquees*, vol. 20, 1909.”

106. I. B. Cohen, who rightly praises Butterfield’s book for its extreme readability, *Journal of the History of Ideas*, 15, 1954, 178, notices the weakness of the chapter on eighteenth-century chemistry. He kindly ascribes this weakness to Butterfield’s “leaning heavily on monographic literature that is better on the sixteenth and seventeenth centuries than on the eighteenth”. But Butterfield refers to the literature that tries to do justice to phlogistonism, and dismisses it as based on the mistaken assumption “that it is the historian’s function to be charitable”, first edition, 180; second edition, 198. Hence, I would suggest that his error is the result of his erroneous view that only one revolution occurred in science, one of a methodological nature, and of his correct view of Lavoisier’s advance as a revolution.

107. That both faculties, reason and observation, play their role in the process of learning is equally platitudinous for Democritus and for Bruno, each of whom tried to specify the role of each faculty, and the ways in which they coordinate. But Bacon himself usually preferred propaganda to the discussion of problems; and one could hardly expect Sarton not to follow suit; *op. cit.*, 36-38.

108. I have discussed in the text the application of the continuity theory to the history of science (Duhem), methodology (Cassirer), and metaphysics (Jammer); its application to the history of technology can be found in J. Jewkes, D. Sawers, and R. Stillerman, *The Sources of Invention*, 1958, 223-5 *et passim*.

109. Max Caspar, *Kepler*, 1959, 67-8, says of Kepler's *Mysterium*,

> "To be sure, in his book he speaks of an "*anima motrix*", a moving soul; but already in a letter of this period he uses the word "rigor", force. In this idea is hidden the first germ of celestial mechanics."

110. F. Y. Loewinson-Lessing, *Historical Survey of Petrology*, 1954, 8.

111. I do not wish to imply that connections or glimpses into the future are totally uninteresting. I found it most interesting, for instance, to find a continuity from Leibniz via Boscovitch, Priestley, and Davy, to Faraday; but this is no more than a historical fact to be studied, not a framework (as Duhem calls it; cf. note 28 above). One glimpse into the future that has impressed me is Biot's attempt to quantize the spin of the photon, so to speak, in order to rescue Newton's particle optics. This is a thought-provoking fact, not a reason to award Biot a medal; and certainly there is no continuity in this case.

112. Marie Boas speaks of his "almost Galilean touch" and praises his "approaching his material analytically", *Isis*, 46, 1955, 304. Angus Armitage says the book is "at least a notable milestone on the road to understanding", *Annals of Science*, 11, 1955, 101.

113. E. N. da C. Andrade, *R. S. Newton Tercentenary Celebrations*, 1947, 16, 19. See also Andrade's *Isaac Newton*, 1950, Chapter 7. In his *Sir Isaac Newton*, 1954, Chapter 8, "Newton the Man", the evidence is further shrunk and a letter from Newton to Boyle is quoted saying that he had "so little fancy to" alchemy that he would not have written anything about it but for Boyle's "encouragement". To return to Hall's allegation about Newton's consuming interest in alchemy: certainly Newton did have a considerable interest in alchemy; but how deep and wide, and from what angle, is far from being clear as yet.

114. J. B. Conant, *Science and Commonsense*, 1951, 25 ff.

115. J. H. White, *op. cit.*, 41.

116. Sir W. Ramsey, *Joseph Black, M.D.*, 1819, quotes (87) an incredible letter from one Dr. Eason to Dr. Black: "Doc.[r] Priestley, not having anyone to steal from at present, I believe is quiet, unless it is to trouble the world with his religious nonsense." Evidently a prejudiced person like Priestley can only steal or talk nonsense, not discover!

117. *Annals of Science*, 5, 1952, 193. Dr. Thomson states, *op. cit.*, vol. 2, 261, that Becher and Stahl knew that calcination increases weight of metals. See also J. H. White, *op. cit.*, 55.

118. J. R. Partington and D. McKie, "Historical Studies on the Phlogiston Theory", *Annals of Science*, 2, 1937, 361; 3, 1938, 1; and 4, 1939-40, 117; 135; and J. H. White, *op. cit.*, 59-92, discuss a number of ad hoc hypotheses. The "Historical Studies" are very interesting in that they capture the spirit of the age; namely, classical inductivism (that the authors endorse); see especially the closing paragraphs.

119. J. P. Hartog, "Joseph Priestley and his place in the History of Science", *Proc. R. I.*, 1931; also published separately in *R. I., Weekly Evening Meetings*, 1931. See also his "New View of Priestley and Lavoisier", *Annals of Science*, 5, 1941, 1-56, and note 128 below.

120. *Transactions of the British Association*, 5, 1837, 163. See also Th. Thomson, *Hist. Chem.*, Vol. 2,, 261.

121. *Experiments to Make Fire and Flame Ponderable*, Additional Experiments, No. V, Exp. iii, Corol. iii.

122. D. McKie, "Boyle's Essays on Effluviums", *Science Progress*, 29, 1934-35, 253-265. In his conclusion, McKie rightly claims that Boyle was not misled because the experiment was improperly conducted, but that Boyle's "studies were evidently directed by a belief in the corpuscular nature of flame", which is quite true, "and in more fortunate circumstances the results here presented would doubtless have been accompanied by others tending to prove that light too was corpuscular."

    See also D. McKie, "Cherubim D'Orleans: a critic of Boyle," *ibid.*, 31, 1936-37, 55-67. McKie here refers to Boyle's repeated mistake in the following manner. "This malobservation … was unfortunate, for it confirmed Boyles[false] explanation. It is tempting to speculate as to what theory Boyle would have put forward if he had made the correct observation here, but such is not the part of the historian." Not being a historian, I have no hesitation in speculating that the speculation hinted at is that Boyle would have developed a theory intermediate between those of Mayow and Lavoisier.

123. Hèléne Metzger, *Newton, Stahl, Boerhaave, et la doctrine chimique*, Paris, 1930. "The phlogiston theory in its day was a distinct step forward.", writes J. B. Conant, *op. cit.*, 167. See also notes 153 and 175 below.

124. Stephen E. Toulmin, "Crucial Experiments: Priestley and Lavoisier", *J. Hist. Ideas*, 18, 1957, 205-220, reprinted in *Roots of Scientific Thought: A Cultural Perspective*, ed. P. P. Wiener and A. Noland, 1957.

125. See, for example, Bacon, *Sylva Sylvarum*, §1000; see his *Wisdom of the Ancients*, Ixion, for "impure wish for glory"; see C. W. Lemmi, *The Classic Deities in Bacon*, Baltimore, 1933, 102, for the alchemical origin of this doctrine (which goes back to Cabbalism).

126. Richard Kirwan, *Essay on Phlogiston*, second edition, 1789, translator's preface (by W. Nicholson). The second edition contains the translation of the comments on the first edition that the French chemists had added to the French translation of the book.

127. Joseph Priestley, *The Doctrine of Phlogiston Established*, 1800, Conclusion. I cannot escape feeling that Priestley's general feeling was not a sense of hankering after the past but some profound sense of tragedy that seems to me quite justifiable in view of the immense significance of Baconianism to the scientific tradition: Priestley saw the destruction of a tradition, but not so much of the phlogistic tradition as of the Baconian, which he rightly identified with the scientific tradition. He writes:

    > "There have been few, if any, revolutions in science so great, so sudden, and so general, as the prevalence of... the Anti phlogistons, over the doctrine of Stahl, which was at one time thought to have been the greatest discovery that has ever been made in science. I remember hearing ... that there had hardly been anything that deserved to be called discovery subsequent to it. Though there had been some who occasionally expressed doubts … nothing had been advanced to justify a revolution."

    The following moving thoughtful passage, however, written in the same typical admirable Priestley style, refutes Baconianism — although Priestley himself did not notice this result at all:

    > "But I check myself. It does not become one of a minority, and especially of so small a minority, to speak or write with confidence; and though I have endeavoured to keep my eyes open, and to be attentive as I could to everything that has been done in this business, I may have overlooked some

> circumstances which have impressed the minds of others, and their sagacity is at least equal to mine.
>
> "Though the title of this work expresses perfect confidence, in the principles for which I contend, I shall still be ready publicly to adopt those of my opponents, if it appear to me that they are able to support them. Nay, the more satisfied I am at present with the doctrine of phlogiston, the more honourable shall I think it to give it up upon conviction of its fallacy; following the noble example of Mr. Kirwan, who has acquired more honour by this conduct than he could have done by the most brilliant discoveries that he could have made."

128. This has been noticed by Davy, *Elements of Chemical Philosophy, Memoirs*, 1836, Vol. 4, 26. J. P. Hartog, Priestley's modern defender, we may remember (last section and note 119), also noticed this; but he put it very derisively. See also Robert E. Schofield, "The Scientific Background of Joseph Priestley", *Annals of Science*, 13, 1957, and compare with Sir Phillip Hartog, A. N. Meldrum, and Sir Harold Hartley, "The Bicentenary of Priestley", *J. Chem. Soc.*, 1933.

129. *Loc. cit.*:

> "In my opinion there can be no compromise of the two systems. Metals are either necessarily simple or necessarily compound, and water is either resoluble into two kinds of air or not."

130. Whittaker, *A History of Theories of Aether and Electricity, The Modern Theories*, 1959, 36.

131. They are summed up in Priestley's *Autobiography*, 1806, 269.

132. W, Cruickshank, "Some observations on different hydrocarbonates and combinations of carbon with oxygen, in reply to some of Dr. Priestley's objections to the new system of chemistry," *Nicholson's Journal*, 1801.

133. Dalton discusses the results of Clement and Desormes that seem to me to be the same as Cruickshank's, saying that they had not "taken notice of this remarkable result", i.e., the multiple proportion of CO and CO_2; notes for his nineteenth lecture at the Royal Institution, quoted in *Harvard Case Histories*, Vol. 1, 245. The situation here needs more clarification.

134. John Davy, *The Fragmentary Remains of Sir Humphry Davy*, 1856, 359 *et seq*. See also his *Memoirs*, 1836, 155-7.

135. To give one example, I shall quote in translation from a letter from Schönbein to Liebig, *Briefwechsel, 1853-1868*, 1900, 96:

> "The present day's playing about with atom-complexes, substitutions, rational composition-formulae etc. will not appeal to me, I confess, and I fear that I am not the only one who does not find an especial taste for them; when a man of the deep spirit of Faraday can tell me … 'I am too stupid to understand the organic chemistry of the present day ', this has to be hardly an expression of praise or admiration."

136. A. H. Lorentz, *Lectures on Theoretical Physics*, 1927. Whittaker's thesis concerning special relativity is that Poincaré provided its physical conception, Lorentz its mathematical conception, and Einstein nothing at all. I would like to think that this was said in the name of continuity, and indeed inasmuch as the myth that special relativity is largely an achievement of a single individual is still alive (as expressed, say, in Hans Reichenbach, *Philosophical Foundations of Quantum Mechanics*, Berkeley CA, 1944, v), there is merit to Whittaker's observations. Yet, in complete

accord with the continuity approach, he could have attributed something to Einstein. In particular, he could have suggested that Einstein unified the physical and mathematical conceptions. After all, one can claim that the physical conception of the classical theory of gravity is due to Gilbert, or Gilbert and Kepler, and that the mathematical conception is due to Hooke! This is in some sense acceptable, and not in the sense of belittling Newton's achievement. Perhaps what angered Whittaker in the first place was the revolutionary aspect of Einstein's work — even though in his preface he speaks plainly about the revolution in physics that includes special relativity. Yet he certainly did wish to minimize the revolutionary aspects of the revolution. He wrote, *op. cit.*, Vol. 2, 42-3, "It is clear … that the theory of relativity has its origin in the theory of the aether and electrons" though it was later attempted to deduce it from plausible axioms. Nonetheless, he did not refer to Einstein's effort in this direction, he pooh-poohed Einstein's deduction energy-matter formula as an approximation and overlooked Einstein's revolutionary deduction of Newtonian kinetic energy from his energy-matter formula as an approximation. Einstein's second paper on relativity, in which the energy-mass equation is rigorously deduced for the first time, Whittaker did not even mention; rather, he attributed this success to others. And he did not attribute to any author the relativistic law of addition of velocities, although he knew that it is Einstein's and although he emphasized its significance (44). Although these injustices to Einstein are negligible in comparison with Dingle's — who once wrote a book on relativity without so much as mentioning Einstein in it even once — they are still remarkable, especially from a writer like Whittaker who looked for various contributors to each forward step. What Whittaker, as well as A. O'Rahilly (*Electromagnetism*, 1938) and Dingle disliked most about Einstein is his revolutionary approach to science. But at least O'Rahilly stated his criticism openly, as Dingle has also done in recent years.

137. R. G. Collingwood, *Autobiograph*y, 1939, 70. See also Ludwig von Mises, *Theory and History*, 1957, Lionel Robbins, *Essay on the Nature and Significance of Economic Science*, 1930, K. R. Popper, *The Poverty of Historicism*, and my "Methodological Individualism", note 95 above.

138. *Novum Organum*, Book I, Aph. 109.

139. Arthur Koestler, *The Sleepwalkers*, 1959, 19: "we can add to our knowledge but we cannot subtract from it."

140. Arthur Koestler, *Insight and Outlook*, 1949, 251-255.

141. This question is neatly answered in Dingle's thought-provoking essay on the history of thermometry, "Some Reflections", *op. cit.*, 30. I confess I find his answer unsatisfactory. He says, no one before Black measured and saw that the temperature of ice remains constant while it melts. This may be true but it is not relevant here. The temperature of boiling water had been conceived much earlier as a given fixed degree of heat; and the fact that more water takes more time or a bigger flame to boil away completely has always been common knowledge. Black himself speaks of "the inconsistency" of the accepted views "with many obvious facts"; W. F. Magie, *A Source Book in Physics*, 1935, 140 and also lines 6 and 5 from the bottom of page 142. Moreover, the fact that melting ice and boiling water do not increase their temperatures can be easily "proved" *a priori* as Galileo "proved" that big and small stones fall with equal speed. Yet Dingle's error is already to be found in Black's memoir, *Harvard Case Histories*, Vol. 1, 147. Black also says that the discovery had puzzled its discoverers and had led them to ad hoc explanations of it. We may surmise then that they held a theory that persistently misled them; what it was I do not know. The same error that Black and Dingle make, is expressed more confidently in Duane Roller's *Case* History "Temperature and Heat", *ibid.*, 126.

142. *Novum Organum*, Book I, Aph. 112. Spedding discusses this point in his introduction to Bacon's *De Interpretatione Naturae Proemium, Parasceve*, and *De Sapientia Veterum*. Helmholtz discusses the nature of force and the structure of matter in his Faraday Lecture, adding, (133), "The discussion of this question … is not yet finished, although, I think, it approaches its end."

143. A. Einstein, "Autobiographical Notes," *Albert Einstein, Philosopher-Scientist*, 1949, 19; M. Planck, Scientific Autobiography, 1949.

144. N. R. Hanson, "Discovering the Positron", *Brit. J. Phil. Sci.*, 12, 1962, 299, states the problem explicitly with regard to the discovery of the positron. His answer tallies very well with Popper's theory presented here in section 16. He adds interesting comments on this idea from Oppenheimer and Bohr (306n) that provide good illustrations for my view that we are wise after the event because we condemn all errors indiscriminately. In any case, this paper deserves close study. Abdus Salam's inaugural lecture, *Elementary Particles*, Imperial College, 1957, discusses the oversight of non-parity for at least one decade.

145. H. T. Pledge employs this proposal — look for a technological innovation behind the appearance of a new discovery or new theory — in an extreme way in his *Science Since 1500*, 1939, but he is not the only one to use it. Marxist authors are, of course (see section 7 above), particularly prone to commit this kind of error. Pledge declares (145) that the discoveries of spectral lines "were due to using spectra focused by Telescopes. Unfocused spectra do not show the lines." By analogy, we might argue that the discovery of electricity in metals is due to using insulators. Uninsulated metals do not show electric effects. Funnily enough, we have historical evidence to refute Pledge's claim: Charles Babbage tells (*The Decline of Science in England*, Conclusion) that Herschel focused the microscope and showed him the lines, yet he could not see them until he got further instruction from Herschel. Pledge makes extensive use of the idea that technological development precedes discovery. This is trivially true in the sense that it is essential to its coming out of hiding and trivially false in the sense that at time the discoverer invents the technology to that end, as Young did when he invented photography in order to discover the wave character of invisible light (although without a fixer, so that the interference patterns he observed disappeared at once).

    The methodological rule of the inductivist historian — look for a technological innovation behind the appearance of a new discovery or new theory — is more clearly seen when the historian is off guard. Thus, J. U. Nef declares, *Cultural Foundations of Industrial Civilization*, 1958, 27, that "the chief instrument which enabled Brahe to go beyond the Arabs in accuracy was the telescope." Nef's source is A. R. Hall, *The Scientific Revolution, 1500-1800, op. cit.* A hasty reading of page 121 of this book may indeed suggest that. But Hall only allows it; he does not say it explicitly. It is a part of a tradition discussed in section 5 above.

146. See Sarton's interesting remark in *Horus: A Guide to the History of Science, op. cit.*, 17.

147. G. Gamow, *One, Two, Three, Infinity*, 1947, Chap. 6, §2, 122. Measuring the size of molecules by spreading a layer of oil over water until it breaks rests on the theory of monomolecular layers.

148. There is a popular misconception against the admission of the role of luck in research: Bacon's famous, repeated assertion that discovery is accidental does not mean that it is due to luck, as he stressed. You can have accidental discoveries by luck or by method, namely, systematically; *Novum Organum*, Book I, Aph. 108. But all discovery is accidental, he said, whether made due to luck or due to method,

in the sense that it is a genuine novelty. Hence, it is made independent of theory— as I explain in section 16.

Popper stresses in his lecture courses that were discoveries made in accordance with a method, none should know the method better than great discoverers, yet such individuals often cease to make discoveries at a relatively early age. I was curious to see what inductivist writers would say about such cases. The inductivist methodologists agree about the principle: Herschel, for instance, says, *op. cit.*, that the scientist is "accustomed" to discover and is "deeply imbued with the best principles and sound philosophy." It is not surprising, then, that if such powerful people quit the battlefield, then it should arouse indignation. Volta is a traditional object for the indignation of inductivist historians: a person so highly qualified, it is maintained, should not have ceased to conduct his researches. Ørsted was also censured in this fashion — although he continued industriously but unsuccessfully to the last. Mach censured Galileo for not having discovered the vacuum; and Cajori, *op. cit.*, endorsed Mach's censure. Laplace says, *op. cit.* Newton was lucky to be born in a time ripe for the grand generalization and Herschel comments, *op. cit.*, that it was sad for Boyle and Hooke that they missed the same opportunity; Dampier-Whetham says, *op. cit.*, Faraday, though he followed Ampère — which he did not — was not as easily satisfied, went deeper into the causes of electrochemistry, and was justly rewarded (which sounds like a gentle censure). One possible reason for the inductivist dislike of Priestley (especially among the British) is that he was worthy of discovering antiphlogistonism and would have done so if he had not been so pigheaded.

149. Ernest Gellner, in his very interesting essay "On Being Wrong", *The Rationalist Annual*, 1955, distinguishes clearly between being mistaken and being wrong. Yet, drawing a complete parallel, he does not distinguish between being wrong and thus condemnable, and doing the wrong thing because of a reasonable error (be it in the factual or the moral sphere) and thus not. The result is strange. Far from noticing that the application of fallibilism to moral philosophy engenders moral standards more attainable than the traditional ones, he conveys the mistaken feeling that the application of fallibilism to moral philosophy makes life totally intolerable, as it entails that we have moral faults even though we may not have found them as yet. The contrast between responsible error and condemnable one, so well known in democratic everyday life, is still obscure to the traditionally minded moral philosopher. (To my surprise, Popper disapproves of my view on this.)

D. O. Hebb, "Alice in Wonderland", *Biological and Biochemical Bases of Behaviour*, ed. H. F. Harlow and C. N. Woolsey, Madison, Wisconsin, 1958, 463 ff, is perceptive. He notices the strain on research students created by "perfectionism", and he suggests that "we must not let our epistemological preconceptions stand in the way of getting research done". Instead, he advocates the encouragement of serendipity. "Serendipity" means here expecting to find one thing and finding something quite different. He does not notice that here it means finding refutations instead of confirmations to given hypotheses. Undoubtedly, he is right in contending that this is less perfectionist, more reasonable, and perhaps even more fruitful. It is, however, regrettable that he does not demand the outright rejection of the "epistemological preconceptions" that have given rise to the problem.

150. H. L. A. Hart, "Prolegomena to the Principles of Punishment", Presidential Address, *Proc. Arist. Soc.*, N. S. 60, even claims that from the viewpoint of legal principles this point was over-emphasized in English law. The distinction must be quite ancient; at least the Talmudic law makes it explicitly, in matters of criminal law as in civil law.

151. Contrast with Holton and Roller, *op. cit.*, 220: "the word 'error' does not have in science the connotations 'wrong', 'mistaken', and 'sinful' which it so often has in everyday speech." They add that the scientist is exact when he knows the limit of his error. This is true, but such situations seldom occur. See also notes 172 and 175 below.

152. L. K. Nash gives the title "Confusions and Dawning Clarifications of the Atomic Theory in the Period 1827-1857" to his section on the refutation of the theories of Dalton, Avogadro, and Berzelius, and their replacement by better chemical theories *Harvard Case Histories*, Vol. 1, 313. (Incidentally, these newer theories were refuted later.) Nash prefers "confusion" to "refutation" ─ perhaps the latter sounds to him more condemning. And perhaps he confuses the two.

153. "The Phlogiston Theory: A Block to a New Concept" is J. B. Conant's title in his *Science and Common Sense*, 1951, 181. this is true of all false theories, including Newton's theory of gravitation; and not only when the theory in question is unpopular (as phlogistonism still is).

154. De *Caelo*, 309 b 20 and 311 a 35.

155. See the very end of *De Caelo*.

156. *Ibid.*, 311b5.

157. For the Platonic-Euclidean tradition that Archimedes was following, see the reference in note 87 above. The view expounded here accords completely with Galileo's , as expounded in the last and historical part of his *Discourse on Bodies in Water*, trans. Thomas. Salusbury, new edition by S. Drake, Urbana IL, 1960. It also accords fully with the view taken by Lane Cooper, *Aristotelian Papers*, Ithaca, 1939, 98, whose great advantage is in the knowledge that "of course it is fun to feel wiser than Aristotle, though the fun commonly entails a loss of historical perspective." Cooper, *Aristotle, Galileo, and the Tower of Pisa*, 1935, 48 and note invite discussion. He criticizes Galileo's presentation of Aristotle's dynamics, and strangely enough, he even endorses Heath's view according to which Archimedes "had no predecessor in hydrostatics". Nonetheless, he rightly adopts Emil Wohlwill's suggestion "that Galileo's issue with Aristotle should be traced back to Hipparchus". He adds boldly, "I suggest that it may well go back to Archimedes, if not to the Academy". It is greatly to be regretted that due to hero-worship Cooper was attacked for criticizing Galileo and his positive suggestions have never been taken up.

As blaming Aristotle is still all too common, I should now mention Sarton's praise for him, *A History of Science*, Harvard, 1960, 516, although it is equally pointless.

Friedrich Solmsen, who has the last word on the subject, *Aristotle's System of the Physical World, a Comparison with his predecessors*, 1960, notes that Aristotle condemns Plato's theory of the light as the less heavy as based on a misunderstanding. Following Jaeger (280n), Solmsen endorses Aristotle's claim (281). He tries to relate Aristotle's doctrine not to facts but to Aristotle's cosmology, and he admits failure (285); but he claims that Aristotle's faults are usually to be found already in Plato and other of his predecessors (286). This tallies with his intention (Preface) to link Aristotle with his predecessors so as to show that he was "neither as free nor as arbitrary as some students of his thought appear to believe", even if this must be done at the cost of showing him ─ in accordance with Cherniss' view ─ to be "not as invariably original as he himself … may lead us to think." I accept the point about Aristotle's lack of originality, and am ready to see some value in tracing Aristotle's muddles to his predecessors as much as possible. Yet the title of

Solmsen's book leads one to expect a discussion on Aristotle's muddles on weight, even though they seem to be Aristotle's own.

158. Sarton rightly claims *A History of Science, Hellenistic Science and Culture in the Last Three Centuries B.C.*, Cambridge MA, Harvard University Press, 1959, 79,

> "It is clear that the Euclidean mode of exposition which Archimedes used is as dogmatic or didactic as can be, and the order is certainly very different from the order of discovery".

The expression "dogmatic or didactic" here is a variant on Bacon's "rational and dogmatical". namely, deducible from self-evident principles (*Novum Organum*, Book I, Aph. 85).

159. J. B. Cohen, *The Birth of a New Physics*, 1960, 145ff. 1985,

160. Holton and Roller *op. cit.*, 383 consider Proust's theory as "a generalization" and of its atomic interpretation as Dalton's contribution!

161. Quoted approvingly by Nash, *Harvard Case Histories*, Vol. 1, 240 — approvingly simply because the up-to-date chemistry textbook says solutions, amalgams, etc., are not compounds.

162. *Harvard Case Histories*, Vol. 1, 222-224: "it often struck me with wonder how a compound atmosphere, a mixture of two or more elastic fluids, should constitute apparently a homogeneous mass." To this remark by Dalton the commentator L. K. Nash adds: " 'compound' is used here not in the sense of chemical combination, but merely as synonymous with 'mixture' "; he does not say why. "Newton has demonstrated clearly … that an elastic fluid is constituted of … atoms … which repel each other," continues Dalton. "But modern discoveries … that the atmosphere contains three or more elastic fluids, of different specific gravities, it did not appear to me how this proposition of Newton would apply to a case of which he, of course, had no idea."

Newton "had no idea", or, as explained above, was mistaken; but what was the mistake? What has the number of gases to do with his atomic repulsion, whether of the same specific gravity or not? An elastic fluid with atoms all repelling each other equally, but with unequal weights, will immediately separate into strata according to weight. Indeed, Dalton explains: "The same difficulty occurred to Dr. Priestley, who discovered this compound nature of the atmosphere. He could not conceive why the oxygen gas being specifically heaviest, should not form a distinct stratum of air at the bottom of the atmosphere, and the azotic gas [nitrogen] at the top. Some chemists upon the Continent, I believe the French, found a solution to this difficulty (as they apprehend). It was chemical affinity … "

If the word "chemical affinity" means a force causing chemical combination, then it is clear why Dalton calls air "a compound"; this was the common view from which he started: all gas mixtures are compounds. Hence in this context he uses the words "compound" and "mixture" as synonyms because the difference has not yet been created, because from the Newtonian viewpoint all stable fluid and gaseous mixtures must be compounds, sustained by atomic forces, namely, by the forces of chemical affinity.

Nash's explanation of the meaning of the words "chemical affinity" is this: "an attraction or tendency that was believed to cause substances to combine, react, or form solutions with one another." This seems to be the claim that chemical affinity is one cause of both chemical combination and solution. If this is so, then he is mistaken: chemical affinity is the force that causes combination alone. Hence, assuming solutions to be caused by chemical affinity is tantamount to saying that all solutions are compounds. Dalton continues by attributing to his opponents the view that chemical affinity causes solutions of gases in fluids and in other gases; he goes on to discuss a known difficulty that this view involves: diffusion shows no alteration of chemical properties. Why, if "chemical affinity" is a name for forces causing either combination or diffusion, rather than only combination, why then should the existence of forces causing diffusion lead to the conclusion that diffusion ought to lead to chemical changes?

Dalton continues by reporting that the criticism he quotes was met by the suggestion that solution is caused by weak chemical forces. This made him think of balances between chemical forces and weights of atoms; he tried all combinations and failed. Finally, he gave up the idea that all mixtures are necessarily compounds, or, in his words, "the hypothesis of the chemical constitution of the air". Had he lived today, he would probably conjecture molecular forces between oxygen and nitrogen; but these he never dreamt of. He was obliged therefore to abandon the idea of any interaction between atoms of atmospheric oxygen and nitrogen (other than the gravitational interaction that is negligible).

163. L. K. Nash, *op. cit.*, 240, 262, 292.

164. In following Newton's explanation of the elasticity of airs, whether strictly as in his early days, or in a modified form as later, Dalton deviated from Lavoisier. Newton explained the elasticity of air by assuming that its particles repel each other with a force proportional to the inverse of the distance between them. Lavoisier rejected Newton's explanation, claiming, *Elements*, sixth edition, 1806, 42,

> "the same body becomes solid, or fluid, or aeriform, according to the quantity of caloric by which it is penetrated; or, more strictly, according as the repulsive force exerted by the caloric is equal to, stronger, or weaker, than the attraction of the particles of the body it acts upon.".

The reason for Lavoisier's deviation is possibly this: Boscovitch had shown that the collision between absolutely rigid bodies involves infinite forces, and he therefore dismissed rigid atoms. Lavoisier, on the other hand, retained the rigid atoms but prevented them from colliding by using caloric as the elastic medium between them *, ibid.*, 38:

> "particles of bodies do not touch each other in any state hitherto known. Though this be a very singular conclusion it is impossible to be denied."

For the purposes of the present discussion, the difference between Newton and Lavoisier may be insignificant. I do not know why Dalton simultaneously clung to Newton's theory of the elasticity of air and added to it Lavoisier's theory of the caloric-bath. The existence of the mystery is evidently rooted in deference for Newton and the confusion of criticism with disrespect. It is nonetheless possible to solve the mystery. My suggestion is this. In his chapter "Theory of Specific Heat", Dalton presents three alternatives and eventually chooses the one according to which each atom of any gas under given pressure and temperature is associated with the same quantity of caloric. This enables him to reconcile the views of Lavoisier and of Newton, as he implies quite clearly, *New System*, Manchester, 1818, Part I, Vol. 2, 147-8. It is interesting to note the order of Dalton's work: from specific heats of gases (67-75) to their elasticity (145-150), to the problem of diffusion (150-153), to his new theory (153-156). This is a logical, and perhaps also a chronological, order. (See also note 5 above.)

165. See for example Dorothy Stimson, *Scientists and Amateurs*, 1950.

166. "Considerations upon the Reputation, Loyalty, Manners and Religion of Thomas Hobbes" in Sir William Molesworth, editor, *The English Works of Thomas Hobbes*, 1839-45, Vol. 4, 409-40.

167. Thomas S. Kuhn goes so far as to speak of "quasi-inevitable accidental discovery" *Isis*, 49, 1958, 133. Koestler speaks of the "ripeness" of time for a discovery.

168. Voltaire, *Huron*, Chapter 14:

> "The ingenious youth was making a rapid progress in the sciences, and particularly in the science of man. The cause of this sudden disclosure of his understanding was as much owing to his savage education as to the disposition of his soul; for, having learned nothing in his infancy, he had not imbibed in prejudice ... He saw things as they were."

169. *Hist. R. S.*, 472: Priestley's ignorance of chemistry may have been bliss, but his ignorance of human nature, it seems, was an impediment. He was, according to Dr. Thomson (474), rated too high at first and too low later. Priestley himself testifies that his relative ignorance was "favourable" to him, since it made him devise new instruments (*Autobiography*, §101). His view here was still naively Baconian, but not so naive as Thomson's orthodox Baconian view — particularly since Priestley speaks of "apparatus and processes adapted to my peculiar views." Robert E. Schofield, "The Scientific Background of Joseph Priestley", *Annals of Science*, 11, 1959, records the survival of Thomson's myth in our century and its endorsement even by people like A. N. Meldrum, and he criticizes it.

170. Paul de Kruif, *Microbe Hunters*, 1926, Chapter 1, i: "his ignorance was a great help to him, for, cut off from all the learned nonsense of his time, he had to trust his own eyes, his own thoughts, his own judgment." Helmholtz said the same on Faraday, referring to his ignorance of mathematics (*Nature*, 2, 1870, 51).

171. Although Bacon, Whewell, and Popper all tried to characterize discovery by its logical relation to theoretical knowledge, none of them stated this program explicitly. See also my "How Are Facts Discovered?", *Impulse*, 10, 1959, republished in my *Science in Flux*, 1075. René Taton, *Reason and Chance in Scientific Discovery*, 1957, distinguishes between a chance observation of a new fact — a chance discovery — and a flash of thought occasioned by a chance observation of well known facts (such as Archimedes' alleged observation of the spilling of water when entering his bath) that is a psychological affair, as noticed in the review of it by R. F. J. Withers, *Brit. J. Phil. Sci.*, 12, 1961, 258-259. A. N. Meldrum, *The 18th-Century Revolution in Chemistry*, Chapter 5, §23, explicitly abandoned the logic of discovery in favor of a possible psychology of invention in order to avoid being wise after the event.

172. One example: Weber claimed that Faraday's discovery is independent of Ampère's theory (see notes 78 and 79). It refutes it. This logical error runs through the whole of the literature, and the greater the wish to express the demand that refutations be accepted, the more it seems to exhibit this error on this point. Claude Bernard says,

> "it is necessary to obliterate one's opinion ... when faced with the decisions of experiment; ... we must accept the results of experiment just as they present themselves with all that is unforeseen and accidental in them."

He confuses here counter-expectation with non-expectation, while demanding the abandonment of refuted theories; *Introduction to the Study of Experimental Medicine*, quoted by Pierre Duhem, *The Aim and Structure of Physical Theory*, Princeton, 1954, 181-2. The translation in the English edition of the book, 1949, 38, is remarkably different. (See also note 175 below.)

In this context of persistent logical error, the one that stands out is the inference from Kepler's theory to Newton's that the vast majority of methodologists and historians of science still consider valid, perhaps under some conditions. Whewell criticized Hegel's claim that Newton plagiarized from Kepler by proving the incompatibility between their theories. He suddenly noticed then that this incompatibility between these theories comprises a problem for him, as he deemed both true. He dismisses the problem in a Hegelian fashion, saying that they are incompatible on one level but dependent on another. See William Whewell, *History of the Inductive Sciences*, Vol. 1, 1837, 415n; *Philosophy of Discovery* 1860, Appendix H, On Hegel's Criticism of Newton'*s Principia*.

173. K. R. Popper, *The Logic of Scientific Discovery*, 1959; *Conjectures and Refutations*, 963.

174. A. N. Meldrum, *op. cit.*

175. J B. Conant, *Harvard Case Histories*, Vol. 1, 81. Concerning the allegation that there are no refuted hypotheses, see for instance Priestley's *History* of Electricity,

> "Every experiment in which there is any design, is made to ascertain some hypothesis … An hypothesis absolutely verified ceases to be as such … [Otherwise,] … new facts correct the hypothesis … [and] bring [it] … nearer to the truth."

It is not surprising that a good thinker with such a bad idea would prefer the so-called modifications of phlogistonism to Lavoisier's overly novel idea.

The fact remains that Lavoisier said all sorts of combustion and acidulation involve oxygen, and on this Davy corrected him. To date a number of authors defy this fact and a few circumvent it, at time in sophisticated and even interesting ways. See Arthur Donavan, "Lavoisier and the Origins of Modern Chemistry", *Osiris*, second series, vol. 4, 1988, 214-31, and its erudite notes 8 and 9. Donavan's thesis is that Lavoisier's real contribution is his research program that Donavan does not judge so that he is free of the need to say whether in his books Lavoisier is black or white. To be precise, Donavan speaks warmly of Lavoisier's "programmatical commitment" (224); he also praises his "commitment to reform" (239). Readers interested in the meaning of this concept are directed (note 29) to the writings of Larry Laudan. Wishing to avoid a priority dispute here, I leave it at that.

176. See H. E. Hoff, "Galvani and PreGalvanian Electrophysiologists", *Annals of Science*, 1, 1936, and W. Cameron Walker, "Animal Electricity before Galvani", *ibid.*, 2, 1937. See also J. Munro, *Pioneers of Electricity*, 1890, 94n.

177. *Isis*, 47, 1956, 237. For an account of the regular way in which occasions of discovering X-rays were missed, see Whittaker, *A History of Theories of Aether and Electricity*, The Classical Theories, revised edition, 1951, 358n. See also note 147 above.

178. In this section I have omitted many references, since almost all the relevant literature is referred to in the interesting paper, R. C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism", *Isis*, 48, 1957, 33-50.

179. Pierre Duhem, *Leçons sur l'electricite et le magnetisme*, Paris, 1891-2, Vol. 3, 433.

180. Philipp Lenard, *Great Men of Science: A History of Scientific Progress*, 1933, 214.

181. H. Bence-Jones, *Life and Letters of M. Faraday*, 1870, n, 395:

> Professor Hansteen to Faraday.
>
> "Observatory, Christiania: December 30, 1857.

"... Professor Oersted was a man of genius, but he was a very unhappy experimenter; he could not manipulate instruments. He must always have an assistant, or one of his auditors who had easy hands, to arrange the experiment: I have often in this way assisted him as his auditor. Already in the former century there was a general thought that there was a great conformity, and perhaps identity, between the electrical and magnetical force; it was only the question how to demonstrate it by experiments. Oersted tried to place the wire of his galvanic battery perpendicular (at right angles) over the magnetic needle, but remarked no sensible motion. Once, after the end of his lecture, as he had used a strong galvanic battery to other experiments, he said "Let us now once, as the battery is in activity, try to place the wire parallel with the needle"; as this was made, he was quite struck with perplexity by seeing the needle making a great oscillation (almost at right angles with the magnetic meridian). Then he said, "Let us now invert the direction of the current", and the needle deviated in the contrary direction. Thus the great detection was made; and it has been said, not without reason, that "he tumbled over it by accident". He had not before any more idea than any other person that the force should be transversal. But as Lagrange has said of Newton in a similar occasion, "such accidents only meet persons who deserve them".

"You completed the detection by inverting the experiment by demonstrating that an electrical current can be excited by a magnet, and this was no accident, but a consequence of a clear idea. I pretermit your many later important detections, which will conserve your name with golden letters in the history of magnetism." [Compare this letter with James F. W. Johnston's interesting 'Men and Institutions in Copenhagen', *Edin. J. Sci.*, 1, 1830, 198ff.]

This letter refutes Lenard's silly suggestion that once the relevance of the current was guessed "the discovery then was easily made" (*loc. cit.*). Perhaps he made it on the strength of a text of Dr. J. A. Paris, *op. cit.*, 377, who, however, was too careful to state it explicitly, and who was not too credible on the matter anyway.

182. F. C. Bakewell, *Manual of Electricity*, 1859, 38.

183. Ørsted to Hansteen, quoted by Stauffer, *Isis*, 46, 1953, 309.

"Copenhagen July 22, 1820

"I feel a special pleasure at being able to send you, my greatly respected friend, the accompanying communications concerning the magnetic effects of galvanism. It appears to me that the consequences of my discovery could be very extensive. Let me hear your opinion about this soon.

"*totus tuus*

"H. C. Ørsted"

This covering letter cannot help decide whether Hansteen was present at the lecture he describes in his letter to Faraday. Stauffer's first essay (note 26) deems Hansteen's story very plausible but not eyewitness evidence; his second essay (note 178) accepts Ørsted's claim that the discovery was no accident. He is not inconsistent here: his question is historical: did Ørsted make a prediction or not? He makes no attempt at a rational reconstruction. (See also note 59 above.)

184. *Op. cit.*, 81. On the previous pages Berzelius' speculations are recorded; but instead of discussing the possible relevance they might have to Ørsted's discovery,

Whittaker provides some apology for their incorrectness and changes the subject commenting (80),

> "It is scarcely expected that anything so speculative as Berzelius' electric conception of chemical combination would be confirmed in all particulars by subsequent discovery; and, as a matter of fact it did not as a coherent theory survive the lifetime of its author. But some of its ideas have persisted such as the conviction which lies at its foundation, that chemical affinities are, in the last resort, of electrical origin."

(Berzelius' theory "did not as a coherent theory survive the lifetime of its author" because Faraday developed Ørsted's idea and performed a series of crucial experiments; cf. note 57 above.) Yet the "conviction … that chemical affinities are … electric" rested on the discovery of electrochemistry (1800) and is universally held ever since. It led to the problems studied by Davy, Berzelius, Ørsted, and others; but Whittaker scarcely reports problems. And he ignores the difference of opinion between Ørsted and Berzelius, just as he ignores Ampère's claim that electromagnetism is demonstrably impossible — a claim quoted by Stauffer. (See also note 59 above.)

185. The similarity between Ørsted's ideas and the Kant-Boscovitch model may seem obvious; the difference should also be stressed: the different manifestations of the primordial force are not functions of distance alone, but of unknown set-ups; this is an intermediate step towards electromagnetic fields. Priestley's anticipation of Ørsted's idea of the relation between galvanism and magnetism reported in Robert E. Schofield, "The Scientific Background of Joseph Priestley", *Annals of Science*, 13, 1957, 158, may be due to his following Boscovitch.

186. The following are the reports that Ørsted has published. Their variance is of great interest. (Even the use of third person singular in the third and most definitive is.)

I. "The first experiments respecting the subject which I mean at present to explain, were made by me last winter, while lecturing on electricity, galvanism, and magnetism, in the University … But as these experiments were made with a feeble apparatus, and were not, therefore, sufficiently conclusive, considering the importance of the subject, I associated myself with my friend Esmarck to repeat and extend them by means of a very powerful galvanic battery, provided by us in common. Mr. Wleugel, a Knight of the Order of Danneborg, and at the head of the Pilots, was present at, and assisted in, the experiments. There were present likewise Mr. Hauch, a man very well skilled in the Natural Sciences, Mr. Reinhardt, Professor of Natural History, Mr. Jacobsen, Professor of Medicine, and that very skilful chemist Mr. Zeise, Doctor of Philosophy. I had made experiments by myself; but every fact which I had observed was repeated in the presence of these gentlemen."

II. "Since for a long time I had regarded the forces which manifest themselves in electricity as the general forces of nature, I had to derive the magnetic effects from them also. As proof that I accepted this consequence completely, I can cite the following passage from my *Recherches sur l'identite chimiques et electriques*, printed at Paris, 1813. 'It must be tested whether electricity in its most latent state has any action on the magnet as such.' I wrote this during a journey, so that I could not easily undertake the experiments; not to mention that the way to make them was not at all clear to me at that time, all my attention being applied to the development of a system of chemistry. I still remember that, somewhat inconsistently, I expected the predicted effect particularly from the discharge of a large electric battery and moreover only hoped for a weak magnetic effect. Therefore I did not pursue with proper zeal the thoughts I had conceived; I was brought back to them through my lectures on electricity, galvanism, and magnetism in the spring of 1820. The auditors were mostly men already considerably advanced in science; so these lectures and the preparatory reflections led me on to deeper investigations than those which are admissible in ordinary lectures. Thus my former conviction of the identity of electrical and magnetic forces developed with new clarity, and I resolved to test my opinion by experiment. The preparations for this were made on a day in which I had to give a lecture the same evening. I there showed Canton's experiment on the influence of chemical effects on the magnetic state of iron. I called attention to the variations of the magnetic needle during a thunderstorm, and at the same time I set forth the conjecture that an electric discharge could act on a magnetic needle placed outside the galvanic circuit. I then resolved to make the experiment. Since I expected the greatest effect from a discharge associated with incandescence, I inserted in the circuit a very fine platinum wire above the place where the needle was located. The effect was certainly unmistakable, but still it seemed to me so confused that I postponed further investigation to a time when I hoped to have more leisure. At the beginning of July these experiments were resumed and continued without interruption until I arrived at the results which have been published."

III. "In composing the lecture, in which he was to treat of the analogy between magnetism and electricity, he conjectured, that if it were possible to produce any magnetical effect by electricity, this could not be in the direction of the current, since this had been so often tried in vain, but that it must be produced by a lateral action … The observations … of magnetical effects produced by lightning, in steel-needles not immediately struck, confirmed him in his opinion. He was nevertheless far from expecting a great magnetical effect of the galvanical pile; and still he supposed that a power, sufficient to make the conducting wire glowing, might be required. The plan of the first experiment was, to make the current of a little galvanic trough apparatus, commonly used in his lectures, pass through a very thin platina wire, which was placed over a compass covered with glass. The preparations for the experiments were made, but some accident having hindered him from trying it before the lecture, he intended to defer it to another opportunity: yet during the lecture, the probability of its success appeared stronger, so that he made the first experiment in the presence of the audience. The magnetical needle, though included in a box, was disturbed; but as the effect was very feeble, and must, before its law was discovered, seem very irregular, the experiment made no strong impression on the audience. It may appear strange, that the discoverer made no further experiments upon the subject during three months; he himself finds it difficult enough to conceive it; but the extreme feebleness and seeming confusion of the phenomena in the first experiment, the remembrance of the numerous errors committed upon this subject by earlier philosophers, and particularly by his friend Ritter, the claim such a matter has to be treated with earnest attention, may have determined him to delay his researches to a more convenient time. In the month of July 1820, he again resumed the experiment, making use of a much more considerable galvanical apparatus. The success was now evident, yet the effects were still feeble in the first repetitions of the experiment, because he employed only very thin wires, supposing that the magnetical effect would not take place, when heat and light were not produced by the galvanical current; but he soon found that conductors of a great diameter give much more effect … "

187. R. C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism", *Isis*, 48, 1957, 35. All dogmatists identify every heterodox view with the official heterodoxy; just as for many Communists all deviationists are Trotskyites, so all deviationist Newtonians were Cartesians in the

eighteenth century and *Naturphilosophen* in the nineteenth. It is alleged that Helmholtz's paper on conservation of force (that is a kind of reconciliation between the orthodox Newtonian views and those of Ørsted and of Faraday) was rejected because the various editors regarded Helmholtz an advocate of *Naturphilosophie*. Whittaker, *op. cit.*, 1951, 214n, endorses this allegation. It is equally unreliable. (See also note 59 above.)

188. Lenard (*loc. cit.*) suggests, perhaps correctly, that Ørsted does not even state explicitly when the discovery was made, in winter or in spring 1820. If we allow that he did not even claim to have made the discovery before July 1820, then the reference to a public lecture of Professor Gilbert, the German translator of Ørsted's report (see text), must be to the demonstration before the distinguished gentlemen; this would accord with Hansteen's view.

189. Presumably Hansteen was aware of the demonstration with irregular results in the lecture in the winter of 1820. All evidence is explicable on the assumption that he was speaking of the demonstration before the distinguished audience. In that demonstration, according to Ørsted, the assistant was Esmarck; it is not known whether Hansteen was present too. What has greatly puzzled me is why Hansteen wrote to Faraday about the discovery. Faraday's letter to Hansteen, to which Hansteen was replying, has no reference to Ørsted. Was Ørsted perhaps mentioned in the conversation Faraday had had with the person whom Hansteen had sent to him before their brief correspondence started? Or was Hansteen eager to belittle his teacher? The second assumption is, I think, much less credible.

190. Popper's model of explanation is his *The Logic of Scientific Discovery*, 1959, Section 12; his application of it to historical explanation is in his *The Poverty of Historicism,* 1957, Sections 30-32, and in his *The Open Society and Its Enemies*, Chapter 25.

191. The problem leads to interesting results in that it undercuts historicism by showing a possible root of its attraction in the erroneous idea that as history makes no use of universal laws it needs its own laws.

192. The comforting power of inductivism is enormous. "We do not understand all the processes involved in the formulation of working hypotheses," write Duane Roller and Duane H. D. Roller in *Harvard Case Histories*, Vol. 2, 572, although "it is usually clear — as in the present case [of Stephen Grey's "working" hypothesis] — that they stem from observation and experiment …"

Explanations according to origin were once very popular; Popper discusses the pedigree aspect of this in his "On the Sources of Our Knowledge", *The Indian Journal of Philosophy*, 1, 1959. 3-7. In social anthropology this kind of explanation has been discarded since the advent of functionalism. The refutations of the theory that all good scientific theories originate from fact are abundant.

"Strange!" writes Voltaire, "we know not how the earth produces blades of grass: how a woman conceives a child; and yet we pretend to know how ideas are produced!" *Newton Versus Leibnitz*. 1764, 59.

193. The problem of induction is, how do we learn from facts? The problem of the justification of induction is, how can facts help us to show that our theories are true (probable, sound, reasonable)? Taking the second problem seriously is a sensible approach only if we ignore the first; for the answer to the first entails the answer to the second. Maxwell — who was seriously concerned with the problem of justifying induction — explicitly discarded the problem of the origin of scientific theories as irrelevant. "Mere speculations", he asserted, *Scientific Papers*, 1890, Vol. 1, 189, "may be turned to account in experimental science." And he spoke (*ibid.*, Vol. 2,

211) of atomism as "the branch of physics which not long ago would have been considered a branch of metaphysics."

194. Popper calls this the magical attitude towards science. We take it for granted that present-day scientific theories are better than old ones and better than present-day non-scientific theories; for the former were designed chiefly to avoid the pitfalls found in the latter. Historians of science defend present-day science by beautifying and romanticizing its history; this way they do it a disservice. For, they at once conceal the problems and difficulties on its way, and miss the opportunity of showing the merit of present-day science by comparison with those of yesteryear. It was Cohen's brilliant idea to open his *The Birth of a New Physics,* Garden City NJ, 1960, with a discussion of the fact that, unnoticed, commonsense is still geocentric, and with "the need for a new physics." The fault lies even deeper. It is the dogmatic manner in which we teach schoolchildren about Archimedes and Lavoisier — a way that makes it hard to avoid being wise after the event. I shall not enter this here, but merely quote a passage expressing an idea that ought to be less rarely noticed (H. Visick, "The place of science in the school curriculum", *Journal of Education,* University of Hong Kong, 1954, 12, 35):

> "... in discussions on science teaching in schools we raise the question of syllabus and method but usually ignore a more fundamental question. The how and what of science teaching are dependent on the why, and the primary question for educationalists, administrators and teachers is "why should we teach science?" On the answer we give to that question will depend the extent, method, and substance of our teaching of science. This basic question is not often raised nowadays, for Science in the last half century has risen from neglect to the position of one of the most important subjects in the school curriculum. It has become in spite of much opposition an established tradition. Tradition rightly plays a great part in determining the scope of education, for education must be a stabilizing force in a community. But tradition, unanalyzed and unappraised, can be a brake on essential progress especially in these days of an accelerated rate of change when quick adaptation to changing social conditions is essential. So it is very important to consider afresh what can be said for the modern tradition of science teaching."

195. "How are experiments to be mistrusted, after [their] imposing [errors] on Boyle and Newton!" Voltaire, *op. cit.*, 62.

III.

HISTORIOGRAPHIC ESSAYS

1. A Retrospect

One of the greatest monuments to the human intellect is the Babylonian Talmud, compiled, we are told, about the year 500. In it the two chief compilers congratulate themselves, with understandable pride, saying, we are no mere woodcutters in the swamp. The chief compiler's father is characterized elsewhere in that vast work as a master dialectician. He was, it is said, one who would raise a whole palm-tree and then take an axe to it. The question, what good does it do to put all the labor required into the act, what is gained by raising a palm-tree and then cutting it down, this question could not possibly occur to Talmudic scholars. They knew that preoccupation with the Law is the highest form of activity, the best way of life. The idea of progress was alien to them, if not actually distasteful. Echoing Plato, the Talmud says, if the former generations were like angels we, the later ones, are human; if the former were human, we the later ones are like asses. Yet, clearly, they saw in the corpus that was compiled an achievement. How does one reconcile the idea of progress with a static system of thought? I do not know. I do not think it possible.

The question is not, is the compilation of this or that scholarly work progress? The question is, does a certain philosophy permit progress at all? This question is a bit vague. Clearly, the Talmudists denied the possibility of progress in some sense yet affirmed the possibility of progress in another sense. Moreover, both views are stated quite emphatically. Nor is there a need to declare this a contradiction. Clearly, before one could judge matters one should be told in what sense the Talmudists saw progress as impossible and in what sense they saw progress as possible and even present. It is hard to say; at least I find it so. Clearly, in the sense in which I can elucidate matters somehow, I may employ the jargon popular today among philosophers and historians of science, a jargon usually ascribed to Thomas S. Kuhn, who made it trendy. But I am using here a popular jargon, not Kuhn's own views, since there is no consensus about them. I have heard him often say he was misrepresented, and I would rather not represent him at all here because of that recurrent complaint of his. And as a jargon word it has a loose meaning — which fully accords with Kuhn's recurrent complaint.

Well, then. The Talmudic scholars operated within a paradigm. They denied that progress is possible in the sense that they deemed the paradigm absolute, contrary to contemporary views of paradigms as transient. The reason they saw the paradigm as unalterable is, of course, their attribution of divine origin to it. They felt, quite rightly in my opinion, that without divine authority it is frivolous to cling to a paradigm. One may, of course, adhere to a paradigm for pragmatic ends. This possibility is, historically, the most popular second option. The two options, incidentally, are known in the philosophic literature by a number of catch-phrases, such as nature and convention, absolutism and relativism, dogmatism and pragmatism.

Common to absolutism and relativism is the idea that progress remains within a paradigm. What they differ about is the number of paradigms available, whether it is one or many, to use another philosophical catch-phrase.

As historians we may wonder, what offers a historian more scope, the one or the many. The attraction of the many comes forth: many paradigms present many epochs: from the very start the historical dimension is introduced. Yet this is the historian's view. The philosopher's view is quite the contrary: the one is the resting place, the home our spirit yearns for; the many only disturb us by placing before us an insoluble problem of choice: which of the many should I choose? The historian does not choose. For the historian, as a fact, Tom was a mechanist and Dick a vitalist, Reuben a plenist and Simon a vacuist. It is given; it is no problem of choice. Some great thinkers have tried to make use of this fact, to approach philosophy historically. Prominent among them is Martin Buber who claimed that the choice of a religious paradigm is a matter of historical fact. Following him Michael Polanyi declared that the choice of a scientific paradigm is also a matter of historical fact. Polanyi did not mention Buber, as far as I know, but he used Buber's unmistakable idiom. Anyway, their idea is that we can choose only those systems, conventions or paradigms, that exist as cultures that some living communities practice. Moreover, they said, you need not like any existing option: you can try to alter it. But you can do that only from within, i.e., by endorsing it (or its paradigm) to begin with.

When comparing Talmudic scholars with Martin Buber, we may see a remarkable shift in the very concept of the paradigm. The Talmudists declared as central an unchangeable core doctrine, the divine word, and change and progress as occurring only under its umbrella, say, in practical applications, in interpretations, and further applications. According to Buber, and Polanyi too, as long as the socio-cultural base maintains continuity, all is well; the doctrinal changes are matters of the community in question to decide upon, and no outside philosopher, certainly not a Buber or a Polanyi, has the ability, much less the authority, to dictate any rule on that matter. (They were authorities within their traditions, but not meta-authorities. As Jean-François Lyotard has put it, there is no meta-narrative.)

This refusal to criticize a culture from the outside is, briefly, a cop-out. It is taking society as primary and its culture as secondary. This order of priorities limits the ability to understand the culture whose social background is under careful analysis. For, living societies face problems, problems rooted in their social backgrounds that are not soluble by looking at them. Problems beset any active member of any living community, be that community the Catholic Church, the local Jewish synagogue or the International Society for the History of Science. The problems are diverse. Some are soluble within the paradigm in a reasonable manner, some not; is reform then the order of the day? Neither Buber nor Polanyi had an answer. Neither had a criterion.

They even explicitly declared that no criterion exists but that the spiritual and intellectual leaderships have to take responsibility and act with no criterion to guide them. Buber and Polanyi knew that leaders err; they may then lose their position at the helm, said Polanyi. Much worse, they may take the whole ship to a reef, let me add. All this is useless to leaders Buber and Polanyi added. Averse to having advice rammed down their throats, they are not averse to advice, and not amused when advised not to seek any.

Perhaps I am too rash to suppose that they are willing to listen to advice before they make up their minds. As a budding philosopher studying science and moving in the company of normal scientists I was told many a time, and in a blunt manner not meant to spare my feelings, that philosophers of science are individuals trading in advice that is both unasked for and inherently useless. But what they meant thereby to affirm was their status as normal scientists, as individuals who take the paradigm of their scientific community as given, and as individuals happy to perform tasks valued by their community, no questions asked.

There is something terribly important shared by the ancient Talmudic scholars and today's normal scientists, something that I consider the root of their peace with themselves and profound inner sense of well-being. It is the obviousness of their choice. The individuals in question know that they had a choice of a paradigm. Many Talmudic scholars were steeped in the local culture within which they lived in exile, yet they never stopped to consider their choice: the moral and intellectual superiority of their Jewish paradigm over the local culture hardly invited articulation. The same holds for my scientific friends. They could choose religious teaching as their way of life or any other profession, but they chose science: they had a calling.

All this holds for the rank-and-file, for the normal scientist or the normal Talmudist. The leading ones faced more basic problems and sought criteria by which to face them. One of the very earliest Talmudist leaders, Raban Gamliel, decided to rely on scientifically calculated calendars in preference to traditional ways of determining dates by observing the new moon. He was challenged. He used his authority and demanded of his challenger to publicly yield to his — scientific — ruling. How did Raban Gamliel know that the heathen scientific calendar is reliable? How did St. Robert Cardinal Bellarmino know that the Copernican calendar is more reliable than the Julian calendar instituted by Raban Gamliel? (For, the Gregorian calendar is but a variant, well within the Julian paradigm.)

These are the obvious questions to ask anyone who is not quite content to be a normal member of a given intellectual community. Thomas S. Kuhn is right about one point in this context: he noticed that at times this question is not pressing, at other times it is. And when the pressing need for a change is felt and the leadership successfully implements change, then the sooner the rank-and-file take notice, the more easily they can pretend that nothing much has happened. That is to say, Kuhn views paradigm shifts from the viewpoint of the obedient rank-and-file. For, by definition, they are content to leave the big tough questions to others and merely follow in their wake. They want no advice from outsiders; they want instructions from their leaders. But not all rank-and-file can stay as obedient as they wish and as Polanyi and Kuhn describe. Kuhn wrote about the Copernican revolution and he wrote about the quantum revolution and in neither case did he notice the enormous qualms and perplexities that even simple and very normal researchers may suffer. When you are perplexed, says Maimonides, the author of the great *Guide to the Perplexed*, go and consult an acknowledged wise person. How do I know whether the acknowledged wise is wise? Once you ask this question, you are obedient normal rank-and-file no longer. The obedient normal rank-and-file have no trouble knowing this. You go to a convention and snoop around. In no time you find out. I think this is true. Appearances to the contrary notwithstanding, Thomas S. Kuhn's *The Essential Tension* is in fundamental accord with Maimonides' *Guide to the Perplexed*. Indeed, he said the adoption of a paradigm is an akin to the act of religious conversion. Life is much harder for one who rejects their counsel, to speak from personal experience, for the unable to follow people whom the community considers wise.

So much for background information. I apologize for my lengthy and round-about presentation, but only now do I feel comfortable — somewhat comfortable — coming to my point. My *Towards an Historiography of science* was published in 1963 and reissued in 1967. It is still popular — perhaps my biggest public success. I was and still am very happy with its reception. Yet looking back, I feel the need to say what is wrong with it and what I do not like about its message. My brief work presented the two paradigms concerning the writing of the history of the physical sciences, rejected them and proposed that the views of Karl Popper and some of the work of Alexandre Koyré and his associates offer a better paradigm. Readers concluded that I endorse Popper's paradigm with no qualification although I hint there that I prefer another paradigm, one that weds some ideas of my teacher Popper with ideas of Émile Meyerson and others whom Meyerson influenced, including Koyré and P.P. Wiener. I have expounded this paradigm elsewhere — in my *Science in Flux*, for example.

Some historians of science dismissed my *Towards an Historiography of Science* as small fry, either because they stuck to a paradigm I was criticizing or because I was an outsider. Also some critics who said kind and appreciative words about it criticized it from the viewpoint of a given paradigm — for example Nicholas Rescher and Edwin G. Boring. I will not respond to this, since the quarrel between paradigms is an elaborate, wearisome matter. I should also mention that some of those who said kind and appreciative words about my book complained that I was an outsider, particularly Kuhn and Derek J. de Solla Price. And, I suppose, they were right.

What characterizes an outsider, what is it to be an outsider, and why does it matter? Let me show that these questions signify, discuss them, and apply them to the place of my work in the literature.

The question signifies: reforms, to repeat, come from within a culture or a cultural community. Continuity, so goes popular prejudice, is maintained by the society in question sharing a paradigm ─ sharing a certain prejudice, I will say. To remain both flexible and prejudiced, a community needs an intelligent and a strong leadership. The leadership, then, prescribes a paradigm-shift when need be, and only it can do it. Query: what if an outsider offers a paradigm shift? So as to avoid comparing myself to other outsiders, and for presentational simplicity, let me present the biggest examples around. These are Moses the law-giver and Einstein the patent-tester. For more examples one can use Colin Wilson's *The Outsider*, though it is a cheap romantic essay, once extremely popular and now all but forgotten. The usual way to assimilate ideas of outsiders is for current leaders to endorse them. Aaron endorsed Moses, Planck endorsed Einstein. Smaller examples are more abundant but less useful as examples. Myself, I am no example at all. No leaders have endorsed my views. When I will present my dissatisfaction with my book, my satisfaction with this fact will be apparent.

Why, then, the complaint that I am an outsider? There is no problem if my views are useful and the leadership endorses them or useless and the leadership rejects them. Yet neither is the case. Why? I do not know, but I have a conjecture. The field of the history of science in general and of the history of the physical sciences in particular has no adequate leadership and it does not recognize any. True, there are the trappings of agreement about common and shared prejudices, trappings of a totem pole, of territorial claims respected by others, and so on. Yet all this is unsatisfactory and may be ignored.

Unsatisfactory to the rank-and-file who need leaders, I mean; not to those who would rather get lost without a leader than follow one. It is easy to ascertain that the field is still dominated by diverse paradigms, that a few authors are purists who stick consistently to the paradigm of their choice, and that a few write with disregard for all paradigms. (For example, Kuhn has offered a new parading for the writing of the history of science, but he did not follow it, as he explicitly said.) The followers of single paradigms or no paradigm have the virtue of consistency; the eclectics may be inconsistent, but then, as they announce exciting finds, others may clean their works of inconsistency. On the whole, what I propose as the Koyré-Popper paradigm is by now a reasonable contender in this multi-paradigm field, and I am proud of my little share in this change. Not to lose my sense of proportion, let me observe the immense popularity of Kuhn's paradigm (not discussed in my *Historiography* that appeared but one year later). It is more popular than dominant: there still is no dominant paradigm in the field.

What characterizes an outsider, what makes one an outsider, and why does it matter? I take my own case as an example. My books and research papers in the field of the history of science proper did not make me an insider. I follow a venerable precedent here. In his later years Einstein still counted as a physicist, yet neither rank-and-file nor a leader. So he counted as an outsider. I still remember the sense of shock I experienced when I learned that my physics teacher had no wish to read Einstein's latest. An outsider, then, is neither a leader nor rank-and-file. Most outsiders, of course, simply play no roles in the field, but not all of them: those who have something to contribute not endorsed by the leadership may also be viewed as outsiders. What keeps them outsiders is a different matter.

The interesting cases are always the problematic ones. For example, a person may be a partial insider, as was Einstein. Michael Faraday's case was more complex: he was very much of an insider as experimentalist and an outsider as theoretician. James Clerk Maxwell, his leading disciple, was surprised to learn that he was a theoretician. Yet this was kept secret. This secret is still not known. This is what is called, since Watergate, the cover-up of the cover-up. It includes the refusal of reviewers of my *Faraday as a Natural Philosopher* to report this the book's message.

This brings me to the heart of the matter, namely to the significance of it all. Although I do not like the institution of intellectual leadership, I do recognize the existence of communities of scholars, and the need of any community to have leaders. I also recognize that communities are often identified by shared prejudices, by paradigms, but I dislike and consider no longer valid for pluralist society.

Communities often play certain social or intellectual roles and their leaders need to know the consensus of their communities on these matters. Kuhn says, the role of rank-and-file scientists is to solve certain problems that serve the community at large: the theoretical investigations of leading scientists, then, serve the rank-and-file in their discharge of their recognized task. If this is what Kuhn says, then he articulates a sentiment widely held by physicists in the period of large-scale United States Federal Government research and development funding, roughly between the rise of the Manhattan Project and the big budgetary cuts. If so, Kuhn is out-of-date by now. If this is not what he says, then I do not know what the task of puzzle-solving is that the rank-and-file scientist's are supposed to perform. One way or another, this question, what is the current role of the community of physicists, is an example of the general question presented here, perhaps also of the subsequent or the derivative question, what is the priority order of today's agenda within physics?

All this concerns the history of physics, not physics. And I have argued in my *Historiography* that regrettably the task that many historians of physics undertake is to orchestrate the process of hero worship that the community at large is supposed to perform with the scientist as the hero. I find this task distasteful. I admire Popper and Koyré who, following Einstein, have taken it as a central idea that an error can be an excellent intellectual adventure and a great contribution to the progress of science. Yet this view is still quite unpopular, and the ascription of an error to a thinker is quite often taken as a way to belittle the contribution of that thinker. This is intolerable and intolerably common in the literature of the history of science. My distaste for hero worship, although I share it with a few historians of science, still brands me an outsider in a way: those who perform the task of presenting scientists as if they had never made an error do not like to be told what it is. Nor are they wrong: many social tasks suffer an ambiguity that is essential to their proper functioning.

Such social tasks may be necessary (I hope not), but they are never enjoyable. And what I tried to show is that historians of science have better and more enjoyable tasks to perform — and I offered examples. This put me in the position of an underground advisor for quite a few historians of science who, I know, have read my work with pleasure, and deem it beneficial for their work, yet on the condition that they pass over this in silence. As historians they know that future historians may, if they put their mind to it, discover some unmentioned facts. But as members of their community they know what is useful for them to do and what not.

A radical change took place in the writing of the history of the exact sciences in the last few decades. Koyré and Popper are the chief sources of inspiration here. Even historians of science who reject Popper's ideas, such as Gerald Holton, admit this, not to mention Kuhn, who said clearly both that he had no quarrel with Popper and the contrary. Philosophers and historians of science, notably Dudley Shapere, consider the in-depth accord between Popper and Kuhn despite surface difference harbinger of the new paradigm.

This is the time for me to quit. When I wrote my *Historiography* I defended Popper as an outsider. Now he is increasingly becoming an insider and I want to disengage from him. Here, briefly, is my central point of dissent, written from the viewpoint of particular interest in the history of the exact sciences.

What gain is there in raising a palm tree and taking an axe to it? What gain is there in the game of conjectures and refutations? Popper identified it with the game of scientific research; his answer then is, the gain is science and its fruits.

My major critique of Popper is this. Some conjectures are worthless though highly refutable and at times refuted; other conjectures are of much (technological) use although of little (scientific) interest; they concern only engineers, and possibly also mathematicians, ones concerned with complex differential equations, approximations and their theory, and more. Here I wish to stay a little with science as an intellectual adventure à la Popper, as the major point of criticism of his view of science.

Popper opposes scholasticism or Talmudism, as do most philosophers of science. It is well-known that scholastics dogmatically cling to a petrified paradigm and merely add to it patchwork upon patchwork. Dogmatism was characterized by Bacon as disregard for refutation and as making light of it and as meeting it by minimal adjustments: "frivolous distinctions" is his memorable expression. Amazingly, W. V. Quine endorsed this view. I say "amazingly" because the Duhem-Quine thesis is the claim that one can always both be dogmatic and accept criticism, which claim is an obvious logical truth: Duhem proved it with ease.

Popper advocated conjectures and refutations while rejecting the scholastic disposition to make light of criticism; he had to explain how. He did. He said, scientific conjectures are highly refutable, scholastic ones are not. This is very beautiful; yet it is false. For example, it is possible to amend the Newtonian law of gravity by adding to the inverse square an inverse cube (and other factors if need be) and this amendment is practiced in space-programs. It is very useful, highly refutable, yet, scientifically viewed, it is scholastic. Einstein said repeatedly, he never considered it a serious option. Popper, therefore, was in error. He said he favored bold conjectures and he hoped that boldness is assured by high refutability. Not quite, though.

Popper also said, scholastics cling to their views while forgetting the problems that they were meant to solve. This is true in social studies. Thus, radicalism was endorsed as a quick solution to social ills and Herbert Marcuse clung to it despite his acknowledgement that no quick fix is possible. Is the same true of scholasticism in science? If science is explanatory, then, yes, Ptolemy's astronomy still explains what it was meant to explain. But if science is refutable explanations, which is Popper's choice, then no, ptolemy's theory is irrefutable as Norwood Russell Hanson argued in detail (*Constellations and Conjectures*, 1973). Can explanatory power and refutability diverge? Popper said no, and he was in error.

In science conjectures are usually chosen within some paradigm, until it gets tired and exhausted: it is no longer a source of inspiration for new conjectures that may explain older ones. The existence of hierarchies of theories as series of approximations is the reason we may consider them both as series of conjectures and refutations and as progress — in explanation and in shifts from good paradigm to better ones. Historians of science should not conceal refutation, but they may ask, what ground was gained as our heroes raised a whole grove of palm trees only to take axes to them? No one seriously questions that science advances in diverse ways. This is one factor, but only one factor, that makes the history of science exciting. My *Historiography* was fairly adequate as a guide, but not all the way. If science is, among other things, liberating our thinking from past constraints, and if some individuals are helped by it to become autonomous, then the future autonomous historians of science will have enough exciting work on their hands.

Perhaps the point from my *Historiography* that I like best is that of asserting autonomy — of both the historians and their heroes — by the clear contrast of the opinions they happen to represent and their own opinions. To my regret, till today too many historians — especially those who are still under the influence of Henry Guerlac — identify with their heroes instead of sympathizing with them, worship them and beautify their results in violation of scholarly custom while claiming the status of scholars. We should know that our heroes advocated views quite differently from ours, and we should present their work both from their won viewpoints and from ours. In writing my study of Faraday I attempted to do this, critically presenting his social background, his inner conflicts, his methodology and his paradigm — while reminding my readers that all of Faraday's ideas are by now superseded. The ideal history of science today, I still think, is the very opposite of identifying one's ideas with the ideas of one's hero. We should be problem-oriented and record that most past theories are refuted, and do so without being apologetic in the least. Yet this is not enough, and I wish to supplement my earlier work. We may want to contrast our own way of choosing problems with how past thinkers chose theirs, and how they approached them. At times this would lead us to consider social history, at times the spirit of the age, including the popular prejudices shared by our heroes, including, particularly, their metaphysical and methodological views, and always in contrast with today's. We may wish to offer integrated portraits intellectual portraits of our heroes, but we have to stay on guard: no person is fully integrated and portraits should not do too much.

Some studies approach this idea. The current concerted studies of Galileo and of Newton, in particular, come close to it. But as yet too few historians notice that Newton was influenced by conflicting philosophies — by mystic doctrines, by inductivism, and by Cartesian ideas, and more. To ascribe conflicting views to our heroes and to ask how much these ideas helped and how much they hindered the progress of science is still regrettably uncommon. The history of science is as open and challenging as ever.

2. The Place of Metaphysics in the Historiography of Science

Historical and Epistemological Background

The legitimacy of the history of speculative metaphysics as a component in the field of the history of science follows a sweeping and far-reaching revolution: the field has thereby shed its two major traditional characteristics, the servility to the inductivist methodology and the hostility to speculative metaphysics prescribed by this methodology. We are in debt for this to a few historians of physics: E. A. Burtt, Alexandre Koyré, Max Jammer and I. Bernard Cohen.

The most popular view of science is the inductivism that Sir Francis Bacon fully articulated in 1620: scientific ideas emerge slowly out of their empirical basis. This idea was still the dominant one among historians of science in the mid-twentieth century though it was overturned already in the early nineteenth century by William Whewell, the once celebrated and then forgotten great philosopher and historian of science. He noted the influence of metaphysics on scientific research. His example was Kepler's Platonism. He used this example to refute Baconian inductivism and legitimize heuristic (this word was his invention), his view of metaphysics as ancillary to scientific discovery. According to Bacon and his followers, a theory is inductively justified by reference to its past history, by the claim that it has emerged out of experience. Whewell refuted this.

Whewell proposed an alternative: regardless of their source, he said, theories are put to rigorous tests and the rare ones among them that pass the tests successfully are then declared scientific, verified, proven, certain. The inductive basis of a theory, he said already in 1835, is therefore divorced from the history of its origins, and its verification or proof is of necessity posterior to its invention. Under the pressure of Einstein's replacement of Newtonian mechanics the view that a theory comes before its verification had to be admitted, and the idea had to be given up that success in passing empirical tests is full proof. As final empirical proof or final verification was given up, it was declared that the hallmark of the empirical success of a theory is not its verification, but its mere probability. This was described in detail in the classical work of John Maynard Keynes (1920). But something was lost in the reshuffle. First, whereas by Whewell this success is having standing up to tests, by Keynes it is logical probability, which depends on some relations between any given theory and all available evidence at the time of the assessment of its probability. Second, Keynes ignored metaphysics altogether; he did not denounce metaphysics, but he did not legitimize it either — as heuristic or as anything else. This was re-established only in the second half of the twentieth century.

This idea of Whewell, that scientific status is granted a theory not by reference to its origin as Bacon had taught but by reference to its success in subsequent tests, is usually ascribed these days to Hans Reichenbach, who initiated the famous slogan, of any idea, the context of its discovery differs from the context of its justification. This is significant to our story, as Reichenbach was notoriously hostile to metaphysics and so his influence blocked the possibility that Whewell's writings would influence thought concerning the role of metaphysics in the growth of science. Yet possibly this is why an idea that was clearly and forcefully advocated by Whewell in the mid-nineteenth century is regularly ascribed not to him but to Reichenbach. Yet even if we leave Whewell in his historical limbo, we should note that Reichenbach was preceded by Keynes. So Whewell's idea of heuristic lied dormant until the historians of science developed it a century later. The reason is this. Whewell justified the intellectual framework of science in the traditional way, not by empirical proof but by *a priori* proof. This was reasonable at the time, but not later, since the major component of any intellectual framework for physics was the theory of space, and the classical theory of space was viewed then as proven by Euclid and Newton. This was altered with the discovery of non-Euclidean geometry. Thinkers were reluctant to admit Kant's view of the knowledge of it as synthetic yet *a priori* valid when they knew that Euclidean geometry is not the only conceivable one. All this was vividly described in Bertrand Russell's terrific first vintage, his *The Foundations of Geometry* of 1896.

Soon afterwards Henri Poincaré pointed a new possible way: he showed that geometry cannot by itself be tested, and if any conjunction of it with other propositions will meet a refutation, then it will be possible to explain the failure as due to the other propositions, so that it can always be saved. Hence, he said, it is not open to empirical refutation, and so, by fairness, geometry is also not open to empirical support. It is, he said, chosen to stay as a mere convenience. This, he said, holds for any statement taken in isolation. (This is known as the Duhem-Quine thesis.) Hence, we may decide to rescue any law of physics from refutation and consider it a part of our *a priori* intellectual framework. To take a concrete example, just then the law of conservation of energy in the version that Poincaré knew and applied to the conduct of uranium salts was refuted. It was at once rescued, of course, by adding to the list of possible energies nuclear energy. Hence, he said (*Science and Hypothesis*, Chapter 5), the law without the specifications is irrefutable. It is "almost a tautology", he said.

Nevertheless, the privileged status of Euclidean geometry is gone. It was abolished by Einstein in 1917 (the theory of general relativity) or in 1919 (Eddington's eclipse observation). In 1920 Keynes tried to justify science as probable on empirical grounds while ignoring its intellectual framework. (At about the same time Sir Harold Jeffreys offered similar ideas, and even made allowance for background hypotheses, but he did not elaborate and this aspect of his work remained unnoticed.) The climate of opinion in the period was hostile to metaphysics. And so the role of the framework was neglected; it was left to E. A. Burtt, Alexandre Koyré, Max Jammer and I. Bernard Cohen to show it in history. This is clearly indicated in the very titles of their books: that of Burtt is *The Metaphysical Foundations of Classical Physical Science: Historical and Critical Essay*, 1924, 1932; of Jammer is *Concepts of Space: The History of Theories of Space in Physics*, 1954 (see also Einstein's magnificent preface to that book; it deserves song and dance); of Cohen is *Franklin and Newton: An Inquiry Into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof*, 1954, and of Koyré, *From the Closed World to the Infinite Universe*, 1957. Burtt's classic was ignored for decades; Koyré, more than anyone else, changed the fashion: his *Galilean Studies* of 1939 started the methodology of the new historiography of science as it describes the trial and error of empirical research as firmly planted in its intellectual framework.

Background Material for Historians of Science

The quantity of writing on the history of science has increased in the second half of the twentieth century beyond expectation; it branched out and developed in many new ways. Of the many innovations, none is intellectually as significant as the presentation of the history of metaphysics as a component of the history of scientific research: it was the wedding of the history of science with the history of ideas, contrary to the inductivist taboo. Since few have made this revolution, the references to the literature on it are quite scant. In his *From the Closed World* Koyré refers in this context only to Burtt and Jammer! The question here then is not historical but pragmatic: how much non-science should a historian of science notice, and why?

Today the influence of the pioneers mentioned here is a part of the tradition; the picture has altered irreversibly, and historians of science can go back to the classics in attempts to quarry from them whatever ideas they can use in their studies. And they can present all sorts of additional material that they find in the archives. Of course, such an activity can lack discrimination, and then it might be disappointing. There is no guarantee for success, anyway, in the introduction of items from the history of ideas into the history of science or anywhere else, and every brilliant move can lead to poor imitations of it. So why is there any value to the study of the history of scientific ideas in the light of the history of different ideas? It may be particularly useful to ask, what sort of metaphysical ideas are historians of science advised to examine?

Examples should be useful here. The most conspicuous example is from the history of religious ideas, particularly religious philosophy and theology: did any of these ideas influence the growth of science? Usually scientists had religious upbringing; most of them belonged to one Christian denomination or another; most of them also practiced their religion, though some of them were irreligious, and some of them were even anti-religious. The question therefore naturally arises: does this matter? In some cases the answer is in the affirmative: Some of Darwin's disciples were anti-religious and this did color their work. Are there also instances in the opposite direction? Did the religious background of science play any role in the development of any of its ideas? The historiographic point of this example is dual. First, traditional historians of science avoided this question by writing as if religious ideas do not exist; were they right? Second, in the wake of Robert K. Merton, sociological studies of the religious background of science are popular; they are irrelevant to the present inquiry, as it concerns interrelations of scientific and non-scientific ideas, not of the conditions that made science possible.

Some sociology of science is obvious. A conspicuous example is the case of Giordano Bruno, who was condemned by the inquisition for heresy for his view that the universe is infinite. He was burnt on the stake in the year 1600. The chief inquisitor responsible for his legal murder was St. Robert, Cardinal Bellarmino, who also threatened Galileo and demanded that he should desist from teaching the Copernican hypothesis as true, though he himself instructed his underlings to teach it as a mere instrument for prediction. Such an atmosphere is obviously constrains the free practice of scientific research. Some writers have suggested that as a result of Catholic intolerance, the center of scientific research traveled from Catholic Italy to Protestant England. This is problematic, since on the way the center stopped at Catholic France, and at least Descartes admitted that he feared prosecutions and therefore also postponed the publication of his more daring works. Let us not go further into the discussion of this matter (see however Michael Segre, *In the Wake of Galileo*, 1991): it is not relevant to the question, was the development of scientific ideas helped by theological ones? This question was taken up by Koyré, who studied the influence that Jacob Bohme has exerted on Newton, and by Jammer, who presented the influence on Newton of some Cabalistic ideas about space.

Though these ideas influenced Newton, they were not in any way comparable in their value to those of Kepler and Galileo, that influenced him much more decisively. This should be said, and it not having been said clearly and emphatically enough raised the protest of Edward Rosen, the great Copernicus scholar who was a fairly orthodox Baconian. He denounced the allegations that Copernicus was under the influence of mystical ideas, and he did so because he found this humiliating this great thinker. He had a point. A reasonably competent historian of science has to be familiar with some scientific ideas in some depth, that is to say, with some instance of some sort of scientific thinking. Equipped this way, one may be blind to the high quality of Greek (Pre-Socratic) metaphysics, but not to the low quality of mysticism, including standard Cabbala, and the notorious *Naturphilosophie* that in the late eighteenth century and the early nineteenth century Schelling, Fichte and Hegel dished out. Now it is usually not recorded in standard histories of science that Schelling influenced Hans Christian Ørsted, the great discoverer of electro-magnetism, one of the greatest experimental scientists of all times.

Such historical facts, to repeat, seemed to Rosen not worthy of mention; their mention, moreover, seemed to him to demean science. Was he right? This cannot be judged without the examination of the simple question, how can an inferior thinker significantly influence a superior one?

To see this we may return to the background of scientific research not necessarily related to its working, at least not directly and not forcefully, such as the influence of some religious ideas on research in physics. Still, scientists did discuss the laws of nature as they understood them at the time as evidence for the existence of God. Was that of no significance for dispute *Psalm 19*, "The sky tells the glory of the Lord, etc.", that Giovanni Pico della Mirandola cited (*Oration on Human Dignity*) as an argument in favor of scientific and technological research. Different astronomers read it in ways that support their diverse views. Is this relevant or irrelevant to the history of scientific research?

George Sarton suggested that historians of science should add to the history of science proper some local color, to provide their stories with human touch. Perhaps this is condescending, and then it is not too pleasant. Yet the alternative is not too pleasant ether. A text in the history of science that describes first only empirical facts and then some related theories sounds as if the individuals whose findings are thus described were all orthodox Baconians. This is why inductivist histories of science are at their best when describing eighteenth-century science, since that was the high tide of Baconianism within the scientific community. In a historical chapter written by the mighty Pierre Duhem, actors sound like instrumentalists philosophers of science, and this is why histories of science of his kind are at their best when describing mediaeval science, since that was the high tide of instrumentalism within the learned community. Yet the mediaeval flavor of the writings that he and his followers use is so sharply expurgated in their reports on them, that contrasting the color of the original texts with their reconstructions is bound to shock ingenuous readers.

It is not that reconstruction is forbidden. It is excessive to demand from a historian of science to cite Newton's or Maxwell's mathematical formulae in the original wording rather than in translation into modern idiom. Yet this too is distortion. For, many classical theorems initially gained with much blood and sweat, now look obvious because of terminological innovations. Terminological innovations often incorporate novel characteristics of the mathematics involved. Think of the advantages of the vector notation or, even better, the tensor notation, of electrodynamics, as compared with the traditional language of partial differential equations in which they appeared in Maxwell's original publications. Maxwell's admirers should admit by that some of Duhem's criticism of him is valid. This is not to endorse his dismissal of Maxwell, of course. Indeed, it is amazing how severe Duhem was to his rivals, especially in view of his leniency to his revered predecessors.

Distortion is inevitable, but this is no license to willful distortion, and so we do not want to cover up the inevitable distortions, and so we should not hide them from our readers. We need not fuss about matters. Historians of literature need not repeatedly remind their readers that the very act of reading was much facilitated by myriads of innovations less than two centuries old. But at least readers are informed of this and have some idea of what this means in terms of the publication and marketing. Historians of science do not describe science as it appeared in different periods, how philosophy, metaphysics, even literature, made science appear so differently then. Not nearly enough attention is paid by historians of science to the help that good encyclopedias rendered to science, for example. How much of all this do historians of science owe it to tell to their readers? When Edward Rosen reported the bare facts of Copernicanism without reference to its historical background, he did not mean to exclude discussion of the poverty of the empirical techniques that astronomers employed at the time, the poverty of the system of scientific or any other communication at the time, yet he did intend to exclude the religious ideas of Copernicus, particularly the mystical ones. He took recourse to the current disregard of mystical ideas even if the works of a modern astronomer were influenced by them. Perhaps. This does not mean that these ideas were disregarded, that they should be disregarded. The view of them as not scientific is granted, but that they are therefore irrelevant to it is under dispute.

The History of Science and of Ideas

The question is more theoretical than historical: it is not so much the first but the second of the following two questions that is at stake here. (1) What part of the history of ideas is relevant to the study of the history of science? (2) What is this relevance and is its recognition sufficient for the partial merger that may be beneficial to the study of the history of science and in what way? The second question is significant and exciting.

To repeat, in his *From the Closed World* Koyré refers in this context only to Burtt and Jammer. He should have referred there to Cohen too, and I do not know why he did not. He also overlooked other writers, such as Arthur Lovejoy, the author of the familiar *The Great Chain of Being*, and Ernst Cassirer, the famous student of the Enlightenment movement, who also wrote on the theory of relativity and on quantum theory. Perhaps such studies are irrelevant to the history of science, but this is contestable. Indeed, some writers have meanwhile discussed them in the context of the history of science. The outcome seems to contribute more to fog and haze than to clarity and illumination. In any case, this is a possibility and it merits study. The reason that Koyré did not refer to some studies or other in this context, however, is not any indication that he viewed them unfavorably; some of them he did not view as pertaining to the history of science; it is that such valuable works in the history of ideas do not belong to the history of science. To see this, let us observe that in Cassirer's *The Problem of Knowledge*, as well as in his *Determinism and Indeterminism in the History of Science*, both metaphysics and science are mentioned, and so they could be construed as belonging to the history of science proper, yet these works are deemed more relevant to the field of the history of ideas than the field of the history of science. Why? What makes Koyré's *From the Closed World to an Infinite Universe*, and no less so his "Galileo Platonist", belong to both the history of ideas proper and the history of science proper?

That these works belong to both the history of science and the history of ideas is not surprising, nor is it too problematic; yet it is worth showing why these works belong to the two fields. In particular, we should explain what makes them such exciting integrative works. How do the works of Koyré, of Jammer and of Cohen belong to the history of science? Or perhaps we should ask, do these works at all belong to the history of science? And does the work of the philosopher E. A. Burtt fall in the same category as those of these august historians of science? These works claim that the two fields of human concern grow together symbiotically. Obversely, it is because Edward Rosen rejected this view (on the strength of Baconian arguments) that he did not speak well of their works. Hence, this view is both significant and problematic: we do not know whether the progress of science does in any way depend on the development of speculative metaphysics.

The strong affinity between the two fields of research, the history of science and of ideas, is all too obvious; the claim that they are independent of each other still is popular, but let me ignore it. The claim here is that they are distinct, and that their developments significantly intertwine. It is probably impossible to characterize the field of the history of ideas except historically, and the characterization of science is a task that engages philosophers now for centuries. It is agreed that science is empirical. Not all empirical studies are scientific, however. Clearly, most folklore studies, such as folk medicine, are highly empirical, and even astrology is full of pointless empirical *data*. But even if we ignore all questionable and all pointless empirical *data*, there remain vast tables of valuable, uninteresting information, such as technical catalogues. Even if we ignore these as not given to any serious study, most empirical fields worthy of empirical study remain unscientific but technological: they belong to applied mathematics plus the tests of its diverse theories that are so significant technologically but not scientifically, for example, aeronautics and astronautics. What additional factor is required to render a theory scientific was traditionally seen as the mechanistic framework of science, especially of physics. After Michelson this cannot be upheld in its original form; it may, however, be weakened to refer to any metaphysical framework. We will return to this later on.

There is no clearer evidence for the strong affinity between these two fields of research, the history of science and of ideas, than that the joint ancestry of their contemporary manifestations. Émile Meyerson, whose work is usually classified as belonging to the philosophy of science rather than to either of these two fields, drew his inspiration from both, and influenced both greatly, in his personal influence on both Alexandre Koyré and on P. P. Wiener, the founder of the *Journal for the History of Ideas* (1940) who single-handedly established the history of ideas as a prestigious academic field of study.

I will not discuss here the history of ideas: it is too wild to characterize. It is even hard to say why a certain study belongs neither to the history of metaphysics nor to the history of science, yet it does belong to the history of ideas. Rather, what is of interest in the present context is the opposite: what study belongs both to the history of ideas and to the history of science? How come that in the middle of the twentieth century only a few authors qualified as contributors to both as so many do today?

The approach presented here is controversial. It is told, with how much truth I cannot say, but with an obvious moral that seems to be true: Koyré read his essay on Galileo's Platonism in the University of Chicago, in the presence of Rudolf Carnap, then famous for his hostility to metaphysics. Not a single sentence of this paper, he is reported to have said in the discussion ensuing the reading of it, has any cognitive meaning. Now the reason Carnap said or was presumed to have said this is the famous dogma that at times he espoused, the hallmark of the "Vienna Circle" to which he and Reichenbach belonged as leading members: this school was famed for its grounding or alleged grounding of the traditional hostility to metaphysics in the claim that in principle metaphysical utterances are inherently confused, that in principle any speculation, even the wildest, if it is well worded, then it is capable of being verified or refuted, that traditional metaphysics comprises not of speculations but of inherent verbal confusions.

Were the dogma of the "Vienna Circle" true, then there would be no value to the idea that metaphysics and science interact. The new trend in the study of the history of science is at all possible only because it is false. Indeed, this thesis was stated by Karl Popper, recognized by the leadership of the "Vienna Circle" as its official opposition: metaphysical ideas, such as atomism, he said, have played a role in the growth of science; hence, they cannot be the mere confusions that the "Vienna Circle" had said that it is. This argument is powerful enough for the debate in which Popper was engaged, and indeed, he won this debate, even though the leaders of the "Circle" saw to it that news of this victory did not leak out for decades. Yet Popper's claim is not sufficient for the history of science: what is needed there are two more items: the one is the detailed exposition of the influence of ant metaphysical idea on the history of science, and the other is the presentation of sufficiently many examples of this, to show the methodological import of the one or two rubber-stamp examples (atomism and Platonism), so as to argue that such cases are not freaks. For, even if influences on science such as those exercised by atomism and of Platonism were important, if they were exactions, then historians of science could safely ignore them.

This last point is important, and places the history of science in a different category than any other history. If a political leader, for example, was significantly influenced by astrology, political historians should mention this fact or else they may be branded as biased, as ones who beautify their heroes. Not so the historians of science who freely ignore such facts as that the great astronomer Ptolemy was also a leading (and innovative) astrologer. The clearest example is the study of the life of Benjamin Rush by Lyman Henry Butterfield, the prestigious historian of the early period of American history: he did not conceal the great error of his hero who taught that blood-letting is the best cure for all ills, but he dismisses it as a purely personal affair that has nothing to do with science. He was utterly unmoved by the fact that Rush had explained his view rationally, and that he had won a law suit against those who accused him of excessive blood-letting; Butterfield knew that the medical views of Rush were erroneous and that sufficed for him as the basis for the judgment that all the medical opinions of Rush are to be ignored.

Demarcating Science as Status

Frazer called myth and magic pseudo-science. He did not mean that these are disguised as science and scientific technology, the way phrenology was at its heyday: unlike our society, societies that are myth-minded and magic-minded usually do not have individuals who intend to imitate modern science and technology. When they do, they admit their inability to emulate the success of modern science and they institute rituals to be performed in the hope to achieve just this, rituals known as cargo-cults, which is a new kind of magic. What Frazer meant when declaring that magic is pseudo-science was, clearly, that magicians claim falsely to be in possession of some knowledge and/or of some proficiency. So those who profess real science and really science-based technology are those who make true claims to be in possession of some knowledge and some proficiency. Simple. The question, what is good science is then different from the question what is good art in that not all art is aesthetically pleasing, but we can speak of their aesthetic value all the same, whereas the question what is good science is the question, what is good among all the scientific truths that we are in possession of? and so deserve acclaim, also deserves special acclaim? In other words, whereas good art is membership in an exclusive club, good science is either any science or membership of an inner club; whereas good art is excellence, good science is double excellence.

The first question is, whose knowledge-claims are true? The second question is, whose true knowledge-claims are (highly) significant? We have to answer the first question first. To know that we have to be in possession of the same knowledge as the claimant has: to say that we know that what Newton says is true is to say that we know that Newton's theory is true, namely, that we know what it says. Query: can we know that without being scientists ourselves?

To say that should know that Newton's or Einstein's theory so as to judge it scientific, is to limit research activity in these fields. Since people sufficiently familiar with science are usually busy doing research, they can be active in the philosophy and history of science only incidentally, in their spare time and in old age. This was the view repeatedly and forcefully advocated by leading physicist Max Born, for example. He admitted (to me, incidentally) that it is not likely that a retired researcher will have the energy, disposition, and qualifications to do good historical research, but he felt strongly that the needs of science are more urgent than the needs of its history.

George Sarton flatly and bravely denied that opinion. He said, suffice it if historians are familiar with science in general, even without the possession of the familiarity with the detail required for proper understanding of the intricacies of science that they may have to discuss. How then can historians assert that some ideas are scientific proper? Sarton said we can rely on scientists. The idea is that scientists are no liars, that science-based technology is operative and open to all to see, that the proof of the pudding is in the eating.

This is how it comes to pass that historians of science rely on the standard science textbook. Whenever the textbook changes, they have trouble on their hands. Science proper, they say, is modern science. The first modern science textbooks were Newtonian. But then Newtonian optics was replaced by wave optics, electricity was replaced by electromagnetism, and then Newtonian mechanics was dethroned. Scientists have some explanation of this fact or another, and they need not bother themselves with the question, how good their explanations are. Yet the historians of science know too well that in the age when Newtonian mechanics reigned supreme, views on it were different from those held today, as it was claimed then to be the very last word in physics the way no physicist today allows. Why do most historians of science overlook this important fact?

This is clear: whatever we think of Newtonian physics, whatever our explanation for its scientific status — today or yesterday — we will never compare it to myth and magic: it never was and it will never be a pseudoscience in Frazer's sense or in anyone else's. Why? It is the same question as the one we began with: what makes it scientific? What grants it its scientific status?

History of Science New Style

The discussion here sounds too abstract for some simple-minded historians of science who wish to go about their business without too much sophistication. The claim made here was that unlike the history of art, the history of science requires first that we decide what idea is scientific and then what ideas are relevant to the history of science that we happen to be studying and why. This turns out to be a tall order.

Different demarcations of science are available, all competing, some legitimate, some not. Thus, the demarcation of science as demonstrable theories or as probable theories or as useful technology are unacceptable. We may endorse the demarcation of science as the class A of theories that are descriptions of the world and are also empirically testable; alternatively we may limit it to the subclass B of class A, to the class of theories that were tested or are going to be tested, as well as to the subclass C of class B, of theories that have stood up to test, and even to the subclass D of C, that of corroborated and unrefuted theories. When Jammer says that a scientific theory is a king for the day, then he has in mind demarcation D, of course. By this demarcation Newton's theory of gravity is no longer scientific. By demarcation C its scientific status cannot be taken away from it ever. This appeals to me greatly.

We can circumvent this by saying explicitly what is at stake. E. A. Burtt did not discuss the demarcation of science and this does not obscure his meaning. He observed that at its hay day Newton's theory exercised more authority than any other theory ever did. This is no longer so. I asked him once if he wrote his book under the influence of the success of Einstein's theory. He smiled beautifully and admitted this in the humility becoming to one not sufficiently well-versed in modern physics. Koyré held a similar view of the tentative character of scientific theory: in his *Galilean Studies* he refers in this connection to the ideas of the lovely philosopher-physicist Gaston Bachelard. Jammer said the ruling scientific theory is the king for the day, and Cohen elaborated on this theme in his preface to the 1952 Dover edition of Newton's *Opticks*.

Nor is this all that there is to it. We may deny that Newton's theory — or some other theory — is refuted: as long as it is in use in technology, for example, or as long as it is prominent in the up-to-date science textbook, or as long as some other condition holds, we may see it as unrefuted. In my view it is much easier and simpler and nicer to declare it refuted but still scientific.

There is still another traditional condition for a theory to count as scientific: in addition to being empirically testable, the additional condition is that it has to comply with certain metaphysical ideas, such as to be not magical, or mechanistic, or atomistic, or conform to some other specified picture of the world, or that its equations should be invariant to some set of transformations. Each metaphysical system, whatever it is, promises by its very dominance to insure some measure of coherence between the different scientific theories that are supposed to comprise a coherent whole. The demand for a coherent whole is often tacitly assumed.

Metaphysical systems that can function as such unifiers can often also function as generators of agendas: theories that present threat to the unified image of the world present problems; they may be open to reinterpretations in the light of this or that unifying thesis, and the reinterpretation is then crying to be presented as a competing scientific theory to be tested as against the initial problematic theory in a crucial experiment. (The standard objection to crucial experiments is due to their inability to settle matters once and for all; this objection — due to Duhem — is no longer acceptable.) This, briefly, is the idea that some general — metaphysical — ideas of the world play as grand-scale research projects or research programs.

The traditional sets of conditions for empirical theories to count as scientific, partly compete and partly complement each other. They serve as means for the presentation of unified stories covering portions of the history of science, and such stories can be more successful or less, and if successful, they can be tested as against historical material. Older views of researchers were often stuck on reefs since the programs they were working on were impossible. Now we can simply record these programs and their successes and failures. This makes the study of the history of science both exciting stories and philosophically enlightening.

3. Rationality: Philosophical, Social, and Historical Aspects*

The problem of rationality is the problem of the choice of ideas, of courses of action or of lifestyles. More cannot be said without loss of generality. Clearly, there is a great variation here, as the example of the rationality of historians of science indicates: they have to choose which idea to ignore and which to discuss and how. Also, there is a variety of solutions to the problem. Rather than offer a critical survey,[1] let me deal briefly with some solutions to the problem and try to apply them to the case of the rational choice which a scholar from one periphery may face when preparing a lecture to deliver to another periphery, taking it for granted that the history of science is peripheral almost everywhere. It will turn out soon that almost no philosophical solution to the problem of rationality applies to this case; the sociological theory of the interaction between centre and periphery (of Edward Shils) does apply to it but for that it requires far-reaching modifications. I will only hint at wide field of application of all this to the history of science.

My concern here is with the following, unpleasant fact. As the intellectually hungry in remote places look up to the affluent, august centers of learning for much needed sustenance what they receive is almost invariably inferior. Instead of best products available they receive not even the best that the centers can offer: almost invariably they receive, and take as some sort of gospel, what passes in the prominent centers of learning as fickle intellectual fashion or worse, what serves there as public relations, with the inevitable disastrous malnutrition that results from the replacing of main dishes with dubious, piquant snacks. The peripheries fare worst in the philosophy and the history of science in that these are even in the centers almost purely public relations; they fare less poorly in the social sciences, as these are so often legitimating the regimes at the centers that then they legitimate the much worse regimes at the peripheries. The "soft" sciences are ill-treated this way most, the "hard" ones least, but not well enough. The menus in the natural sciences in the peripheries are closer to the centers in content, but without the accompanying methodology, without the critical apparatus that is required for their proper digestion. Leading natural scientists who usually take their critical approach for granted are shocked to the realization of its absence in the peripheries. This should suffice to suggest that the problem of rationality, the search for universal canons of rationality, is of some practical significance. The practical concern here is the peripheries known as the philosophy and the history of science, whose leading practitioners all too often take as gospel true the wrappings of the great scientific advances that often are just popular prejudices. This way they legitimate the worst in the peripheries that take public relation myths as universal truths and as *sine qua non*.

Rationality in Philosophy and in the Social Sciences

The most popular theory of rationality, the most common in the philosophy of science and in the scientific myth, lore, and fashion is the traditional, classical Baconian-Cartesian theory: rational people assert all and only what they can back up by good arguments, whether proofs or factual knowledge empirically obtained. The defects of this theory are obvious. We do not know what is proof and we would not know how to back up any view of what proof is without begging the question; this is called "the skeptical critique from infinite regress". Moreover, this theory does not apply to the act of determining agenda, including agenda for research. This is deadly.

The study of the problem of rationality is the nearest to the choice of intellectual procedures. In most of its versions, it is broken down into two problems of rational choice: of ideas and of actions. Judging actions rational whenever they rest on ideas endorsed by their actors, the problem of rationality of actions is reduced to the problem of choice of ideas to endorse. (This evades a vital question: what does the endorsement of ideas amounts to? Often philosophers and historians of science endorse ideas that they are utterly unfamiliar with; how is that possible?) Assuming that rational endorsement is of what we know reduced this problem of rationality to the problem of knowledge. He view that knowledge is scientific, the problem of rationality is further reduced to the problem of demarcation of science: what theory is scientific? Rationality is then the proper choice of the best scientific theories available, for both endorsement and action.

Historians of science usually do not solve this problem. They have a shortcut, or a touchstone: science is whatever the up-to-date science textbook presents. They dismiss with ease other ideas and call them superstitions, prejudices, or metaphysics.

Philosophers of science usually present the problem of rationality thus: what theory should I choose to believe in? In its more careful wording, the problem is broken into two: which system of knowledge is preferable, and, what is the criterion of choice of hypotheses within the chosen system. The system can be of the search for knowledge or some other kind of system. Thomas S. Kuhn has broken the problem of choice of a hypothesis to diverse paradigms: the problem appears within each paradigm and as relative to it.

(What is a paradigm? Kuhn tried to answer this question, admitted failure, and dismissed it as insignificant. Nevertheless he gave up the term. This is shows his not having fully digested the ideas of Michael Polanyi that he was following. By the philosophy of Polanyi, masters do not have to articulate their views but allude to them by displaying an example, a *digma* to use the Greek term, at times a chief example, a paradigm. This breaking down of science to systems is also known now by the term of Imre Lakatos, "scientific research programs". It is odd that in the vast literature on this idea of Kuhn and Lakatos there is barely a discussion of what it adds to the discussion. It adds an important item, it answers the question, how do we determine the agenda for research? Alas, it does so wrongly, by deciding matters too soon and without rational deliberations.)

It is obvious why the problem of rationality so often centers on the choice of theories within science. The concept of rationality is traditionally identified with that of scientific method and that concept with the concept of objectivity. And so all other systems of thought were dismissed as inferior to science, within which the choice of hypotheses is supposed to be utterly objective. Traditional historians of science take it upon themselves to illustrate this. They cannot. Their task is tragically doomed to failure

Let me mention a lovely sociological concept of objectivity that is nowadays almost forgotten and seldom invoked: the objectivity of "the observer from Mars". That objectivity was deemed secured by the distance of Mars from the earth both in interest and in prior knowledge. The concern of the observer from Mars that has a parallel in the disinterestedness of science, has, perhaps regrettably, ceased to be of primary interest; the question of prior knowledge and prior belief regarding the criteria of valid knowledge are now central to the debate on rationality. The observer from Mars, then, serves as a metaphor for the utterly rational, scientific investigator who is not impeded or constrained by the biases that may dwell in personal interest and in the peculiarity of some prior knowledge. Let us ignore personal interest here, on the assumption that in scientific investigation the search for the truth is the paramount interest. What are we to do with the peculiarity of the prior knowledge, of the knowledge that is different in different societies or the different ideas taken as knowledge in different societies?

We may also illustrate the phenomenon of rationality and the problems involved in the classical theory of rationality by reference to the individual marooned on a desert island, Robinson Crusoe by name. As he was once a member of human society, he cannot be bereft of prior knowledge as the observer from Mars is. He is not without the knowledge that he possessed prior to his present perceptions. The question then is, would he perceive the same way were he a member of a different society? If not, can he be stripped of that knowledge so as to avoid the bias specific to his original society? The bias that Robinson Crusoe may have is only a part of the possible bias that some social scientists attempt to avoid. In addition to the knowledge that human beings possess wherever they go, and that may come into play and introduce some bias into research, there is a kind of power not given to Crusoe but that resides in objects that are repositories of knowledge, handbooks, encyclopedias, and such. This is objective knowledge, knowledge of "World 3", or knowledge without a knowing subject, as Karl Popper has called it.[2]

The problem of rationality thus appears in two variants, represented by the observer from Mars and by Robinson Crusoe respectively. The variant relating to the observer from Mars is the problem of the rationality of that observer; it is recognized as a very difficult problem, perhaps as insoluble in principle. The variant relating to Robinson Crusoe is recognized as a problem that is all too easy to solve. The reason for the tremendous difference between the two is obvious. Whereas Crusoe has inherited from his society a criterion of rationality, the observer from Mars is supposed to be an observer rational like us and superior to us in being utterly rational. Hence, the observer from Mars is able to create an utterly disinterested criterion of rationality: *ex nihilo*. Nevertheless, generations of philosophers were puzzled by the fact that the problem of rationality seems so easy to solve and is so difficult; the easy and the difficult problems are very similar yet not identical. Rationality is generally assumed to be a matter of thought and of action; rational action is based on the knowledge that rational actors possess when they act, and when the actors are students of new problems, this includes the knowledge that they possess prior to their research. Rational research is action in accord with prior knowledge, and this is particularly useful, as the knowledge that is prior to research is supposed to include the objective criteria of rationality. The search for the criterion is all too hard; the assumption that rational people have it makes it all too easy

This is so not only for studies that break new grounds. Every extant social system known to us has its own fund of knowledge and this fund includes some standard, some criterion of rationality, as understood by members of that society. This is taken to be the case even if the individuals in question, as any other member of that society, cannot articulate the standard of rationality of their society. The standards are then declared tacit. The question then is, are the standards given by prior knowledge correct? We do not know, but there is a repeated empirical observation that they vary from society to society, to a small or large measure. This observed variability was considered from antiquity to the present to be the evidence that all standards are suspect: in the light of this consideration, the choice of one standard amounts to the assumption that one society has a privileged access to the proper standard. This line of argument is the classical claim that nature is one and conventions are many and that only nature counts.[3]

One may create a shortcut and suggest that the differences between the canons employed in different societies are superficial, that at bottom all humans follow the same rules.[4] Right or not, this is not to the point, as the point is that only the universal is valid, not the particular, and no one has denied that behind the particular stands the universal, behind appearance stands reality. Classical philosophy demanded that all extant standards be rejected as not sufficiently objective, since only thus will it be possible to replace them with the one, absolute, scientific standard: in principle, only the universal is valid. This was meant to hold for the rules of any society whatever and for any idea whatever: as long as they are not strictly universal, they are defective. This is similar to the myth of the Muslim attitude to the great library of Alexandria: either its contents are already included in the Koran and so may be replaced by it or they are wrong and should be disregarded. This, so the myth ends, led Caliph Omar to order the burning of that library.

The only difference between the myths is that the faith of the scientific observer is not in the Koran but in the truth. But the faithful know that the Koran is true. So it is not the claim for the status of the truth but for its validity. Traditional philosophy of science requires proof with no recourse to revelation or to any other claims limited to some particular society.

It is the demand that we reject all prior knowledge and replace it with the proper, rational, objective scientific alternative; this renders the problem of rationality so very difficult. When all extant, diverse bodies of prior knowledge other than scientific knowledge are rejected, then all extant, diverse criteria of rationality are thereby rejected as well, and room is made for the attempt to create one *ex nihilo*. Yet the very demand to start utterly afresh is what renders the problem difficult and, at least seemingly, insoluble. This would render the very idea of rationality impossible and may even amount to the denial of any sort of rationality, which is plainly absurd.

Social Theory and Rationality

The reef upon which classical rationalist philosophy was wrecked is known traditionally as the skeptical critique. The claim that rationality is impossible is usually called skepticism. All skeptics assert that rationality in the sense of creating criteria for rationality *ex nihilo* is impossible; they back this trivial assertion by the obvious observation that without prior knowledge there can be no proof of the validity of proofs, so that every proof is question-begging. Among skeptics, Pyrrhonists assert further that rationality is impossible for the reason just given, and they further recommend that therefore one should never assent to any opinion. When young Ludwig Wittgenstein held skeptical views, Bertrand Russell asked him to assent to the claim that there was no rhinoceros in the room in which they were discussing the matter at the time. Wittgenstein refused. Russell found this ridiculous. So, with most rationalist philosophers, he undertook all his life the task he knew was impossible, regarding it as necessary to refute the trivial and unanswerable skeptical criticism or provide an alternative theory of rationality that would be immune to that criticism. He admitted failure.

Many individuals devoted to scientific research dismiss the skeptical criticism as sheer annoyance. Philosophers agree but they cannot dismiss that criticism, as their self-appointed task is to try to refute it. Historians of science used to have the happiest attitude towards it: limiting their studies to science they could ignore it in good conscience. But then came Einstein and declared his indebtedness to David Hume from whom he learned of this criticism that he found most useful in his researches.

Instead of the observer from Mars or from a desert island, then, let us consider a concrete ordinary case that individuals may encounter as rational beings. Real individuals usually come with the prior knowledge that they share with their neighbors, and this may include criteria of rationality. To accept any criterion of rationality as a valid component of prior knowledge is a complete solution to the problem of rationality but it also begs the question.

Despite the obvious stringency of the demand for utter rationality, such as would be exercised by the observer from Mars, the philosophers of the Enlightenment took for granted; most philosophers of science today still do. Yet the rise of the romantic philosophy was a reaction to the philosophy of the Enlightenment. Its adherents rejected the traditional concept of rationality and instead suggested the preference for the mode of thought of one's own social framework. This encouraged the study of different systems of thought, and with it the study of different sets of views and values and criteria of rationality. This was the progress in the study of rationality that irrationalist philosophers brought about. They (especially Hegel) first preferred history, but social anthropology followed suit (as ancillary to history) and sociology.

Certain of the major figures of the history of empirical sociology and social anthropology interested themselves in diversity of systems of knowledge, and they justified it in different ways. The most important of these were Émile Durkheim and Max Weber around 1900. Durkheim refused to take systems of beliefs literally; he viewed them as social institutions. In modern monarchies where citizens reject the myth of the superiority of the royal family this view holds; otherwise it is obviously false. Weber took the different systems literally and asserted that we can study them without endorsing them. Even if one studies one's own system, Weber added, one should study it from without, not confusing one's study of it with one's endorsement of it.[5]

One thing about both Durkheim and Weber is astonishing. Neither studied the contacts or interpenetrations or interactions between different systems of thought. Their contemporary Georg Simmel did. He was interested in the conflicts generated in contacts between different social structures (and the systems of thought associated with them). He offered a brief treatment of social change through interaction between different societies. This is his theory of the stranger. It is the theory that the agents of such change are individuals equally at home in different societies, or rather equally strangers in them. He avoided the problem of rationality. The stranger is a member of different societies who might mediate and transfer knowledge between systems views, values and criteria or standards. The idea is that a stranger raises the question, what parts of a given system are transferable and which of these is advisable to transfer? Simmel did not discuss the rationality of strangers or of the societies that they influence. It is hard to see how and to what extent one can be a stranger, i.e., be at home in different systems as well as in the sets of criteria that go with them. As long as the stranger follows consistently one standard, one set of criteria, their validation is assured. The case of the stranger has to be different: the stranger happens to hold competing systems and to face the choice between them. But why should others follow that particular choice? This obvious question is scarcely noticed.[6] Perhaps strangers succeed, perhaps changes in systems of views and values vary because the strangers are not consistent to begin with and so they have to change [7]

The possibility that standards or criteria are inconsistent and must change should be carefully considered and studied. Another possibility to be considered is where two societies share the same criterion of rationality. This could occur with regard to the scientific sectors of different national societies. They differ in content but share the values of scientific objectivity. This is where the theory of centre and periphery is relevant and this is where the history of science enters. The centre can cause the periphery to change its judgment on particular matters just because they share the values of scientific objectivity.[8] The center is “in the structure of society”: it is not quite geographical and membership in it is not quite territorial even though it “almost always has a more or less definite location”. Edward Shils discusses the consensus concerning values, and asserts that affirmation of the center’s values can be instrumental, partial or attenuated (p. 10), that the affirmation of apolitical scientists is quite weak, that compared to traditional society, modern society has dissent as both more explicit and more domesticated and restricted by attachment to the central system. He thus declares that the dissent can be fully expressed as it rests on a deeper consensus. The consensus can be on the agenda for discussion, and it is usually this kind of consensus that rules the scientific community. The consensus in the scholarly community is its affirmation of the current scientific research agenda given its background-knowledge, and this is based on a value-system, (belief in Shils’s sense; note 1 of his “Tradition”, *Center and Periphery, op. cit.*, 185), yet, perhaps contrary to Shils’s text, it is an affirmation by apolitical scientists, and it is both strong and instrumental.

Possibly we are seeking the impossible. Possibly communication takes place only within any one of the different systems of views and values, not between them. Karl Popper called this idea “the myth of the framework”.[9] This myth is ancient and most prevalent (although Popper was criticizing its version that Kuhn was advocating). It usually came as a rider to the view that one of these systems is indubitable and it is the one to which the individual expressing it belongs. This view is called “parochialism”. The myth of the framework was altered to become anti-parochialism with the advent of relativism, whose mark of distinction is that it approves of every system within that system’s own domain and refuses to ask in the abstract the question of the validity of any system. Relativism destroys the very source of the problem of rationality, since the problem arose (in ancient Greece) out of the rebellion against parochialism, out of the realization that the diversity of cultures renders the adherence to the views and values inherent in any one of them a matter of accident rather than of choice, or else it renders adherence quite arbitrary — rather than a matter of intelligent choice that should be independent of any extant culture. Put differently, the problem of rationality is the problem of intelligent choice between different systems of thought; between different criteria; between different paradigms; relativism sanctions every option and so it dissolves the problem. The philosophers who do not consider the problem serious tend to dismiss it as spurious. Their dismissal is thus highly plausible and, under the influence of Ludwig Wittgenstein and G. E. Moore, many philosophers have devoted many pages to show how very plausible this dismissal is. Once this dismissal is favored, it puts an end not only to the discussion of rationality, but also to the whole tradition of Western philosophy, which deems this problem central. The proposal of Wittgenstein and Moore to replace all philosophy with the plausible thus amounts to the dismissal of all traditional philosophy; it is therefore known as the revolution in philosophy. As far as the diversity of cultures and even sub-cultures is concerned, if Wittgenstein ever spoke of it, he declared all of them equally legitimate. The idea of rationality as embedded in one's given background, in the system of views and values taken for granted in one's social environment, appeals to many philosophically minded social anthropologists, as it is rightly customary in their field to reconstruct the systems of views and values of the societies that they study with indifference to the conflict between the views and values current in these societies and in their own. Relativism is therefore quite popular among social anthropologists, and it might be plausible, yet it is no solution to the problem of choice of a system of views and values. It is only a redundant, *post hoc* justification of any choice already made.

That relativism is untenable is obvious; the cases to which it does not apply at all are many and varied. They are the standard topic of concern both for social anthropologists and for sociologists, under the name of "the clash of cultures". Individuals who live in different cultures each faces the choice between cultures, i.e., between systems of views and values and criteria that go with each culture. Relativists tell them that their problem is insoluble (Hegel) or that they have no problem (Wittgenstein). [10]

The Idea of "Background Knowledge"

The meeting of cultures, in "clash" or not, occurs in the meeting of different systems of knowledge each of which is taken for granted in its own society. Such systems of knowledge, when taken for granted each by itself, are called by philosophers "systems of background-knowledge".

The background-knowledge in any situation is not any knowledge that any individual may bring to that situation; it the instituted prior knowledge, the knowledge that is presumably commonly possessed. Individuals presume themselves and their peers to share certain knowledge, ideas and information easily found (as in *The Book of Common Prayer* or in the standard science textbook). This is their background-knowledge. Even Robinson Crusoe, the individual who was marooned on the desert island, and who serves as a metaphor for an individual utterly detached from any society, possesses the background-knowledge acquired in the society in which he once lived. (Defoe had the original Crusoe salvage a Bible from his wrecked ship; this made him distinctly different from the observer from Mars.) The troubles besetting foreign visitors unfamiliar with the local system of background-knowledge and who undertake to give lectures may serve to illustrate the contrast between the different systems of background-knowledge that belong to different societies.

The problems that foreign lecturers may face when they are ignorant of local background-knowledge may sound artificial to those who have not been in that position. Also there are many who have much experience as foreign lecturers and who have not experienced the difficulty discussed here. What is it that burdens some foreign lecturers so much and how do other foreign lecturers escape it? It may appear that the ignorance of background-knowledge that some foreign lecturers manifest when addressing local audiences is easier to overcome than the ignorance that other foreign lecturers do, and that the variance is the same as some people face when they wish to master a new specialty. Perhaps Michael Polanyi's view merges the two problems: addressing members of one's community and addressing members of one's profession, both require certain background-knowledge, and in each case the prior ability to acquire that background-knowledge is baffling for some but not necessarily for all. In Polanyi's view the two problems are identical as they require familiarity with some background knowledge, and the required background knowledge is generally impossible to pin down. He called all knowledge that we cannot specify and that we acquire through long personal experience "personal knowledge".[11] Even the need to become familiar with the (relatively simple) background-knowledge of preliterate tribes has baffled social anthropologists for generations. It is not any particular cognitive proposition, true or putatively true. An idea belongs to the background-knowledge of a given group if and only if its members take it for granted and they consider it as taken for granted by other members of the same group: background-knowledge is thus a part of the institutional systems of a society (or a sub-society, a social class, a group, a guild etc.).[12]

To help focus our discussion on material entirely within the scope of the problem of rationality, let us consider lecturers who speak on the problem of rationality, yet without being equipped with sufficient information about the background-knowledge of their audiences. (As usual, it is almost impossible to specify *a priori* that background-knowledge.) To lecture before an audience in a foreign land may be an embarrassment because, lacking background-knowledge, lecturers may assume too little or too much about the knowledge that their audiences possess. They may be stating things that their audience know to all too well, and, even worse, things that their audience can criticize with ease. Alternatively, they may be taking for granted things that their audiences are not familiar with, but happen to be well known in the corner of the world where the lecturer happens to live.

To make things worse, us assume that the guest lecturer and the learned audience are historians of science. Surely, all historians tell old stories, material that at least in principle is known. The principle is here very strong. A lecturer in science may repeat new material that is already published, so that it is in principle known, since in fact it is not known to the learned audience and they may wish to discuss it in public. Public lectures, said Erwin Schrödinger, are pretexts for public discussion. Suppose, by contrast, that there is an old book that no one in the audience has read and the guest lecturer reports. This is barely excusable, since the audience may be told to read that book — or an abstract of it — and save the lecturer the visit. Suppose we say, but the learned audience want to discuss that book with the visiting lecturer. To that end the lecturer may indeed give a brief summary of the book and its background and discuss the problem that it presents to historian of science today. Otherwise the lecture is an embarrassment. And they usually are, I am afraid — in most fields.[13]

The problem of visiting lecturers has a familiar solution; it is Shils's theory of centers and periphery: centers of learning are recognized by the periphery and they can and do declare what is the valid background-knowledge everywhere.[14] Thus, visiting lecturers from the center come to enlighten their colleagues in the periphery. This solution is particularly applicable in the case of a paradigm-shift Kuhn-style. It does not work in history, least of all in the history of science.

This theory, despite its presentation of very familiar facts, is strikingly novel as a solution to the problem presented here.[15] International scholarly communication is a new phenomenon; the international commonwealth of learning became significantly more extensive and more heterogeneous only after World War II. The theory of centers and peripheries differs from the classical solutions to the problem of rationality in that it does not describe, much less validate, the content of any background-knowledge. Rather, it describes how background-knowledge is determined, treating it as part of a complex of institutions, and its powers of coordination as emanating from centers that coordinate scientific activity. Background-knowledge, then, is not declared true; it is declared to be what should be known wherever its objects are discussed, even if only in order to put it to critical examination. To sharpen this distinction, I should note that ideas may be declared as background-knowledge and as open to criticism. Hence, background-knowledge is not necessarily identical with ideas assented to. Even strict, zealous intellectual traditions (religious, Marxist, Freudian, or functionalist) often require familiarity with some heresy plus some of its refutations. The idea of background-knowledge is of course as old as philosophy. Yet its use in a solution to the problem of knowledge first occurs, as far as I know, only in Michael Polanyi's works from the late 40's onward.[16] Explicit reference to it as part of the solution to the problem of knowledge was first presented, as far as I know, only in 1956 by Karl Popper.[17] The rationality of the preference for one scientific theory over another depends on their degree of confirmation. (Popper's example is the fact that the degree of confirmation of the Einsteinian theory of gravity is assessed as higher than that of the Newtonian theory that it has come to replace. The assessment of the degree of confirmation of a theory, Popper tells us incidentally, is done relative to some unspecified background-knowledge. The casualness with which he introduced the concept of background-knowledge into his system and its immediate adoption in the current literature explain the diversity of nuances it has there. In part, at least, this immediate adoption is due to the fact that Polanyi, too, laid stress on the role of background-knowledge in rationality, except that according to Polanyi that knowledge is tacit and can never be made explicit. Imre Lakatos, in particular, has contributed much to the popularity of the term, as he regularly conflated Popper's and Polanyi's opinions.[18] Being familiar with the fact that competing answers to given questions (such as Newton's and Einstein's theories of gravity) as well as some theories plus their criticism, Lakatos declared background-knowledge internally inconsistent.[19] This is his abdication of reason. This was unnecessary, as we have a set of alternative sets of ideas, not always explicitly stated, each of which can be put to different uses, and hopefully none of them inconsistent[20] This accords with Polanyi's idea of tacit knowledge since background-knowledge is often tacit; Polanyi insisted that background-knowledge is always tacit, although only a part of it is; also, he called tacit knowledge personal, though, as he has stressed, it is not personal as it is incorporated in and transmitted by institutions of learning. Regrettably, Polanyi discouraged all effort to render tacit knowledge explicit. There is important to this: background-knowledge, any knowledge, cannot be made totally explicit.

When visiting lecturers disagree with some opinion or criticism commonly where they lecture, all they have to show is that they are familiar with the background material that they reject. Lecturers may then proceed to present and explain objections to the received material, and these objections might be interesting or not. There is no assurance of interest in either the acceptance or the rejection of any idea. The concrete problem presented here has to do less with assent or dissent and more with the communications across barriers of background-knowledge (that Kuhn declared impossible to overcome except by acts of quasi-religious conversion). This problematic fact is a new variant of the ancient, perennial problem of rationality.

I suggest that lecturers may dissent from background-knowledge and advocate the rejection of a scientific opinion commonly expected to be assented to, on the condition that they acknowledge that they speak against received scientific opinion. This is important and not as trivial as it should be. Its non-trivial character may be seen in its repeated, rude violation by individuals who are neither rude nor ill-willed.

Example. In the beginning of his *Paths in Utopia* Martin Buber reports that once, after he had presented his doctrine to fellow-socialist scholars at a centre of learning, he was dismissed by the moderator at once — as utopian who overlooks the standard critique of utopianism — without bothering to notice that, or to find out whether, Buber was familiar with that critique. Buber's point in narrating this story was not to complain about a moderator's rudeness but to illustrate the fact that many scholars take it as a matter of course that the standard critique of utopianism is valid and they attack their opponents' views by abuse rather than by rational dispute.[21] Thus, the acceptance of the validity of the standard critique of utopianism is an instance of the established — Marxian — authority of a centre of learning, and the rudeness that Buber describes is an instance of the failure of centers of learning to establish good intellectual manners. Although it might be taken for granted that a critique (of utopianism in Buber's case) is valid, when Buber presents his views it would have been better to find out whether he was familiar with that critique and, if so, whether his response to that critique was of any value. Buber's point was that the moderator had no right to dismiss him as he did. In order to discharge their useful service, then, centers do not have to impose uniformity of opinion. This point is of great importance. Philosophers of science who advocate the doctrine of rational belief violate it systematically even if unintentionally.

Background Knowledge is no Dogma.

Although centers prescribe only some background-knowledge, not assent to any particular proposition or any particular criticism, the question of the validity of particular criteria of rationality and validity remains unsettled. Intellectual backgrounds alter; centers shift; what is taken for granted before a revolution, scientific, intellectual or cultural, may alter radically afterwards.

Does the theory of centre and periphery disallow the variability of background knowledge and vagaries of centers? No; on the contrary, a part of the established discourse on centers and periphery is the material that Joseph Ben-David presented as he spoke at some length of the shifts that the location of centers of learning undergoes in time. He hardly discussed the question of what centers prescribe to the periphery.[22]

Admitting the alterability of agendas and the content of scientific discourse and assuming the theory of centre and periphery, we may ask, is the received research agenda the best, and does the centre of learning have the right to prescribe it to the periphery? The answer is that the decision on this is in the hands of the periphery. Yet we can judge their decisions wise or unwise, and we can do so intuitively, as historians of science do all the time. But we want to have a criterion for that, or discover the tacit one that they employ. Unlike the traditional discussion of the problem of rationality, the present discussion does not raise the problem of knowledge and it does not seek criteria for the rational acceptance of any opinion. In accord with the theory of centre and periphery it does not insist on the validity of any particular proposition of our background-knowledge; it merely examines the requirement of familiarity with whatever the centre takes to be required prior knowledge. The theory of centre and periphery does not eliminate the question of the validity of the criteria of validity and rationality, as it still makes some requirements, even though these are narrower than the traditional rationalist ones. Are these requirements acceptable? How does the commonwealth of learning determine its agenda and is it rational to conform to the implied directions contained in that agenda?

The traditional presentation of the problem of rationality is inherently individualistic.[23] Thus, research program put in classical terms would refer to individual students, not to the commonwealth of learning.[24] The result will be what Popper has called Crusonian science.[25] Now surely individual students are at liberty to choose any program they like, but unless their concern is shared by the commonwealth of learning, it has no safety measure against sinking into oblivion.[26] It is therefore essential to discuss the problem of rationality, including the problem of the rationality of the agenda for the advancement of learning and the problem of knowledge, as social problems.[27]

Here the problem of the validity of the criteria of rationality is narrowed down to a simple, technical problem, the one faced by a foreign lecturer, especially a historian of science, whose familiarity with the local background-knowledge is limited. What is that lecturer to do? The standard view of the matter is that the lecturer should follow the instructions that hold generally and show familiarity with what the centre prescribes to the periphery as obligatory background-knowledge. The material that should serve as obligatory background-knowledge need not be right, and at times the background-knowledge includes its criticisms. For example, Marx' ideas and their invalidity should belong to our background-knowledge. Today, some texts of Marx are obligatory background-knowledge. This does not mean that they are endorsed; it means only that they should be known, since they have now become an integral part of the required background-knowledge in discussions about modern societies. This deserves notice, since the influence of Marx among historians of science is valuable when take critically and pernicious otherwise.[28]

Centers of learning are prominent: this is what makes them centers. They offer background-knowledge that may safely be taken for granted by a lecturer from one periphery performing in another. There are good reasons and empirical evidence — though no guarantee — that directives of the centre regarding the right background-knowledge will overcome misunderstandings, even when the periphery happens to reject what the centre expects them to promote. This then may count as the standard solution to the problem at hand as most scholars endorse it as a matter of good practice. As it is a solution to a practical problem, it seems utterly reasonable, especially if all the lecturer has to do is mention the items of background-knowledge presupposed by the lecture, the items necessary for the comprehension of the lecture.

To be prominent, centers of learning should avoid parochialism. This is impossible to do all the way; it is a human limitation; this limitation should be recognized. It behooves those who occupy positions of authority in centers to be aware of human frailty, especially their own, and more especially the intellectual frailty that is the inability to avoid all error. The awareness of the inability to avoid all error raises at once the question, as to what error is tolerable. Codes of civilized societies, ancient and modern, distinguish between negligent error and a defensible error; it is time for the modern philosophers to take this into account in their theories of rationality.[29]

In the Western philosophical tradition, sincere efforts to avoid parochialism dictated the rejection of all tradition and the endorsement of only the purely rational. This was deemed possible as there was an optimistic faith in the ability of science to prove its assertions beyond doubt.[30] By this standard truth and rationality are united by proof, by the ability to avoid all error. As Thomas Babington Macaulay and Augustus deMorgan said of Sir Francis Bacon, his theory was not so much that of the finding of the truth as that of "error avoidance". Error cannot be totally avoided. Relativism is a wild effort to save the task of "error avoidance" by declaring that those errors that are hard to avoid because they are generally accepted as parts of the parochial framework are not errors at all and need not be avoided. Allegedly where they are generally accepted they should linger.

The only serious exceptions to the two traditional, abstract theories, of rationality as perfect or alternatively as relative, are the views of Popper and of Polanyi. In Popper's view both traditional views of rationality are too abstract, too unrelated to concrete historical cases of its practice. His view that rationality is a matter of a tradition, as presented in his *Conjectures and Refutations*, agrees with the idea that we cannot operate without background-knowledge, imperfect as it surely is; imperfect background-knowledge includes imperfect theories of rationality. Yet Popper's theory of rationality, as presented in his *The Open Society and Its Enemies*, is also abstract: it includes the repudiation of all earlier theories of rationality as if they were not commonly received in the tradition that he described as rational. This is a contradiction that can be settled with little difficulty, and in an interesting manner. But first we should acknowledge it.

Polanyi deemed impossible the attainment of a satisfactory explicit theory of rationality: if we articulate one and it meets with some criticism, then we need some means with which to assess the situation, and this means will be the tacit or unarticulated idea of rationality. This is a profound idea.

Weber had a better idea: refusing to relativize truth he relativized rationality — in the sense that he promoted the theory of degrees of rationality while renouncing the theory of degrees of truth.[31] Yet Weber spoiled this by offering a two tier theory of rationality: sadly, he could not free himself of the opinion that the rationality of science is maximal.

Popper's view of rationality as involving criticism can offer quite a few suggestions to add to the theory of centre and periphery. Had the centers of learning proceeded according to his critical view, they might put on their agenda not only criticism, but also self-criticism as centers of learning. They would then put on their permanent agenda the question, since centers of learning cannot avoid misleading the periphery altogether, how can they reduce it to the barest minimum? How can the errors of centers of learning be least communicated to the peripheries? It is too optimistic to assume that centers of learning can and will be very self-critical. Had the peripheries of learning proceeded according to the view of rationality as involving criticism, they might put on their agenda the question, how can the periphery criticize effectively the centers of learning? Moreover, they could ask, how can the periphery control the centers of learning against too much folly? As the centers of learning determine the background-knowledge for the study of any problem, one can only hope that the centers concerned with the problem of rationality will put the facts here mentioned on their agenda. If they act responsibly, they will. They do not.

As Popper presented standards of interpersonal criticism, he supposed that parties to a critical debate are sincere and ready to offer and accept criticism that is as severe as possible. This is not consistent with Popper's views that science works through institutions that encourage criticism and foster safeguards for its maintenance, unless the encouragement and safeguard of rationality are merely parts of the sincerity of individual scholars and their readiness to have their ideas severely criticized. Yet more is needed, such as the readiness to defend critics against personal censure[32] and the readiness of centers to give criticism a fair hearing by putting possibly valid criticism on the public scientific agenda — amicably and generously, and in disregard for self-interest. to some extent all civilized people meet this need, but some institutional reform is urgently required all the same. Popper compares the ability to reform institutions to the ability to correct hypotheses.[33] Hence he should have admitted that the institutions of science are open to reform too. Since they embody scientific rationality, it too is open to reform. His presentation of the standards of the rationality of science in the abstract is understandable, not to say unavoidable, but a limitation it is all the same.[34]

Peripheries do not always follow the centre. When they do, and when they follow poor suggestions, they may still find new worthwhile ideas, since even the dwelling on stagnant ideas may produce new insights; misunderstandings can sometimes do so too. The centre seldom recognizes ideas that emerge in the periphery, and then only after prolonged delay.[35] this is unfair, and it is reasonable to ask whether and how improvement is possible. Can the periphery exercise some control over the centre? Yes, but on the proviso that it displays some autonomy. It is easy to see how to apply these observations fruitfully to the history of science, especially to the history of the social background of science. In particular, when did science become academic and how? Was it a Good Thing or a Bad Thing? Did it strengthen the centers or render the periphery more independent? Does the internet help here?

Peripheries naturally find it very difficult to maintain autonomy. Members of peripheries feel isolated and they naturally seek communion with the rest of the commonwealth of learning, and it is only reasonable that they try to do so through communication with recognized centers of learning. This can cause needless delays in the transmission of any new ideas between peripheries, if these have to pass through the procrastinating centers. Centers are likely to procrastinate even more before they add new ideas from the periphery the body of required background-knowledge. Recognition of significant contributions from peripheries might be interpreted by centers of learning as threats to their status as centers of learning. Centers can act properly despite such threats. This is why the more evolved a science, the less the centre allows itself to procrastinate in recognizing new ideas from the peripheries.[36] Here the internet certainly helps.

Let us return to rational choice, then. Most general is the choice of a way of life. The choice of specific issue for critical discussion is much more specific. The case studied here is in between: it concerns the choice of agenda for discussion. To make it more concrete we can ask, how can scholars increase the rationality of their intellectual way of life, their critical discussion, their scientific and scholarly agenda, the agenda that the centre of learning prescribes to its peripheries? How did they do this in the past?

Replacing the dependence of learned peripheries on centers of learning by some measure of mutual interaction requires the study of procedures and an examination of the degree of their rationality, as is done already in international conferences of some learned associations. This may permit the achievement of a higher level of rational discourse than was earlier arrived at, especially when the agenda includes some very simple and obvious criticism of received procedures. One may ask, how can the peripheries acquire rapidly the procedures that are essential to science and that are accepted in the more advanced industrial parts of the world but are wanting there?

The Rationality of Criticism

Tradition deemed identical standards of rationality and of science, on the questionable supposition that there is but one standard of rationality, practiced best in science across the board. The standard of scientific discourse had to be instituted. Science is at present triumphant, at least in the modern industrialized world, despite all hostility towards it. Though universities managed to keep science out until the French Revolution, since the beginning of the twentieth century they deem science an academic monopoly; by now science and the academy are identified, and as the proper place for education and research. Today the academy controls all cultural, artistic and the technological education. The reform of the standard of rationality is thus becoming increasingly important. Standards of criticism were established and reformed to prevent unwanted repercussions of the introduction into the market of technological innovations. These standards too are in need of criticism and improvement.

And so, whatever rationality is, the prime *desideratum* of it is that it should provide for criticism, that it should raise the standards of criticism and that it should help raise the readiness to be criticized, especially when making far reaching proposals. Discussions of international coordination of research should concern relations between centers of learning and their peripheries and be the most far-reaching. These relations should alter to cater for one urgent need, the need to devise institutions that will raise the readiness to accept criticism and to debate critically and democratically any agenda, including particularly the choice of agendas for debates. Here the study of the history of the institutional reform of science and of technology may be of great use. Here historians of science can perform a valuable practical role, and they should debate this matter with every possible visiting scholar — after their lectures or in the corridors or in faculty clubs.

To conclude, all writers on rationality admit as a matter of course that criticism is valuable, but except for Popper and his followers, their views on rationality are meant to include criticism as one of its items, but they hardly make room for it. At best they leave it as marginal and as ancillary. All theories of rationality are oblivious of the matter of agendas for rational discourse, except for the theory of center and periphery. The merging of the different theories — the theory of rationality as (largely) critical discourse and the theory of rationality that requires rational discourse about the agenda of rational discourse, and the theory of centers and peripheries — combined they should lead to some fruitful and far-reaching reforms of the institutions of the commonwealth of learning.

NOTES

* Edward Shils Scrutinized many earlier versions of this chapter and made extensive comments and suggestions on each and every one of them.

1. Joseph Agassi and I. C. Jarvie, *Rationality: the Critical View*, 1988, Part II, Rationality and Criticism, including my essay “Theories of Rationality”, pp. 249-63 there.
2. Karl Popper, *Objective Knowledge*, 1972.
3. See the commentaries on Greek philosophy from John Burnett’s classical *Greek Philosophy* to all introductory works which follow Bertrand Russell’s classic *The Problems of Philosophy*. See also my *The Siblinghood of Humanity: An Introduction to Philosophy*, 1991, that elaborates on this classical point.
4. This retort is handy. Some philosophers used it to support their claim that commonsense is universal and that it embodies the canons of rationality; some moralists used it to support their claim that morality is universal and that it appears in the diverse moral systems of diverse societies; all generative linguists used it to support their claim that grammar is universal and underlies the different grammatical systems of different languages.
5. See my *Towards a Rational Philosophical Anthropology*, 1977.
6. See, however, Dan V. Segre., *The High Road and the Low*, 1974. See also my “Technology Transfer to Poor Nations”, in Edmund Byrne and Joseph C. Pitt, editors, *Technological Transformation: Contextual and Conceptual Transformation*, 1989, pp. 277-283.
7. I. C. Jarvie, *The Revolution in Anthropology*, 1964, his *Rationality and Relativism*, 1984, and my *Technology: Philosophical and Social Aspects*, 1985.
8. Edward Shils, “Centre and Periphery” in *The Logic of Personal Knowledge: Essays Presented to Michael Polanyi in Honour of his 70th Birthday*, 1961; republished as

the opening essay of his *Centre and Periphery: Essays in Macrosociology*, 1975, pp. 3-16 (and in *The Constitution of Society*, 1982, pp. 93-109).

9. Karl Popper, "The Myth of the Framework", in Eugene Freeman, editor, *The Abdication of Philosophy: Philosophy and the Public Good (The Schilpp Festschrift)*, 1974, reissued in his *The Myth of the Framework*, 1994.

10. Hegel labeled "alienated" people not rooted in their tribes. This is Pinel's neologism for the mentally ill (out of respect for them). Wittgenstein's later philosophy is obscure. Perhaps he endorsed relativism as a solution to the problem of rationality. This will make his obscure term "forms of life" mean lifestyles, more accurately, the views and values inherent in some culture. Peter Winch first proposed it (*The Idea of A Social Science*, 1958). Philosophically-trained (not anthropologically-trained) Wittgenstein scholars consider it *passé*. This makes Wittgenstein too obscure to invite useful hermeneutics. See my "Wittgenstein and Physicalism", *Grazer Phil. Studien*, 1992.

11. Michael Polanyi, *Science, Faith and Society*, 1946; *The Logic of Liberty*, 1951; *Personal Knowledge*, 1958; *The Tacit Dimension*, 1955. See also my "Sociologism in the Philosophy of Science", reprinted in my *Science and Society*, 1981, pp. 85-103.

12. See my *Science and Society*, mentioned in the previous note, p. 201 and the reference to Polanyi there.

13. L. Pearce Williams' famous paper, "Should Philosophers be Allowed to Write History?", *Brit. J. Phil. Sci.,* 26, 1975, 241-253, presents history as inductive, thereby blocking my solution to the problem, what role can visiting lecturers in the history of science play?

14. See note 8 above.

15. The oversight of the novelty of the problem is not surprising, though it is a manifest aspect of the sociology of current learning, as the reluctance to pose problems is rooted in traditional dogmatism, and dogmatism is averse to posing both disturbing problems and innovative solutions. See my "On Novelty", reprinted in my *Science in Flux*, 1975. See also my "Sociologism in the Philosophy of Science", mentioned in note 11 above. It is an important aspect of the sociology of questions that usually questions are not posed to the public until they are solved. See my *Towards an Historiography of Science*, above.

16. see note 11 above.

17. Karl Popper, "Degree of Confirmation", 1956, reprinted in his *The Logic of Scientific Discovery*, 1959, Appendix *ix.

18. The comparison between Polanyi's and Popper's views and the way Lakatos intentionally conflated them is the subject-matter of the preface to my *Science and Society* mentioned in note 11 above. See also my *The Gentle Art of Philosophical Polemics* mentioned in note 2 above, especially pp. 327, 335 and 372.

19. Imre Lakatos, "Changes in the Problems of Inductive Logic", in Imre Lakatos and Alan Musgrave, editors, *Problems of Inductive Logic*, 1968, pp. 75-6.

20. My *Technology*, mentioned in note 7 above, Chapter 11 and Nathaniel Laor and J. Agassi, *Diagnosis: Philosophical and Medical Perspectives*, 1990, 27-28 and 34-35. See also 101 and 152 there.

21. Martin Buber, *Paths in Utopia*, final paragraph of opening chapter, "The Concept". See Abraham Schapira's new, annotated German edition, *Pfade in Utopia*, 1985, pp. 27-8 and notes there.

22. Joseph Ben-David, *The Scientist's Role in Society: A Comparative Study*, 1971.

23. My *Towards a Rational Philosophical Anthropology*, note 5 above.
24. My "The Methodology of Research Projects: A Sketch", reprinted in my *Science and Society*, mentioned in note 11 above.
25. Karl Popper, *The Open Society and Its Enemies,* 1945, Vol. 2, Chapters 23 and 24, pp. 219-20, 225.
26. My "The Nature of Scientific Problems and Their Roots in Metaphysics", reprinted in my *Science in Flux*, mentioned in note 14 above.
27. My "Sociologism in the Philosophy of Science", mentioned in note 11 above.
28. The literature on this is too vast to survey here. Let me mention one item: Robert S. Cohen and Marx W. Wartofsky, Editors, *Methodology, Metaphysics and the History of Science*: *In memory of Benjamin Nelson*, 1984.
29. When is error culpable? The philosophical discussion of this legal question is prevented by the philosophical idealization of rationality to the level of "error prevented", to use Augustus deMorgan's words (see his *A Budget of Paradoxes*, second edition, p. 78). See my "The Confusion Between Science and Technology in Standard Philosophies of Science" and "Imperfect Knowledge" both reprinted in my *Science in Flux*, mentioned in note 14 above.
30. See note 27 above.
31. See my "Bye Bye Weber", *Philosophy of the Social Sciences*, 21, 1991, 102-9.
32. Kuhn said, normal scientists who do not conform can lose their jobs, and rightly so. Popper said the opposite: their assertion is not quite true and it should not be.
33. Karl Popper, *The Open Society and Its Enemies*, ii, 222
34. Hans Albert rightly criticizes (*Traktat über Rationale Praxis*, 1978, Part II, Chapter 5) Popper's identification of the rationality of science as an institution with abstract logic. Yet this identification is the crowning glory of his achievement. See my *The Gentle Art of Philosophical Polemics*, 462-3.
35. My "Cultural Lag in Science", reprinted in my *Science and Society*, mentioned in note 11 above.
36. The observation that in the better sciences new ideas spread faster is the core of Kuhn's theory of the paradigm, though he puts it in absolute rather than graded terms. This observation is easily confirmed by even a superficial examination of the state of different departments in universities in undeveloped countries, though it certainly merits a closer examination.

4. Between the Philosophy and the History of Science

The oldest histories belong to myth-systems, and all myth-systems are all-encompassing: they come whole; they are not divided into categories or to subject-matters or to any other divisions. The opposite of the myth world is the world of science. Let me discuss jointly the science of myths and the myth of science. The former is fascinating. The latter is most potent and dangerous myth in today's world. All sorts of people perpetrate it, prominent among them are philosophers and historians of science. The following was said first in a colloquium in the philosophy and the history of science, as it seemed to me the right place to express my alarm.

The myth of science is an odd fish, since science traditionally opposes all myth. Myths are always hard to articulate, as they often express mere attitudes. Take the attitude of awe, for example, expressed in many a myth. The myth is the attitude, not the story that carries it. The attitude of awe towards science is expressed in myriads of ways, including, in particular, the total trust of physicians, especially surgeons. The result of the excessive trust is often no less than the avoidable loss of life. This is neither surprising nor a novelty; Bernard Shaw expressed it in the Preface to his *Doctor's Dilemma*, and a recent program about it on public television states this clearly and in detail. My contention here is that philosophers and historians of science, on top of being scholars, are dangerous myth-makers who contribute significantly to the myth of science. This is no accusation: had they known what a danger their contributions constitute, they would shudder. But they are often exceptionally self-righteous so that they are quite inaccessible to critical debate. This is then one more effort on my part at a plea for a rational debate.

The following important theses are now admitted by almost all political and cultural historians, by all social anthropologists, by all post-modernist philosophers and historians, and by a few philosophers and historians of science as well.

(1) The myth world does not disappear overnight. The process is gradual, even though at some stage it may accelerate and even bring about the collapse of traditional myth-centered society.

(2) We are still not free of all myths; no modern society is free of individuals and sub-cultures living in the myth world, and none of us individually is fully free of all myths.

(3) Nevertheless, there are some (relatively) myth-free cultures and some (relatively) myth-free individuals. When speaking of the myth-free, we refer to those who are relatively, not wholly myth-free.

Myth-free cultures and individuals, then, have evolved fairly gradually: the process of emergence from the myth world is slow, arduous and never ending.

There are exception to the acceptance of all this, of course. Apparently, most post-modernist philosophers and historians usually speak as if in they advocate the view that since there is no myth-free individual or culture, even being relatively myth-free is impossible. Not so. They will, of course, admit the existence of relatively myth-free cultures, but only under pressure, and then only temporarily, as long as the pressure lasts. This mode of conduct is to no avail: it is a significant characteristic the admission of myths: under pressure, their advocates may well deny them for a while. Therefore, possibly we should class the post-modernist philosophers and historians who speak that way as still in the myth world rather than as myth-free. So that what they say, though silly, is still true of them.

Another kind of exception is the historians and biographers who regularly perpetrate all sorts of myths as history. These myths are of diverse kinds, but they fall nicely into categories, as most of their perpetrators are historians of specific religions. Among these, I tend to include those whose religion is science, those who masquerade as historians of science

Let me mention the earliest myth-free western histories — regrettably, I know too little about other cultures. The earliest western histories not quite universal and not quite in the standard mythic style, are of Hebrew and Greek origins. Karl Popper has suggested that history proper — rather than a mythical history, at times called scientific — can only emerge out of a critical tradition. By “critical” he meant the readiness to examine some descriptions; we should modify this either to include in the objects of criticism also prescriptions, or else to include in the deviations from mythical history both criticism and admonition. For all we know, prescriptive admonition came before descriptive criticism. But perhaps I am finicky, since in the myth world, from which the less mythical history has to emerge, of necessity hardly differentiates between the two, since the myth world is more integrated than the scientific. Not only is the Egyptian goddess of truth also the goddess of justice. Anyone steeped in biblical idiom knows that the Lord is the God of Truth and Justice.

The earliest western histories are religious and political. There is no need to seek a profound explanation of this: it has a simple and straight forward one. Writing was initially found in temples and in courts. Their function was first and foremost mnemonic: the Egyptian goddess of writing was initially the goddess of memory. Most of the oldest documents extant still are religious, including first abjurations, amulets, and similar magical texts; later on older sacred texts were written down. Next come records of inventory, and then monuments, mainly records of victories and other great deeds. The legend of Judah the patriarch indicates that records of inventories were also kept elsewhere, not only in courts and temples. Yet Judah’s records were not written: they were marked on sticks and knots on strings, and identified only by the personal seals that accompanied them. Writing proper was practiced in courts and in temples: hardly anywhere else could it be practiced.

We find history proper narrated in the biblical Book *Samuel II*, in the narrative of the admonition by Nathan the Prophet of King David: he took his soul in his hand and bravely pointed his finger at the king and said to him, "Thou art that man!" The story of King David's son and successor, King Solomon, is narrated as a myth in the old style. Nevertheless, admonition went on, and so did history. *The Book of the Kings of Judah* and *The Book of the Kings of Israel* are both lost, and so we know little of their contents; it is reasonable to assume that they were mythical, but not fully so, though probably they comprised history proper intertwined with myths or semi-myths, as does the biblical *Book of Kings*.

This leads us to the problem of the demarcation of myths and more so of semi-myths, especially if we are brave enough to entertain the suggestion that historians of science are court chroniclers who are engaged in the making of semi-myths. It is easy to demarcate myths proper: a myth proper is a part of a myth system proper, and myth systems proper are universal or undifferentiated; they belong to the world of myth, so-called. The demarcation between the myth-world and the science-world is unproblematic. This much is generally admitted today.

We can place some conditions on what we would consider a possibly adequate demarcation between science and semi-myth: we want it to admit any history that is split into church history and state as history as a departure from myths, perhaps only the slight beginning of such a departure. What should count as a more significant departure from the myth world, what is needed to be fairly outside of it, is a critical apparatus that enables one to disassociate oneself from one's narrative: the opposite of myth is disinterestedness, and so the grading of the distancing of oneself from myths should be not so much rationality as disinterestedness. The conditions that give rise to this wonderful phenomenon are harder to discover than the conditions that make it possible. (The confusion of these two searches, of the necessary and the sufficient conditions for disinterestedness, incidentally, is mythical, yet it also characterizes Marxism, and incidentally also what makes so seemingly enlightening. The ability to mix science with myth or semi-myth is irresistible for ones uncomfortable with disinterestedness or criticism.) What is disinterestedness? It is the readiness to entertain the possibility that we are in the wrong and/or in error. At the very least, it is the ability to consider for a while the interests of others in preference of one's own. This neither kings nor priests tolerate. The myths of Homer, although they praise their hero, nonetheless, when in a fit of anger and envy, they describe him as using shamelessly any weapon against a best friend, including the critical abilities of that friend.

Claude Lévi-Strauss said, myths think for us. This idea can be used not only for the myth-world as intended, but also for myths in the age of enlightenment: myths are thinking patterns that cannot easily be broken, that are occasionally broken by criticism and by an increase our disinterestedness.

Today we find it hard to see what difficulty myth-ridden people have to distance themselves from some myths about nature, such as creation myths. We see no difficulty there, as we see no personal involvement in the myth and so no more difficulty involved in the readiness to affirm or deny it. The difficulty is different in the myth world and in the science world. In the myth world the difficulty is obviously dual: people there find it difficult to be critical in the first place, and they find it particularly difficult where the denial of any single thesis leads to a collapse of the world view received in their society (since it is an integral part of it, since the myth world is well integrated). Once the myth world crumbles, the picture is different. For intrinsic reasons, Marxism, for example, is incompatible with a physics that permits the unleashing of forces strong enough to destroy the world, since it is optimistic; but there is no intrinsic reason for it to affirm or deny that the universe is infinite, since Marx' view goes no further than his endorsement of the physics of his day, whatever it was. He endorsed the physics of his day for the same reason that he would have endorsed a different one had he lived in a different time. Yet it is nonetheless difficult for a Marxist to deny the infinity of the universe contrary to Marx' own view; this is a different kind of difficulty: it is the difficulty to hold heretic views on whatever question and regardless of their being marginal. The inability to alter one's view under the pressure of criticism is dogmatism. Dogmatism is contrary to the spirit of science, yet it belongs to the science world (as an aberration), not to the myth world.

The ideology of science, whatever it is, is opposed to myths and to whatever myths constitute. Let us endorse this for a while without debate. This is the theory of the Great Divide or of the Big Ditch, still endorsed by Max Weber in the dawn of twentieth century and of Ernest Gellner in its twilight. It is hard to hold this theory, since the view of the presence of a gap between magic world and the science world suggests that the transition from the one to the other is and must be in one great leap, both ontogenetically and phylogenetically: the beginning of science must be abrupt, both for a culture and for an individual. In fact, few of us remember such a sharp transition in our lives. We can still meet individuals who live in a pre-critical intellectual environment, some of them adolescent and some of them adult. Efforts to break to them some scientific views, such as Copernicanism, do not shock them; those of them who in due course do move to science do so not in a state of shock. Intellectual shocks do occur. The discovery of religious heresy as well as the discovery of some crazy ideas such as solipsism, and the discovery of the critical spirit and of moral and intellectual autonomy, these processes can be and often are met with a shock: intellectual shocks do occur in our present society. But, to report matters empirically, the discovery of some ideas does shock, but the possibility to replace myth with science does not.

Why? Why is the discovery of autonomy shocking but not the discovery that magic is possibly false? I do not know. But it is evidence, and a very strong one, that the commonly admitted association of science with the critical spirit is not necessary. Indeed, many autonomous science students who are critically minded as a matter of course still have to learn of the critical canon and of the idea that it can and should be applied within science and to science itself. Some respond to this idea with great pleasure and others in annoyance and in a declared wish to ignore it. Richard Feynman describes in his autobiography science education in the underdeveloped world as intrinsically different from that in the developed world, as there the critical spirit is so scarce, yet like many physicists he disliked the philosophy of science because it preaches the critical canon.

There is almost no literature on this. The only paper I found is by the famous early-twentieth-century Canadian psychologist D. O. Hebb, called "Alice in Wonderland". Graduate students are disheartened, he said, by their refutations rather than confirmation of their hypotheses. Hebb suggested to them to take it lightly and use commonsense. These students of Hebb believed in the myth of scientific success not as a myth but as a fact, and so they were shocked to find that scientific success was not guaranteed.

The idea of guaranteed success in science has characteristics of a myth, of a cargo cult, yet we may take it for a proper theory or set of theories: under some specified conditions scientific success is guaranteed. And then each specification of these conditions makes for a different theory, and one that we can put to test. If we wish to take it as a myth, we better keep the statement of the conditions vague. The theory of Claude Lévi-Strauss that myths think for us is based on the suggestion that myths are complemented by counter-myths, since what is not covered by the myth is covered by the counter-myth. Moreover, when it is hard to decide which to apply, the myth or the counter-myth, tradition offers a mediating myth, Lévi-Strauss observed. The myth of guaranteed scientific success is due to Sir Francis Bacon. It was the myth of inductive science-making machine: the machine's input is factual information, gathered with no assistance from any theoretical apparatus (that is to say, as accidental discoveries); its output is verified theories. The myth is complemented Lévi-Strauss-style by the myth of the powerful penetrating intuitions of great researchers. This myth was made as palatable as possible by William Whewell, but as a theory, not as a myth, as he sharply denied that any guarantee of success is possible. He also denied that accidental discovery is possible, as all factual information is theory-laden. He declared that as all scientific ideas are hypothetical, there is no reason to suggest that they are true, so that they must undergo the most careful and rigorous tests. When refuted they must be rejected, and when corroborated they are verified.

The myth of accidental discovery survives all criticism. It is still popular and far-reaching. It dominates the study of child development, on the basis of the crazy idea that infants possess no myth, since they are inculcated with it early in their education that begins only after their early infancy is over. Early infancy is viewed as the pre-educational, though, obviously, education begins at birth and in many senses the world of infants is a proper myth world. The idea of accidental discovery is the idea of the innocent eye, the idea that the innocent and the naive realist are the same. Its most sophisticated expression is called "logical atomism". It is the theory that when Ludwig Wittgenstein developed its most rigorous expression he thought this was the demise of philosophy. Bertrand Russell disagreed, but still never could shake it off and advocated it to the last as the hypothesis that offers the best foundation for science. Willard Van Quine called it a dogma of empiricism. The myth of accidental discovery survives, both as a part of the impressionist theory of the visual arts that and as a part of the myth of science as inductive: the innocent eye sees the world in the naive realist mold. That this is a myth is clear from the fact that people can speak of the counter-intuitive character of modern physics and endorse inductivism without noticing the glaring inconsistency between them.

Baconian inductivism implies a very important thesis: science and its history are one. The reason for this is very strong: within science both factual information and theories are final. Hence, all that a historian of science can do is report the dates of the diverse discoveries and perhaps some of the accidental circumstances under which they took place. These circumstances, however, must be irrelevant to science strictly speaking. In particular, all ides other than the scientific ones are utterly useless for science at best, and usually they are detrimental to it. This idea of Bacon's is his once so famous doctrine of prejudice. It is a combination of two separate discoveries: first, that facts are theory laden, and second that people will pay a lot to sustain their self-deception that they are infallible. Hence, says Bacon, it is important to start with a clean slate. Then we can see facts as they are and have theories evolve from them without the intervention of any hypothesis. (On a second thought he allowed researchers steeped in facts of nature to make small conjectures, and Robert Leslie Ellis, the greatest Bacon scholar, declared this move a cop-out and am admission of a bankruptcy.)

Bacon's idea that science and its history are one was refuted. Even the very rise of Copernicanism contradicts it, since Copernicus never cleaned his slate. This raised the question, does the innocent eye see the truth of Copernicanism? Does it at least harmonize with it? John Locke said, yes, since the innocent eye does not see the motion of the sun in the firmament: this is a deduction from seeing the sum in different places. The idea developed into the view of sense *data*, the view that what we really perceive cannot be contrary to Bacon's theory.

The next step was taken by Laplace. His *Systeme du monde* is a reconstruction of Newtonian astronomy as if it has emerged from sense *data*. The last chapter of the book, the famous historical part admits that the real history diverges from its reconstruction. This is the end of Baconian inductivism as a theory and its transformation into a myth proper. As a myth proper it invited its opposite pole. This was supplied soon, when Whewell invented his view of science as hypothetico-deductive, a view that he claimed scientific status for as it was testable by reference to history and by tests corroborated.

Though it was largely corroborated, it could be questioned, and indeed it was. That a theory of science is corroborated by its history is not very surprising, since, in full accord with what Bacon had said, whatever historical event that does not fit into a theory of science has to be declared as unscientific and that secures that the theory about science will be only corroborated and never refuted by facts from the history of science. For example, all false theories have to be declared unscientific by both Baconian and Whewellian historians of science.

There is a limit to this exercise, yet quite a number of historians have gone over the limit. We cannot declare Newton's theory unscientific, and so some scientific theories are false — although approximately true nonetheless.

The question that all this raises is, how should historians of science select their material? This question does not obtain for political history. Political historians know what political power is, and if they find a detail that is not a matter of political power but pertains to it, then they do not find any difficulty describing it and discussing its relevance to power politics. Even if they do not understand it, they can report it. The controversy over the Eucharist, for example, is obscure: what is the difference between saying that the bread and wine served in church during communion is symbolically the flesh and blood of the Savior or literally so due to a recurrent miracle? We do not have to fathom this for us to know that the religious wars were largely centered on this controversy. Not so in the history of science: whatever idea had an influence on science has to be reported and explained by the historian. But this is a tall order. Do historians of Copernicanism have to discuss his view that the sun is the center of the universe because it is God's throne, or Kepler's variant of it that presents the sun as the symbol for God the Father? The wish to understand this leads one to study the cabalistic system and symbolism, or Pythagoreanism, that is no small matter. Even Frances Yates, who only dabbled with the cabbala, found much objection to her studies, and she studied the cabbala through the spectacles of Gershom Scholem, whose views were centuries out-of-date, as he held an eighteenth-century Baconian idea of the cabbala as non-science that had no influence on science and its similarity with classical astronomy is insignificant.

The traditional study of the history of eighteenth-century chemistry is a paradigm of Baconian historiography. Its students reported the great scientific discoveries of the period while taking it for granted that phlgoistonism is unscientific and so useless for scientific research. This seems *a priori* implausible: the great researchers of the time declared phlogistonism scientific *par excellence*, and so they naturally used it in designing the experiments in which they made their great discoveries. By Popper's demarcation of science, as phlogistonism is refuted, it is refutable, and so it is scientific. To be precise, it was a series of theories, each of which was refuted by some great discovery. Let me mention one lovely example. Joseph Priestley's greatest discovery was that of dephlogisticated air that later Lavoisier called oxygen. He insisted that he made this discovery quite accidentally. Indeed he was one of the most serious followers of Bacon, who could not allow that the theory hailed so enthusiastically as a great scientific achievement was refuted, since refuted theories are superstitions and prejudices, of course. He had proof that his discovery of oxygen was accidental. He said, it was an accident that while he prepared his air he had a candle in the vicinity and placed it in the container where its bright light was indication of the discovery that the air was highly conducive to combustion. He had evidence that the candle was there by accident: it was there because he expected the air in the container to be foul (detrimental to combustion). This is clear, then: having had only two categories to choose from, deducibility and independence, as he could not choose the one he found the other the obvious choice. But there are three options, not two: in addition to deducibility and independence there is contradiction. And as Priestley himself noted, he had an expectation that was disappointed, so that the experiment was not without any expectation, he was in error declaring it accidental: it was a refutation. I have discussed the refutation elsewhere and illustrated the significance of the refuted theory. I discussed the in *Historiography*. I also discussed there Lavoisier's theory that was also refuted. Consequently, Dr. Thomas Thomson declared Lavoisier's theory a prejudice. As I wrote this, most historians of chemistry were Baconians and so they insinuated or even asserted that it is true. Today there is an alternative: it is declared a paradigm in Kuhn's sense and so irrefutable. Kuhn's theory is a myth. Lakatos tried to recast it as a different myth, that of distortion viewed as rational reconstruction.

Rational reconstruction need not be a myth. It is anyway problematic, however, as it is done in frank disagreement with historical records. In political history this is at times necessary, as when we possess conflicting reports of a conflict provided by chroniclers from the opposite sides. But there the views of the chroniclers are admitted and when historians contradict them they express their opinions openly. Is this the same in the writing of the history of science? Do historians of science disagree with their heroes about science and its methods? Pierre Duhem said, they must, since different researchers had advocated competing opinions on this matter. This is problematic, as it amounts to saying that researchers act rationally without knowing what they were doing. This made Lakatos offer an irrationalist idea: he said, Hegel's theory of the cunning of history takes care of it.

Rational reconstruction need not be objectionable, even when it goes way out. One of the boldest and most significant reconstructions of Newtonian dynamics is to be found in Einstein's *The Meaning of Relativity* (1921) where it is described as the simplest differential equations invariant to Galileo transformations. Newton had no idea about invariance. Yet Einstein did not distort Newton's ideas: he was offering his own new version of an old idea. To iron out the differences between Newton and Einstein, like Howard

Stein, do not violate history if and only if they do not deem their results as the true meaning of the old heroes of the past. It is easier to

The following observations are all true, and their combination is not very palatable as it leaves too much undecided. First, science is always mixed with myth. Second, there is no theory of myth other than of myth proper, which is not relevant to the history of science; much less is there a theory of the role of myth in science. Third, it is theory that helps us decide what ideas that may be of historical import we should count as science. If we select a detail that our demarcation of science excludes from science, then do we have to recast the demarcation or ignore that theory? As we use our demarcation of science to decide what historical factor is relevant to it, and as we do not have a proper theory of science and myth, things are pretty much in mid-air. It is not that we cannot do anything in the meanwhile: we are not paralyzed, and we can still develop much of our understanding of the history of science by applying Popper's views and even later ones. And at the very least we can notice that the history of science is written from different viewpoints and includes some gross errors that we can try to eliminate. My main advice from 1963 still stands: let historians of science write clean histories and say explicitly in them what motivates them and declare as openly as they can in the light of what rule of selection they write. But we still need a better idea of what science is and how it operates.

Let me conclude. The view of myths as parts of the integrative myth world frees us of demarcating single myths but not the myth world itself. This was characterized in ancient Greece as truths by conventions, demarcated from truths by nature, and these are demarcated as proven theories. This is unsatisfactory. It is too easy for people who live in one myth system to translate and assimilate diverse myths. There was never any trouble identifying Ishtar with Aphrodite and Venus, for example, and myths travel, as the myth of Gilgamesh that appeared in diverse pleases in the Fertile Crescent. How far this inclusiveness went or could go is a difficult question. What is obvious is that not myth but ritual varies in an uncompromising manner. Thus, when Herodotus discusses the matter he contrasts burial customs and he describes hostility to magic. That hostility to magic is insufficient to describe the myth world is obvious from the Bible that is hostile to magic but is full of it — presumably only rites performed by a member of the Cohen family in accord with the right code are admissible.

Opinions other than scientific did not interest Greek philosophers overmuch. In line with this, Bacon and all his modern followers spoke with one breath of superstition, prejudice and dogma. Dogma, however, is the conviction that one's views are proven, and so it is more akin to pseudo-science than superstition that is an idea that even it advocate does not quite adhere to, a sign that may or may not prove right, like much magic that is always in doubt since a blessing or a curse may have effect or not depending on ever so many unknown factors. The idea characteristic of the magic world is that these are objects that move about and succeed or fail to have effect. This cannot depend on accident, but on the precise way they were generated and on the possibility of there being a more powerful magic act to destroy its potency, and more.

Prejudice, incidentally is a bunch of possible opinions that are neither dogmas nor superstitions. They are often endorsed without notice, or they are preferences or discriminations, friendly or hostile dispositions toward someone and more. What they do not share with science is the wish to explain and the readiness to accept criticism and change one's mind.

All this makes the adherence to phlogistonism possibly scientific and possibly dogmatic, depending on one's ability to rethink. But then the same holds for Newtonianism and for Marxism. It is hard to deny that few advocates of Marxism early in the twentieth century were offering it as scientific researchers. Piero Sraffa and his followers stand out as almost the only exception. Most of most advocates of Marxism early in the twentieth century were dogmatic. Not so their twenty-first century heirs: they are scarcely familiar with Marx's writings (or else they heavily qualify their assent to them) and so it is hard to know what opinion they advocate and whether it is a dogma or a superstition. By contrast, after 1919 few researchers advocated Newton's mechanics, among them such great thinkers as A. A. Michelson and H. L. A. Lorentz. Obviously, we have to view them as prejudiced by the criterion that Boscovitch offered in eh mid-nineteenth-century: an opinion is a prejudice if it is preferred over the known alternatives to it only because it appeared first. But it is hard to condemn them since they tried hard to develop versions of Newtonianism that would not be vulnerable to the standard criticism of it.

All this is fairly canonic. One need not assent to it; one should know it. If historians of science will, the result will be very beneficial.

5. Scientific Disagreement

1. Attitudes to Dissent

Why do historians of Science Disagree? This question is a part of a wider one: Why do people disagree? Four centuries ago, Sir Francis Bacon offered general answer to this: people disagree because they are ignorant of the truth. We do not disagree about what we know, which is the truth. Even if we do not know but we think that we know, then we do not disagree. In science, Bacon said, in true science, there can be no disagreement. If there is no disagreement about science, then there should be no disagreement about its history. For example, we may disagree about the date on which some important scientific event has taken place. Even that is not quite possible: since in principle scientific events are public, dates in the history of science are hardly ever contestable.

The most famous dispute about dates has to do with the discovery of the calculus: Leibniz claimed that he had a manuscript with a date on it that proves that he had it first; others said that the date on that manuscript is forged. This is a fascinating dispute. It has a number of interesting implications, yet it does not belong to the history of science proper, be it physics or mathematics, since priority is a matter of publication, not of discovery in private. This is a very strong argument. Even if we deny that science imposes unanimity, the unanimity about its public character renders it very hard to sustain a dispute about it.

One may contest the barely contestable. Kahneman and Tversky have shown empirically that people regularly contradict the calculus of probability; some of them refuse to be corrected, insisting that facts and theory differ. We may ignore these since we hope that obvious errors are not so endemic as to prevent progress.

The general puzzling case is that of disagreement between experts. Bacon said, experts are ignorant too — even in the area of their expertise. This is true but not very much to the point: ignorance is a necessary condition for disagreement, not a sufficient one. Most questions to which no answer is known are hardly ever asked. At times people ignorant of the true answer venture to guess it; and then they may guess different answers. This is unusual, since wild guesses are seldom expressed in science. When different guesses are not wild but also not well corroborated, they may find their way to the print and then dispute about their truth or falsity may but need not ensue. It seldom ensues, since people seldom wish to hear views different then their own, and then they have little patience to hear arguments that go against their views.

Paul Feyerabend was famous for his excessive liberal permission to all to assert whatever pleased them to assert; he admired both Lenin and Mach yet he never mentioned the one's critique of the other. The other is reported to have considered the one's criticism as resting on a misunderstanding; he would probably have said the same of Planck's criticism, as both critics disapproved of his phenomenalism as anti-realist. He never explained. This is a pity, since it would be very interesting for all to learn why he thought the criticism rests on a misunderstanding and what were it roots.

In the eighteenth century it was often said that dispute is like fire, it gives off much heat and little light. It was also observed that the first bout of a dispute may be of some interest, rarely also the second, but then it becomes repetitious and unenlightening. The idea that one should avoid dispute altogether is rare. It can be found in a very central essay of Robert Boyle, the founding father of the Royal Society of London, who laid for that Society the rules that was later generally accepted and became the standard: the discovery of a fact that constitutes a refutation of some hypothesis should be published, preferably with no mention of that hypothesis, since the advocates of that hypothesis will notice the refutation and saying so out loud will only make them leave research. The best item that reflects the attitude of the age towards dissent is the wonderful autobiography of Ben Franklin. He reports there that in his youth he was known as a master dialectician. He also reports, with pride, that he refused to notice the expression of dissent with him made by Abbé Nollet, the person whose views Franklin had empirically refuted.

The social background to the difference between attitudes to criticism now and then is obvious. First, researchers were then amateurs who had to be enticed to do research. Second, criticism was deemed an expression of contempt. Sir Francis Bacon said, professors disagree and debate for ages and there is no progress, so amateurs should do research and stick to facts since the targets of criticism never yield as they rightly take it to be expression of contempt.

Disagreements seldom happen; they are less frequently expressed as clashes of opinions, and then hardly ever debated about. Those who preach for harmony and against discord tend to identify discord with disagreement and conclude that disagreement is very common. This is obviously not the case, but then Bacon, at least, was speaking of the academics of his day, and they certainly did spend most of their time in barren disputes.

The opposition to the publication of controversial papers is still strong, and the instructions to authors that appear in some respected journals say, they do not publish controversial papers, and the same goes for papers that describe controversies. This is by no means generally the case. Debates raged in learned periodicals of economics during the decades of after World War II, regarding both the dispute between the followers of the neo-classical (Chicago) school and the Keynesians (Yale) and between the advocates of the view that economics is an empirical science and their opponents. Most scientists suggest that there are too many and too long controversies. When pressed they admit that some controversies are beneficial, and these, they declare, are sooner or later closed. The perennial ones are pernicious, they add, and they are mainly metaphysical.

The opinion is voiced that too few controversies are published is dismissed with the assertion that we all experience constant disagreement. When those who make this assertion are asked to offer examples, however, the cannot. They first refer to cases of conflict. Two children in a play-pan quarrel about a toy are in conflicts not in disagreement. Examples then are provided from politics: surely political parties do disagree. But political parties make conflicting proposals. To the extent that these express conflicts, they do not count. Yet at times behind conflicting proposals stand differences of opinions. Thus, proposals may express different opinions as to what is best for the national interest, say. This kind of disagreement is less about values and more about facts: will the nationalization of the means of production bring prosperity? After World War II most British political leaders said, yes. Nowadays hardly any of them does.

Still, since many people expressed this view in the past, it shows, perhaps, that disagreement is not so rare. Regrettably this is not so. Though opinions differed, most people did not express opinions often and most of them did not have the occasion to meet people with different opinions: disagreement is more than a difference of opinion: it is the contrast of them. We have, then, knowledge, ignorance, opinions held in ignorance, differences of opinions held in ignorance, contrasts between these, and finally clashes of opinions or debates about them. This is last stage is rare: most people are more ready to express opinions or to explain them than to listen. Yet clashes of opinion demand tolerance, readiness to listen, and critical exchanges or proper disputes. Those who seek harmony and confuse disharmony with dissent, they disapprove most of critical exchanges, less of contrasts of opinions, and less of difference of opinions. Yet most of them are opposed even to the very expression of opinions: we should teach ourselves to be responsible and say only what we know; this will eliminate all disagreement and all dispute.

Why stick to saying only what we know? The answer is the traditional theory of rationality. It says, rationality is scientific character and scientific character is provability: one should say only what one can prove. My own teacher, Sir Karl Popper, has made a revolutionary suggestion: replace proof by disproof: try to say only what can be disproved and then try to disprove it. In line with his idea I tried to go further: traditional philosophy valued rationality as a method of consensus, of agreement. It is better to begin with disagreement and to admit as a basic *datum* the fact that we recognize some disagreements as rational, some not.

1. On Disagreement in General

Opinions differ. Hardly any assertion (perhaps other than utter platitudes) will gain unanimity. Yet difference need not elicit disagreement. Differences of opinion are different from disagreements and these differ from exchanges about them. An exchange may be personal, it may explain and clarify a disagreement, or it may be critical: different sides may attack and defend their different views. This is what wins the greatest and deepest diversity: few face it with love and appreciation; others with dislike and disdain

Even the personal aspect of a disagreement, the quarrel regarding it, is not quite irrelevant to the matter of that disagreement. Agreement is valued and disagreement is frowned upon much too often and as a matter of course. Disagreement spells discord: friends agree; disagreements is between opponents. This idea was shared by the great fathers of modern philosophy. Sir Francis Bacon said, you cannot disagree with and respect a person, he said: disagreement suggests contempt. Perhaps the most famous example for this is the case of Sir Isaac Newton: he considered personal enemies all those who disagreed with him. Even his suspicion that someone may disagree with him sufficed for him as a basis for a profound hostility. Adam Smith wrote a wonderful obituary of his friend, the great philosopher and historian David Hume. He said, of his philosophy I will say nothing, as those who agree with it admire it and only they.

Bacon said, the reason for this is that no one wishes to lose in a debate. So everyone sees facts as if they accord with their theories, and everyone is ready to dismiss an argument against their views as insignificant, to justify this by "frivolous distinctions". The result is that every idea becomes a prejudice: its advocates becomes addicted to it and get their whole worldview distorted by it, even if they advocate it tentatively to begin with.

Bacon considered the professors in the universities as his evidence. They argue for their views and against the views of their colleagues; they do this for centuries and there is no end in sight. What should be done is, first and foremost, to give up all received opinion, and start afresh, carefully, without allowing any hypothesis into one's system, so as to remain utterly free of prejudice.

Descartes developed his philosophy after he realized that what they taught him in college was full of error. So he decided to give up all opinion and start afresh, accepting nothing except what he could prove. He said, his philosophy will bring peace to the world. In the same direction Leibniz developed his idea of a fully formal language that can be at least as rich as ordinary language, so as to build a computer in which competing claims could be assessed so as to be resolved without violence. (His slogan was, *Calculemus*!)

All this did not prevent differences of opinions from taking place. Yet they need not be confronted. This is traditional. In the eighteenth century disagreements were often not expressed, and when they were, there was no debate and when there was it was fast truncated, as dispute gives off little light and much heat.

This classical view of disagreement and of controversy is still popular, so that there is little need to expose it in detail. To say to scientists that the classical theories of their field are false, they take it at once for granted that this is an aggressive expression of hostility to science and even a personal dislike for the individual who has originated the classical idea that is declared false.

2. Viewing Criticism as Friendly

There are many stories about the sensitivity of leading intellectuals to dissent. I have mentioned Newton's sensitivity. But this is a matter of the distant past. So let me mention that Sigmund Freud was no better. His biographer, Ernst Jones, reports that once he, Jones, published a review of a work of Melanie Klein that included one passage of tentative, qualified praise. It nearly caused Freud to ostracize Jones. But things did change, as the following anecdote shows. Einstein once wrote to Freud that he was glad to report having found some corroborations to his ideas. Freud answered saying he always knew that Einstein's expressions of respect were mere lip service, since he could not endorse his views earlier and so he could not respect him till then.

Morris Raphael Cohen was one of the most famous philosophers in the United States. His leading disciple, Sydney Hook, reports this about him. As his first publication he had to write a review of a new book by Cohen. The review was highly favorable, of course, and it contained only one short, mildly critical remark. Hook was worried about the possible reaction of Cohen, so he consulted Cohen's best friend and closest colleague. That person encouraged Hook and assured him that Cohen would not be offended. But he was. Very much so. Very upset, Hook said, but even so-and-so said this is all right. I always knew he was a traitor, answered Cohen.

This story illustrates a change of ethos: Hook clearly considered Cohen's attitude unusual, though at the time it was quite common, even for later authors. This despite Plato's praise of criticism. In his *Gorgias* Socrates says, the critic helps the criticized to get rid of their error and so they are the ones who benefit from the criticism and so they should be grateful. In his last and most dogmatic book, *The Laws*, Plato again says one should not take criticism as affront but as help. Criticism, said Popper repeatedly, is a sign of taking an opponent seriously. Einstein had the same view. *The Bulletin of Atomic Physicists* asked him to respond to a political letter received from some Soviet physicists. He did, and he opened his response by saying, it was very hard for him to do so, as he could not follow in that case a habit that he used to follow: before attacking a position, he tried to adopt it as best he could. He clearly meant that he wanted to limit his critical responses to ideas that he found false but valuable, yet the political views of these Soviet physicists not serious enough. If criticism is a mark of appreciation, why then is it so upsetting to be criticized in public?

The answer was already given above: critics and their public often assume that criticism is contemptuous, and so they express it with contempt. Try this: ask a critic whom you knew personally, did you find nothing valuable in the work that you have criticized? They usually have no hesitation in answering in the affirmative. Ask them then why do they not notice this in their critical publication. This will puzzle them. In some cases critics are unaware that they use insulting language in the expression of their criticism. This shows that there is a need for some discussion of criticism and of their styles. This is intriguing, as it shows that the development of the proper style of criticism was a conscious process of discovery. Our standards of acknowledgment do not demand acknowledgment of criticism. Authors often mention individuals who have read and criticized their manuscripts, but they hardly ever mention what the criticism is: they deem the criticism a personal favor, and the acknowledgement a personal courtesy. Yet it is easy to show that some criticism was essential for some breakthroughs that followed them. Most effects that physicists name after their discoverers are refutations of previously received opinions and thus the heralds of new ideas (that explain these novelties, and that inductivists say rest on them).

Plato's dialogues are still the models for the critical style, especially the early, short ones. They are often misinterpreted: many commentators see the famous irony that Socrates exhibits there as license to lie: he is very learned but he pretends to be ignorant. One reason for the refusal to take the style of Plato's dialogues as a model in general — it would make dialectics the standard — is the refusal to take Socrates seriously. One reason for that is that in these dialogues Socrates or Plato often defends strange ideas. Now historians are often called to explain old oddities, of old opinions, customs, and religions. How successful they are is hard to decide, as there is a great variety here. But clearly the basic assumption is that there are ideas that we know too well that they are false, so that holding them today is not laudable, but that were reasonable enough to hold at their time. This shows that it is not intelligent to dismiss Socratic dialectics as a matter of course just because at times Plato defends ideas that we reject as a matter of course.

A conspicuous example is Plato's dialogue *Parmenides*. The famous English philosopher Gilbert Ryle wrote a terrific paper on this dialogue, trying to refute the view, very popular at his day, that this dialogue is not serious. He also wrote a terrific paper in which he tried to reconstruct the rules of dialectics as it was practiced in antiquity.

Both Galileo and Kepler were great dialecticians. Galileo wrote dialogues, and Kepler reported his series of errors that led him step by small, painful step to his famous results. Bacon, we remember, opposed dialectics as he saw it as casuistic. Boyle instituted the inductive style that Bacon had recommended in the statute books of the Royal Society of London. This landed Newton in a difficulty. It is obvious that if Newton's theory is true then both Galileo's and Kepler's theories are approximately true. In the theories of the time, however, the approximation to the truth was not considered good enough and in Bacon's system there is no room for anything that is in between truth and prejudice. So Newton had to pretend that he endorsed both views as absolutely true. This mattered little for a time, as Bacon's theory and Newton's theory together were sufficiently challenging and useful. In the eighteenth century they ruled the day, and the only important new idea was the phlogiston theory.

That theory was first taken to be true and then a prejudice, because in Bacon's system, to repeat, there was no room for any other status for an idea. So when Lavoisier overthrew phlogistonism, it was taken a great success and his wife threw a copy of the leading phlogistonist text into the fire. Some people, prominent among them was Ørsted, the discoverer of electromagnetic induction, said it was unfair to dismiss phlogistonism as a prejudice, since it was a good approximation to Lavoisier's system. But this was ignored. Historians of science presented his ideas as if they are absolutely true. They are not: every oxidizer that is not oxygen refuted him. He himself knew of such an oxidizer, since the strong attraction of hydrogen to chlorine was well-known. So Lavoisier decided that chlorine is a compound of oxygen and some element. Sir Humphry Davy made his name by refuting Lavoisier. He introduced the name "chlorine" for the chemical he declared an element.

Faraday did not suppress criticism though he was an admirer of Bacon and Boyle (he even called himself a Baconian). Davy held the view that water is essential for electrolysis. Faraday discovered alternative electrolytes. He reported that he had refuted Davy's view. Davy's brother protested in the name of his deceased brother. He said, his brother had made no false statement but was reporting his failure to find such an electrolyte. Faraday responded. He said, had I claimed that only water can serve as an electrolyte, Dr. Davy would have claimed priority for the idea to his brother.

Faraday noted that the prohibition for scientists to make an error forced them to make claims for new ideas by mere hints at their conjectures. Faraday opposed this. He decided to make his conjectures openly and express readiness to be corrected. He corresponded with others about the need to be frank about these things and not to play infallible. At about the same time William Whewell advocated a new philosophy according to which science cannot proceed without hypotheses, and so he claimed that scientific progress is a matter of trial and error. Both Whewell and Faraday were ignored by the establishment. Those who allowed for hypotheses in science, like Faraday's friend and successor John Tyndall, demanded that scientist make right guesses. This makes life intolerable. It is much simpler to agree with Faraday and Whewell that science includes conjectures and. As Whewell showed, though possibly many people refuse to accept criticism, out of pride or out of dogmatism or for any other reason, science does accept criticism, or else Newton's optical theory would have never been replaced by the new wave theory (1818).

3. Science and Its Rationality

What is wrong with the publication of all this? The answer is that this refers to controversies: they are not nice, as we remember, and publishing material about them is also not nice. It is like publishing the story of a scandal that involved a splendid person in a biography of that person. Many consider this bad taste. Is it? And if it is, is controversy scandalous?

Many important people say that it is. Clearly, the history of science is full of unpleasant items, but we can ignore them as not too useful to record. So the sociology of science as taught to the general public and to the scientific public is expurgated. This gives an impression that is quite erroneous, and very harmful to boot. This is so particularly as the history of the social aspects of science is told in bits and pieces, apropos of other matters. It leads to the taking for granted that scientific ideas are usually received with no opposition from the scientific community and that if there is such a resistance then it should be blamed on some prejudice of individuals who failed to live up to their scientific commitments. The story of the resistance to Davy's ideas was clearly not that: clearly the defenders of Lavoisier's ideas were convinced that they were defending science, as do the historians of science who pretend that Lavoisier's ideas are true. John Herapath was an important researcher — he revived the statistical theory of gases in the early nineteenth century — and James Clerk Maxwell considered this a major event in the history of thermodynamics. When historians of science discovered that he was important and that his way to publication was blocked, they censured the individual who was directly responsible for that blocking. This is most unfair. To begin with the Establishment tried to block all revolutionary ideas as they assumed that revolutions in science are impossible. Already Priestley offered this argument as his explanation to his resistance to Lavoisier. He was dismissed as prejudiced by many historians of science. Those who tried to rehabilitate him were sacredly able to justify the dismissal of his ideas, yet clearly although their condemnation of him was erroneous, it was neither foolish nor vicious. For, by the Baconian canon his error is the best evidence that he was prejudiced.

We are here dealing with two different items: the history of scientific discoveries and the history of the social conditions and psychology of these discoveries. And historians of science have views on them that need airing and criticism.

This does not answer the criticism of my view from eh fact that the history of science is full of unpleasant items that are better ignored. There is much truth to this criticism. Science is a distilled product. Like all distilled items, it is purified according to what we value most. So science is an artifact not only in the sense that it is a human product, but also in the sense that it is distilled clean. How clean should it be? What is the criterion of distillation? What makes a report of an observation or of an idea science? Its rationality. To answer this criticism, then, I have to discuss the question, what is rationality?

Consider a different distillation, say the success story of any non-scientific search, be it a detective story or a legal battle or a political campaign. No doubt, not all stories are of success. There are admirable reports of failures too, be these of detectives or of legal battles or of political campaigns. But as the history of science is a success story, let us ignore these for now. The simplest way to go about a success story is to describe the successful event and to go to its immediate antecedent and then further back until some starting point of the story is reached. The beginning can be the story of the acceptance of the challenge, or of the rise of the challenge, or of the education of the hero whose success is the subject-matter of the narrative at hand, or even the story of the hero's ancestry. The question is, how much of the failures on the way to success should enter the narrative? Suppose the challenge is to bring a murderer to trial. Suppose the list of possible suspects is given. The police have no choice but to put detectives on all of them. Only one detective will then succeed. Do we have to report the story of each? If yes, do we have to report all the details?

In detective stories, in truth or in fiction, the decision is given to the discretion of the author. In recent decades, philosophers of science repeatedly compared scientific research to crime detection. Can we leave the decision to the discretion of the historians of science then? Will the outcome of the one be judged by the same criteria as the other? I do not know.

This, however, is obvious: writers of histories of science, and more so of biographies of researchers in which discovery is prominent, do make such choices, and so their works doe not avoid all arbitrariness. And they owe it to their readers to say so clearly and say something about their choices.

Freud, we remember, refused to believe Einstein's expression of support. So it is scarcely surprising that commentators on my writings refuse to believe my expressions of admiration for Bacon and respect for some of the historians of science who follow him. Let me put this then in a language that they tend to use. Although Bacon's ideas are somewhat dated, they were valid and useful when they were dominant. This is their proof: the proof of the pudding is in the eating. What then is scientific proof? Baconian historians of science have a simple answer the problem, what historical detail should they should include in their histories of science? It is that they should discuss significant scientific ideas and their proofs. Fortunately for them, they have a shortcut here: they can take the up-do-date-science-textbook as science, namely, as proven, and add to each item a date and a name and then order them chronologically. This will append to each canonic science text a canonic history. Why is this so difficult to do? I do not know. I try to present the difficulties that one who wishes to perform this task may face, but surely one may ignore these.

4. Why do Historians of Science Disagree?

Historians of science who have done some original research should know this: it is impossible to find an old book, a science book or any other (save perhaps the Bible) that one can agree to all that it says. So historians of the Baconian persuasion try to sift from such books what they agree with. This way they overlook something important: they forget that the rationality of a book is often to be found in it as a whole, and this gets lost when a historian of science chops up the old book that way. The author of an old science book also displays a conviction about its contents: it is proved and so it will never be altered. Thus, in particular, most scientists in the nineteenth century were convinced that Newton's mechanics is proven, and so they repeatedly adjusted their researches to his. This was rational. After Einstein this is no longer so.

Enter Thomas S. Kuhn. He said, the scientific revolution proves that scientific truth is no truth at all. Scientists believe the dogma of the day because they are told to, and they are told to because it is useful that they do: the system of scientific research is the most useful possible. This is false. Kuhn could be indifferent to my calling his ideas false, as he said there is no absolute truth. So let me say, his idea is not useful either. Science is impossible without free exchange of opinions, and this means open controversy.

Suppose that Kuhn is right. Then, just as scientists are productive when they believe what their leaders tell them to believe, so historians of science are productive when they believe what Kuhn tells them to believe. Is this all right?

I do not know. Is Kuhn the leader of the historians of science? Perhaps Bernard Cohen is, and he says, do not believe Kuhn. How do we know who is the leader? We can say, Cohen was there first, and he was a leader. This is no argument, as Kuhn says, leadership is not for life. So how do we know if Kuhn is a leader? A leader is obeyed. We know what Kuhn orders historians of science to do: they should believe his idea and try to apply it to solve small problems in the field of the history of science. Is he obeyed? We do not know. The literature includes papers by leading historians of science who say that even Kuhn himself did not obey Kuhn. But we need not go into all this.

Do we need the concept of truth? Do we have examples of it? Yes, we do. Quine opened a lecture of his with the stunning logical observation: truth is ubiquitous, he said: of every couple of sentences one of which is the negation of the other, exactly one is true. So we do have as many examples of true sentences as you like. But we usually do not know which of the two is true. This upsets many thinkers. This cannot be helped.

Another example of a remark that annoys people is this: we do have demonstrably true sentences, such as "all chairs are chairs". This annoys people, since they seek true informative sentences. They want proof. This shows that they say, either there is scientific proof or there is no truth at all. This is false.

To cut things short, let me observe another annoying fact: we have examples of disproof: whatever the truth is, contradictions are false: if one admits a report of observation that contradicts one's theory then one has a disproof: one has thereby admitted that the theory is false. This does not satisfy many readers, as they want to know that the refuting sentence is true: they want proof. They find it difficult to allow that a theory is refuted, that disproof will do. The field of the history of science proves them in error: if the proof of the pudding is in the eating, then the history of science proves that disproof should do amply. Read the original texts of some grand old disputes: some of them display eminently rational disagreements.

The greatest innovation in the field of the history of science is the study of rational disputes within science. This leads us to the greatest and the loveliest of questions in this context: what dispute is rational and when and how does it cease to be rational and how can it be rendered rational again?

Historians of science can contribute to the growth of the philosophy of science by studying such cases in depth.

6. Kuhn's Way*

Anything printed is ipso facto out of date.

[Whittaker, 1913, 26]

* Thomas S. Kuhn, *The Road Since Structure: Philosophical Essays, 1970-1993, with an Autobiographical Interview*. Edited by James Conant and John Haugeland. Chicago: Chicago University Press, 2000. viii + 325. No bibliography; no index.

This review of the posthumous collection of essays by Thomas S. Kuhn is my personal obituary of him. I offer some background to his scholarly career and a coherent story in it, and come to a revised conclusion. I am not neutral, since I fancy myself a rival. (He was my senior by a few years.) We wrote on the quantum revolution [Agassi, 1967; Kuhn, 1978] and on the historiography of science. [Kuhn, 1962; Agassi, 1963] His second book was the first on that topic; my first book came second. We reviewed each other's book. [Kuhn, 1966; Agassi, 1966] Gerd Buchdahl reviewed both books and noted a trend. [Buchdahl, 1965, 69] The trend was mostly Kuhn. (Compare pages 28 and 168 of [Kragh, 1987].) His success was immense. His book "influenced … scientists, … economists, historians, sociologists and philosophers, touching off considerable debate. It has sold about one million copies in 16 languages and remains required reading in many basic courses in the history and philosophy of science." [Gelder, 1996]

He good-humouredly indulged my unruly histories and crude manners. Our casual meetings were few but pleasant. He invited me to speak to the departmental seminar in Princeton. He then received me at his home. We crossed swords in meetings. His book on the quantum revolution [Kuhn, 1976] had earned many reviews, and he answered all of them [Kuhn, 1984] but mine [Agassi, 1983]. We met last at the international history of science meeting at Berkeley, 1985. I talked there about willful distortions. As an example, I named works of Guerlac. [Agassi, 1987, 102] There and then, Kuhn broke off relations with me. Guerlac was a friend, he briefly explained. It was nothing personal. He just was frank. I valued this frankness. This was our last meeting. He ignored all my efforts to appease him.

Traditionally, historians of science considered open criticism hostile. They therefore concealed their criticism. (Bernard Cohen is the first to have noticed this custom. [Cohen, 1954, 164]) Guerlac told me that his review of Douglas McKie [Guerlac, 1954] contains criticism that has caused hostility. I find none there. Both pour scorn on the phlogiston theory as it is false, and praise Lavoisier's alternative to it as if it were true. Both masked the familiar refutations of Lavoisier, implying that only his terms need updating. [McKie, 1952; Guerlac, 1961; Agassi, 1963, 17, 30, 41, 43, 46; notes 3, 22, 34, 36, 63, 91, 119] Kuhn noted rightly that some distortions are unavoidable and thus excusable. [Kuhn, 1962, 139-43, 173] He ignored the systematic ones. My book offers many examples of this kind. In his review of it, he dismissed them *en bloc* as dated. [Kuhn, 1966] Here he reports on his discovery of them and on his having learned from this discovery to avoid inflicting up-to-date readings on old texts. (276-7, 291, cf. 276, 278) Obvious now, it took courage to notice this when distortion was the rule. His censure was of my criticizing colleagues by name and of my disregard for the reputation of the field.

His histories are above the ordinary cut, as he did not conceal controversy and error. Regrettably, he played them down. The central theme of the present summing up is this. Controversy is a vital and regular factor in the scientific tradition. Kuhn did not do it justice. He said, for most of the time leading scientists rightly shield from criticism the ruling scientific idea of the day. This limited his vision. "I am never a philosopher and a historian at the same time", he claimed. (316) He was in error. We are all victims of philosophical limitations; they are the chief source of distortion. The description of Galileo's significant errors — by Alexandre Koyré [Koyré, 1939, Introduction to Part 2]; [Koyré, 1965, 2] — is a major event in the historiography of science. (Still many ignore it, e.g., [Kragh, 1987].) Kuhn openly denied that we all need criticism. To criticize scientific leaders is unseemly, he taught.

Glossing over criticism creates confusion

I first met Kuhn in 1962, at Guerlac's international history of science congress in Cornell. My paper for the occasion concerned simultaneous discovery. Historians of science often blur differences between distinct ideas by identifying them with their up-to-date variants. [Agassi, 1963, 7-8, notes 29, 34, 40, 80; Fuller, 1989, 130] Genuine simultaneity is rare. It results from the use of similar tools for testing one theory. Kuhn's 1959 essay [Kuhn, 1977, 66-104] depict the simultaneity of a discovery as due to time being ripe for it. This is obscure and useless. I showed Kuhn my paper. He pleaded with Guerlac to ask me to scrap it. This puzzled me. I let go, perhaps because my impromptu substitute paper won praise. (It appeared in Guerlac's *Proceedings*.)

I once postponed commenting on a lecture of Kuhn from the public discussion period to a private chat. He thanked me — as a gentle hint, I suppose. Again, I was puzzled. After all, he was a skillful contestant. Later I found out that he regularly implied that he had the consensus on his side. He viewed dissent from him as merely verbal variance. "Inevitably, the term 'cross-purposes' better catches the nature of our discourse than 'disagreement'," was his response to Karl Popper's criticism. "There is not a great deal to choose between us." (126, 136, 141) Popper's choice of words seemed to him too harsh. (126) Popper called failed predictions "refutations". Kuhn preferred "anomalies". (He borrowed it from Hans Reichenbach. [Reichenbach, 1944]) This matters little. (142) By any name, refutations of successful theories are genuine discoveries. The value of a theory spills over to its refutation.

Kuhn's view of dissent as verbal variance had a high cost. The more he managed to defend it, the more he came to view all dissent as verbal. Had he rewritten his famous book, he confessed, he "would emphasize language change more and the normal/revolutionary distinction less." (57) This renders merely verbal the conformity that he required of researchers. This is good. It also renders merely verbal all revolutions. This is not good. Rudolf Carnap had an idea that he called "the principle of tolerance" (see below). By this idea, all disagreement is verbal. [Wedberg, 1975, 163] His tolerance then allows for the choice of variants of a received theory, not for dissent from it: he deemed it obligatory. W. V. Quine suggested that this idea of Carnap rests on two assumptions: that perfect translation is possible and that evidence decides the choice of theories uniquely. He criticized these assumptions. (46-7, 279, 306) Kuhn endorsed Quine's critique. Hence, he could not endorse Carnap's principle of tolerance. He came as close to it as he could. (104) He was a positivist *malgré lui*.

The stake was high. Kuhn deemed general assent essential for becoming the leader in a field. He wanted to be the leader in the field of philosophy. To that end, he voiced as much accord as he could. He voiced accord with Hempel. (208, 309). He voiced accord with Popper. (133, 135) He voiced accord with Margaret Masterman his nemesis. (137, 169n, 300) He voiced accord with me on the historiography of science, forgetting my view of the great value of scientific controversy. [Kuhn, 1966]

"Controversy about scientific matters sometimes looked much like a cat fight", he said. (108) He deemed it a communication barrier. (124) "When I received the kind letter in which Carnap told me of his pleasure in my manuscript, I interpreted it as mere politeness, not as an indication that he and I might usefully talk. That reaction I repeated to my loss on a later occasion." (227) The expression "to my loss" here does not signify a change of view. It refers to his ignorance at the time of a "deep parallels" between his views and those that Carnap had allegedly formed late in life. He did not divulge the content of this "deep parallel" beyond mentioning an obscure paper by a fan of both. There is no such deep parallel. Carnap's pleasure in Kuhn's manuscript is simple. He could appreciate rival views. Regrettably, he had also shared the common practice of flaunting spots of accord, as he had no room for controversy in his philosophy. (See below.)

Kuhn linked assent with approval. He enjoyed a "very considerable rapprochement" with Hempel. (247) He flaunted spots of accord with Hempel. They found that their views "were perhaps not quite so different as we both then thought." (225) Hempel learned to agree with Kuhn. Carnap had endorsed the dichotomy between descriptive concepts: they are "purely" observational or else they are "purely" theoretical. Hempel agreed. Kuhn disagreed. "A few years later" Hempel moved to Kuhn's view. Referring to the traditional dichotomy as if it was Carnap's distinction, Kuhn said that Hempel had replaced it with a distinction between old and new concepts. (226) This way he "implicitly adopted a developmental or historical stance."

(226) Implicitly. He then put things “in a sort of historical developmental perspective.” (309) Sort of. Kuhn tried hard, we see, to present a sort of agreement with Hempel. He was a friend. (209-10, 224-6)

The story involves a misreading and a distortion. The distinction between observational and theoretical concepts is innocuous. All distinctions are. Carnap’s error is in the view that some descriptive concepts are “purely” observational. Kuhn reported appreciatively that Carnap too had given up this view. (227) This report is fantasy. [Carnap, 1963, 964-6; Carnap, 1966].

Hempel backed a theory of Carnap’s known as the theory of reduction sentences. Kuhn backed Hempel on this. [Kuhn, 1977, 259] This is puzzling. Let me explain. In 1935, Popper criticized the claim that observational terms can be “pure”. They are all dispositional. The term “glass” implies “breakable”. Observation reports it partakes in are thus testable, and so not “pure”. In 1936, Carnap offered his theory of reduction sentences that reduces dispositional terms to “purely” observational ones. It says something like this. Glass is breakable if it breaks when the pressure on it is above a certain minimum. But is “break” “purely” observational term? I do not know what Carnap’s answer to this question is. He did not try to present “purely” observational terms. The literature is still open on this. (See [Hintikka, 1975] for a conspicuous example. See, however, [Murzi, 2001].) Kuhn had no business endorsing any theory of Carnap. He did so only because of Hempel. Carnap was an inductivist to the last. [Carnap, 1963, 998] So was Hempel. Kuhn was an anti-inductivist. He should have respected inductivism without giving it his consent. Linking accord with respect causes confusion.

Kuhn did not always conceal his dissent. His reluctance notwithstanding, he expressed dissent from Popper, from Carnap and from Reichenbach. (127) More importantly, he dissented from the two revered traditional views of science that positivism allows. (One is inductivism: inductive inferences are from observed *data* to unique theory-choice. The other is instrumentalism: theories are empty formulas used for housing observed *data*.) On this Kuhn was “an unrepentant Popperian.” (128) Assent to Popper imposes some dissent.

The scientific tradition encourages glossing over criticism

Plato expressed admirably the right view on criticism (*Laws*, 635a, translation by Trevor J. Saunders). “There is no disgrace in being told of some blemish – indeed, if one takes criticism in good part, without being ruffled by it, it commonly leads one to a remedy”. In another passage Plato expresses the same view (*Gorgias*, 506c, translation by W. D. Woodhead).”If you refute me, I shall not be vexed with you as you are with me, but you shall be enrolled as the greatest of my benefactors.” He put down rhetoric (positive criticism) as pleasing cookery but as trite. He extols dialectic (*Gorgias*, 472a, 473d-4a, 476a, 521e) as dissuasive (negative criticism) and as a friendly, beneficial purgatory medicine. It may be bitter, but it should not be.

This friendly view of criticism has emerged repeatedly and led to great accomplishments. There are two more popular views that repeatedly submerge it. One is the view that curbs criticism drastically, as it makes principles immune to it. *Contra negantem principia non est disputandum*. Aristotle endorsed it, it seems. (*Met*., 1006a7; *Anal. Post*., II, 3, 90b) [Popper, 1945, II, 287-88] So did Wittgenstein. So did Kuhn. He called principles "paradigms". The other view equates criticism with aggression. Kuhn endorsed it too.

This second view is the product of Sir Francis Bacon. Criticism conveys contempt, he said. [Bacon, 1620, Preface to *The Great Instauration*, his projected collected works] Hence, to accept criticism is to admit weakness. Hence, there is a great incentive to ignore criticism. Hypotheses are thus potential dogmas and obstacles to progress. Hence, one should avoid error and wait for proof before publishing a new idea. This view became popular. People who had new ideas soon tried to be content with mere allusions to them, to claim priority only after their ideas had won acclaim. Faraday opposed this practice. He suggested replacing it with frank admission of error. [Agassi, 1971, 123, 133, 147-9] Kuhn did not speak of scientific error. Following his mentor, James Bryant Conant, he declared it unrealistic to expect people to have no prejudice. [Conant, 1953, 35-7] Following Michael Polanyi [Polanyi, 1958, Ch. 6, §5], he declared it obligatory to endorse the dogmas of scientific leaders. He saw science as a profession that makes great demands on its affiliates, yet he did not include among these the demand that they should respect rivals. Kuhn declared that science recognizes no rivalry. As a historian, he opposed the concealment of controversy within science; as a philosopher, he advocated its suppression. This is neither possible nor necessary. Rather, we should all learn to argue in dignity. All we need for this is suitable procedures and sensible, skillful moderators.

Bacon's view that criticism is divisive is self-reinforcing. It urges critics to express disdain for their targets. Criticism and blame thus regularly mix. We should separate them. This is important, mainly in history. Our rational heritage comprises a stock of noble and wise ideas and of noble and wise criticism of them, mostly valid.

Robert Boyle valued criticism but not its public display. [Fulton, 1932, 101] Open, criticism makes its targets desert research, he said. Veiled criticism allows them to improve. [Boyle, 1661, Proëmial Essay] At the time, only a small band of amateurs conducted empirical research. As their leader, Boyle respected their feelings. He did not try to sustain the veiling of criticism. The Royal Society of London unwittingly entrenched his demand to veil criticism by making it customary. Newton tried to banish criticism. [Manuel, 1968, 344-8] It became normal to compare dispute to fire. It gives little light and much heat. If so, then efficiency should rise as the cold light of reason replaces the fire. Scant effort went into attempts in this direction. Diverse means can serve that end. Honor to objects of criticism from pens of leading thinkers may help. To some extent it does. Reconstruction of great past disputes may help too. To some extent, that does it too. [Agassi, 1963, 61]

Faraday presented his new theory in the usual way ─ avoiding a clash of opinions. He was ignored. He became increasingly explicit ─ in vain. He tried to institute a new, critical style of scientific discourse (in the British Association). He had limited success. Tradition demanded that old respected theories should be vaguely assimilated into new ones. The model is Newton's vague sketch of the level of accuracy of Kepler's laws as both full and partial. [Newton, 1687, III, 13; Cohen, 1974, 325] Oddly, William Whewell was the first to note this, and only apropos of some polemics. Kepler's laws are not accurate, he said: they contradict Newton's law. This was ignored, as he still tried to insist that the views of both Newton and Kepler are true. John Herschel and Pierre Duhem noted this too. Also to no avail.

Einstein is the first to put older theories as approximations to newer ones. He sought crucial tests between them. He respected all criticism. [Einstein, 1949, II, last words] Newton's theory, though superseded, is a great feat that still guides thinking. [Einstein, 1949, I, 30] Popper developed this idea. [Popper, 1963] Koyré said, "Cartesian science, for us, belongs entirely to the past, whereas Newtonian science, though superseded … is still alive. And very much so." [Koyré, 1965, 54] Bernard Cohen then endorsed Popper's ideas and discussed Newton's vagueness about the status of Galileo's and of Kepler's laws in detail. [Cohen, 1974]

New theories meet the empirical criticisms that had hit the old ones. Science progresses by series of approximations. This idea is plain and powerful. Public notice of it lagged behind by a couple of generations. Moritz Schlick, an eminent physicist-philosopher in Vienna, belittled it. He charged Popper with self-aggrandizement. He thus managed to secure extra time for positivism. This allowed new contenders to appear. Michael Polanyi offered a traditionalist view that was further from positivism than that of Popper. He defended science and religion on a par ─ as traditions. Kuhn offered an austere version of his views, offering no theory of tradition and nothing at all on religion. The positivists could come to terms with this.

Kuhn used commonsense to fill gaps in his philosophy

Kuhn was a means for stopping Popper. His oversight of tradition and of religion was helpful and backed by commonsense. Discussing tradition raises controversy. Admittedly, any rounded view on the rise of science takes notice of the great role that religion has played in the process. Even Otto Neurath, the leading positivist, admitted that the rise of modern science is much due to religious upheavals. He hated religion, and he followed Duhem, in whose view religion and science are utterly detached. Even so, he would not ignore history. Kuhn did. Paradigms are social beings. To discuss them with no sociology of science is odd, especially since so little is known about them and since the little that is know does not sit well with Kuhn's view of science as authoritarian. [Finkelstein, 1984, last pages]

Kuhn witnessed the vast growth of the authority of science. His image of it fits this. It is a rounded, convincing insider's view. Seemingly, he omitted only technical stuff. One cannot grasp it, he said, without years of hard training. Normal scientists are competent professional researchers. They solve reasonably challenging puzzles. They emulate the lead theory of the day — the paradigm. This way, they make puzzles manageable. A paradigm can become obsolete, though. Leaders then spend sleepless nights to rectify matters. They design a scientific revolution that is a paradigm-shift. Observations influence shifts only partly. They resemble religious conversions. (108-9, 174-5) [Cohen, 1987, 464, 468; Fuller, 1989, 67] Controversy may flair up in the process. As a new paradigm settles, consensus re-emerges. (108, 169n; 223, 288) "Paradigms had been traditionally models, particularly grammatical [?] models of the right way to do things." (298) They are "what consensus was about." (299)

Kuhn insisted that, nonetheless, science is empirical. He did not explain. Rather, he appealed to common sense. Not much of his philosophical output is devoted to exposition. Much of it is of ideas he shared with others. Most important of these is that there are no "pure" observations and so no "pure" observational terms. (107, 311) Most of his philosophical texts comprise examples from the history of physics. Next come corrections of misreading of old texts. Next come "damaging misrepresentations" of Kuhn's own texts. (156) He complained and showed surprise. (53-4, 106, 123-4, 133-5, 156-7, 160, 228, 307-8, 311, 315, 322 and more) He was surprised to hear, "Well, Tom, your biggest problem now is showing in what sense science can be empirical." (159n) He did not name his source, though he mentioned that she had written a favorable review of his book, thus targeting Mary B. Hesse. [Hesse, 1963] The story reappears thirty pages later, where he names her. (186) She repeated her message over a lunch we three had one day. What troubled her, I understand, was his view of the leaders as mediators between *data* and research.

Leaders impose paradigms, he said. They thus decide what projects the rank-and-file should pursue. He did not say what or whom science serves. He never mentioned grant donors. Presumably, he did not favor gratifying them. Traditionally, research serves the curious, the seekers after the truth. Kuhn dismissed them as "fossils". (120)

Paradigms help solve puzzles. They undergo small revisions. This somehow makes them increasingly clumsy. Small revisions give way to gigantic ones — to revolutions. Leaders decide how much clumsiness to allow before going for a revolution. Einstein did not allow any. [Einstein, 1949, I, 65] Kuhn reports that Einstein did (154) — on the assumption (Kuhn's) that at any time only one paradigm prevails. He (Kuhn) later withdrew this assumption, but he forgot to withdraw the corollaries to it. He finally allowed for many paradigms and for small revolutions. (143) As these changes are gigantic, what he was finally allowing for were small giants.

This is what Norwood Russell Hanson said. [Hanson, 1964, 180-1] Kuhn had good case histories, he said, but no idea for them to illustrate. After Kuhn had caught the public eye, he took back all that he had ever said, observed Hanson. He was quick to notice Kuhn's way, yet he exaggerated. Kuhn did have a theory. It is, leaders impose a shared belief on all professional scientists. True, he also took this back once, but we should overlook this as a mere slip. He said, science requires dogma, as some dogmatic conduct is beneficial. [Kuhn, 1963] This justification will not do. When dogmatic conduct is useful, then one can behave dogmatically without dogmatism. [Bendix, 1970, 68; Agassi, 1977, 338] At one point Kuhn said so too. (141) This must have been a mistake, as it amounts to relinquishing the demand for shared belief. And then, nothing of Kuhn's philosophy remains. Abner Shimony has ascribed to Kuhn the "sleight of hand" of a systematic "abortion of a viable line of reasoning at exactly the moment that it became embarrassing to the author!" [Shimony, 1993, 309] This discussion peters out unless someone presents a consistent canonic version of Kuhn's philosophy. A sketch of its genesis may help them.

Conant influenced Kuhn significantly

Traditionally, empirical science was a loose network of amateurs. In the scientific revolution, the network became voluntary groups. (Boyle called his group "an invisible college".) They became prestigious clubs. They called themselves "the republic of science", "the commonwealth of learning". Change followed the American and French Revolutions, the subsequent secularization of some universities, and the industrial revolution. Technical universities appeared in the mid-nineteenth century. Interest in science grew. Academies still ignored research. Until World War I, the chemical industry employed only a few researchers, and research institutes employed fewer. The military stepped in significantly only during World War II, and more so in the Cold War. " … for good or ill, the cold war is in large measure a war of the laboratories." [Danhof, 1968, 1] Almost all of today's vast science-based industry came during the Cold War. Kuhn's familiarity with the social history of science did not stop him from portraying research as a profession linked to political power. (149, 252) He even declared this "necessarily permanent". (252)

To identify profession with competence is to overlook incompetent professionals and competent amateurs, not to mention outstanding amateurs (Charles Darwin, Alexander von Humboldt, Émile Meyerson, Michael Ventriss). Kuhn collapsed quite a few distinctions. Here are some. Proficient vs. dilettante. Professional vs. amateur. Qualified vs. unqualified. Polymath (von Neumann) vs. specialist. Reliable vs. sham. Trade specialist vs. academic specialist. Specialism vs. sub-field. [Zuckerman, 1988, E 4b] Research activities vs. research projects. [Bunge, 2001, 170] Preference for an idea vs. dogmatic adherence to it. [Bendix, 1970, 68] His concern was with prospective leaders. They must work hard and imitate top physics professors. These oozed authority and boast top reputation (as well as security clearance). A lively passage in Kuhn's book on the quantum revolution [Kuhn, 1978, 215] pictures young, hardly known Einstein visiting a famous university, the professor showing him respect, and the students realizing that he counts.

All this reflects the new mentality of the Cold War. Harvard University president Conant made new conditions for academic jobs. He demanded professional authority and political conformity. [Hershberg, 1993, 391-554; Danhof, 1968, 281, 316, 320] Polanyi cautiously defended this authority. Authority "grows out of mutual control and criticism", he said. It "enforces scientific standards and regulates the distribution of professional opportunities". Above all, it is imperfect. [Polanyi, 1969, 44-6, 53-5, 94-5] "For scientific opinion may, of course, sometimes be mistaken." [Polanyi, 1962, 61] Kuhn's defense of authority is unqualified. Science is "in certain circumstances the most authoritarian", he said. (308) The proviso in this sentence allows some laxity. It is his license for controversy in inter-paradigm times.

Conant was Kuhn's mentor. He had standing in Washington, in the Pentagon, and in the academy. [Hershberg, 1993, Chapter 28; Lipset and Riesman, 1975, 302, 305 ff.] He wished to remain an academic but was burnt out. So, he opened a program for teaching popular science. The idea is worthy but weak. It can scarcely be improved without "overall direction and planning." [Conant, 1964, 51] Excuses for its weakness abound. Were they serious, they and the obstacles that they depict would be worthy of investigating. [Conant, 1964, Chapter 5] His program did not suit his temper and his other activities. The rigorous science teaching programs of his battery of reputed top physicists (266) left popular teaching for duds. He sought new ideas about education. [Conant, 1964, 4] He lacked a "nationwide policy adequate to meet the challenges of the new and awesome age in which we live." [Conant, 1964, last sentence] Instead, he developed an innovative program for teaching the history of science. It had notable success. [Hershberg, 1993, 409-11] When Kuhn joined it he was a rising star with a fresh doctorate in physics. History of science was barely a profession then. He had some difficulties settling down. Overall, however, according to his report, Conant had assured him of a career. (278)

Conant's view of criticism is conservative [Conant, 1963, 110]:

> "At the risk of incurring the everlasting hostility of the American Association of University Professors, I suggest that the time is more than ripe for lay boards to ask searching questions of the experts. These questions, needless to say, should be addressed to the faculties through the presidents and the deans."

Controls, said Conant, flow from boards through presidents, through deans. Kuhn agreed in part. Controls start at the top. The top is not the board, but the scientific leadership. I assume that Kuhn was referring to leading intellectuals, not to presidents and deans. I am uncertain. He said nothing about presidents and deans and their part in wielding and molding intellectual prestige and power. He said, leaders are always right. In the absence of democratic controls, this holds for administrators, not for researchers. [Danhof, 1965, 298] Kuhn ignored democracy.

The success that physics then had was most unusual. Kuhn's choice of it as a paradigm is unhappy. [Crane, 1972, 39; Reed, 1987, 226] The same goes for his backing of rigid instruction. "Scientific education should be particularly careful to avoid this dangerous rigidity." [Ziman, 1968, 70-71] Kuhn ignored Robert Merton on egalitarianism in science. (287-8) [Zuckerman, 1988] Derek J. de Solla Price spoke of "Diseases of Science". [Price, 1961, Chapter 8] Harriet Zuckerman discussed deviance in science. [Zuckerman, 1988, V, C and D] Popper said, we have no guarantees for success and we need training for criticism. [Popper, 1945, Ch. 10, n. 71] Kuhn was unmoved by all this.

The Cold War initiated a social revolution. [Weinberg, 1963; Kowarski, 1977; Agassi, 1988a] The academy began to offer to its members much in terms of worldly success. Academics increased their efforts to gain worldly success. [Zuckerman, 1988, V: C, D] Competition in the academy increased. [Burke, 1988, 114-32] A reversal is hopefully now in store. Old wounds are healing. Interest in nuclear weapons is waning. The need for democratic control over the public institutions of higher learning is gaining recognition. The republic of science needs reconstruction. Giving up Kuhn is a first healthy step.

Hempel failed to reconcile Kuhn with rationalism

Kuhn was a frank authoritarian. So, he invited the charge of irrationalism. The scientific leaders are rational, he replied, and so are their edicts. He offered no theory of rationality. He thus looked like a clandestine inductivist or a clandestine irrationalist.

In a symposium in honor of Hempel at the meeting of the Eastern Division of the American Philosophical Association, Boston 1983, Wesley Salmon and Kuhn paid homage to Hempel. (Chapter 9 here) Hempel was the commentator and Israel Scheffler was in the chair. In the discussion period, I criticized the hostility to metaphysics that positivists display. Hempel replied that even if their hostility to metaphysics is excessive, their hostility to religious dogmatism is beneficial. This is no answer. So I may have misheard him. I also heard him say, Kuhn was stuck in a dilemma between inductivism and irrationalism. Later I casually reported this and elicited a hostile denial from Adolf Grünbaum. Scheffler sided with me. I checked it with Hempel. He said I had misheard him. At least Kuhn took some responsibility for the fact that so many take Hempel to have described him (Kuhn) as an irrationalist. He said,

> some of the difficulties with my published accounts of theory choice would be avoided if desiderata like accuracy and scope, invoked when evaluating theories, were viewed not as means to an independently specified end, like puzzle solving, but as themselves goals at which scientific inquiry aims. (209-10)

This assertion is clear. It says, were Kuhn ready to admit that science aims at increased comprehensiveness, then the charge that he was an irrationalist would die down. If he did not admit it, the charge stands. If he did, then he did so not consistently and without a clear indication. [Sankey, 1997, 306-7; Toulmin, 2001, 215-16] Either way, this undermines his complaint of "damaging misrepresentations". (156)

Hempel attempted to help Kuhn out. [Hempel, 1979] To that end he had to discuss Kuhn's demand for conformity in science. Moreover, the conformity that Kuhn demanded is full. He said, this was necessary, in order to "maximize efficiency". (209) Bohr regularly wanted "crazy" ideas. Popper wanted respect for criticism boosted. Kuhn wanted full conformity.

Hempel said, Kuhn demanded conformity only where reason fails. [Hempel, 1983, 87-8] Now, irrationalists do not deny that reason is valuable. They only declare that it is limited, and that authority should supplement it. Did Kuhn agree? Hempel's excuses for him make him agree. This is no help. If anything, it aggravates matters.

Kuhn wanted to escape both positivism and irrationalism. To that end, he wished to replace individual rationality with group rationality. Classical rationalism is the view of science as a "one-person game". (243) This is an important error. Most philosophers of the rationalist school regrettably emulate Carnap, Hempel and Grünbaum. Rationality, by their prejudice, comprises individual acts of deliberation that rests on extant evidence and leads to wise choices of hypotheses to believe in. Yet, science is not faith.

Kuhn tried to do without a criterion of rationality. He said science is "a language game", "intrinsically a community activity". (215) He said, suffice it to consider rational whatever "the observed norm" is. (209) What "the observed norm" is we do not know. Many say, it is the quest for comprehensiveness. Kuhn set aside these "older, more comprehensive modes of practice." He wrote them off as "fossils". (120) The most broadly recognized quality of a scientific theory is empirical verifiability. [Piaget, 1965, 11, 226] As Hume has shown, this is an error. Hempel interpreted verifiability as confirmability. Kuhn disagreed. Einstein interpreted it as falsifiability. Scientific theory should be "verifiable (viz. falsifiable)", he said. [Einstein, 1949, II, 676] Popper amplified this. He advocated "steady criticism." Kuhn deemed this absurd. (136) With no consensus, all criticism is barren, he said, relying on the consensus. (141) Popper never discussed it, nor how it emerges. He stressed that whatever it is, criticism provokes efforts to improve. Polanyi had more to say on this.

Kuhn borrowed traditionalism from Polanyi

Kuhn ignored his debt to Polanyi. (296-7) Earlier he had admitted it. His term "paradigm" is synonymous with Polanyi's "tacit knowledge". [Kuhn, 1963, 392; cf. Kuhn, 1970, 44n, 191] It is not. For one thing, Newton's system is the paradigm of a paradigm. [Kuhn, 1963, 356] It is not tacit. [Cohen, 1956; Bunge, 2001, 170] More generally, Kuhn admitted Margaret

Masterman's observation that his term is ambiguous. "I seldom use this term these days, having totally lost control of it." (221) "Paradigm was a perfectly good word until I messed it up." (298)

What imposes unanimity? Inductivists say, shared information. Full sharing of information is impossible, however. Duhem said, without scientific realism, unanimity is natural. If theories are mere tools, then unanimity concerns only the information on the degree of their utility. He aimed at freedom of choice limited only by freely chosen tasks (and by logic). [Duhem, 1954, 206] He still agreed that realism is vital for science. So he viewed it as an ideal. [Duhem, 1954, 31-2, 217-18, 265-70, 285, 296; Agassi, 1957] Polanyi said, leaders are expert and largely trusted. Their arbitration produces unanimity, he added. Rules that govern skills of great artists are tacit. So are the rules that govern handing skills over to apprentices. [Polanyi, 1958, 183-5] The same holds for science, he said. This deserves admiration, but also criticism. Admitting the usefulness of tradition, Igor Stravinsky rightly advocated student autonomy too

> "No matter what the subject may be, there is only one course for the beginner: he must at first accept a discipline imposed from without, but only as the means for obtaining freedom for, and strengthening himself in, his own method of expression." [Stravinsky. 1936, 20]

Polanyi left small room for dissent in science. [Polanyi, 1969, 80, 93] Kuhn left none. As in art, he agreed, so in science, knowledge is tacit. [Kuhn, 1977, 340-51] Unlike art, however, science aims at unique optimal solutions. (209) To achieve this, we should maximize scientific discipline, he said. This is crucial for him, and it is dead wrong. Not before the final truth will be to hand will total authority be justifiable. Until then, all authority should be under check. To echo Polanyi, "I can accept the … [conception of] Kuhn only as a fragment of an intended revision of a theory of scientific knowledge." [Polanyi, 1963, 380]

Polanyi is famous for the idea that some knowledge is tacit. It is prominent already in works of Pascal (*esprit geometrique*), Hume (*je ne sais quoi*), Kant (*Takt*), Duhem (*esprit de finesse, bon sens*) and others. Polanyi combined it with ideas of Buber and of Husserl. [Polanyi, 1969, 149, 222] This led him important new messages. Valuable tacit knowledge introduces large doses of tradition into all valuable discourses. All tradition is imperfect yet it deserves trust. The scientific tradition is but a special case, then, however important. He encouraged criticism as long as it is not comprehensive. [Polanyi, 1958, Ch. 9: "The Critique of Doubt"]

Kuhn expressed blanket agreement with Polanyi. [Kuhn, 1963, 392] He agreed with him only on the authority of leaders, not on the freedom to criticize them. Polanyi criticized them for their radicalism. Kuhn had no right to join him, having granted them unchecked power. He dismissed their philosophies of science silently — as outside their narrow specialties. He never said so openly. This led to the "damaging misrepresentations".

We are all trapped within traditions. We are all frustrated by failures to articulate. These are familiar limitations. Efforts to transcend them are regularly afoot. Polanyi discouraged them. He judged them futile. He taught learning to bear them rather than trying to beat them. He was only halfway right. We cannot fully transcend them. We can do so to some extent, however. This is risky, said Polanyi. Risk is common to all innovation, however. So, applying new ideas is wise only after they stand up to tests. Applying critical philosophy is hardly risky. Just imagine: no more bullying, only free exchanges of ideas. Not too efficient, Kuhn might grumble. Perhaps. But it will be fun again. (130)

Kuhn borrowed incommensurability from Duhem

Kuhn ignored his debt to Duhem — though he respected his leading followers. (286-7) Responding to a query of mine on his neglect of Duhem, he said he had never read him. Commenting on this, Bernard Cohen said, it is impossible. Members of Conant's circle were familiar with Duhem. Here Kuhn hardly mentions the Conant circle, and he mentions Duhem as an inventor of a term. (235) The same goes for Whewell. (212) This is a common token tribute that inadvertently is an insult. [Agassi, 1963, 10] More so is Kuhn's expression of gratitude to Popper for the advice to read a book by a Duhem fan. (286) (When Popper was a visitor in Harvard, young Kuhn attended his seminar.)

Kuhn's image of positivists jars with the case of Duhem. He derided them all for their lack of historical perspective. This is true of Schlick and of Carnap. Duhem however was a great positivist and a great historian of science. Also, Kuhn did not discuss the cause of the neglect of the historical perspective. It is that verification renders knowledge a-historical. Bacon, the first of the modern positivists, explained this. Duhem disliked Bacon, but he was gracious enough to note his popularity. [Duhem, 1954, 86-93]

Kuhn said of incommensurability, "the notion still seems to me the central innovation introduced by" his famous book. (228) This is puzzling. The label denotes an important idea that Duhem explained in some detail. It is that we do not forget old theories even after they are dated. Scientific realism is the view that the aim of science is a comprehensive image of the world. [Duhem, 1954, 81, 103, 171, 173, 176] Duhem rejected it as naïve. [Duhem, 1954, 31-2] It restricts truth to at most one member of a set of alternative theories. Tradition overrules this restriction, as older theories continue to serve. If realism is overruled, theories cease to compete. They then become complementary. [Duhem, 1954, 101, 294] Kuhn endorsed every step of this reasoning. The error in it is the refuted hypothesis that usefulness goes with truth. Tradition takes it as self understood. It permeates the writings of Duhem as well as those of Kuhn. Its refutations are countless.

Logic demands that we separate alternatives. We comply if we view them as languages — since perfect translation is impossible. [Duhem, 1954, 133] (Duhem limited this to the physical sciences, to exclude the life sciences.

[Duhem, 1996, 78]) Choice between different theories is then between languages. No amount of information suffices to settle it with finality. [Duhem, 1954, 187-8] Crucial tests do not, as they carry no assurance. Possibly a faulty working hypothesis (say, about measuring instruments) is involved in the deduction of the tested predictions. It may then tip the balance erroneously. [Duhem, 1954, 185, 187-90, 220] (Duhem's wording is misleading. He said, there are no crucial tests, meaning, there are no decisive crucial tests. They are all fallible. [Hempel, 1966, 25-8; Adam, 1992])

In science conclusive decidability is not possible. Here is why. (1) Radical untranslatability: there is no perfect translation. (2) Radical under-determination: information never imposes a theory. (3) Empirical irrefutability: isolated hypotheses are irrefutable. These theses are named after Duhem and/or Quine. Duhem precluded and Quine included the possibility of false scientific theories. [Jaki, 1984, 370; Vuillemin, 1986, 595-8] (Quine learned late about Duhem. [Quine, 1988, 118] He then gladly learned that their views differ. [Quine, 1986, 619]) Each of these theses has two different readings. They are demonstrable but with limited application. Satisfactory translation is obviously possible. Ordinary translations of scientific texts are so reliable that in the present context Duhem and Quine have overlooked them. Nor can one preclude all perfect translation between perfectly formal systems. Likewise, information cannot determine the choice of a hypothesis only in the abstract. Within received frameworks this happens regularly. And hypotheses are irrefutable only in abstract isolation.

Kuhn elaborated. Paul Feyerabend, and Imre Lakatos agreed. Criticism cannot succeed, they all said, unless a better alternative to it is available. Hempel agreed. [Hempel, 1966, 40] Belief in a false theory is rationally obligatory, then, even past its refutation — until something better emerges. Sandra Harding considered this folly a breakthrough. [Harding, 1976, Preface]

By its own light, this criticism of criticism should come with a proposed criterion for choice between alternatives. Hempel appealed to experience. The others appealed to authority. This way they succumbed to irrationalism. [Russell, 1917, I., end of §1]

Duhem needed no such criterion. He valued criticism highly. He equated physics with applied mathematics, whose aim is expected utility. So, he allowed for the errors that engineers commit. Kuhn too equated pure and applied science. He had to: most normal scientists today are technologists. They have no ruling paradigm, and they usually apply refuted theories. Most of Kuhn's historical examples are from pure science, not from technology. Science and technology overlap, of course. They do so in basic research, as it is theoretical and for technical ends. [Danhof, 1968, 172; Agassi, 1980] In rare cases, basic research serves pure science too. The most famous instances of this are in nuclear technology, the nub of Kuhn's philosophy.

Researchers may ignore paradigms. Thus, Bohr's 1913 model of the atom is not relativistic. Sommerfeld's variant of it is. Kuhn noted that only

Bohr was revolutionary. Sommerfeld merely retained Bohr's quantum jumps. (141) Schrödinger did not. His equation, too, is not relativistic. The same holds for matrix mechanics. The concern of the dispute about them is not formal. (They are quasi-equivalent.) It is about interpretation. This may evolve into a research program. [Agassi, 1957] Kuhn has noted that Schrödinger's equation rests on some relativistic finds, ignoring its being non-relativistic. (153-4) He also ignored the inconsistency between his demand for conformity and the fierce controversy over quanta. He said that on this matter he was a "trouble maker". (140) Popper's "critical strategy seems to me the very best available", he also said. (137) Hence, conformity be damned and farewell to paradigms.

Popper encouraged troublemakers. Kuhn discourages them. This is where the differ. David Budworth said, reading Popper made him regret that he had moved from research to administration, and reading Kuhn made him glad that he had. [Budworth, 1981, 177]

The consensus is complex

If theories are viewed as languages, then alternatives may be held consistently. But then every theory change, however minute, must create a new language. This precludes scientific revolutions. [Duhem, 1954, 32, 36, 39, 177, 220 ff.] Kuhn was hesitant about this. (143) His concern was with the scientific consensus. How does it survive scientific revolutions?

Confusion on the consensus abounds. Inductivists see it as given: unanimity, proper belief, the belief widespread among scientists, or expert opinion. Kuhn said, the consensus made by is decree. Not so. The consensus is not unanimity, since dissenters recognize it. Opinion leaders have much to do with the way it sways. The public may test their abilities to lead. [Katz and Lazarsfeld, 1955, 281, 315-16, 322] They may influence public judgment on trivia and on important matters. [Rogers, 1962, 308-16] They adjudicate when controversy rages. When they suspend judgment, doubt lingers. Philosophers of science often wish to be right on ideas that are beyond their skills. [Laudan, 1983, 118-19] They then need opinion leaders most. Consensus may hold for parts of a controversy. An example is the force of an experimental argument, to use an idiom of Faraday. [Faraday, 1839, §§1799, 1788, and 2010; Agassi, 1971, 64, 132, 137, 147, 176, 295] Einstein discussed the value of a theory, not its credibility. He found it unimportant whether a theory gains credence or not. He liked intelligent disagreement. The superiority of one theory over its competitors, he suggested, is broadly recognized. [Einstein, 1949, II, 680] In this he was somewhat generous to his peers. If alternatives lead to a crucial test, then its evaluation will win consent. Credence for a theory is not so important.

Unanimity is scarce. Newton came closest to winning it. He tried to impose it and failed. [Manuel, 1968, 344-8] His criticism of Cartesian physics did not stop terrific efforts to revive it. (This Einstein and Bohr achieved.) During the Cold War, the Pentagon assigned to Edward Teller the project of developing thermonuclear weapons. He needed the cooperation of Robert J. Oppenheimer. He met with a blank refusal. Some Pentagon big shots then decided to bully researchers and to teach them a lesson. They demanded cooperation and resorted to violent, un-American means. Academics folded fast. (Rovere, 1959, 17, 24, 208) And then even the gods could not help America. Decline set instantly. Academic officials forced faculty to seek research funds. The Pentagon demanded security clearances and controlled much of the funds. For every grant, an added bonus (of fifty percent of the grant) went to the successful applicant's home institution. This made researchers academics, academies into research institutions, cultural institutions into academies, and famous intellectuals into faculty. Grantmanship became a tool for securing academic appointments. The initiation of peer review added power to windbags with no compunction to raise the pressure to conform. [Agassi, 1990a]

Polanyi's valiant struggle for scientific freedom [Polanyi, 1958, 145, n., Ch. 6, §5] is admirable. His struggle was consistently against planned science. Regrettably, he did not combat academic officials for their failure to resist the bullying of academics who exercised their freedom of opinion and of association by choosing the wrong side. He should have issued a warning against the dangers of all control over research. Future historians will write about the incredibly great and important influence that his fight for the freedom of science and of culture has exerted. Had he fought against the American academic bureaucracy too, he might have had success in that venture too. We do not know. We do know that the political bureaucracy of the United States managed to intimidate him by trumped-up charges (alleging that he had some association with communists, no less).

Kuhn's incommensurability is redundant at best

Newtonian mechanics is the most famous Kuhn-style paradigm. It had met opposition, mainly from Leibniz. Kuhn blamed Leibniz for insubordination to the ruling paradigm. (290) He did not blame Einstein for his siding with Leibniz. [Einstein, 1954] His is a different paradigm. Thus, much depends on how Kuhn demarcated between paradigms. He could not say. He viewed this as a serious setback (187n) and as no setback at all. (142-3)

Two ideas, of incommensurability and of the paradigm, express "the primacy of the community over its members". (104) Fortunately, "groups do not have minds." (103, 242) So, leaders must adjudicate. These two ideas are at their disposal. One of them reconciles competing theories. The other views one as dominant. One allows free choice between theories. The other imposes one theory as dominant. One drains theories of meaning. The other soaks them in it.

Supposedly leaders impose conformity to the paradigm. How then do they use incommensurability? They cannot. It is redundant. The view of theories as languages merely blocks conflicts between them. This can be achieved with greater ease otherwise. Suffice it to give different senses to a term shared by competitors. To take Kuhn's paradigm case (70-4), he assigned different senses to the term "mass" in the systems of Newton and of Einstein. This already reconciles them. Hence we can amputate the idea of incommensurability from Kuhn's system. More than that. Since the domain of applicability of the later theory is wider, relativistic mass (whatever exactly it means) is variable to a higher extent than classical mass (whatever exactly it means) is constant. It is more accurate. Increased accuracy is progress. Calling it increased verisimilitude or not matters little. [Newton-Smith, 1981, 176-7]

How do we compare two systems? Duhem said, we compare their domains of application. Kuhn promises us a few times that incommensurability does not preclude comparison. So now we may reintroduce comprehensiveness as the aim of physics. By Duhem, comprehensiveness is the condition of universal applicability, the quality of the ideal theory. (This condition is necessary but not quit sufficient. But let us not be finicky.) Duhem's view of systems as empty shells is thus redundant too. He has ascribed to theories relative truth — depending on their domains of applicability. We can then perfect his philosophy by making use of his admission that the relatively true is not absolutely true: it is false. His system and Popper's will then merge. Kuhn added imposition to all this. This is undesirable. The consensus can do without it. There is no objection to relative truth, then, as long as it does not oust the absolute truth. Kuhn, however, did oust it. To see why, we have to examine his theory of truth. It will transpire that he had none.

Bacon demanded of scientific research that it should be free of error. Whewell said, research is trial and error. Duhem said, domains of applicability are found by trial and error. Kuhn forgot to discuss error. Obedience to paradigms is error-free, he said. "Paradigms had been traditionally models … of the right way to do things." (298) They are guarantees for success. So his view explains success. (129, 132-3) Is it incommensurable with the view of science as inductive? Should contrasting them lead to crucial tests? Kuhn wanted incommensurability to be grammatical (211): "Paradigms had been traditionally models, particularly grammatical models of the right way to do things". (298) Can grammar explain history? Is Kuhn's grammar incommensurable with its standard alternative or should they undergo crucial tests? (44, 77, 200)

Kuhn's critique of approximationism is disappointing

Realism has variants. Of these only approximationism is viable. Science approximates the truth. This is the demand that a theory should outdo the explanatory success of its predecessors. Russell endorsed it. [Russell, 1940, 280, 303] It is a corollary to Popper's views. The explanatory success of the predecessor refutes its competitors that do not share it. A new competitor that does share it challenges its predecessor. It thus invites a retrial, a crucial test. [Popper, 1972, 200, 358]

Kuhn denied that the older theories approximate the later ones. (188-9) He adamantly rejected approximationism even while stressing that in some sense science progresses: it does as newer theories are superior to older ones. (74) They do so in many ways. One of these is increased precision. That is to say, they are better approximations to the truth. He denied this even while comparing Kepler's and Newton's theories. (150) As to the comparison between Newton and Einstein, which is the heart of the matter, he said this already early in the day. Newton's laws as a part of Einstein's system are not the same as the original "at least they are not unless those laws are reinterpreted in a way that would have been impossible until after Einstein's work." [Kuhn, 1970, 101] This is not contestable. That this is no argument for incommensurability Kuhn himself said, more or less at once. He then explained (after the word "But" below) why he opposed the alternative view (of theories as alternatives).

> Our argument has, of course, explained why Newton's Laws ever seemed to work. … An argument of the same type is used to justify teaching earth-centered astronomy to surveyors. But the argument has still not done what it purports to do. It has not, that is, shown that Newton's Laws to be a limiting case of Einstein's. For in the passage to the limit it is not only the forms of the laws that have changed. Simultaneously we have had to alter the fundamental structural elements of which the universe to which they apply is composed. [Kuhn, 1970, 102]

This is a terrific passage, and it shows clearly that approximationism does not do its job without the assumption that the competing theories apply to the same universe. [Scheibe, 1997, 338-9] This is what Duhem said all the time: realism is at the basis of the view of alternative theories as competing. So Kuhn rejected realism. The trouble is, this rejection makes him a relativist. He tried to wriggle out of this consequence. He failed.

Kuhn invented a new argument against approximationism. (106, 161, 188-9, 243, 280) A new theory may resemble less its immediate predecessor than an older one. (Consider theories of light going back and forth between waves and particles. If each approximates its successor, then they progress towards the truth, yet with no decision between waves and particles.) Now Kuhn was satisfied with any progress in any respect. Yet he demanded of approximationism to progress on all questions. (189) This is not exactly fair. As long as new theories do empirically better than their predecessors, verisimilitude increases. (Agassi, 1981) Each stage leaves open questions.

Though as an argument Kuhn's new point is unfair, as an observation it is true and significant. (Agassi, 1990a) A theory may serve many ends. Each of them can be used as a criterion for valuation. Progress proliferates. Kuhn and Popper are thus somewhat reconciled. Change is generally a mixed blessing, and this should hold for scientific change too. The old reluctance to give up Cartesian physics is an example. Nevertheless, Kuhn has erred. Approximation to the truth is central to the life of science. His objection to it is sheer stubbornness.

Kuhn equated the quest for comprehensiveness with absolutism. This is not bad, although total comprehensiveness is necessary but insufficient for realism. Relativism is not the acceptance of relative truth; it is the rejection of absolute truth. Like young Carnap and like Arthur Pap, Kuhn introduced synthetic *a priori* knowledge. Unlike them he rejected the absolute truth, and thus he rejected knowledge as justified true belief. Science explores the real world but there is no Kantian thing-in-itself, he said. (7, 71, 207, 245, 264) Seemingly, this is absurd. He dodged it with the old, defunct logical positivists' (pre-Tarski) exclusion of the question from the agenda ─ as senseless. "I am not suggesting … that there is a reality which science fails to get at. My point is rather that no sense can be made of the notion of reality as it has ordinarily functioned in the philosophy of science". (115) He was a positivist *malgré lui*.

"What replaces the mind-independent world about which scientists were once said to discover the truth is a variety of niches which the practitioners of … various specialties practice their trade. Those niches are … real …" (120)

Perhaps this is what makes his view consistent. I do not pretend to understand what a niche in this context is, nor what Kuhn meant in his assertion that it is real. He praised Hempel as "a man who intends philosophical distinctions to advance truth rather than to win debates." (208) What niche did Hempel occupy? Does advancing a truth increase the size of a niche or reduce it or replace it altogether with an incommensurable one? It is a mystery to me.

I also do not see what (Ernst Cassirrer [Cassirrer, 1910] and) Kuhn could offer as synthetic *a priori* knowledge flexible enough to suffer the wear and tear of scientific revolutions. (264) "I go round explaining that I am a Kantian with moveable categories", he said. (264) Things do not get better. Here is an especially puzzling passage that, it is clear from the context, is not a slip of Kuhn's pen, and not merely a passing aside.

> … I got some very important tools out of that, and one of them was to go back and think about the Copernican revolution … . … it turns out that some people, to the extent that surprises me and others, simply say, "in the Ptolemaic system the planets go round the earth and in the Copernican system they go round the sun." But that's an incoherent statement! (312)

The statement that Kuhn declared inconsistent is consistent. Because of the importance that he laid on it, let me elaborate. Compare the sum of angles of any triangle and the sum of two right angles. They are exactly equal in Euclid's geometry, and not in its alternatives. This was proved by Felix Klein. (One geometry can embed another.) The two statements, Klein's and Kuhn's, are strictly analogous. Hence he is in error.

Of the extant alternatives to absolutism and relativism, the more detailed their presentation, the more apparent their troubles become, unless they collapse into relativism or approximationism. Ilkka Niiniluoto has recently discussed this in detail. He found these the only possible options. [Niiniluoto, 1999] Those who disagree would be wise to try to rebut him. The editors of this book write as if Kuhn had developed his alternative to absolutism and relativism and as if he had criticized in detail diverse alternatives to it. (6-8) They exaggerate. Let me present his fragments on truth and on meaning to depict their sketchiness.

Kuhn had no theory of truth

One philosophical problem fascinated Kuhn: what is truth? (278, 312) He could be a physicist (273); he was a top historian of science (276); he could be a historian of philosophy (316); but he was a born philosopher. "I like doing history … [but] philosophy was always more important [to me]." (314) He sought a new epistemology.

"My goal is double. On the one hand I aim to justify claims that science is cognitive, that its product is knowledge of nature, and that the criteria it uses in evaluating beliefs are in a sense epistemic. But on the other, I aim to deny all meaning to claims that successive scientific beliefs become more and more probable or better and better approximations to the truth and simultaneously to suggest that the subject of truth claims cannot be a relation between beliefs and a putatively mind-independent or 'external' world." (243)

Cognition is of an object out there, in the "mind-independent or 'external' world." The view that science is cognitive clashes with the disclaimer of all relations between statements and a putatively mind-independent or "external' world. Hence, the second part of this account repudiates the first.

Kuhn suggested that semantics should be limited to "intra-theoretic applications." (162) One begins with declaring a theory true and proceeds to seek more truths. Its logical consequences are likewise true. Other statements are independent of the theory. Kuhn ignored them. He wanted competing theories to be separate-but-equal. To that end, he called them languages. This will do. (Hence, "theories are languages" is but a restricts metaphor.) The mathematical theory of embedding allows full embedding of some older theories in newer ones. This permits perfect translations. [Vuillemin, 1986, note 28 (regarding Euclid) and Note 34 (regarding Newton); Scheibe, 1997, 341] Though Kuhn's idea is so very sketchy, it already fails repeatedly.

Duhem suggested not ascribing truth-values to theories — in order to avoid making them probably false. (Popper suggested the opposite for the same reason.) This is intriguing. First, we void a theory of content and thus of truth-value. Consequently, it is mathematical, and thus vacuously true. We may then give it any meaning that renders it true. Henri Poincaré took up this idea. He viewed axiom systems as implicit definitions of their descriptive words. David Hilbert endorsed this and made it a part of the study of the foundations of mathematics. [Jaki, 1984, 315, 335] Duhem also sketched a new theory of partial truth, to reflect empirical testability. [Duhem, 1954, 184, 206, 208] A hypothesis is true for the domain for which it is successfully applicable. Tests are of the precise meanings of hypotheses, namely, of their precise domains of applicability. This way Duhem combined (mathematical) certitude with (scientific) doubt. [Duhem, 1954, 174, 181] It is a splendid achievement. Admittedly, the consent to have false scientific theories supersedes it. It still is active in the study of the foundations of mathematics. Kuhn has ascribed it to a critic of himself and dismissed it casually. (249) This is an amazing feat.

Frege identified meaning with possessing truth-value. Wittgenstein agreed and further identified it with decidability. The wish to allow for meaning with only partial decidability, in defiance of Wittgenstein, invited deviations from Frege. Carnap allowed partial verification and so partial meaning. [Carnap, 1963, 963-6; Carnap, 1966] Reichenbach suggested intermediate values between truth and falsehood. [Reichenbach, 1944] Both ideas are worthless as they ignore error and so the incompatibility between scientific theories. Kuhn, too, ignored error. But he addressed incompatibility. (161) He strongly dissented from Carnap and ignored Reichenbach. He dissented from Duhem, wanting incompatible theories to be informative and true. (73n) He dissented from Tarski and Popper. He offered no alternative. His endorsement of the demand for constructive criticism annuls his criticism of all theories of truth.

Kuhn had no theory of meaning

Kuhn claimed that he had linked incommensurability, meaning and translation. He did not. He understood Quine's view on translation as limited to nouns and descriptive phrases. "Quine's analysis of translation suffers badly ... from its inability to distinguish … ", he said. (48) His references display no need for distinctions or analysis. (37-40, 47-9, 61, 189) Quine had a mere sketch of a theory of meaning and translation. [Quine, 1988] He viewed dictionaries as sets of loose, circular definitions. This is hardly contestable, least of all by Kuhn. Dictionaries employ theories, Kuhn rightly added, implying that Quine would disagree. Trying to raise difficulties for Quine, he pointed at famous difficulties that compilers of dictionaries face. He derided "Quine's conception of a translation manual" with no ground. (47, 74, 165) Whether Quine is right or wrong, Kuhn's comments on language are disappointing

Ian Hacking ascribes to Kuhn a view that he (Hacking) names "revolutionary transcendental nominalism." [Hacking, 1993 and 1999] On Wittgenstein's authority, Hacking identifies it with the classifications implicit in common discourse. (72) It resembles Saul Kripke's essentialism-of-sorts. Kuhn responded by rightly expressing disagreed with Wittgenstein: common views are useless for science. (78, 229)

Aristotle viewed science as classification. He deemed one classification natural and right. This is essentialism (Platonism). The traditional alternative to it is nominalism. Both equate meaning and denotation. They differ about class names. Essentialists say, they denote classes. Nominalists say, they denote their members. This makes language overflow with homonyms and with synonyms. Frege refuted both views by refuting the view of meaning as denotation. It makes the identity of the evening star and the morning star purely verbal. Kuhn observed that the discovery of this identity rests on the discovery of planetary orbits. (220) Did he suggest that this is an argument for or against Frege? Russell's theory of definite descriptions is an alternative to Frege's, but it is incomplete. Kuhn rejected it (198), so as to exclude strict synonymy. So did Quine. Kuhn rejected Quine's theory too. (47-8) He offered no alternative. This matters little, as he accepted [Popper's] methodological nominalism. (232) For, this idea moves the search for a theory of meaning from philosophy to science.

Though all classifications are legitimate, they may smuggle theories, and these may be false. They may also be hard to detect, as they often appeal to intuition. Kripke has suggested that this makes us endorse them. He was in error. Their appeal to intuition is due to its approximation to some scientific theory. [Agassi, 1995, 255] Ernst Mayer told me proudly that he managed to convince Popper that the dispute among biologists about classification is significant. Later, David Hull expounded on this significance. [Hull, 1999, 496-9] The literature that he refers to ignores common intuition. It thus also ignores Kripke, Putnam, and Hacking ─ not to mention Wittgenstein.

The book

Foreword by Jehane R. Kuhn. She says touchingly that her late husband would have altered some of the text here, "not so much from discretion, which was not high among his virtues, but from courtesy." (viii) Here is a clear example. His put-downs of Quine and of Putnam differ in tone only. A book by Quine is "going off the rails"; "there isn't much of an argument" in it. (279-80) Not so Putnam: "nobody could reasonably show anything but respect for" him. His book is not exactly Kuhn, but it is "a big step". (312-13) Putnam is a friend.

Editors' Introduction opens with "Shifts happen", a pun on a sophomoric flyer. The flyer also includes, "why does this shift happen to me?"

Chapter 1 is on scientific revolutions. They are rare. Small ones are common. (143) This raises the serious problem of "discrimination of normal and revolutionary episodes". (146) On this Kuhn had no more than "a mere *aperçu*". (187n) "We must first ask", he added, for whom is an episode revolutionary. (146) This would render the concept relative. This is funny, as the rationale of this discussion was to maintain conformity to a consensus.

The best suggestion is by Mario Bunge [Bunge, 1968, 342]. Start with a problem. Try to solve it by a small change. Failure raises the stake and invites a more drastic change and a greater talent.

Chapter 2 concerns verbal changes. Revolutions are far-reaching verbal changes. Small ones comprise mini-revolutions, or they belong to normal science. He could not say. This is odd, as he forgot that scientific change partly depends on decisions (of leaders). (32) Moreover, mini-revolutions are not too revolutionary. New intellectual frameworks make for real revolutions. Kuhn considered these verbal. He made them paper tigers.

Chapter 3 is on possible-worlds semantics. Kuhn asserts that its "worlds" are not theories. They do embed some theories, however. By definition, a "possible world" is an alternative comprehensive description of a conceivable world. Each of them should include the theories that hold in the conceivable world that it describes. The literature on possible-worlds confuses two senses of "possible", possible given the laws of nature and possible in a broader sense (Popper, 1959, Appendix *10]). This is not trouble for modal logic. It is disastrous for possible-worlds semantics.

Chapter 4 describes Kuhn's progress since *Structure*. He increased emphasis on incommensurability and centered on its linguistic aspects. "The ways of being-in-the-world which a lexicon provides are not candidates for true/false." (last sentence) This is below Kuhn's standard. It sounds deep but is trite: whatever a "being-in-the-world" is [it is a human being], quite obviously, "ways … are not candidates for true/false." Only assertions/statements are.

Chapter 5. "The historical philosophy of science … has undermined the pillars on which the authority of scientific knowledge was formerly thought to rest." "Observations of facts are prior to and independent of the beliefs for which they are said to supply the evidence" and "what emerges from the practice of science are truths, probable truths or approximations to the truth". (118) True. "The authority of scientific knowledge" is thus gone. What "emerges" is freedom of thought. Kuhn's effort to limit it is pointless.

Chapter 6 comprises Kuhn's replies to critics. He dismissed (139) odd paradigms, such as mediaeval theology (Watkins) and safe cracking (Feyerabend). They are unproblematic, he said. They would be, were he interested in the demarcation between science and non-science.

Popper's comments on his claims, he said, are only seemingly critical. They display verbal diversity. Otherwise, they are counter-claims. The latter option invites crucial tests. Kuhn preferred the former. It rests on Carnap's principle of tolerance. (164) Kuhn was a positivist *malgré lui*.

Chapter 7 is a valid critique of a stray, once-famous, mock-formalization of Duhem's theory. Kuhn never cared for it. (318-19)

Chapter 8 connects with Max Black's famous paper on metaphors. Wittgenstein's view of ordinary theological terms as metaphorical challenged Black to develop a theory of metaphors. Their suitability depends on some loose (tacit) "networks of associations", he said. This is most interesting. It is problematic, however, as it makes allegory the best metaphor. And it does not serve his purpose, as it does not help Wittgenstein. Arguably, it may help with Our Father, and then also with Thou art in Heaven, but not with Hallowed be thy Name. Wittgenstein suggested that we replace religion with religious attitudes. This is unacceptable. Nor is any theory of metaphors relevant to it.

Chapter 9 concerns the choice of theories. Why does it matter? Bacon said, belief influences observation. Kuhn presents this as a modern invention. (107, 311) Bacon demanded that observers should shed all preconceived notions (= unproven theories). Kuhn disagreed. He nonetheless lauded control over the beliefs and conduct of normal scientists.

Alvin Weinberg did better. He spoke not of individual choice of theories but of “scientific or institutional choice between science and industry” and between “different branches of basic science” — as matters of allocation of resources. He also considered the option of postponing such decision indefinitely. [Weinberg, 1963, 159-60] The neglect of these insights is sad.

Chapter 10 concerns the humanities and social studies. Kuhn voiced broad agreement with arch-conservative Charles Taylor. He charmingly confessed ignorance. Interest in social affairs had cured him of positivism. (216-17) He had intrigued some leaders in social studies, as they wished to impose unanimity. They were ignorant of his view of their fields as too arid for growing paradigms. (57, 223) This way he assented to the criticism of Feyerabend, Watkins, and Hesse. Unanimity is insufficient. What more is needed, then?

> … the Greek heavens were different from ours. … the transition between them was relatively sudden … resulted from research done in the prior version of the heavens … . … the heavens remained the same while the search was under way. Without that stability, the search … could not have occurred. But stability of that sort cannot be expected when the unit under study is a social or a political system. No lasting base for normal, puzzle-solving science need be available to those who investigate them … . (223)

This is an moving speculation. Despite esteem for Koyré, Kuhn ignored the neo-Platonism of early modern science. He was a positivist *malgré lui*.

Chapter 11 is from a conference in Kuhn’s honor in MIT. It comprises responses to papers on him. It includes his acceptance of Hempel’s support and his refusal of Hacking’s.

A discussion with Thomas S. Kuhn is a long interview (69 pages) that is a pleasure to read. He spoke there in a marvelously uninhibited and cooperative manner. Apropos of his life story, he talked of many things — education, psychoanalysis, social and political affairs, including the bomb, metaphysics, religion, history, and art. The discourse is slight, conveying typical middle-of-the-road contemporary American intellectual attitudes, with very mild sympathies with the American left. Its scattered highlights on the academy of the day are of some use.

Publication list. Kuhn’s publications (325-35) helped link philosophy with history. Early in his career, the philosophy department at Berkeley insulted him by trying to move him to the department of history. (300) These were then separate fields. They merged too late for him (309, 311, 315-17) — partly due to his great success. This was a real and significant contribution.

His influence is not profound but marked. His publications amplify important ideas for which he rapidly won public endorsement: science has no justification; it involves repeated revolutions; scientists may have political ambitions; and their authority rests — or should rest — on competence. The social background of science matters too, since competence requires nurturing.

His publications contain valuable historical material, including reviews and surveys of difficult literatures. He argued with some of the sharpest intellects around. He was admirably candid as he admitted that he refused to play guru, as "it scared the shit out of me." (321) He could have rightly said, "It is beneath my dignity". And he should have. His fame allowed him to be a power broker like Conant. Laudably, he did not care for it.

He was not as innovative as Duhem, Popper or Polanyi. He did not write as innocently as Hempel or as gracefully as Koyré and Cohen. Yet, he wrote engagingly, worked with tremendous verve, and made a difference. He chose the right predecessors and brought some of their better ideas to large audiences. Trying to convince, he also appealed to the ability to exercise judgment.

He was far too decent to drive his ambition to success. His wanted recognition as serious, not as merely popular. I confess I did him systematic injustice by repeatedly considering his views a mere vulgarization of Polanyi's while ignoring his ambition. Though a leader in the field of the history of science, he wished to be a leader in philosophy. He failed in this. He was much more subtle than he appears, but also much less systematic. He tried hard not to fool himself. He did not need me to remind him of his shortcomings. I must have been a thorn in his side, I now realize. I regret this.

He crusaded for the idea that the authoritarian turn in physics heralds a new era. Had he been successful, much of the inadequacy of his writings would be exempt as blemishes for time to heal. Fortunately, the democratic view of science has not lost this round. Kuhn deserves the accolades that we, his chivalrous democratic challengers, can bring ourselves to award him as we bury him with full honors.

May he rest in peace.

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IV. Historical Essays

1. Who Needs Aristotle?

It has been said repeatedly that Aristotle is the most commonsense philosopher who has ever lived. This allegation, it seems, amounts to two claims; first, there is such a thing as the peak of commonsense, and second, Aristotle came closest to it. This peak commonsense, or ideal commonsense, or arch-commonsense, or core, or epitome, or distillation of commonsense — choose any metaphor you like — is what a philosopher will call the essence of commonsense. That it exists is the claim of Plato and Aristotle. In other words, the high praise of Aristotle comes from his own stable. Recently it has become a popular trend to find new ways to defend the idea of essentialism as strong commonsense.[1] This, I fear, is not only objectionable on the ground that commonsense is not the supreme court of reason; it is also objectionable on account of its circularity. For, as is frequently observed and reported, commonsense comes in a great variety. We may, of course, appeal to the best commonsense, to wit that sense that is common to myself and my peers, that is very appealing if I am Oxford or Harvard; but not if I am London School of Economics or Boston University. Even without belonging to the elite, I may still appeal to the best commonsense: I may try to appeal to the universal essence of all commonsense. (I shall talk of the universal later on.) It stands to reason, then, that the ability to appeal to the essence of commonsense — to any essence for that matter — postulates essentialism. But then, putting essentialism itself on the basis of commonsense is rather circular.

The beginning of philosophy, says Aristotle, is wonderment, problems. This makes my criticism of the new Aristotelian trend brief, unanswerable, and devastating. Members of this trendy school do not pose any problem; their essentialism solves no problem, nor is it meant to; rather, they offer revised versions of classical essentialism that, they argue, does not suffer from criticisms leveled against the classical versions of essentialism, and this surely is the proper mode of arguing. But the time-honored canons of dialectics require this to be the second step. The first step is to show that one's proposed theory is sufficiently strong to solve the problems that its predecessors have come to solve. If the new essentialism would a) solve the initial problems, and b) be impervious to criticisms leveled in the past against past solutions, then my task as a commentator would be c) to attempt to look for newer criticisms. As it turns out, there is no need to take the commentator's obvious third step as long as the first step in the discussion is missing. Let me, then, raise the problem that the classical versions of essentialism come to solve, and examine whether the new version solves this problem, in order to show that the new version solves no problem at all and gains its plausibility from being too weak to be open to criticism while its consequent inability to solve any problem hides behind its adherents' claim that it is immune to the criticism of the old version. To repeat, this claim is true due to the mere weakness of the new version.

I

Arch philosopher Thales wondered, we are told in Aristotle's *Metaphysics Alpha*, what is it that makes Tom or Dick or Harry the same from one day to the next in spite of his having changed from one day to the next? Let us call this problem The Problem of Identity. The identity of Tom as an individual may be contrasted with the identity of this table as a table. In this case, and it's historically important, the problem of identity splits into the problem of individuation and the problem of universals. There is no doubt that commonsense supports this problem, or this pair of problems, by the way, and it is reflected in all literature repeatedly: even Wittgenstein in his *Tractatus Logico-Philosophicus* referred to a German fairy tale in allusion to the problem of universals. In the United States the Problem of Identity was reflected in the folk tale of Rip-Van-Winkle who is the same, when old, as his younger self, yet his younger self is so much more like his son than like his older self. Commonsense postulates a theory of continuity — Rip's identity must reside in a connected space-time region and hence he can never become his son. The continuity theory is so strong that some proponents of the theory of the transmigration of the soul try to modify it so as to minimize the violation of continuity through transmigration: the death of the old Dalai Lama and the birth of the new one occur in the same space-time — or as nearly so as possible.

The theory that people possess individual souls itself already offers a solution to the problem of identity: young and old Rip have the same soul, and it differs from that of his son; the theory of the soul is a generalization of the theory of space-time continuity, then. But let us examine this a bit.

The soul theory and the continuity theory are both fairly commonsense. They both came to solve the same problem. And so at the very best one of them is redundant. At the very best means here, if they are compatible. Descartes' theory of the soul, for example, or Leibniz's, is not compatible with continuity. Kant, who was a Leibnizian, was forced to change his mind by the criticism that Euler had launched against it: there is no conflict between Leibniz' theory and the assumption that my soul resides in the body of some African rhinoceros. But there are versions — two, as far as I know — of the theory of the soul, which seem compatible with, and so may be generalizations of, the continuity theory. First, the claim that the soul is born at birth or at conception and dies with the body's natural death. Unlike Descartes, today's dualists accept this assumption as they grant a ghost to the machine. Second, the soul resides in the body and comes and into it and goes out of it on proper continuous world-lines, like in Hollywood movies. The corollary from this second theory is that Tom's body is never identical with Tom and that when we speak of Tom we mean his soul, never his body; otherwise we speak of his body or of his corpse.

But I fear the theories of continuity and of the soul concerning personal identity are in contradiction just because they are different solutions to the same problem. The point is that an explanation (*explicans*), far from entailing the explained (*explicandum*), actually contradicts it. I shall not discuss this point in detail, since it is presented in Popper's "The Aims of Science", Lakatos's *Proofs and Refutations*, and my "Sensationalism". In brief, the situation is this. Consider any crucial experiment between two theories, say, between Einstein's and Newton's theories of gravity; it is possible if and only if the two theories are in a conflict. This everybody admits, whether they affirm or deny the possibility of crucial experiments. Also, everybody admits, a conflict is the outcome of having two answers to the same question. Now the crucial experiment is possible only because the two theories present observable facts somewhat differently. In other words, scientific explanations (*explicans*) modify the observable facts they come to explain (*explicandum*). What this amounts to is that a new explanation does not entail the old observation report taken verbatim, and hence, strictly speaking, the celebrated deductive model of explanation is false, as claimed by Feyerabend and by myself, but as vehemently denied by Popper and by Lakatos. I think they abdicate logic here. The contradiction depicted in the present discussion of identity, incidentally, is not empirical as in the Einstein-Newton example; it is conceptual, as in the cases discussed so masterly by Lakatos. For, the continuity theory but not the theory of the soul will declare the identity of a thing retained through all gradual changes; including the gradual change of each and every property and aspect of a thing: the continuity theory makes spacio-temporal continuity a necessary but not sufficient condition; the theory of the soul demands that the soul retains spacio-temporal continuity and on top of this ascribes to the soul its identity quite unconditionally.

II

To return to good old Thales, he postulated the existence of the unchangeable Tom within the changeable Tom in order to solve the Problem of Identity — he postulated, that is, the existence of the unchangeable small man within, of the soul. How this works in psychology we do not know, nor even whether the psychological example was offered by Thales or by later writers, perhaps even Aristotle. Thales' view, all is water, is understood by Aristotle to be a metaphysics, a grand theory of the physical or material world, and thus he considered Thales to be the father of the *physiologoi*, of the Greek physicists. His disciples, still according to Aristotle, agreed about the existence of the unchangeable, but denied that it is water. This goes for Aristotle as well. In brief, Thales postulated a theory that says, the world comprises varied, complex, and changeable appearances and a single, simple, unchangeable reality. We may call this idea Thales' essentialism, or we may call it by its traditional name, the theory of the substratum or substance.

Thales' essentialism, alas, does not solve the Problem of Identity at all. For, it identifies matter but not a single material thing, be it a chemical substance or a building — except perhaps water. Hence the force of Parmenides' criticism and his conclusion that only reality exists, not appearances (which are but illusions); namely his conclusion that there is only one entity that has true identity, The One, The Unchangeable. Democritus postulated the identity of every atom to be that of an essence and concluded that atoms are unchangeable, eternal. He allowed, Aristotle tells us, to identify a thing, say a chemical or a building, as the variety of shapes and orders of atoms. This opened the way to Plato's version of metaphysics according to which a thing is matter and form. According to Democritus the atom, including its shape, is unalterable; according to Plato only the shape is unalterable. This is an advantage of simplification (since the shapes of all congruent atoms collapse into one shape) and the disadvantage of the added assumption of a Platonic realm of pure shapes (since the shape of an atom is no longer attached to that atom as it shares its shape with other atoms). Plato noticed that this is a solution to the Problem of Identity but an objectionable one: two things partaking in the same form are identical, whereas the problem of identity arises from our recognition of the difference between young rip and his son despite their sharing a form. Leibniz was disturbed by this and postulated his principle of identity of indiscernibles in order to get out of trouble: Rip's son cannot entirely resemble Rip; no two entities, he said, can partake in the same forms exactly. He had two proofs of the principle of identity of indiscernibles.[2] The one invoked the richness of God's imagination. The other employed the theory of continuity referred to above: if young Rip were identical with his son they should occupy the same space-time region.

If we reject Leibniz' principle of identity of indiscernibles as the *deus ex machina* that it is, we shall admit the criticism of all past efforts to solve the problem of identity: Norbert Wiener has declared it logically true that any characterization of an individual that may be deemed a complete specification should be one that offers us a recipe for the reconstitution of that individual from the elements. Hence, any theory of individuation that is at all satisfactory should deny the possibility of such a thing as an individual.[3] But the Problem of Identity arises from the puzzling empirical observation that, however changed Tom is, we still recognize him as the same individual! The idea that there are no individuals is strongly supported today by many disciples of Wiener who are cyberneticists and information theorists of all sorts, on the ground of their claim that in principle we can reproduce anything and anyone. And by now this idea is popular enough to appear on T.V. The beams in Star Trek are information beams (loaded with energy, of course); a few episodes of that series of science fiction rest on this idea. They describe an accidental transmission of a beam to two places creating doubles, on machines that build robots that duplicate individual humans, etc. This shows dangerous moral implications — ones that Norbert Wiener was the first to warn us against, and in each of his books he claimed that we can only reproduce artificially what we can specify, but there is the not given to specification. Apply this to the Problem of Identity and you may hear Wiener say, the Problem of Identity is inherently insoluble for irreproducible individuals like Tom or Dick, whereas a reproducible table or chair is hardly an individual. Where a Rembrandt painting stands is an open question as yet, all protestations of information theorists to the contrary notwithstanding.

III

We have thus arrived at the deadlock that R. G. Collingwood declared, in his *Speculum Mentis* of 1924, to be the one that constantly besets religion, science, and art, and that only art has thus far managed to evade to any extent whatsoever. To notice the severity of the deadlock we must notice the facets of the problem at hand. What makes Tom one and the same, yet different? The very problem is rooted in the observed fact that we have changed in the midst of constancy. The Parmenideans, ancient or modem, deny the problem by denying the existence of change. The Heracliteans, ancient and modem, deny the problem by denying the existence of constancy. Ludwig Wittgenstein's *Tractatus* is Heraclitean, and in his Investigations he admitted this to be false by recognizing the existence and validity of an intuition about the identity through change of, say, a game: when we change the rules of a game far enough, it becomes a new game and our intuitions tell us so. Hence, according to Wittgenstein, we need not worry about identity, as our intuition tells us fairly reliably what is constant-through-change and what is not. Hence, he suggested, we may ignore our problem.

I dare say Wittgenstein is fairly right about our intuitions. If we ever mistake Rip for his son then we are corrected without the aid of philosophers and without the use of explicit criteria. Of course, our intuitions can be stretched to their limits and get confused. Remember Star Trek. And, of course, Wittgenstein will protest against the stretching. Indeed, he blamed illegitimate concept-stretching to be the source of all philosophy. Some of us, however, wish to enlarge our intuitions and — perhaps quite intuitively — in agreement with the view of Émile Meyerson, R. G. Collingwood, and others: all past progress, artistic or intellectual, comes from worrying about the Problem of Identity, and by subsequent attempts to stretch our intuitions. We may agree with Collingwood that to do so we may try to articulate our intuitive criteria in order to be able to apply them more consciously, to criticize them, to improve them. We may ask not only how long is Tom still Tom rather than Dick. We may ask, is the sound produced according to John Cage's specifications still music? Still in the tradition of Bach and Beethoven? Here Wittgenstein is of no help and his reliance on intuition is poor.

IV

The above discussion has slid into another theory of essences, the theory of quiddity (whatness) so-called, of methodological essentialism,[4] as Popper calls it. Aristotle ascribes it to Socrates who "fixed thought for the first time on definitions" (*Met*. 987 b; he admitted some priority to Democritus and Pythagoras, *ibid*., 1078 b). This is the starting point of methodological essentialism, of "seeking the essence" and of "inductive arguments and universal definitions [of essences], both of which are concerned with the starting point of science". Aristotle, again, ascribes the idea to Socrates (*loc. cit*.), and again with some hesitation. We ask what is *x*, when *x* is a familiar object; namely, we perform the task of searching for those properties which make *x* an *x*, whose absence will deprive *x* of its *x*-ness or *x*-ity. Here we have used a technique: instead of varying in time, we vary by shifting from one member of a set to another. For example, what is Man? You cannot say Man is a biped, as some are one-legged; Man is rational animal, says Aristotle: rational, since without reason one is a donkey; and animal, since without flesh one is an angel. Ergo, Balaam's ass is human. Accepting Aristotle's definition is accepting as human Balaam's ass, as well as Aesop's cock and bull. This, I submit, has both its great attractions and its great discomfort. If these examples are too unscientific, consider Mr. Spock of Star Trek fame, or any other extra-terrestrial rational animal of your choice. Methodological essentialists must debate the question, are they human? I will not.

One can be an ontological essentialist without being a methodological essentialist: Galileo and Bacon, for example, but not Descartes. If we reject methodological essentialism, yet admit the intuitions of identity — of constancy in the midst of change — we may still hope to be able to explain (rather than explicate) these intuitions, test these explanations, etc. If we succeed in catching the constant in the midst of change, shall we call it the essence of the thing? Do things possess essences? Is not the very preoccupation with the Problem of Identity the admission of the existence of essence?

For my part, I will say, no. And thus I plainly reject what I have labeled Thales' essentialism; his claim that the world is comprised of reality and appearances. In contrast with this, we may claim that there are things more ephemeral and things less ephemeral; that there are levels or degrees of reality (Popper, "Three Views Concerning Human Knowledge"). Take the essence of man, and take Tom who essentially partakes in it. What is the essence of Tom? Not only that he is human, or else he will be essentially the same as Dick. And so we come again to the individual soul of Tom. Is it unique? Can it be copied? Here is a philosophical morass that we are stuck in for twenty-five centuries. These questions shake methodological essentialism by attacking all ontological essentialism, namely by attacking the very polarization of everything .to appearance and reality, the very dichotomy between the two. This dichotomy was first rejected, I think, by Franz Brentano, Edmund Husserl, and Bertrand Russell. (Russell said, we have to make do without the concept of substance or substratum: the laws of nature should do. Husserl investigated the essence of the appearances; this puzzled many people because, according to the dichotomy, appearances cannot have essences but things have both appearances and essences.) We can no longer ignore the fact that the dichotomy is false, and so all essentialism prior to the 20th century, resting on Thales' essentialism, on Thales' dichotomy, must go.

V

On this issue Aristotle's work is the worst and most confused philosophical discussion ever. There is the question, are Aristotle's essential definitions verbal or ontological: are human beings rational animals because otherwise we shall not call them human, but by some other name, or is it that when they lose their reason they lose their very humanity? I shall remind you that already ancient commentators have noticed this slippery quality of Aristotle's argumentations (see W. and M. Kneale's *The Development of Logic*). In *Metaphysics, Zeta*, 1030a, Aristotle feels uneasy and tries to limit his theory of essential definitions expressly in order to prevent too much arbitrariness. He confines essences to cases of a species of a genus only. So he solves the Problem of Identity at most only for humanity as a whole, not for Tom or for Rip-Van-Winkle. Further, he identified essence with cause, and he had a theory of the four causes and of the substance or substratum, in addition of the theory of essence. How do they all go together is itself unclear: I do not even know how clear is his theory of the substratum — that of things as being and becoming, as potential and actual.

Briefly, Aristotle's substance as potentiality and actuality is this. The essence of an acorn is its potentiality, its ability to become an oak, its very oakness. Essences can thus be hidden, or occult, or potential; or else it is manifest, or it actual. Essences are potentials, becoming actual and fading away, generating and corrupting. Essences can move from the backstage to the limelight and back to the backstage. In this theory, substance is the stage of blooming and wilting essences. In it there is alas! little room for accidents. Also, as the existentialist Raymond Polin says, too little room for freedom. Also, may I add, though this picture of the world is not static, it is stationary; It does not offer much hope for progress — much contrary to Aristotle's grand theory of the unmoved mover which (supposedly) tops his grand metaphysics.

Thus, to sum up this point, it is never clear how integrated, or even consistent, are the various parts of Aristotle's view. This lack of clarity of Aristotle's leads to some astounding results. I shall mention the worst example, from *De generatione et corruptione*, 328a, discussed in Sambursky's *Physics of the Stoics*, 12. The essence of wine is its ability to intoxicate us. It is lost in dilution. Hence diluted wine is wine no longer; hence the essence of wine is (*inter alia*) its not being too dilute; whereas dilution is, we all know, a mere accident. This is absurd. The essence, concludes Aristotle, comes in indivisible units, and so there is an essential limit to dilution and the absurdity is removed. After a whole volume of attacks on atomism, Aristotle endorses it — declaring essences indivisible — so as to overcome a rather marginal difficulty! Clearly, even in Antiquity this was noticed as a major fault. It forced the Stoic physicists to make dilution central to physics and thus took them back to the Ionian school — to Thales' disciple, Anaximander, to be precise. I do not know how many more revolts against Aristotle we need before we see how out of step he always was.

VI

How do the neo-Aristotelians face the difficulties outlined here? I do not know if they do at all, though the literature includes some attempts in this direction.[5] For, in order to give an adequate neo-Aristotelian view one should somehow combine Aristotle's theories of substance, essence, being and becoming, the four causes, and the natural kinds, as well as the covering model of explanation; or else give up some of the ingredients in this list and explain how we can make do without what we give up. Moreover, the difficulty about the essence of wine requires handling too: what is the essence of wine, and what is the essence of methyl alcohol; how this is related to what is deemed the laws of nature concerning chemicals, intoxication; and more. I am utterly unable to decide whether any of the neo-Aristotelians would combine ontological essentialism with a methodological essentialism or not — I find clues going either way — because on the one hand the attraction of essentialism is methodological, yet on the other hand essential definitions are despised by mathematicians, analytic philosophers, and others. The problem here is of necessity, not necessarily of essences, though the two are connected, perhaps. We have necessity, by the laws of logic, by the meaning of words and such, called logical necessity or necessity *de dicto*; and we have the more significant necessity, that of the laws of nature, called natural necessity or necessity *de re*. If the laws of nature are the laws of essences, presentable by the definitions of essences, then definitions, not covering law models, describe natural necessity or pertain to necessity *de re*.

Possibly Man is rational animal *de dicto*, i.e., by mere nominal definition, i.e., by our use of the word "Man". Assuming Man's rationality to be nominally necessary is not sufficient to enable us to decide whether you and I are (rational) human. Hence whether we are or not is undetermined as long as our rationality happens to be in doubt. Nominal necessity is quite different from the one based on the claim that all people, called by any name, but me and you included, are rational animals by some law of nature, by some natural necessity, which is necessity *de re*, or by essential definition. It is difficult to confuse necessity *de dicto* with necessity *de re*, yet the scholastics had to make great efforts to make this distinction since, to repeat, it was confused by Aristotle, as ancient commentators have observed. Clearing this confusion, as anyone trained in modem logic can do, only raises a problem. After the confusion is cleared, the claim that we are rational animals remains a hypothesis, not a necessarily true scientific proposition as Aristotle claimed essential definitions to be. That is to say, once Aristotelians admit all definition to be nominal, then they must view all putative essential definitions as mere hypotheses, and attempting to dodge this view they introduce the theory of natural kinds. Since natural kinds belong to Aristotle's philosophy of science as comprised of essential definitions, this amounts precisely to Aristotle's confusion: by his theory, definitions are at times (when under attack) verbal and at times (otherwise) natural. The basic certitudes of informative theoretical scientific knowledge are, according to Aristotle, those concerning natural kinds; they have nothing to do with strictly nominal definitions. (The confusion of epistemology with essentialism, of course, rests on methodological essentialism, unless it is just any old confusion.) [6]

VII

Insofar as neo-Aristotelian essentialism is trendy, it also benefits from association with other trendy ideas. And, no doubt, the success of model theory and the claim that this theory does not square with traditional nominalism is conducive to essentialism (as long as we ignore methodological nominalism, that is; see note 4 above). And in particular, the essentialist fashion got boosted by the popularity of Saul Kripke's fashionable theory of possible worlds as the foundation of model theory, plus the fact that it has got snarled with the problem of naming, plus the fact that attempts are made to overcome the problem by postulating some sort of identity-through-possible-worlds, so-called. For the question this new situation naturally raises is, how is identity-through-possible-worlds preserved? And, admittedly, essentialism offers a solution to this new problem: essentialism entails identity through possible worlds: all rational animals in all conceivable worlds, including Balaam's ass and Aesop's cock and bull, not to mention Mr. Spock, are all human. This, of course, rests on the assumption that there is meaning to expressions or concepts like 'identity through possible worlds'. Can this be adjudicated? There is no viable theory of meaning to apply to the expression, and intuition may go in any direction. Perhaps intuition does raise a difficulty about possible worlds, one that has to do with continuity: the continuity criterion of identity is intuitive and may easily contradict some versions of essentialism, e.g., the Cartesian, if not all versions of it (as I have argued).[7]

Of course, what one needs in order to apply essentialism to possible worlds is to use Aristotle's theory of meaning, and, indeed, originally essentialism encompassed a theory of meaning. The trouble with the idea of possible worlds, however, is that it is Platonic, not Aristotelian. Plato explained the common element between two different beds (which is the problem of universals), or between two time-slices of a given bed (which is our Problem of Identity) — he explained these as their sharing in the abstract idea of the bed or of bedness. Aristotle wanted bedness to be in the bed itself, not in a Platonic heaven. Yet he wanted it to be universal. He could not have it both ways, as his scholastic commentators noted. Moreover, beds are not species of any genera and so it is hard to see how their essences reside in them, or if they have essences at all. Yet he stuck to individual things since the very notion of possible worlds was too abstract for his taste though it is very palatable to a Platonist. This is why today, as in the Middle Ages, logicians who are not nominalists — Gödel, Quine — are Platonists, not Aristotelians. Aristotle simply falls between the two stools. (There are attempts to put a third stool in the middle, as some versions of conceptualism. All conceptualism, however, is seriously entangled either in a confusion of necessity *de dicto* and *de re* or in frank identification of both: idealism.) It is therefore understandable that increasingly many Aristotelians undertake to examine the theory of possible worlds, nor should one undermine the insurmountable difficulty they face when undertaking this task: the very attempt to solve the problem posed by Kripke's work with the aid of Aristotelian (rather than Platonic) essentialism is inconsistent and doomed to failure.

And yet, somehow, Aristotle's theory sounds most convincing. Certain changes he says, do not affect identity, e.g., one's getting old; while certain changes do, e.g., death and putrefaction. This assertion is the strength of all essentialist claims. And this assertion is true; but it is the *explicandum*, not the *explicans*; it is the initial problem, not a solution. Neo-Aristotelian essentialism is a confusion of the *explicans* with the *explicandum*. We all agree that no matter how many wrinkles I gather, I am still myself, until I die, whereas a plum, after gathering sufficiently many wrinkles, ceases to be a plum and becomes a prune. This is true, and admitting this to be true seems to the admission of essentialism. We want, however, to know why. Aristotelians say, there is an essence of me and there is an essence of a plum, the one has nothing to do with the wrinkles, the other a lot. This is a claim that can perhaps be made good by a fully fledged theory of the world, one that embeds a theory of essences, such that it agrees with our view that wrinkles make a prune but not a human. Aristotle's theory of essences would make good sense as a part of a theory of the universe that is a serious contender for the status of a true theory. This is too much to demand of Aristotle or of his modem followers, yet, the claim that wrinkles make prunes out of plums, is, to repeat, much too little. For, it is too little to say that while we change we may retain our identity, except when a change somehow makes us lose our identity. This small claim is no theory, essentialism or otherwise; it is no essentialism, Aristotelian or otherwise. It is the reiteration of a plain fact; not a solution to any problem, but the source of a problem to be attacked.

Aristotle's theory of essences sounds so very convincing, then, only out of context and in confusion of a problem with its solution. The classical seventeenth-century claim, then, as expressed by such diverse writers of that period, as Bacon, Molière, Spinoza, and Newton, is still the last word on this: Aristotelian essentialism is so facile and so hopelessly *ad hoc* as to be utterly unenlightening.

NOTES

1. It is possible to view the Aristotelian school as a tradition continuous with the deep past, and all Thomist philosophers and all Aristotle scholars as the conveyors of that tradition. Yet I speak here of philosophers who come from the modem anti-Aristotelian tradition of modem logic, of modem science, and of modem enlightened liberal democracy. Of course, some giants, particularly Sir David Ross, stand out as belonging to both traditions. Friedrich Solmsen's book of 1960 on Aristotle's physics seems to me a trail-blazing muddle: despite Cherniss's harsh critique of Aristotle and despite Jaeger's attempt to offer a balanced view of Aristotle, Solmsen's muddled *apologia* for Aristotle was well-received (after Lane Cooper's thoughtful attempt to separate fact from fiction was ignored as too apologetic for Aristotle).
2. There is also Leibniz's proof from causality: indiscernible causes will have indiscernible effects. This proof, however, is either a part of his metaphysical system of monadology, where every monad is in total isolation from the rest of the world, or it is the same proof as that from continuity. Certainly the proof from monadology is the stronger — indeed the only valid one — yet we must reject it along with Leibniz' system.
3. An element, say a chemical element, or an animal species, may still count as an individual without violating Wiener's thesis. But as long as there are individual samples of any element, this option must be excluded. Hence, the only option that Wiener left for individuation is the soul. Wiener himself tended to reject this option and so he took refuge in nescience. This seems to me to be a cop out.
4. Karl Popper, *The Open Society*, Chapter 11, section ii, is still the best restatement of methodological essentialism and objections to it. The same place also introduces methodological nominalism, in order to block the positivistic existential import (or rather export) which draws ontological conclusions from the nominalist method quite illegitimately. The commonsense of positivistic reductionism as resting on nominalism *cum* deductivism is counteracted by the seeming commonsense of the new Aristotelian essentialism: the two become intellectual poles as Levi-Strauss-style myths that think for us. Of course, both extremes stretch commonsense too far, and only mixing them returns us to commonsense. Hence, we better reject both extremes and have no need to mix them to avoid unpleasant extremism.
5. See, for example, Baruch Brody, "Towards an Aristotelian Theory of Scientific Explanation", *Philosophy of Science*, 39, 1972, 20-31, reprinted in E. D. Klemke *et al*., eds., *Introductory Readings in the Philosophy of Science*, 1980, 112-23, where the difficulty is noted.
6. Much confusion arose from the oversight of the role of questions regarding unfamiliar words or objects as requests for information, translation, help, etc. When the request is for the "definition" of an unfamiliar word or to a word that refers to some unfamiliar object, the request has nothing to do with definitions proper, much less with methodological essentialism. Hence Bacon's argument that questions concerning essences should properly come at the very end of a study, not, as Aristotle suggested, at its beginning. This amounts to the rejection of methodological essentialism or, as Bacon calls it, "logic". (Bacon's rejection of "logic" is the rejection of deductivism, or the method of anticipation as he called it; it is not a version of irrationalism.)
7. See my "Naming and Necessity: A Second Look", *Iyyun*, 44 , 1995, 243-72.

2. *The Desire for Reason and the Rise of Modern Science: The Role of Maimonides* *

0. Preface

Studies of the history of science customarily present it with no background ─ historical, philosophical, social or any other. Pre-modern science and modern science are often presented in the same manner. By default, this custom leads to viewing pre-modern science as if it were modern. This can be interesting but it is not sufficient as it prevents the study of the transition to modern science and the scientific revolution. The modern standards of science were fixed during the scientific revolution. What led to this development? Whatever our opinions of it are, it was due to much struggle, and Maimonides took a significant part in it

Medieval science was largely a study of fragments of Greek science, poorly understood and examined with the aim of reconciling them with religion. The little contribution in the Middle Ages to what we would today consider scientific was made futile by the confusion caused by the medieval method of reconciling all disagreements in order to reconcile the conflict between religion and science. The conflict persisted as it was (it still is) the clash between abstract universalism and some living particular (religious) traditions. Maimonides' contribution to this effort at reconciliation is important, as it was most sincere and most intelligent and unusually clear. His effort was the last of its kind. It gave way to newer attitudes that heralded the scientific revolution.

Failed intellectual ventures are traditionally disdained, and wrongly so. This blocks also the appreciation of successful ventures, as these are usually indebted to failures that forced people to seek new avenues. The paradigm case is the failure of James Clerk Maxwell to devise mechanical models of the electromagnetic ether that heralded Einstein's theory that did away with the ether. I view the case of Maimonides in the same fashion.

1. On medieval Science in General

What is specific to medieval science, to medieval thinking about nature, is its bowing to Greek authority. Advocates of science generally express contempt towards the whole of the medieval intellectual sphere. A striking example for this contempt is described in Bertrand Russell's autobiography: having contracted to write a history of philosophy, he tells us, he delved into medieval texts; he was impressed, and this surprised him.

The valuable knowledge available in the Middle Ages is dismissed by most modern historians as derivative. New information to the contrary has not yet altered their verdict that still prevails. It was first disputed in the late nineteenth century by Marcellin Berthelot and by Pierre Duhem, once noted scientists, and now remembered as historians of science. Berthelot, a chemist and an inductivist, described the empirical information accumulated in the Middle Ages, especially the chemical and mineralogical information that we owe largely to medieval alchemy. Duhem, a physicist and a founder of the modern instrumentalist philosophy of science, wrote impressive histories of mediaeval and Renaissance physics and described in detail the history of the background to the Copernican Revolution. Both overlooked medieval religion and art, as well as medieval superstition and intolerance. In the strict internalist mode of writing the history of science they skimmed whole libraries in order to sift what they deemed properly scientific. Each used his own criterion of scientific character as means for this sifting. Berthelot took inductivism for granted: science comprises bare facts and the theories that they support; Duhem advocated the instrumentalist view of science as imaginative hypotheses that serve as instruments for the classification and prediction of factual information. What they shared was strict internalist methodology. [Note: the concept of internalism used here has nothing to do with the concept of internalism used in current debate on the justification of knowledge.] Historians of science usually are internists, but they regularly deviate from strict internalism. (A notable exception is the inductivist *History of Physics* of Max von Laue, 1950.) Here and there they add some external information as means for linking science to its background. This is attenuated internalism, as the external items it allows are marginal supplements prevented from conflicting with science.

Berthelot and Duhem disagreed about the logic of science, but agreed that it separates scientific truth from its background. This very separation is questionable. The scientific truth that is the aim of research Berthelot saw as the absolute truth. Duhem denied this. He said, the absolute truth is not the immediate target of research but its remote ideal. Some oppose both Berthelot and Duhem, denying all links between science and the search for the truth, concluding that there is no scientific method, no logic of science. The paradigm case here is the view of Thomas S. Kuhn. Following A. N. Meldrum he tried to replace the search for the logic of research by the search for a psychology of research. Inadvertently he offered instead a sociology of science (in the wake of Michael Polanyi). Replacing the inner logic of science with its sociology overrules all internal history, as the social structure of science becomes its external framework and the sole means for organizing its history. It is anti-intellectual. Still, it has an asset: it naturally places science in the context of its culture. Kuhn stressed that he was an externalist, since he said that the organizing principle of science is the authority of the scientific leadership, and he conceded that the discourse concerning the leadership belongs to sociology, not to methodology. But he did not develop externalism. He only reiterated Polanyi's view that science is what the scientific leaders say it is. As this varies time after time, science ceases to be timeless. Kuhn unfortunately ignored all this. John Watkins tried to apply Kuhn's view to medieval thinking in a critical mood; Kuhn apologetically dismissed him.

The strict versions of internalism and of externalism pose a dilemma. Attention tends to focus on them, forcing discussions of internal / external factors into the strictly internalist / externalist Procrustean bed. One option is too constraining; the other is too loose; both are clearly defective. Strict externalism is anti-intellectual. It may be ignored with no loss, since it loses its attraction as soon as internalism is attenuated. Internalism is objectionable only when it is strict, as it then abolishes all medieval science that the scientific revolution rejected as pseudo-scientific. The hostility of the fathers of modern science is understandable and of historical value, but it is obsolete. Only two criteria established then are (rightly) still accepted within science. First, a theory systematically rescued from all empirical refutation loses its empirical character. Second, only repeatable experiments count as empirical. By either of these two criteria most of medieval science is not empirical. (The scant information within alchemy that is repeatable was recovered after it was properly sifted. See Robert Boyle, "On the Unsuccessful Experiment", in his *Certain Physiological Essays*, 1661, and the Preface to his *Sceptical Chymist*, 1661. What Duhem reproduced was not empirical.)

Berthelot and Duhem tacitly dismissed the view of science received by all medieval philosophers, from Alfarabi, the philosopher whom Maimonides admired most after Aristotle, to St. Thomas and beyond: it was the distinctly unscientific appreciation of the reconciliation of conflicting texts. Before this tradition was relinquished, confusion reigned. Medieval astronomers had to learned the hard way to become autonomous, says A. I. Sabra ("Configuring the Universe: Problem Solving and Kinematics Modeling as Themes of Arabic Astronomy", *Perspectives on Science*, 6, 1988). E. J. Dijksterhuis notes (*The Mechanization of the World Picture*, 1961, 49, 237) that as long as reconciliation was the norm, confusion reigned. To overcome confusion, he observed, even the distinction between mathematical demonstrations and empirical hypothetico-deductive ones, had to be made more sharply than in the works of Aristotle. These developments took place long after Berthelot and Duhem wrote, but they knew and overlooked the obvious intellectual regress and the comparatively little intellectual progress that took place between Antiquity and the Renaissance. Modern science clearly began with more than a revival of ancient ideas: it expressed a new attitude to them. Yet the chief change is still neglected: it concerns controversies.

Maimonides, was the most famous reconciler of texts, and also the first to block some reconciliation. In particular, he contrasted the biblical story of creation with Aristotle's view of the world as eternal (the antiquity of the universe). This heralded the shift from reconciliation to contrast. It enabled the moderns to revive ancient controversies. Ancient astronomers were in disagreement, Copernicus noted; and as they disagreed it is hard to rely on their authority. Bonamico, Galileo's teacher, discovered that Aristotle and Archimedes had disagreed, and he dismissed Archimedes. Galileo admired him but dissented from his judgment: he followed Archimedes. And he then found in his theory support for the Copernican hypothesis. Berthelot and Duhem had no room for controversy. So the controversy between themselves about the criterion of scientific character annoyed them. The controversy between them is outmoded: they both identified science with scientific progress, though this identification condemns all medieval science as stagnant.

The identification of science with scientific progress was a very important and a radically new idea, due to Sir Francis Bacon, the early seventeenth century author of *The Advancement of Learning*. It was instituted by his followers, the founders of the Royal Society of London. His view of scientific progress as the "mark" of science, though erroneous, was a major lever in the explosive growth of empirical research. Despite all evidence to the contrary, it is still taken for granted. Not so in the Middle Ages, when scientific status is claimed for some intellectual systems due to their antiquity. Maimonides was still much concerned with such claims. It is quite right to apply the criteria of scientific character to ideas advocated by people who held different criteria, but to avoid giving false impressions, presentations of their views should include assertions to the effect that they held criteria different from ours. Hence, any strictly internal presentation of medieval science is misleading, and given its stagnation, it is confusing too.

Even without a criterion for progress, there is little difficulty to judge it case by case. Noticing this (rightly) tips the scale in favor of attenuated internalism, as one needs the context of an idea to help judge it progressive or not. The context is the whole of current scientific knowledge. This is still internalist, but attenuated. As the broader context is deemed essential for the judgment, it becomes externalist, but not strictly so. Moreover, there is more to the progress of science than the mere progress due to some specific innovation. Strictly, scientific innovations are new factual discoveries and new scientific ideas. Yet contribution to the growth of public enlightenment may count as scientific too. These include public approval of science and public possession of scientific knowledge and public maintenance of it. This includes diverse activities and the diverse method that advance public enlightenment, as well as contributions to the growth of these activities and methods. Thus we may deem scientific the founding of journals and encyclopedias and the contributions to the widespread of literacy.

Other examples are closer to science. Consider the nineteenth-century discovery of hygiene: historians regularly link it to the public attitude to medicine. This makes it hard to draw a line between science and science education. The paradigm may be the Atwood machine. This is a very cleverly designed instrument whose function is solely to display Galileo's law of gravity in a manner that makes it intuitive to students. Of course, we better expose students to both Atwood's and Galileo's experiments. Teachers often hate such duplications, as they are in a hurry to convey as much material as time allows. Others claim that this is missing a most important aspect of science, its progress from messy to smooth presentations. Some even suggest that this transition is the heart of science. (See Edwin Hung, *The Nature of Science: Problems and Perspectives*, 1997.)

This is not much different from other matters of dissemination of scientific knowledge. The advancement of the scientific culture may count as scientific progress. An instance of such progress is a letter that one abbot sent to another the depth of the Dark Ages, requesting an explanation of the meaning of a passage in Euclid. Science can do altogether without the Atwood machine, though not without Galileo's law that it comes to illustrate. Likewise, Europe of the Dark Ages may be ignored, as next to it lay the Muslim world that was well versed in Euclid: scholars there understood his work much better.

The study of contributions to the rise of the scientific culture invites the search for external contributions to it: the rise of rationality in Greece that led to Greek science, the rise humanism of the early Renaissance that led to the scientific revolution. Seeking the roots of Renaissance humanism in the Middle Ages, Muslim and Christian, is a harder challenge, and it leads to Maimonides.

2. The medieval Yearning for Antiquity

Strict internalism imposes the view of all that contributes to science as science proper. This brings to light as science proper the medieval progress from ignorance to modern science, through the painful process of regaining of Greek science and through it the idea of the intellectual autonomy of researchers. In this process Maimonides has played a significant role, especially in his defense of autonomy as the chief contribution of philosophy to individual well-being.

Considering as science proper the conditions necessary for it clouds the distinction between externalism and internalism. The view that the distinction is spurious is the view that was not expressed in the Middle Ages because the terms were not invented. (The source of the term "internalism" is Sir Francis Bacon's assertion that mixing science with metaphysics is deadly for science.) Yet it is this view that medieval thinkers took for granted. The greatest difference between medieval and modern views concerns the very possibility of choice between internalism and externalism. Bertrand Russell said (*Religion and Science*, 1935, 12), "The medieval outlook of educated men had a logical unity which has now been lost." But he admitted that the advantage of this unity is outweighed by the disadvantage of dogmatism (13). Let us ignore dogmatism for now.

Internalism and externalism may but need not be in conflict. They do conflict on the shared assumption that scientific status is independent of context: they disagree as to the possibility of context-independent assessment. This renders externalism strict. Strict externalism is anti-rationalist and so it is better ignored: it is better attenuated so as to make it the view that events external to science may be relevant to the study of its history. This is needed for the introduction of the question, what were the non-scientific origins of (ancient or modern) science? It allows for overcoming the inability to comprehend medieval science without knowledge of its intellectual background. In view of the rise of modern science as a process of recovery (Renaissance), we need a differentiation between medieval and Renaissance science. Both comprise parts of education towards science, yet they differ enormously.

Education has its own logic, and it is easy to see that it differs from the logic of research, no matter what exactly each of them is. Some educators recommend the use of the (neat) Atwood machine to illustrate Galileo's law; others prefer the repetition of Galileo's (messy) experiments with the inclined plane. Should we try to resolve this controversy? This question shows the import of the difference in the internal logic between the acquisition of old knowledge and new. The same goes for the diverse kinds of the reacquisition of knowledge, individual and social alike. Galileo did not possess Archimedes' study of floating bodies; we do; should historians of science ignore his contribution in the reconstruction of Archimedes' ideas? Those who do not, do not approach Galileo's study of floating bodies strictly internally. They can hardly do that, as it is not clear what knowledge is to be considered the context of the internalist study of Galileo's researches. But they do ignore the transition from the medieval reconciliation between Aristotle and Archimedes, say, by St. Thomas, to the Renaissance contrast between them, or its rediscovery by Bonamico.

What is lost by today's strict internalist history of science? Can the significant part of the loss be reinstated within attenuated internalism? This depends on the rules internal to research. Both Berthelot and Duhem treated medieval science as if it were modern, as if it followed the modern rules of research. They thus overlooked the struggle to develop these rules, and likewise the controversies that were part and parcel of this struggle: they both refused science room for controversy.

Usually, new knowledge is contested; only old knowledge is not. The admission of controversy as internal history is a radical change: the tradition of the enlightenment deemed controversy merely the painful cause of personal injury. Controversy often caused pain, yet it was unavoidable as it repeatedly contributed to the growth of knowledge. Internalism thus needs new criteria for what is internal to science. This is problematic: if we are not careful, then a piece of research historically linked with concern about both science and religion may impose identifying them. It is not clear, for example, whether the link between astronomy and the Christian calendar signifies for the internal history of religion or astronomy or both.

Strict internalism ignores such links: it is the deliberate omission of most of history. Much unnecessary loss is due to the omission of the very pain and of the concern inherent in all problem-orientation. Studies of the cases of Bruno, of Galileo, and of Semmelweis, become external even in internalist texts. This becomes conspicuous in the contrast between external and scientific (i.e., internalist) biographies. The latter often overlook even their heroes' choice of career or research activities. The result can be confusing. Some reports about laboratory life on how external problems regularly invade research, were deemed refutations of internalism and thus of rationalism. This is in disregard of the fact that even discussions of rationalism spill over into the internal history of science.

Maimonides observed this (*Guide for the Perplexed*, III, 51, 627 in the Shlomo Pines translation): referring to the desire for the Lord (*Psalm* 91:14), the Psalmist employed a verb that has a strong sexual connotation. This is remarkable, considering that Maimonides belonged to the anti-sex league. (The sexual overtone in the *Psalm* in question, incidentally, is lost in the King James translation, which speaks of love, not of desire. The terms are quasi-technical: love is *agape* and desire is *eros*; cp. Pines, *Collected Works*, v, 471.) Hostility to sex is no excuse for the low view of women that Maimonides exhibited (he called them brainless in the opening of his *Letter to Yemen*). His low view of women is not essential to his philosophy. His hostility to sex is, as he viewed the life of reason as conditioned on the possibility of civil society, this possibility as conditioned on the ability to control strong urges, and this ability as conditioned on the prevalence of strict religion. Judaism seemed to him superior to its competitors just because it is strict; yet he praised the Psalmist for his use of a strong sexual metaphor. Clearly his yearning spells pain.

An example of the desire for reason was mentioned above, in reference to a letter written by an obscure, barely literate medieval abbot to a colleague, requesting an explanation of a passage in Euclid. This displays not only the regrettable intellectual poverty of the time, but also its profound, admirable desire for enlightenment. At the end of the *Guide for the Perplexed* (636) Maimonides says, "the perfection of which one should be proud and that one should desire is knowledge of Him, may He be exalted, which is true science." The Middle Ages were steeped with the feeling of inadequacy: in Antiquity the Lord spoke to the Hebrews and Mother Nature spoke to the Greeks; now the wells of wisdom are dried, and the way to revive them is through intense desire.

The basic difference between medieval and Renaissance thought is this. Medieval thinkers assumed they could do nothing to bring Antiquity back except by feeling and expressing strong yearning. The Renaissance evolved in the late medieval workshop, partly because workers feel less inept than intellectuals. Philippo Lapi Brunelleschi revived an ancient method of construction (of copulas) and invented perspective: these still symbolize the Renaissance hope in the human ability to act independently. And then Giovanni Pico della Mirandola spoke of dignity (*Oration on the Dignity of Man*) in a way that would have astounded medieval thinkers; he extolled technology (natural magic) there in a new way. It too was a symbol of hope. Renaissance hope came to replace mediaeval yearnings.

3. Medieval Culture

The pain, helplessness and hopelessness associated with the Middle Ages were quite general. Yet it focused on the loss of enlightenment, scientific and religious. Perhaps because long-range memory is mostly written, and scientific and religious enlightenment in their absence seemed one. This is forgotten nowadays because of the view (initiated by Sir Francis Bacon) that science is progressive. Today a society with no intellectual progress is not considered scientific; by contrast, a society may be religiously stagnant yet deeply religious. Indeed, religion is still preferred stagnant. This should not obscure the fact that stagnation (religious or not) is often painful. It need not be. In stagnant society that knows no change vaguely deems stagnation an asset. Possibly it is, since, as long as it lasts, society looks deceptively solid, stable and reliable. Knowledge of social change filled the medieval Christian doctors with a mixture of longings and fear, expressed as attitudes towards the Second Coming and the Day of Judgment. This attitude, says Karl Popper (*The Open Society and Its Enemies*), made Utopian dreams very potent. Following some hints of Alfarabi, Maimonides developed his utopian dream: he hoped that the Jewish political independence is around the corner. His *Codex* includes the laws of kings.

Scientific stagnation is easier to discuss than religious or cultural stagnation, since only in science progress is relatively easy to spot. Still, historians have no trouble narrating the history of medieval society as stagnant, as it was steeped in religious dogma, from which it emerged very slowly. Most historians of science find it hard to narrate the history of medieval science, as they deny its very existence: they refuse to link science with dogma and stagnation. Berthelot and Duhem found a way around this difficulty: they ignored dogmatism.

This was not easy. Under the prevalent influence of Georg Wilhelm Friedrich Hegel, the history of Christian medieval society is depicted as a part of the story of progress writ large: he presented World History as a continued story of the Progress of the Spirit. This was not meant to relate to the history of non-Western societies, as he largely ignored their history. So these histories are usually told with little or no reference to progress; they are described as passing through phases of stagnation. The passages themselves are ignored or described as disruptions due to some cataclysms, to some external events. Muslim medieval society is a borderline case: an increasing number of historians now describe it as a part of the progress of World History, though they still deem its contribution marginal. Some followers of Hegel and Karl Marx tried to draw a universal image of progress to include all societies. They still compare all cultures to the European one, and with little success. (See Benjamin Schwartz, “Some Stereotypes in the Periodization of Chinese History”, *Philosophical Forum*, 1, 1968, 219-30.)

Medieval Jewish history is the strangest: it usually depicts communities within the frame of reference of their host societies. The history of medieval Jewish scholarship, however, is drawn strictly internally with very few exceptions. Maimonides is a conspicuous exception, as he was influenced by Arab scholars and as he was a religious reformer. Not surprisingly, his books were controversial and even publicly consigned to the flames. His work is cataclysmic.

Some progress took place in the Christian Middle Ages: the High Middle Ages possessed more knowledge than earlier times. Yet little science and less scientific progress evolved then. Rationalist historians took it for granted that medieval culture was stagnant; Hegel and Marx offered theories of progress that changed the picture. To date under their influence some historians consider medieval culture superior to that of Antiquity. This includes medieval science. An increasingly large literature now extols the once despised medieval culture.

Views of medieval learning still differ. Rationalism sanctions the persistence of the negative attitude towards the Middle Ages; Romanticism sanctions the opposite attitude. Nineteenth-century Marxism is superior to these, and to most of its other competitors. For, it was progressive and yet it was appreciative of some medieval contribution to technology, mainly the small advances in agriculture that allowed for feudalism after the collapse of the agriculture system of the Roman Empire, of large estates where hoards of slaves were employed. Yet this Marxist idea is obsolete, since it is based on the Hegelian assessment of the Muslim world as one that had no part in the progress of culture, of the arts and of the sciences, much less of religion and theology, not to mention their contribution to the growth of science. Its merit as well as its refutation should be noted: feudalism was entrenched in the early, dark part of the Christian Middle Ages, when the Muslim world contributed to the rise of the culture that was transported to Christian Europe thus prompting the growth of its culture. (Much of the rise of Muslim culture was due to contributions of Christian or Jewish scholars; as they operated in the Muslim world, they are rightly deemed a part of it.)

Hegel's expressions "Spirit" and "the spirit of the age" are now entrenched. Spirit is his term for military power, religion, art and science combined. The strongest state around he declared the Spirit of the Age. Oddly, this flippant militarism encourages the search for interactions between science and the rest of culture. Rationalist historians of science follow Bacon's exhortation to avoid mixing science with metaphysics or theology. This exhortation is strictly internalist. It leaves the study of the interaction between science and the rest of the culture to other historians, to historians of ideas. They do not disturb the internalist histories of science; they supplement them. Thus, possible conflicts between science and other products of the human spirit are overlooked. This is amazing, since historians should and regularly do report conflicts and the problems they bring in their wake. Why not histories of science? If they were, they would be problem-oriented.

Problem-oriented history of science is internalist in a new, rich manner, since internal problems may rest on external factors.

This applies to Maimonides. His major philosophical treatise, his *Guide for the Perplexed*, appeals to a specific kind of readers, he said (3), pious Jews, well-educated and well-versed in logic and mathematics, but lacking the knowledge of physics out of fear that it will shake their faith. He tried (not to teach them physics but) to allay their fears. Many condemn him as one who has compromised: some of them say he compromised faith, others say he compromised reason: they all see his plan as demanding compromise. This is irrelevant: whether he could allay the fears without compromise is not to the point: suffice it that he honestly tired to do so. He took great pain to argue that what others saw as a compromise (say, his insistence that animal sacrifices are barbaric) is but his defense of the nobility of religion. And he judged this nobility as rationally as he knew how, since, he insisted, proper religion cannot clash with proper reason. Robert Boyle, the ideologist of the scientific revolution, accepted this from him — with proper acknowledgement.

Much has changed since then, in matters religious more than in matters scientific. The desire for perfection was at the root of that change: both religion and science were extolled as leading to perfection. Science is conceived today differently, and so it can afford to be indifferent to religion. This may be admitted even while recognizing that some of the wildest speculations of scientists touch upon matters religious. The great book of Alexandre Koyré about the last gasp of the close interaction between traditional theology and physics proper in the early eighteenth century (*From the Closed World to the Infinite Universe*, 1957) is externalist. Since then science ignored religion officially. Religion, however, can never ignore science: it cares about every significant human concern. And so, the desire to keep religion immune to the assaults of science must stay as long as religion stays. This is the outcome of the desire to be faithful to reason and to religion without compromising either. This desire was born in the Arabic world and transmitted to Europe, thus heralding its Christian high Middle Ages. And this way Maimonides was a significant part of the Arabic world, since his work pertains to faith as such.

4. The Conflict between Faith and Reason

The conflict between faith and reason is said to have been addressed first by Philo Judeus, as the one who attempted to resolve it. His attempt is famous in general, not in detail. As both the Bible and Plato are right, he observed, they must agree. This idea is still very popular, because the conflict persists. It is wider than matters of faith and reason: it is inherent to the general situation created by Greek philosophy, as it created the unyielding conflicting demand to honor both the universal and the particular, both the global and the local.

The diversity of cultures is obvious: awareness of it is universal. Almost universal is the view that one's own culture is the best (perhaps because one's religion is). Some commercial societies have developed toleration, perhaps beyond the mere acknowledgement of the right of others to exist. At times, this led to a cultural relativism, the idea that no culture is superior. At times relativism was developed into some naive version of absolutism: at base all cultures are one. (This idea was entertained by some twentieth-century thinkers such as biologist J. B. S. Haldane, anthropologist E. E. Evans-Pritchard, and sociologist Maurice Ginsberg.) Today's relativism denies the possibility of the absolute truth: all truth is local. This conflicts with the Greek idea that the truths of all cultures (truths by convention) are arbitrary and thus not binding, as only the absolute truth (truth by nature) is. This is the demand that specific cultures should yield to the universal culture that champions the idea of the unity of humanity, the idea of the siblinghood of all humans, the idea that only the absolutely rational is binding, and that it binds all. (Immanuel Kant said, what is binding for all is binding because it should have a universal consensus; he was never troubled by the absence of a consensus, least of all on the supremacy of science. He saw the universal as the rational and thus he directed his discourse to all rational humans, but also only to them, namely, only to himself and to his close disciples.) The ancient Greek ideal (truth by nature is universal, demonstrable, and binding all humans as such) was shared by leading ancient thinkers and by the Enlightenment movement. Anti-rationalists challenged it. To accommodate the challenge, one has to affirm that the absolute truth is attainable, even though it has not been attained as yet. The claim that the truth is attainable thus replaces the desire for it — provided we see some progress; otherwise we may find ourselves as helpless as our medieval predecessors and be driven again to helpless desire. This is attenuated rationalism (Karl Popper). Now whatever religion is, it is an integral part of local tradition (its truth is truth by convention). What does the adherence to attenuated rationalism (the desire for truth by nature) imply for local culture, for local religion?

Hegelian Émile Durkheim said, religion is but the self-acclaim of local tradition, of extant orderly society. This was meant to render irrelevant both the endorsement of religion and the disregard for it. This is diametrically opposed to the view of Maimonides: rational religious doctrine justifies the existence and maintenance of orderly society. As local doctrines differ, their differences are better ironed out. This is what he tried to achieve.

He took his cue from what he deemed the earliest effort at ironing out such differences: the monotheism of the Patriarch Abraham. It did not work: if he did advocate the siblinghood of humanity, this doctrine was seemingly rejected by his offspring, the Chosen People. Their being chosen makes the conflict as irreconcilable as that between monotheism and idolatry. To overcome this conflict Hebrew tradition always longed to see monotheism universally adopted. But the beginning of the discourse has to be the assertion that philosophy and religion are both essentially monotheist. The effort to harmonize the Bible and philosophy could make no sense without judging philosophy to oppose idolatry. The effort of Philo and of Maimonides had to fail all the same, since Judaism is not universally observed. They both argued, however, in the prophetic vein that Judaism is the best religion, so that it should soon be universal. Also, Maimonides described the rule of the messiah as the political regime that will eradicate idolatry (as all gentiles will respect the minimal commands that Judaism prescribes for them).

This solution makes a principle contingent on a political plan. Maimonides defended it as he presented the plan as the very best, and here he was glad to follow the Arab philosopher Alfarabi. He endorsed his reasoning and differed from him only in his preference of Judaism over Islam ─ not in his preference for the universal. This version of universalism – of both Alfarabi and Maimonides ─ is in the intent, not in its execution, and an execution may always be defective. Even science, the paradigm of the universal, is not always as universal as it claims, mostly because it is not always true to itself. (This renders scientific fraud important; see Nathaniel Laor, "Prometheus the Impostor", *Br. Med. J.* (*Clin. Res. Ed.*), 1985, 290 (6469), 681-4.) Philosophers hope that as the universality of scientific doctrines should render adherence to them universal. This did not happen. That it will happen is a pious hope, not much different from that of Alfarabi and Maimonides.

They both lived in a culture painfully aware of a paradox: the uniqueness of Greek culture clashes with its universalism. Alfarabi addressed this paradox as he spoke vaguely of a universal religion here and now, or at least soon. He had Islam in mind, of course. Maimonides' reforms came to enable Judaism to become truly universal: he deemed the Muslims virtual Jews as they were monotheists, and he argued for the rational superiority of Judaism. He fervently awaited the imminent arrival of the Messiah.

5. The Unity of Science and Theology

Maimonides' position is not clear to modern readers because of a forgotten, radical change in background ideas about the necessary harmony between science and religion. Kepler and Galileo effected a great, forgotten transformation in our culture (in the spirit of the modern age). They did so by altering the way to consider the duties of faith and reason towards each other. All those who want them to harmonize are now ready to demand that reason be free of the fetters of any particular faith. The predecessors of Maimonides of course made it, and he went as far as possible in that direction. Kepler and Galileo did something new: Maimonides engaged us in theological disputes about science; they managed to free us of these. They thereby exacted a forgotten great price for that, a price that Maimonides was not ready to pay. They said, since in principle the two truths, of faith and of reason, must harmonize, people concerned with research need not bother about the way science harmonizes with faith; they can leave this to theologians.

This is too cavalier. Admittedly, indifference reduces the dogmatism of traditional theology, but reasoning does this better. Descartes is an example of a person indifferent to traditional theology. Whatever was is attitude to religion, he cared too little about traditional theology, and so he amply won Pascal's complaint that he killed divine providence. This way the relevance of faith to the maintenance of civil society was forever lost.

Maimonides deemed the need to maintain society with the aid of religion a powerful proof that religion antecedes science; but he never took this as an argument for any specific religious view. On the contrary, the variety of religions proves this impossible, as well as the fact that in principle religion can be done away with: he said, truly civilized people will be civilized regardless of divine providence (*Guide for the Perplexed*, 526). In the Age of Reason Boyle exerted a great influence, and he referred to Maimonides as his authority in his view that though religion is above reason, they cannot clash (*Things Above Reason*). This helped the transition to a culture where the intellectual value of religion is played down. The same goes for Boyle's idea that rationality should do, that traditional religion is God's merciful granting the ignorant a second chance. His assertion that the truths of the Bible are moral, not informative, makes all this final.

The influence of Maimonides on Spinoza is better known than his influence on Boyle, and in the long run it was more significant. As Pines, the greatest Maimonides scholar, has puts it (*Collected Works*, v, 341), it is agreeable to ascribe to Maimonides a (Spinozist) pantheist, but it would go beyond the evidence. Spinoza himself was the first influential Bible critic; and so he accepted religion strictly for the uneducated masses. As the philosophy of the Enlightenment won popularity, even the concession to popular demand diminished its significance (as David Hume noted), and when Kant discussed the existence of God he was already dealing with an issue that had lost its import. Heinrich Heine noted this. "Bring the sacraments to the dying God", he said sardonically (*Religion and Philosophy in Germany*).

Maimonides refused to separate scientific and religious knowledge: he saw science and theology in intellectual unity. He tried to unearth this unity by the removal of poor habits of thought (Pines, *Collected Works*, v, 420-24). The key aspect of his effort is his attitude to the question of the antiquity of the world: was the universe created by a miracle, as the Bible says, or is it eternal as Aristotle says? This question was revived by Kant, but it was marginal for him. Maimonides stressed, and commentators agree, that the question was of a vital importance to him, since here he exhibited his commitment to the autonomy of reason.

His program was clearly to interpret the Law in accord with science. Yet, Pines has observed (*Collected Works*, v, 343), "That Maimonides rejected the doctrine of the eternity of the world partly because … it would have destroyed the foundations of religious law may appear to affirm the claim of religious belief to have a decisive voice in theoretical questions …

provided that the intellect is unable to reach a fully demonstrable conclusion … ." Pines cites enough evidence to show that this is not the case. Why then did Maimonides use this argument? And why only once? Because it was the only point on which he limited reason (Pines, *ibid*., 342): the very need for faith is rooted in the limitation of reason (*Guide for the Perplexed*, 67, 69-70), and the one limitation suffices: science cannot explain the very existence of the universe. It is remarkable, incidentally, that Russell agreed with him: faith is needed if and only if reason is inherently limited (*Religion and Science*, 1935, 175). So did Einstein (*Ideas and Opinions*, 1954, 11). It is more remarkable, incidentally, that Einstein also agreed with Maimonides that the limits of science lie here: the very existence of the universe is in principle not given to scientific explanation (*loc. cit*.).

On this point Pines noticed (*Collected Works*, v, 294, 342, 355, 390-1, 467, 419, 458, 473) an inconsistency in Maimonides. He took science to be binding and he took religion to be a *sine qua non* for civil society and he took miracles *a sine qua non* for religion and he could not square all these items, no matter how hard he tried. But his failure was honorable.

He stresses that Creation is miraculous, and that one miracle suffices to sanction all the miracles reported in Scriptures, including the fact that prophecy was given to this person and not to that, and that there is a Chosen People. He stressed that the Kingdom of God will be established soon, and then Judaism will be universally recognized. (See my "Reason Within the Limits of Religion Alone: The Case of Maimonides", in Yoav Ariel, Shlomo Biderman, and Ornan Rotem, editors, *Relativism and Beyond, Philosophy and Religion: A Comparative Yearbook*, Leiden, Brill, 1998.) Though one need not believe the allegation, made at the time, that he intended his book to fit all religions and all philosophies, the very existence of this allegation is quite intriguing.

The idea that both theology and science are limited is pivotal to the tension that is the heart of Maimonides' philosophy: the endeavor to achieve perfection is in principle frustrated yet it must continue. (He narrated a parable, *Guide for the Perplexed*, 618, that was expanded by Franz Kafka as his "The Great Wall of China".) It is also of a great historical significance in that its chief argument against the claim that science can achieve perfection is from the imperfection of the received Aristotelian system, and this is rooted in the clash between terrestrial and celestial physics. "This clash plays a considerable part in Maimonides' critique of some tenets of Aristotelian philosophy" says Pines (*Collected Works*, ii, 365). "Maimonides' exposition of the limitation of human reason — exemplified inter alia by the conflict between physics and astronomy — was his most substantial contribution to non-Jewish philosophy." This is how hard it was to move away from the presumed certitude of Aristotelian philosophy.

6. Conclusion

The science that Maimonides spoke of is largely Greek science, and even this not at its best. He began, Pines notes, as an orthodox Aristotelian. He became increasingly skeptical about the truth of Aristotelian astronomical doctrines (*Collected Works*, i, 180, ii, 355, v, 416 ff.). Yet he left knowledge of the divine and of the mundane on a par, ending with the assertion that only Moses the Law-giver had knowledge,[1] and in both domains (*Collected Works*, v, 468-70): so the science of Aristotle was not perfect! The traditional hostility to skepticism, as well as the reluctance of Christians to acknowledge a major influence of a Jew, make it hard to find clear evidence for it; it "seems to have been considerable;" says Pines (*Collected Works*, v, 348). "The matter has not yet been sufficiently investigated." Details of a failed ventures seldom are. Very often, the assessment of a work as a failure gains sufficient popularity to make it influential. Examples from modern times are the failure of James Clerk Maxwell to develop mechanical modes of the ether and Bertrand Russell's failure to reduce mathematics to logic. The failure of the efforts of Maimonides to reconcile faith and reason led first to the claim that researchers need not go into the details of the reconciliation, and later that there is no need for it anyway. Thus when Albert Einstein said ("Science and Religion"), "in truth a legitimate conflict between religion and science cannot exist", he meant by religion a personal (intellectual and emotional) affair, not a socially instituted body of doctrine. Today not only is particular doctrine taken less seriously; today attitudes to universalism have altered as well. Universalism is replaced with multi-culturalism and with pluralism, both of which necessitate skepticism, but without the claim that doubt paralyzes. And so both cultural and intellectual diversity are allowed without threats to rationalism (threats expressed as the advocacy of dogmatism and relativism). This makes the venture of Maimonides quite outdated, but, of course, much thanks to his efforts.

*Jeanette Bicknell of York University, Toronto, and Malachi Hacohen of Duke University read early versions of this chapter.

[1]. When Maimonides had to allow for something he wanted to proscribe he allowed it to perfect people only. Thus he allowed bowing down in prayer with one's face to the earth, as occasionally practiced by the Patriarchs, only to perfect people like them. Alternatively, he declared a sacred practice — animal sacrifice — a divine recognition of human failing that is no longer necessary and so he forbade it.

3. The Riddle of Bacon

1. The changing evaluation of Bacon

The vagaries of the judgment of history may be amusing and they may be indicative of the changing values within one given tradition. We may want to explain why in some epoch, say, Greek sculpture is deemed the peak of artistic perfection, yet in other epochs it is considered rather superficial and too pretty. Such assessments may be matters of popular taste; alternatively they may be the offspring of the evolution of some theory — say aesthetic theory. The change of taste may be inexplicable, or explicable by some suitable theory of popular tastes. The changing of aesthetic theory may be, at least *prima facie*, easier to handle if it is a matter of the inner logic of aesthetic theory — for example if theory alters in response to some new artistic discoveries, such as the import of works of art from the Far East, or in response to some criticism, say of ancient aesthetic theories. The case of the history of the evaluation of an author, especially a philosopher, somehow seems to depend on internal criteria and so easily amenable to critical analysis. The case of Sir Francis Bacon is different: it is surprisingly problematic.

Bacon was first held in the highest popular esteem that turned into great contempt. Later on his reputation wavered; it still does. Although in his time his writings were either unknown or considered unimpressive, his immediate successors, such as Marin Mersenne, René Descartes, and many others, held him in high esteem. Less than four decades after his death the founders of the Royal Society of London took him to be their spiritual father. His reputation was consequently so high that in the eighteenth century he was the only one whose name was allowed to be coupled in one breath with that of Sir Isaac Newton, tells us Paul Hazard, without it being considered blasphemy. David Hume qualified his praise: Bacon only "pointed out at a distance the road" whereas Galileo "both pointed it out to others and made a considerable advance on it." Immanuel Kant used a quotation from Bacon for the motto of his *magnum opus*, his *Critique of Pure Reason*. The independent Solomon Maimon declared him repeatedly a man of genius. In the year 1818 an essay (by M. Napier) on his reputation inexplicably appeared in the *Philosophical Transactions of the Royal Society of London*; in 1831 influential Sir John Herschel eulogized him as the greatest philosopher of science.

At the same time Sir David Brewster's life of Newton declared Newton not a follower of Bacon and scientific method different from the one Bacon described, since Bacon opposed the use of the imagination whereas Brewster took it for granted that it is unavoidable for science to employ the imagination systematically and critically. Macaulay's excellent, popular philosophical essay on Bacon of 1837 was a review of a collected works of Bacon, in which Bacon's contribution to philosophy is declared valid but hardly impressive, indeed no more than the restatement of the obvious. Not much later Justus von Liebig wrote a classic essay on Bacon declaring his understanding of science defective (he "never heard of the principle of the lever"!), his experimental works either borrowed or fake, and his view of the role of experiment in science utterly erroneous and at variance with Aristotle's true view of experiment as the basis of generalizations ─ of low-level generalizations, to use a newer and more precise technical term.

The real drama concerning the changing evaluation of the works of Sir Francis Bacon is the life-story of Robert Leslie Ellis, whose major life project concerned the collected works of Bacon. Collections of Bacon's major works appeared in the market regularly; the last one then, to repeat, was the cause or pretext for Macaulay's essay on Bacon that was a review essay on it. Yet there is but one definitive edition, that of Robert Leslie Ellis, James Spedding, and D. D. Heath. Ellis was a Cambridge don, a student of the classics, of mathematics and of honey-bees. He died young (of consumption), before he could complete the ambitious project he had undertaken, of editing a comprehensive and complete and scholarly and critical edition of Bacon's works. Before he died he bequeathed the project to James Spedding, whose claim for fame rested on one work, a two-volume defense of Bacon as against Macaulay's slim essay. The major part of Spedding's work concerns Bacon's political character since Macaulay portrayed Bacon as a conniving and treacherous politician, as one who recommended the torture of witnesses at the time when this practice had gone almost entirely out of fashion, took bribes with no inhibition, and shamelessly betrayed his best friend (Essex).

Obviously, it is advisable (when possible) to avoid discussion of both the truth value and the moral value of these allegations. Also, as Heath was called in to help edit Bacon's legal writings, we shall have no occasion to dwell on his contribution to the project. To Ellis then.

It is not clear why Ellis undertook the project of editing so very carefully and of commenting extensively and critically on Bacon's works; suffice it, however, to say that he was fascinated by the colorful personality of Bacon (the author; he was not interested in him as a politician) and by the enormous diversity of assessment of Bacon's contribution to knowledge, by the high assessments themselves, and by the animated debate on them that flared up at the time and that he had hoped to significantly contribute to, if not even to close the debate one way or another; indeed he hoped to find a good reason for Bacon's fame. In this he was greatly frustrated.

Ellis was a scholar of tremendous erudition. What impressed him first was the fact that he could find the source of almost every informative passage of Bacon in the writings of some previous author. This fact, i.e., Bacon's extensive plagiarism, was known at the time of the foundation of the Royal Society. Henry Stubbe, a staunch and vociferous enemy of that Society, said so openly. Let me mention an amusing episode: much embarrassment to Bacon's admirers was caused by his borrowing from Pliny, Stubbe tells us, of the simple observation concerning of the time of year when roses bloom, with no mention of the equally simple observation that this information does not hold for Bacon's England, only for Pliny's Italy. Clearly, Bacon knew amazingly little about gardening!

Ellis related Bacon's texts systematically to their origins. He found only one passage he deemed Bacon's own — his brief *Thoughts on the Nature of Things* that narrates the myth of Cupid. In that text Ellis found an interesting if mythical version of atomism. To complete the embarrassment, we may mention that much later C. W. Lemmi showed *(Classical Deities in Bacon*, 1933) that this text is borrowed too — from an occultist author, then of quite some renown, Natali Conti or Comes: Bacon's text is an expansion of Comes' chapter on Cupid.

That an amateur borrows a learned text is not unheard of. Copying was always commoner than original writing. I consider it permissible: it may be a form of day dream, or a cheap way of beefing up a short text in times when only books were publishable, a forgivable sin before copyright was established (1662), a temptation too hard to avoid and a harmless act of little consequence. As Ben Franklin observed (*Autobiography*), an interesting plagiarized text is superior to a dull original one. Even strikingly original and prolific writes like Robert Boyle and Jack London were caught engaged in it. (John Aubrey describes Boyle in his not-too-reliable *Brief Lives* as a known plagiarist.) Helen Keller did so without the slightest awareness, she confessed (*The Story of My Life*). (See also Robert K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science", *American Sociological Review,* 6, 1957, 635-659.) Yet, like everything Bacon did, he did this, too, with panache and with extra flair. For example, when he was forced to prosecute his friend Essex he pushed him to the gallows, and when he faced the accusation of having taken bribes he similarly condemned himself excessively, with streams of tears flowing from his eyes. (Yet he denied being influenced by the bribes; this is probably true: given his obsessive flair for excess, his verdicts were probably unpredictable.) Yet he also had a nasty streak: when he plagiarized from Pierre de la Ramée or Petrus Ramus, he also poured scorn over him, observed Ellis, thus adding insult to injury. The same, incidentally, he did with William Gilbert.

Bacon's scientific writings, plagiary or not, is as insignificant as the whole of the late Renaissance occult literature to which his they belong. Some recent scholars have spent some effort studying Bacon's views of the world, both in detail and in general — his Aristotelianism, his view of thick and thin essences (presumably echoing pseudo-Geber), his mock-atomism, his theory of heat as motion, his endorsement of alchemy and of magic, and his preoccupation with the prolongation of life — perhaps indefinitely. Some contributions to this literature are somewhat disingenuous: they hardly consider his plagiary, his declared anti-Aristotelianism, his inconsistent endorsement of Aristotelianism and of atomism, and so on. Such defects apart, the study of Bacon's views would contribute at most to our knowledge and understanding of a typical minor Renaissance author.

For Ellis — for a scholar concerned with Bacon's reputation, its vagaries, and his true worth — all this was of little interest. It was of little interest particularly as long as no one challenges Ellis's and Liebig's evidence that Bacon's metaphysics and science contain no novelty and nothing of value. And it is all doubly so due to Bacon's habit of clothing his ideas in aphorisms, myths and fables. (Fortunately, Bacon had no taste for astrology.)

Do Bacon's writings contain any new idea of any historical or intellectual value? I will soon answer in the affirmative, and expand on that answer. First, let me say, most of the Baconian commentaries lead one to the negative reply, to the assessment of Bacon's works on science and its method, not to mention his scientific and metaphysical works, as utterly dull. In particular, there is no value in Bacon's lead idea, his *idée fixe*. He recommended repeatedly and incessantly that we perform experiments, that we perform lots of them, and that we try as wide a variety of them as we can. He preached that everyone spend their free time experimenting, observing and recording. Indeed, his fame in the eighteenth century lay largely in this very claim. Yet it is hardly novel or deep. His most oft quoted fable was, perhaps still is, the fable of the ant, the spider, and the honey-bee. The ant is condemned there as she merely collects; she is the symbol of the empiric, of the student of the facts of nature who has no interest in theory or who has despaired of ever attaining true theoretical knowledge. (This is somewhat unfair to the ant, observes Bertrand Russell.) The spider is condemned there as he cobwebs out of his own head; he is the symbol of the reasoner, of the student of nature who hopes to acquire knowledge with no recourse to empirical experience. (This, we may add, is unfair to the spider; Bacon would agree: he said, no idea is ever free of some empirical origin or another.) When Alexander Pope sang the praise of Newton who, he said, cleaned the sky of the dusty cobwebs it had collected, he was alluding to this fable. The true student of nature, the interpreter of nature (this title, of the interpreter of nature, is the title fitting those and only those who follow Bacon's teaching), is compared to the bee, who both collects and processes what she collects, transforming the nectar of flowers, the facts, into honey, the theoretical part of science.

This fable is a testimony to the poverty of Bacon's thought and to the scholastic bent of his mind: rather than have a theory of the growth of knowledge he simply classified and symbolized. The popularity of Bacon's fable is a testimony of the poverty of the thoughts his disciples themselves could and did attribute to him. They called him the father of experimental philosophy. What is that philosophy? The label, incidentally, of experimental philosophy, was invented after his death — in the middle of the seventeenth century — and it was adopted by at least two groups of English scholars who later founded the Royal Society of London, and who labeled as an experimental philosopher anyone who (a) was modern in outlook, i.e., a follower of Copernicus, Galileo, and Descartes and who (b) admired Bacon and spent time either performing experiments or hearing others about their experiments. Is that experimental philosophy? Did Bacon endorse it?

Bacon could not endorse the ideas of Descartes, as he died in 1626. He knew something akin to that, as he was aware of Galileo's works. Ellis has shown that Bacon never mastered a proper comprehension of Galileo. And he was anti-Copernican and even declared Copernicus something of a charlatan and a threat to science — as he had aspired to be the heir of Aristotle and to replace Aristotelian dogmas with his own. (This sort of accusation, incidentally, became popular: Huygens accused Descartes in exactly the same manner, and the accusation became a standard weapon in the scientific arsenal.)

What then is experimental philosophy? Since not only Bacon is honored as its father (is the father of an idea metaphorically the author of that idea or the person who inspires others to create it?) but also Galileo and even Aristotle, it is hard to know what is at stake. The mere recommendation to perform experiments, even living the life of a researcher devoted to the performance of experiments, was hardly new; even Bacon and his followers had to acknowledge this: he found his predecessors in this respect superstitious dogmatists, and he condemned them, but he never denied that they existed.

In order to get one's teeth into any substance that might be found in Bacon's works, at the very least one has to ask, what role does experiment play in the growth of science? There is no way around this. Perhaps there is no answer to this question. Perhaps there is no single quality characterizing all scientific empirical information, astronomical, physical and biological, ancient, medieval and modern. The idea that all experiences play one and the same role within science was labeled by Ellis's older contemporary in Cambridge, Dr. William Whewell, as the view that one system of logic characterizes all discovery. Some writers deny that it exists. Some writers (Meldrum, Kuhn) declare that there is something like it, something that is not the philosophy of science, whether epistemology (the theory of knowledge) or methodology (the theory of the way to acquire knowledge), but the psychology of discovery. They never specified.

There are a few famous philosophers who attempted to describe the logic of discovery. The first among them is Plato, who said that after series of dialectical exercises the philosopher's mind is purged and thus ready to travel to the world of ideas, where in ecstasy it finds true knowledge. Aristotle is known as Plato's chief critic, and the chief disagreement he had with Plato concerned the place of ideas. Plato declared them outside our ordinary space and time. Aristotle denied that. He thought ideas were in the things which they are ideas of, and he called an idea *ousia*, which is translated as essences. He agreed with Plato that the knowledge of these essences is achieved by ecstasy which follows the purging of the mind through critical debates or dialogues or dialectics. It is not clear what ecstasy is, how to get into ecstatic trance, and how to secure that whatever enters the mind during the trance is science. Yet the view of discovery as a trance was and still is quite popular, and is these days ascribed to Henri Bergson. Also, the doubt about it just mentioned is repeated in Bertrand Russell's works against Bergson, in his superb "Mysticism and Logic" as well as in his study of the philosophy of Bergson. In the Middle Ages the theory was very popular and it was presented in the widely used formula describing knowledge as the parity or unity of the knower and the known (in knowledge). (This formula is repeated in central passage in Bacon's *Novum Organum*: Book II, Aph. 19.)

Perhaps because of the difficulty to even articulate the Platonic-Aristotelian theory, the other theory of Aristotle is better known, his theory of probable knowledge, even though he presented probable knowledge as the mere stop-gap in lieu of certain knowledge. Probable knowledge, he taught, is generalizations based on empirical observations. What Bacon said is problematic and will be discussed soon and throughout this essay. The next thinker is the early 19th century thinker William Whewell, who said, new empirical knowledge is acquired by attempts to test hypotheses, when the attempts end up with positive results. This important idea is at times ascribed these days to Sir Karl Popper who has never stated it.

It seems obvious that the view that the sole role of observation is to serve as the basis of generalization is not Aristotle's: he said observations have two roles, not one: experience plays one role in a dialogue, or in dialectics, namely that of refuting current, pre-scientific ideas, and another in serving as a basis for generalizations. It is clear that Sir Isaac Newton, to take the paradigm of a scientist, had both roles in mind when he wrote his celebrated *Principia*: in Book II of that great work he uses facts to refute the theory of Descartes, and in Book III there he presents his own theory as series of generalizations from the facts. Nevertheless, it is questionable, and it was questioned, whether Newton followed Bacon or Aristotle. The first was Henry Pemberton's highly authoritative (he edited the third edition of the *Principia*) *Account of Newton's Discoveries*, where, in the preface, Newton's approval of Bacon's methodology is recorded. Opinion went his way until Ellis and Liebig offered strong arguments the other way. Today, incidentally, it is obvious that Newton's genius enabled him to fulfill his own neurotic wish and miraculously prove his theories kosher by any current criterion. (This is known in social anthropology as multiple legitimation, or legitimation by multiple lines of descent; see Sir Edmund Leach, "The Legitimacy of Solomon", in his *Genesis as Myth and Other Essays*, 1969.)

Ellis declared that Aristotle's idea that observations serve as the foundations of generalizations is true, and that Bacon had called this very idea induction by enumeration and denounced it a childish and vicious since it offers no guarantee against the possibility of ever finding an instance to the contrary. Thus, Ellis concluded: as far as the logic of discovery is concerned, Bacon did not say the right thing. "That his method is inapplicable cannot, I think, be denied" is Ellis's verdict. What, however, did Bacon say? Almost nothing, lamented Ellis. The prevalence in Bacon's writings of comments on induction, proper and improper, does not mean that he said what proper induction is. Ellis was exasperated by his failure to find Bacon's positive view of induction. True, there are hints. For example, there is induction by exclusion: instances to the contrary, we know, logically exclude generalizations they conflict with. And Bacon recommended drawing tables of exclusions. Yet exclusion is not enough: science must offer not only negations, admonished Bacon, but also affirmations. How does one get to affirmations? How, that is, does one do so correctly?

It is easy to say how one does so incorrectly: one can always be impatient, desire fame, jump to conclusions. This is improper induction, or induction by the anticipation of nature (= hypothesis). Proper induction is not the anticipation of nature but the interpretation of nature, the reading of the laws from information, the coming to the proper and empirically demonstrable conclusion not too hastily and not too slowly. What, then, is the proper speed? No answer.

Worse, still, Bacon offers the wrong answer: In his *magnum opus*, his *Novum Organum*, in Book II, he offers an instance of proper induction, tables of facts, of exclusions, of affirmation too. Scholars still debate the question, is it a proper instance of proper induction (as John Stuart Mill surmised) or merely a fake illustration? It does not matter here unless we have a criterion. The criterion, whatever it be, must tell us what is the induction that is performed too early — the objectionable jumping to conclusions — and what induction is performed at the right time.

In that instance Bacon gave a "permission to the intellect" to perform a guess. This he justified by the observation that "truth emerges from error quicker than from confusion", and this maxim of his was extremely popular before Ellis observed that since Bacon's philosophy is devoted to the growth of true knowledge on the premise that this is conditioned by the avoidance of error at all cost, and on the supposition that jumping to conclusions is forbidden as it may lead to error, Bacon's permission to the intellect is nothing short of a declaration of a bankruptcy.

To complete the picture, Ellis did reconstruct a theory of the logic of science that he ascribed to Bacon, and it seemed to him as good a rational reconstruction as possible; yet unfortunately he found it hardly of any value. He ascribed to Bacon two metaphysical principles about science and nature. The first principle is that the number of candidates for the position for the status of the true picture of the universe is *a priori* finite. This principle that Ellis ascribed to Bacon is the same, incidentally, as John Stuart Mill's principle of the simplicity of nature and of John Maynard Keynes' principle of limited variety. (Keynes acknowledged his debt.) The second principle is that a large stock of varied empirical information is likely to refute all but one of these candidates. As Bacon has put it, God has created nature and the human mind in parallel (on a par) so as to insure the possibility of science. Immanuel Kant, incidentally, whose *Critique of Pure Reason* refers to Bacon only in the motto, declared this principle a lazy hypothesis, and deemed it most objectionable. Ellis found it disappointing, and Bacon's evasiveness frustrating.

Ellis's picture seems to suffer from excess sophistication. Bertrand Russell offers a much simpler picture, much closer to the text, and much more naïve. Referring to Bacon's example of induction in his *Novum Organum*, Book II, Russell says, Bacon recommended that the student of a phenomenon — heat in his example — should list as many and varied facts about it, should make lists of its presence, of its absence, and of its variation; these alone will reveal the cause of heat as absent in the list of absence, prevalent in the list of presence, and varied in the list of variation. That is all.

This picture is excessively Aristotelian, although Bacon hated Aristotle and condemned him excessively. This picture is also much too naïve. Yet it is echoed in the leading Baconian texts, such as Sir John Herschel's *Preliminary Disclosure* of 183l, in John Stuart Mill's *Logic* (his four canons reflect this very Aristotelian idea) and in all the texts that follow its wake, including C. G. Hempel's lovely and popular *Philosophy of Natural Science*. As Giorgio Santillana said, there is a *Homo Aristotelicus* lurking almost everywhere. Incidentally, this Aristotelian view that is so popular, the idea that the cause of a phenomenon is absent at its absence, present at its presence and to varying degrees, is refutable by the fact that causes ever so often misfire — for example, when a piece of weaponry literally misfire. Also, as Russell observes, this (Aristotelian) view leaves no room for hypotheses in science.

Back to Ellis. With all the sophistication of his rational reconstruction, he clearly saw little or no value in the theory that emerged from his reconstruction. (The same holds for Russell.) Yet Ellis's initial intent was to find whatever is of value in Bacon's writings and thereby both explain and justify his fame. Can this be done? How? This was Ellis's problem. Ellis, thus, discovered the riddle of Bacon: whence his great fame?

2. Solutions to the Riddle of Bacon

As Ellis was the discoverer of the riddle of Bacon, his is the very first attempt at solving it. He found a myth in Bacon that he (erroneously) deemed original; yet it is of little interest in itself. He found a passage in Leibniz declaring Bacon original and important regardless of all appearances to the contrary. This is intriguing, but no solution. And he found a passage on Bacon in the works of Robert Hooke, the resident scholar of the early Royal Society of London, who was a staunch admirer of Bacon. That passage praises Bacon for his democratic view: every individual, he said, is a possible researcher and contributor to science.

Liebig poured as much scorn on Bacon as he could. He then obviously felt the problem, as he offered a solution to it: he praised him as a stylist and his *Essays* as his great contribution — to English literature, not to human knowledge. And he cited Bacon himself to say something to that effect.

There are two more nineteenth-century solutions to the problem. One is that of Thomas Fowler, an Oxford Scholar and an editor of works by John

Locke and by Bacon, who said, everybody knew what Bacon preached, but he was the one to "shout from rooftops" the important message. (Russell, incidentally, held a similar view.) Spedding, however, seemingly held the opposite view: he said, nobody has yet tried to effect the proposal that Bacon had made, namely to start a science with an utterly indiscriminate collection of many and varied facts and hope to see theories emerge from that collection; and, he added, perhaps we should try out this proposal. Whewell responded rather high-handedly: he had philosophical and psychological reasons for rejecting Spedding's proposal and he had empirical evidence too. (He used it to refute an similar proposal by Baconian Sir John Herschel): the Royal Navy had collected meteorological *data* quite indiscriminately and they naturally turned out useless ― scientifically and in any other way.

Every proposed solution mentioned thus far is true. It is true, as Hooke and Ellis said, that Bacon's methodology is egalitarian: every theory of science that presents the logic of discovery as an algorism ― as a sausage-making machine, to use Sir Karl Popper' (unjustly, perhaps) derisive image ― has the merit of presenting an utterly egalitarian philosophy of science, one that deprives every researcher of all scientific authority. Egalitarian Popper approved of this trait with no qualification. Traditionalist Michael Oakeshott disapproved ("Rationalism in Politics"). It is likewise true, as Liebig says, that Bacon's *Essays* had a profound impact on English letters and even a favorable one, as they are brief, incisive, bold, and, unlike those of his celebrated predecessor Montaigne, they are free of the sermon-style references to other people's writings. They refer to almost no other author, and when they do so the references are playful and even censorious ― they seldom appeal to anyone's authority. Thomas Fowler is also right in viewing Bacon a powerful propagandist, though his works of propaganda were not so much his essays as his more methodological writings. Finally, Spedding is right too: the project that Bacon envisaged is of such a magnitude that the Royal Navy's collection of climatological *data* can hardly qualify as an attempt at its implementation. What does all this signify? It signifies that we should spend a few words on the question and examine possible criteria for an adequate (true or false) answer to it: what statement can qualify as an adequate solution to the riddle of Bacon? What is required of it?

The high position that a thinker may have in the hall of fame of science may occasionally be due to some irrelevancies. Generations of heads of departments or deans of important academic institutions enter the hall of fame ― yet they usually leave it fast, often soon after their demise. Historians find it difficult to ascribe any ideas to these celebrities. An exception, perhaps, is Louis Agassiz, the powerful Harvard naturalist who was a stubborn and staunch opponent to Darwin in the United States. His fame still holds, partly on the grounds, valid or not so valid, of his (rather meager) contributions to empirical geology, but largely as a token ― a token counter-example to the view that scientists are often very famous merely because they are powerful individuals with little or nothing interesting to have contributed to human knowledge. Also, and quite similarly, many a time author claim fame for their reaffirmations of dogmas held by the scientific community at times when some hope lingered for overcoming these dogmas.

Famous thinkers like Auguste Comte and Wilhelm Wundt, and they are quite a few of those, may have contributed a significant idea here and there, but their chief claim for fame is due to their dogmatic reiteration of old dogmas (the hierarchy of sciences in the case of Comte; associationism in the case of Wundt). Even great thinkers (like Hermann von Helmholtz) are often admired not so much for their great contributions to knowledge but for the regrettable dogmatic trait of their character (like Helmholtz's unwavering clinging to action-at-a-distance in the face of the rapid growth popularity of electromagnetic field theory). So there is no guarantee that the riddle of Bacon should be solved by proving that his fame is well deserved. C. D. Broad's tri-centenary 1926 lecture on Bacon ascribes his fame to the Encyclopedists' erroneous view of him as an artist. Yet, for methodological reasons, we may better look for a deserved reason. At times fame brings about more fame (nothing succeeds like success), and it is of course Bacon's fame that makes historians of ideas and of science pay attention to even his gullible advocacy of magic and thus increase his fame. The Royal Society of London chose Bacon as its patron saint despite his embarrassing traits partly due to sheer political, public-relations reasons. All these are really neither here nor there, since, clearly, he could easily be forgotten already in the 1750's when his sun was in its zenith, yet he was neglected in the 1850's, when his memory was quite intentionally and even savagely obliterated by some nineteenth-century severe critics. (It is not Bacon but Descartes, said then highly popular and influential Heinrich Heine, who is the father of modern philosophy; *Religion and Philosophy in Germany*.)

The riddle of Bacon will be best solved by ascribing to him something of value ─ preferably something that his diverse eighteenth-century admirers deemed valuable and that his nineteenth-century critics did not. This will explain the rise and the decline of his fame. This is hardly conceivable. Thus, his anti-Copernicanism was indulged him first and strongly held against him later. Why? (In between Laplace censure Bacon's anti-Copernicanism, but mildly and incidentally. See his *Systeme du Monde*.) The logic of the situation may easily guide us the other way: in the seventeenth century, when Copernicanism was a live issue, his hostility to it should have hurt the Copernicans and so they might easily have resented it; they did not. In the nineteenth century, when Newton's theory was almost universally taken as the paradigm of science, Bacon's anti-Copernicanism was repeatedly mentioned as his disgrace.

The egalitarianism of Bacon was praised (rather fleetingly) in a passage by reputable Robert Hooke. This, however, hardly signifies; much less signifies Leibniz's cryptic remark praising Bacon for some unspecified profundity. Both are irrelevant to the immense popularity of Bacon in the seventeenth and the eighteenth century. Liebig's and Fowler's praise are besides the point too, since Bacon was admired not only for his style and for his *Essays*, and not at all as a mere propagandist. Spedding's proposal to take seriously a proposal never previously heeded to is even more besides the point. If anything, it renders the riddle more puzzling, though perhaps akin to one that religious reformers repeatedly raise: why do you worship the memory of the founder of our faith if you heed it not?

It is hard to survey the vast literature and fish out all the proposed solutions to the riddle of Bacon. Nor is it really necessary. Since the early twentieth-century solution due to John Maynard Keynes has already been mentioned, let us examine it. He said he owed the principle of limited variety to his predecessors, though they were not sufficiently clear about it, not even Bacon (*à La* Ellis, we remember) and Mill. We should not debate the merit of this principle or its defects. Suffice it to notice that as an explicit and carefully treated principle it is Keynes's, or Mill's at the earliest, whereas the vague idea — nature loves simplicity — is commonplace and so cannot help us solve the riddle. Similarly, William Kneale, as well as G. H. von Wright, both famous philosophers of induction of the post-World-War II era, have both ascribed to Bacon the idea that a contradictory instance to a theory refutes it. One of the most famous and powerful elders of today's philosophy of science, Adolf Grünbaum, has recently done the same. He concluded that this way Bacon has anticipated Sir Karl Popper's criterion of scientific character as refutability and Popper's view of refutations as progress in science.

I am at a loss. I do not know how to approach my readers. I mean no disrespect to them, and I fear that arguing against the views cited in the previous paragraph will greatly offend them: it is not too much to expect of authors to know that their readers may know enough traditional logic, say, the square of opposition, or some Platonic dialogue or another; yet William Kneale, a marvelous and pioneering historian of logic and a great scholar, has solved the riddle of Bacon seemingly in defiance of that fact. And, of course, whereas Popper saw progress in the discovery of refutations, Bacon thought such discovery hardly worthwhile since, he repeatedly said, or even "shouted from rooftops", people scarcely give up their convictions when they are presented with contrary instances, and instead they save their pet theories by making some "frivolous distinctions", which is why the scholastic literature is so complex and so worthless. He repeatedly said that were these professors good researchers they would not have asserted erroneous ideas to begin with so that they would need no refutation of any. He emphasized that negations are of little value, as science must rest on affirmations — a point that Popper is at pain to deny from the very start of his career and to its very end. Whereas Bacon, as Augustus de Morgan observed (*A Budget of Paradoxes*), preached error avoidance, as no hypothesis was ever permitted in his view to enter science (which is why Ellis saw his "permission to the intellect" as self-betrayal), Popper preaches the repeated employment of bold hypotheses and the endless process of error elimination; the contrast between error avoidance and error elimination seems clear enough to invite no further comment. Grünbaum's solution to the riddle of Bacon, then, is no solution; it is but the root of another riddle concerning respected academic contemporary philosophy: how low can it go?

Barring, then, much of the Baconian literature that solves the riddle of Bacon frivolously while uncritically repeating one or another of the inadequate solutions mentioned earlier, we are left with very little.

In the nineteenth-thirties three different thinkers, each exciting in his own way, came up independently with a new kind of solution to the riddle of Bacon: though not original, Bacon represented a significant trait of the late Renaissance, and as the Age of Reason put the Renaissance at the distant background, he represented the debt of that age to its predecessor. These were, R. F. Jones, C. W. Lemmi and Tadeusz Kotarbinski. Jones noted that whereas in the late Middle Ages the theory of universal decay was popular, the Renaissance replaced this by universal progress and so authors claimed that they innovate rather than follow ancient authority. The once famous historian of science Lynn Thorndike has one very impressive essay in which he cites diverse Renaissance authors to claim they were following nobody's footsteps but breaking new grounds. Lemmi and Kotarbinski notice that Bacon attacked extant alchemy but defended some future alchemy. (By the way, Kepler held a similar view of astrology.)

Nor is that all. For, after all, what is the Renaissance heritage for the Age of Reason is hard to say, and whatever it is, we can find it reflected in Bacon's works somewhere ─ he was kaleidoscopic and as he never elaborated he had to reflect as many of the platitudes of his age as possible. An amusing example is the Renaissance obsession with the conquest of nature, that E. H. Gombrich, for example, finds epitomized in the personality and character and ideas of Leonardo de Vinci. The same idea is, of course, repeatedly expressed when the importance of science as the tool of mastery over nature is observed ─ for example in Thomas More's *Utopia*. Bacon did that too, though, clearly, he felt that knowledge is much more valuable than mere power, and he stressed the view that knowledge is power for merely secondary reason. First, he felt that the worth of applied science is a great incentive to people who want to be "benefactors of mankind" to engage in pure research with the hope that the applications of its fruits will be useful. Second, he took applicability to be the "mark" of science, i.e., the proper demarcation of genuine science from pseudo-science. (This is why he attacked the alchemy of his day so aggressively.) Third, he sought a biblical guarantee for the future success of science and found it in God's promise to the sons of Noah of the domination over nature. He took it for granted that only true and comprehensive knowledge guarantees such domination. Nevertheless, Benjamin Farrington, and J. Crowther, the Marxist popular writers of all sorts of non-fiction, who are at times identified (by other Marxists) as historians of science, both see Bacon as a predecessor to Marx in his adumbration of Marx's theory of praxis, i.e. of Marx's theory of the unity of theory and practice, i.e. of Marx's conviction that science and technology grow together. Even H. B. Acton, in his grandiose attack on Marxism (*The Illusion of the Epoch*) solves the riddle of Bacon, though incidentally, by the claim that Bacon stood in the Age of Reason as the symbol of a thinker impressed by the symbol of Prometheus, the conqueror of nature at any cost, and as such he well presented the spirit of the Age of Reason as well as of Marx's theory of praxis.

At this junction it becomes clear: efforts to ascribe to him an intellectual contribution that his admirers recognized and for which they admired him is insufficient: we should decide what makes an idea count as a significant intellectual contribution. Thus, Ellis recognized as original and as popular Bacon's classification of prejudices, for example, but he refused to consider this a contribution, as it is trite: all classifications are. Similarly, reference to his enormously popular *Essays*, parables, and propaganda, will not solve the riddle. His Utopianism — his image of a scientific-technological society — may be different. Possibly it is a bold idea, but as an influential idea it is but a part of his propaganda campaign in favor of science, and propaganda is not exactly an intellectual contribution. What is? What contribution signifies?

3. The growth of knowledge

The latest solution to the riddle of Bacon is a statement that appears toward the end of the Introduction to *Francis Bacon: From Magic to Science* of Paolo Rossi ([1957], 1968): "He was . . . responsible for a new intellectual attitude to science which the Enlightenment and Kant — and later the positivists — maintained." It is not clear what "responsible" means here, nor what that attitude was. Nevertheless, we have here a solution. The difficulty, incidentally, hidden behind the word "responsible" and the opacity of the statement cited, relies on Rossi's view of Bacon's influence as a significant factor in the transition from magic to science mentioned in his book's subtitle (from magic to science), although Bacon was much more taken by magic than by science, or rather much more magically-minded than scientifically-minded: what impressed him about science is scientific technology: it is magic that works. Other writers endorsed a position similar to Rossi's, such as Hans Blumenberg, whose *The Legitimacy of the Modern Age* (1966, 1983) presents Bacon as the promulgator of the modern world, scientism or positivism. (See also Eric Voegelin, "The Origins of Scientism", *Social Research*, 15, 1948, 462-94 and O. Bradley Bassler, "Theology and the Modern Age: Blumenberg's Reaction to a Baconian Frontispiece", *New German Critique*, 84, 2001, 163-192.)

The attitude described here as that of the Enlightenment and of modern positivists deserves a name, and the name Blumenberg uses, "scientism", is as good as any. Scientism is described — I will later contest this description — as the view that science is a set of empirically provable doctrines that command our endorsement.

Can we ascribe scientism as characterized here to Sir Francis Bacon? Some, including Ernst Mach, said, no: he was too magic-minded, alchemy-minded, even a cabbalistic word-magic fancier. Today commentators dismiss such claims out-of-hand: we are speaking of Bacon's ideas, not of his failure to apply them. Can we say that Bacon discovered them? No. Both the idea that science is demonstration and the idea that scientific demonstration is empirical — empiricism — are ancient, and one of the most popular formulas in philosophy ever since it was stated in the Middle Ages is, nothing is in the intellect that was not first in the senses. Why, then, do Rossi and his followers ascribe scientism to Bacon? Is that why Rossi uses the vague word "responsible" in this context? What is modern about scientism?

There is an idea here that is hard to articulate though it is obvious: scientism is distinctly modern even though its ingredients are old. Though the medieval thinkers distinguishes science from opinion, though they declared science empirical, though they denounced opinion and praised science, these ingredients did not combine in the Middle Ages to the scientism characteristic of the modern age. What is the difference, if any? The intuitive idea is that medieval thinkers were gullible and superstitious and confused and fascinated by magic and mysticism, whereas the modern followers of scientism are tough and hard to sway and contemptuous of opinion, especially magic.

Not so. Credulity and concern with magic are, at best, matters of degree, and Bernard Shaw declared (*St. Joan*, Preface) modern credulity worse than medieval credulity. Let us consider this a mere paradox. Bacon was credulous and magic-minded. He did not preach against magic, only against magic as practiced by the superstitions: he deemed scientific magic and scientific alchemy possible, much like Kepler regarding scientific astrology.

The founders of the Royal Society of London were much less credulous and objected to all magic almost unanimously and almost entirely. Were they influenced by Bacon? Can we consider this his contribution to human knowledge? Is such an influence — Bacon's or anyone else's — possibly a contribution to human knowledge?

Consider Goethe's claim that he had written poetry that helped fight bigotry and enlighten readers and even enable them morally and intellectually. One may object: some of Goethe's works represent attitudes that we would not consider morally or intellectually valuable; yet this is no sufficient ground for the dismissal of his claim to have a positive intellectual and moral educational contribution through his poetry. Will it then count a contribution to learning? Consider any moral-educational contribution, such as that of Émile Zola, who challenged his compatriots to be honest and brave (*J'accuse*!); it is a contribution to the growth of science, even if indirect, but it is no addition to scientific knowledge the way the discovery of a law of nature is. Take Bacon's call for hope and his backing of this call by declaring that God's guarantee to the Sons of Noah a domination over Nature is a promise that humanity will have science. Did these ideas influence people to become researchers? If yes, is this a contribution to human knowledge? No.

Take a major contribution to the modern world and the growth of the scientific ethos ─ the idea of universal schooling aimed at instilling universal literacy. Much as this idea indirectly contributed to the growth of science, there is nothing in it that is a new observation or a new law of nature, and so it is not by itself a contribution to human knowledge. Admittedly this idea was advocated by Johann Pestalozzi and it was implemented because it was advocated (by Napoleon, and more out of militarism than out of scientism), yet neither Pestalozzi nor Napoleon thereby becomes a contributor to human knowledge, no matter how significant to its growth their social reform was. Yet this reform rested on an innovation made by Pestalozzi: hence he was an innovator: he discovered what today economists call human capital, and he discovered it as a national asset capable of lifting his country ─ Switzerland ─ from poverty to prosperity. And his innovation and his reform intertwined.

Assuming that Bacon was a reformer is unproblematic: he viewed himself as one, and so did his followers and his detractors. Yet we should ask what in his call for reform was successful, and what new idea stood behind this successful call for reform.

Here Rossi is unable to help us in our search. He commented on the claim [of Fowler] that Bacon was important even if he said nothing new, because he was the first to stress a certain idea that experiment is important: "So many others [before Bacon] stressed the importance of experiment … and insisted on the practical aspect of every discovery", he says (34), yet he ascribes to Bacon some new reform plans, such as the erection of a science as (in Bacon's words) the creation of the establishments and institutions of a science (23), and ones endowed with "a public, democratic and collaborative character" (27). This reform plan never worked. Margery Purver claims, *The Royal Society: Concept and Creation* (1967), that a group that practiced research and then became the nucleus of that society attempted a collaborative effort. Yet the successes of science were hardly collaborative. True collaborative science began at Los Alamos, and it brought with it at once intrigue and politicking that proved forcefully how unprepared for this the best scientists and even the best organizers of scientific collaborative research were (Robert J. Oppenheimer, in particular). But even if one claims that Bacon succeeded as a reformer, and significantly so, we may still ask, was there a new idea behind his proposed reform? If yes, which? Rossi does not say.

Assume that knowledge is empirical, comprising facts and theories based on them, and ask, how do we distinguish a new item in the body of knowledge? What is a scientific innovation? We have to do so because followers of scientism do declare only science important; histories of science written from the point of view of scientism do not ascribe any scientific innovation to the greatest metaphysicians, educators, compilers of most important encyclopedias and handbooks, and their likes. Non-followers of scientism may mention, for example, important philosophical ideas that influenced research, since it is hard to comprehend valuable results of research without throwing a glance behind the scenes and see extra-scientific influences on it.

This is a semi-scientistic attitude to the history of science. Assuming it, we may conclude that Bacon made no contribution at all to human knowledge: no one since Ellis and Liebig ever defended Bacon's scientific works as anything better than sham, and even his most ardent admirers have never ascribed to him any empirical discovery or any innovation of any scientific theory. Some authors propose the false idea that heat is motion and ascribed it to Bacon (although he merely reaffirmed it; he used it as an example of a scientific theory just because he rightly wanted to illustrate his theory of induction with an unproblematic instance: instances are always better unproblematic). Since the standard praise of Bacon is that he discovered the empirical method of science, this very praise forces one to deny his discovery the status of science and so deprive him of his contribution even prior to examining the praise. What does this standard praise amount to? Can his theory that heat is motion count as a contribution to human knowledge? How can we ignore such an important idea and say, no? How can we affirm such an unscientific idea and say yes?

We are stuck.

There is a subtlety here. A number of writers have observed that some general ideas, some "intellectual attitudes", to use Rossi's lovely expression, are now so taken for granted that the tremendous significance of their discoveries are not noticed, and that the tremendous intellectual efforts invested in their discoveries are likewise lost on us. The very discovery of nature, some say, for example Erwin Schrödinger, is such an instance; and we can hardly say who made it except that it is the basic idea of the pre-Socratics, the so-called *physiologoi*, the early Greek naturalists.

Another example is the case at hand. The marvelous and most powerful techniques of modern formal logic were used to restate the medieval idea that Rossi and others seem to have ascribed to Bacon. The restatement of the old idea with the aid of the machinery of modern logic is scientism or positivism or verificationism — the most advanced version of the identification of human knowledge with inductively founded science. It is present in Sir Francis Bacon's *Novum Organum* of 1620. But we cannot insist, as this newest and strictest version of scientism is paradoxical. For, one can apply to it the qualification of Leibniz: nothing is in the intellect that was not earlier in the senses, he conceded, except for the intellect itself, he qualified his concession. To translate: all information is empirically verified except the verification principle. The principle may be devoid of cognitive meaning and it may be empirically founded (truth about humans). As devoid of meaning it is not binding, and as an empirical proposition it has not been tested as yet:

no one knows how to test it. It may be a definition of a word, but that will render it uninformative and so useless: the invention of a word never deserves praise. It may, of course, be an exhortation or a hope (Wolfgang Stegmüller), and so it may be viable even when devoid of informative content. But to say it is sheer exhortation and sheer hope is to allow indifference to it — as well as to Bacon who declared himself the master of the exhortation to perform research and the hope for its tremendous yield.

All these are logical subtleties, perhaps even sheer sophisms, that may annoy scientific researchers, individuals investing the honest sweat of their brows in order to advance science. Let us concede their resentment of true sophisms and approach their labors and the fruits of their labors with due respect. Let us agree with Bacon that contributions to empirical science, however humble, deserve all the respect we can show them and are worth more than the wildest metaphysical speculations.

What do ordinary researchers contribute? What are the ways and means of their contributions to knowledge and what items do they contribute? Are these novelties? What contribution is innovative?

The question is tremendously difficult, and yet we all feel that we know the answer to it. To summarize what hundreds of independent original workers do and characterize the novelty in them is almost impossible, yet we all know what is new. We have information and ideas and instruments and techniques that we did not have before, and so on. Bacon said, any new idea should lead to a new techniques, instruments, or implementations. Now perhaps not every research causes that, but perhaps every set of researches, the output of a concrete research team or the abstract set of outcomes of a few studies of the same puzzle. The idea is that every researcher can make a contribution, however small. This, we remember, is Bacon's democratic bent praised by Robert Hooke in the late seventeenth century and by Robert Leslie Ellis in the mid-nineteenth. Today this idea is ascribed to Thomas S. Kuhn, who says normal scientists perform normal research and normal research is aimed at solving normal puzzles — at finding answers to questions small enough to be amenable to normal attempts at solving them.

Kuhn's democratic bent does not go as far as Bacon's: not everyone but every graduate of a decent university, he says, is capable of research. But this is usually attributed to the vast growth of science that makes now necessary more background knowledge and more competence than when science was young. (This is an error, as science is always young, but I need not insist.) Also, unlike Bacon, Kuhn does not demand that scientific theory should be demonstrable. Yet he had no trouble viewing as innovations the new ways to do things, and the broadening of field of science. Are Bacon and Kuhn contributors to human knowledge, then?

Certain ideas, such as Bacon's or Kuhn's scientism, are very important yet not scientific. *Hence,* Scientism is false. These ideas may be significant and innovative (though Kuhn's ideas were better stated by some of his predecessors, notably Michael Polanyi), but not contributions to science. Contrary to the commonsense of most scientists, false ideas, such as scientism, may count as significant contributions. Indeed, false ideas within science, such as Lyell's creationism or Malthus population theory, are important contributions to science. Even the idea that some contributions to science are false is a very important idea, but it is not scientific and it runs contrary to commonsense. Indeed, the lovely jocular bumper-sticker, "Repeal Ohm's law!" (of electric resistance) comes to illustrate the claim that scientific laws are true, yet Ohm's law, almost true for a relatively short straight wire, has to be replaced by the law of self-induction when the wire is coiled, which is why wires in motor-cars and in radios are coiled, and it has to be replaced by the telegraphic equation if the wire is very long.

Once we realize that an intellectual contribution need not be scientific (it can be meta-scientific, for example) and that an intellectual contribution need not be true (it can be a significant error or an approximation to a better idea, and so on) we are ready to solve the riddle of Bacon. For Bacon's new and ever striking idea was neither scientific nor true. It was the idea of radicalism: innovation can begin in earnest and the growth of knowledge can be effected systematically only after we begin afresh — by wiping the slate of all of our older false beliefs.

4. Bacon's doctrine of prejudice

My solution to the riddle of Bacon is very straightforward; as I do not endorse Bacon's doctrine of prejudice; the major pitfall of exegetes — identification with their heroes — is one I need not be concerned about. It is an idea that he clearly and emphatically advocated, and one for which he has staked his claim for priority. It has influenced posterity most, and that transforms scientism from its medieval to its modern style. Finally, all this explains the preoccupation of most philosophers of science from the seventeenth century to date with rational and irrational belief.

Bacon's doctrine of prejudice says this. Only people free of all dogma and superstition can do proper research, and only people who believe nothing except what is proven are free of dogma and superstition, and any theory that is contested between scientists is dogma so that in any allegedly intellectual dispute all parties are at fault.

This doctrine, let me stress, is extremely popular and incredibly pervasive. Ever since my adolescence I meet it in one place or another in one variant or another in whatever I read about science, if not in whatever intellectual activity I happen to engage in. This doctrine, let me add, is the sole justification of the concern people have in the rationally or irrationality of beliefs. Scientism, then, is not only the advocacy of empirically proven belief, in one or another sense of empirically proven, but the denunciation of any belief not empirically proven — religious, philosophical or magical. Scientism is not only pro-science but, and chiefly, anti-non-science. I will return to this point at the end of this discourse.

The chief difficulty concerning this solution is its very novelty: why has it escaped the notice of earlier commentators? How come the diverse authors commenting on Bacon's works neither ascribed this doctrine to him nor attempted to solve the riddle of Bacon by ascribing it to him? Even though his classification of prejudices is well-known and often noticed (and rightly dismissed as insignificant on the authority of Ellis, perhaps), his doctrine of prejudice is not. Why?

Bacon discussed the question, how novel his philosophy is. He noticed that his views on the inductive method can be found in ancient writings. Yet, he claimed he was the first to observe the ill effects of the employment of this excellent method without a prior ascertainment of having given up all preconceived opinion and a resolute systematical avoidance of the hasty jumping to conclusions. Macaulay, Ellis, and deMorgan noticed this when they presented Bacon as the philosopher of error avoidance. Yet they all clearly thought this idea either unimportant or too obvious to count. As Bacon declared that we must start afresh, Ellis sought in Bacon's writings some fresh start and could not find it. The idea of a fresh start itself he took in his stride. This very idea was taken a bit more seriously by R. F. Jones, the authors of the classic *Ancients and Moderns*, whose researchers, in the 1930's, transformed many fields of study and was seminal in the evolution of the modern field of the sociology of science. Yet Jones was concerned with Jonathan Swift, and he tried to reconcile his own love of science and of Swift with the latter's hostility to the new science (savagely ridiculed in *Gulliver's Travels*, for example.). He noticed that Bacon was the father of modern thinking and that Swift's respect for antiquity led him to oppose the moderns. This leaves all questions concerning Bacon open, as well as most questions concerning the moderns, the *literati* who sided with the Royal Society of London and its dismissal of ancient learning as rather light. This will not do even for the understanding of the battle between Ancients and Moderns: we have to notice that Bacon preached contempt for ancient learning and thus invited contrary measures.

Bacon's contempt is nothing personal: it is a part of his doctrine of prejudice: contempt necessary as defense against unwanted influence. It is difficult for a master to admit error and become the laughing stock of the learned world, particularly when one's conjecture is one's claim for fame. Hence, one has an incentive to look at the world through the spectacle of one's conjecture: the conjecture becomes the blinkers that conceal from its user the facts that go contrary to that conjecture. And when one's attention is drawn to these facts, one is tempted to dismiss them as irrelevant by one excuse or another — and excuses are always available, at least in the form of some frivolous distinction or another, to use Bacon's apt idiom. Hence, one must first reject all doubtful ideas. That is to say, with contempt.

The theory of perception that is part and parcel of Bacon's doctrine of prejudice is impressive. Psychologists and sociologists repeatedly discovered it. Nowadays they label parts of it as Leon Festinger's theory of cognitive dissonance and Morris Ginsberg's analysis of social prejudice. The holders of the theory consider it amply confirmed; they disregard the evidence that runs against it, of people changing their opinions upon meeting evidence to the contrary.

And here lies the difficulty: most thinkers endorse the theory as a matter of course; they neither ascribe it to Bacon nor see it as a very important idea; most of the rest, who deem it false, see no merit in it and no praise in ascribing it to anyone. Yet it is only Bacon's doctrine of prejudice that prevents people from praising a thinker for having invented a interesting false idea or from declaring it false. True or false, Bacon's doctrine of prejudice is easily one of the most important ideas of all times.

Making hypotheses, he said, is a form of conceit, an attempt to appear clever, a tyranny of the mind that imposes its vain notions on other minds, an attempt to put nature into chains, the rape of nature: what are one's pet theories to Nature that She should abide by them? But if one avoids conceit and humbly observes the whims of Nature by attending to her smallest ways, if one stoops to conquer, then one is bound to be rewarded; Nature is bound to show Her charms: only God is allowed to be hidden for ever.

This is no mere metaphor; the Cabbalistic sexual symbolism is supposed to reveal great secrets. Skepticism shows that all search of knowledge is questionable. Hence, researchers must stay utterly passive and allow the Naked Truth to reveal Herself. This idea became dominant: Descartes and Spinoza, Locke and Hume, and all of their followers, advocated it in diverse ways. the Theory of the Naked Truth, when presented without religious, Cabbalistic preamble, raises incredulity: if Truth reveals Herself, how come we are all ignorant? The answer is Cabbalistic: success (in bringing the Messiah) is assured only to the worthy; and the worthy is not only just and righteous, but also pure of the heart. The pure of heart is humble, and humility is essential for success and very hard to uphold in the face of imminent success. This is the moral of the terrible story of the (11th Century) Cabbalistic rabbi, Joseph de la Reina, who nearly succeeded, but became vain, lost, and committed suicide. It is a catch, as we say today. Thus, Dr. Thomas Thomson, the famous early nineteenth-century chemist and historian of chemistry said that Lavoisier himself was a vain person. Proof: he published false hypotheses.

The dispute between Ancients and Moderns was between traditionalists and radicals: should we respect tradition? Bacon's contempt for all traditional learning was rooted in his doctrine of prejudice: unless one rejects out of hand all past learning one's researches will only confirm old opinions and thus entrench all error. Following Bacon, Descartes proposed his first rule of method: reject as false any unproven theory: entertain no opinion, hold no conjecture, and propose no hypothesis. Later in his writings Descartes, too, gave permission to the intellect to feign hypotheses of a certain kind — mechanical hypotheses. Newton, however, sternly disapproved: I feign no hypothesis, he said.

How should we approach tradition? This question remained central. John Locke, echoed Bacon in saying, we should endorse from tradition only those parts of it that have been ascertained to be true. Traditionalists say, we should endorse tradition and only reject those parts of it that have been ascertained to be false. The battle still goes on. Traditionalists such as Hans Georg Gadamer admit traditionalism to be prejudiced and yet they insist on the endorsement of traditions for as long as possible.

For, although Bacon's doctrine concerns scientific method, it has immediate political implication: the very application of it to politics creates political radicalism, as is obvious from the very label radical, which means uprooting (of all tradition). Bacon realized that applying his doctrine of prejudice to politics will create a radical political science. Being a political conservative, he opposed this. The law, he said, must rest on dogma, since any dogma, any superstition, even the Talmud or the Koran, is better than the lawlessness that is chaotic. Lawlessness is attractive to Baconians, just because proper scientific investigation must start with a clean slate and political science proper enables the creation of the best possible regime: Bacon was the father of utopian movements not only in that he deemed the impact of scientific technology on society necessarily good, but also because his disciples dreamt of political science, of the final and unshakeable knowledge of the art of government.

All this, too, has ancient roots. The Greek discovery of the contrast between knowledge and opinion led quite naturally to the distinction between natural law of and human law, with the clear corollary that when the two conflict natural law prevails. And they do conflict, since human law distinguishes citizen from foreigner but the natural law says we are all siblings (*Protagoras*). Even when they do not conflict, natural law is unchangeable and inescapable and human law is changeable. Modern radicalism, however, goes much deeper: human law, being opinion rather than knowledge, is not authoritative, not binding: the doctrine of prejudice, demanding that we should entertain no opinion, is hostile to human law.

The application of scientific method to politics, therefore, yields political science at once: since there is no ground to the claim that the king is a superior being, not only are we not bound to obey him: we are bound to reject his authority! Political radicalism was hardly avoidable once Bacon's doctrine of prejudice won popularity. Especially when we consider reason to be the central human quality and when we consider the tremendous impact science may have on society, we can scarcely block the growth of political science, and so we find ourselves to be uncompromising political radicals.

Bacon's doctrine, thus, spread from natural science to politics and bred revolution. At first it bred an intellectual revolution, declaring scholasticism worthless, since its doctrines are controversial, and the universities poisonous, since they were scholastic. Bacon claimed that controversy is pointless and endless since no party to a dispute can give up their opinions by admitting error, and since the refusal to admit error engages one is ever smaller distinctions and narrows one's outlook all the time. He thus promulgated a general distaste for dispute that still rules much of the scientific tradition.

This was a blessing at the time, since scholastic disputes were all that Bacon said about them. Galileo Galilei, himself a master of dispute, held the same view of scholastic disputes as Bacon. Yet it was Bacon, not Galileo, who claimed that all, all disputes are bound to degenerate to scholastic quibbles and personal vituperations. Moreover, once Bacon's negative assessment of the value of disputes is endorsed, it is regularly reinforced in line with his doctrine of prejudice: if one is convinced that dispute is personal vituperation, one responds in a personal hostile manner to any criticism of one's views. Scientism is the idea that science precludes dispute; hence, dispute is of no value: it consists of mere bickering.

Nevertheless, even Bacon's leading disciples, not to mention others, all admitted the value of criticism, and so to some extent they engaged in some dispute and condoned some. Even the one most neurotically opposed to dispute, Sir Isaac Newton, was engaged in dispute, and the middle part of his great *Principia* is almost exclusively devoted to a dispute against Descartes. His hostility to dispute nevertheless gave the authority to Bacon's idea: truth precludes dispute, so that all parties to a lingering dispute are prejudiced and their conduct is evidence that they are.

Bacon's idea that medieval learning is worthless and better forgotten evolved into what the modern traditionalist historian Huizinga called the myth of the Middle Ages. This idea is the source of the claim that Bacon is the father of modern science and thus the root of the enormous admiration for Bacon that so impressed Ellis. Of course, Ellis was impressed also by the hostility to Bacon that was fed by the same radical doctrine as applied to Bacon's writings. Indeed, Bacon's scientific writings were suppressed by his earliest admirers as quite embarrassing. His Essays and other writings were popular, but his science was ignored because it was scholastic (and inferior even by scholastic standards).

Bacon's claim to be an innovator does not matter overmuch. In this he has many predecessors and successors. His only innovation, and the only one he made a claim for, is his idea that unless one starts afresh and rejects all traditional learning one's work only reinforces traditional error. He was in error: his idea cannot be tried out. Yet it was a great contribution — though erroneous one. It led to the rise of the modernist movement and the new, anti-university, institutions of learning. (The rise of secular universities had to wait for the French Revolution, and the traditional universities allowed science to enter their *curricula* almost everywhere less than half-a-century later. Even the traditional English universities had then to reform.) The new scientific movement took it for granted that everyone who can give up what one was taught can join the scientific world and engage in research. The movement soon engaged in social studies, including politics, and led to militant political radicalism. The failure of the French Revolution led to the Reaction, to a new traditionalism, to a continuity theory of history. Yet the history of science was considered a radical's domain until the great reactionary philosopher, Pierre Duhem, argued during the period of the revolution in physics that scientific revolutions are impossible, that the history of science, too, is that of continuous reform of tradition, not of hostility to tradition. Duhem invented medieval science, and his popularity among historians of science eclipses Bacon's popularity there.

A strange cleavage remains. Political history used to be traditionalist but the history of the natural sciences remained radical. Later Pierre Duhem invented traditionalist philosophy and history of science and it became popular to some limited extent until the explosion of its popularity in the version given to it by Thomas S. Kuhn. Yet many philosophers of science continue to discuss the traditional radical problems. Why?

The answer is, of course, Bacon's doctrine of prejudice: one who believes a false doctrine disqualifies as a researcher. And the doctrine is refuted by all deviant researchers whose achievements were publicly endorsed but not their heresies. It is likewise refuted by researchers whose work fits current views but who reject these views all the same.

Nevertheless Bacon's theory prevails. The reason for this is that it has much to recommend itself: much controversy is defensive and thus worthless. Bacon's theory is false, but it is a good approximation to the truth and it is also a powerful deterrent. And even were it now with no merit, its historical merit remains.

Bacon's idea eroded slowly. Newton, who allegedly opposed the making of hypotheses, only opposed calling them scientific. (His famous *Scholium Generale* puzzled scholars, as it begins with a warning against hypotheses and proceeds to making bold ones. The situation is clear; he opposed Bacon's doctrine of prejudice, but still insisted that only verified theories are scientific proper.) In the scientific tradition it continues even after Faraday's expressed deviation from it. The radical political philosophy that results from its application to politics has been exposed as populist or as technocratic — since both are opposed to the democratic practice of airing controversy prior to making a majority decision according to the rules of debate of parliamentary democracy. Radicalism gave way to democratic reformism. The radicals historiography of science that denounces all learning prior to the scientific revolution with very few exceptions (exceptions that should count as refutations but that were not taken to be such) gave way to the Duhemian traditionalist theory that postulated continuity in the history of science; the reformist attitude to the history of science is these days also slowly evolving. Einstein put an end to the idea that science should purge all views on every issue as prejudices and superstitions with the sole exception of the ones proven to be true. Its first substitute was the idea that received scientific theory need not be proven; it only has to be proven to be the best. Later the irrationalist claim won popularity that a refuted theory need not be rejected since any refutation can always be blunted by a frivolous distinction.

The idea that every hypothesis can be rescued by some frivolous distinction is Bacon's discovery that explains how prejudices can withstand refutations. But he used it to bolster his doctrine of prejudice that requires the rejection of all hypothesis and the utter suspension of judgment until demonstrable truths come along. Popper called the willingness to employ frivolous distinctions the conventionalist stratagem. He recommended not to use it. The availability of the conventionalist stratagem was then elevated to the level of a principle — the Duhem-Quine thesis, it is nowadays called. This very availability has made the traditionalist view of science very popular, much through the writings of Kuhn who, however, recommends a mixed strategy: traditionalist in quiet days and radical in days of upheavals.

Thus, no matter how often we may wish to reject Bacon's doctrine of prejudice, we may come up with a mere variant of it or, worse, with a predecessor to it that he had superseded. The only way to lay it to rest is to offer a theory that has at least all of its merits while overcoming its defects. And as long as the only alternative to Bacon's theory is the permission to be prejudiced and to make frivolous distinctions and to defend one's prejudices on pragmatic grounds, there will be a tremendous incentive to block these follies to some extent by some version of Bacon's doctrine of prejudice. Is there another alternative? This question should be high on today's philosophical agenda; almost all philosophers today ignore it. But then, as William Gilbert said four centuries ago, philosophy is but for the few.

5. Conclusion: Bacon's philosophy today

Imre Lakatos rejected my interpretation of Bacon as possibly true but irrelevant to today's philosophy. He said, Bacon has no influence today: only the antediluvians take his ideas seriously.

The most important influence of Bacon is his hostility to hypotheses and to jumping to conclusions; his hostility to controversy was derivative to it: were the parties to any dispute willing to suspend judgment until the truth be clearly seen by all, there would be no room and no need for dispute. To this day many scientific periodicals are run allegedly on the policy (a) of accepting for publication every factual discovery, (b) rejecting all controversy and all controversial material, and (c) demanding of every theory that strong ground for it be given prior to it being judged fit for publication.

These days Adolf Grünbaum (a leading if not the leading American philosopher of science) claims that we need not know precisely by what principle we ground our hypotheses in factual information: suffice it that some such principle does exist, since, in manifest fact some but not all hypotheses are well-grounded. Does he suggest that scientists are hostile to baseless hypotheses so that these are absent from the body of science or at least rare there? And if baseless hypotheses are rare in the shop-window of science, are they also scarce in the workshop of science? This is the most central and basic question. Bacon said, once a hypothesis is permitted to enter the workshop of science, even most tentatively, then it may easily be expected to appear soon enough in the shop-window of science dressed up as amply founded in empirical experience! What, then, is the case? Do scientists allow hypotheses into the shop-window and into the workshop of science?

The answer to these questions is not straightforward: scientists are ambivalent about hypotheses. The context and manner in which researchers pose their questions may easily bring them to answer them one way or another.

This ambivalence is not necessarily a matter of psychology. It is true that Freud did explain ambivalence: he deemed neuroses the ones hard to solve, the confused clinging to a refuted hypothesis that is too painful to examine because it was formed under stress. (Freud's theory of neurosis, and more so his view of the individual cured of it as healthy, although health is so unusual, is but the translation of Bacon's doctrine of prejudice into the field of psychopathology.) But we do not have to examine psychology more than superficially when we study scientific method. It is likewise true but not relevant that ambivalence may be explained sociologically. (Marx's theory of class-prejudice is also a translation of Bacon's doctrine of prejudice.) The methodological and epistemological analysis of ambivalence to hypotheses is extremely simple and obvious. Assume a love of the truth and a hatred of falsehood, and take a hypothesis whose truth-value is not known, and you find an ambiguity that easily may turn into ambivalence.

What insures this process is the catch: the endorsement of Bacon's theory leads to the hatred of error not only after it has been exposed as such but also in retrospect. It is ahistorical all the way.

Combating the powerful ideas of Bacon requires strong measures. We need a pluralist criterion of goodness of hypotheses that can be applied prior to the efforts to put them to empirical tests. Hence, we need a criterion to tell us (roughly and tentatively, of course), what distinguishes a good controversy from a poor one. It is an empirical observation that most of us — rightly or not — think we know of instances, historical and contemporary, scientific and artistic, political, religious and metaphysical, that go either way: we know of some good instances and of some bad ones of each of these broad categories. What then distinguishes good controversies?

I have discussed this question elsewhere. Here I repeat that it is inherently pluralist and so irreconcilable with Bacon's doctrine of prejudice. When it will become a major item on the philosophical agenda, then the current philosophic preoccupation with inductive logic and with the rationality of rationale belief will reasonably diminish. With this process we will finally witness the waning of scientism and the growth of a scientific philosophy that is more democratic, more tolerant of religion, metaphysics, and political pluralism, at least by comparison with the days of classical modern thought.

The Royal Society of London (and subsequently the commonwealth of learning at large) had good reasons for their adoption of Sir Francis Bacon as the patron saint of science. The democracy of Bacon surely is one of them —

the ideal that unprejudiced individuals need no background training or information to become scientists. As Paul Hazard has observed, this created a bias in favor of scientific activities open to all. Yet there was more to it. The idea that unprejudiced people do not mix science with politics, religion or metaphysics was very important in Restoration England, as Macaulay observed long ago. The same idea looked very soothing also in France of the Ancient Régime. Ostracized La Mettrie observed in bitter disappointment that the ideology of the scientific community was more of a convenient excuse than a proper guide for action. As he was biased he probably grossly exaggerated, yet we should not make our life easy by dismissing his observation out of hand. Yet history proved him more wrong than right: the taboo against mixing science with politics and religion was a thin veil that barely covered the radicalism of the commonwealth of learning, including the hostility to religion that political radicalism is so infected with. This led to the French Revolution and the almost immediate disappointment with it. (See the intriguing essay by L. Pearce Williams, "The Politics of Science in the French Revolution", in Marshall Clagett, editor, *Critical Problems in the History of Science*, 1959, 291-308.) When Darwin's disciples violently attacked religion, their excuse was that science was attacked by a religious leader who thus canceled the *status quo*. Today the claim that scientists can avoid mixing science with politics is still taken seriously despite many counter-examples that make them take the claim in some unspecified reading, in the light of some barely stated frivolous distinction.

Giving up the doctrine of prejudice includes give up the taboo on scientists expostulating in matters political or religious. Pluralism permits it, though it should be subject to some reasonable rules of conduct.

This will be a revolution of sorts. The Baconian revolution that led to the rise of the Enlightenment movement condemned earlier philosophies; the new philosophy will have to recognize its debt to the past; the New Enlightenment should not ignore the greatness of the Old Enlightenment and the importance of the contribution to it of Sir Francis Bacon's doctrine of prejudice.

4. Who Discovered Boyle's Law?*

Preface

The present chapter illustrates a historiographic point and makes a historical point. The historiographic point is this: in the history of science, unlike other histories, error and redundancy tend to proliferate, perhaps due to the absence of a traditional requirement from writers to declare their interests, state their problems, express their viewpoints, and list the difficulties they leave not yet solved to their own satisfaction. (See [Agassi, (a)], especially final section.) My example here is an error that tallies very well with the 19th century climate of opinion, according to which Robert Boyle observed facts that his assistant Richard Townley generalized into the celebrated gas law. This error has been criticized by Gerland in 1909 and in 1913, but his criticism was ignored. Later writers have made extensive studies, culminating with those of Webster and Cohen. Yet the error was not corrected in all important texts on the matter. Partly this is excusable because the extensive documentation only confuses matters because authors so not openly declare their criteria, and different writers use different ones. The chief testimony concerning the attribution of the law to Townley is allegedly Boyle's original text. And as I shall show, this is a simple misreading of the text that perhaps lingers because it fits the views about science popular among its historians.

Let me say at once what the misreading is. Boyle claimed to have formulated and tested the gas law for pressures over one atmosphere, and attributes to Townley the extrapolation of the law to lower pressures. This does not make modern sense, but in the 1650's there were even special names for these two domains. Boyle had a model of atoms of air as springs, and a spring can be compressed and dilated; and Boyle's assumed that the zero point between dilation and compression obtains under the pressure of exactly one atmosphere.

Should we or should we not insist on a difference made once and abolished long since? This question can be generalized, and historians might be fascinated by the generalization. Their criteria, the criteria that they employ in their studies of secondary sources, and criteria that they employ in their studies of primary sources, may vary, and they often do. The present case study may serve as an illustration.

The final discussions of this essay revolve around two prominent essays, by C. Webster who claims priority for Townley and one by I. B. Cohen who claims priority for Hooke. Both throw different light on different *data*; proper comparison between these and diverse primary and secondary sources is impossible except by setting the various criteria and the obvious rules of translation from one set of criteria to another. Perhaps this justifies those historians who cling to the error and pretend that there is no criticism of it.

1. Standards of Credit and Acknowledgment

Boyle's law is very important in the history of science for various reasons, not the least of which is that it was published (in different versions) between 1660 to 1662, when the Royal Society came into being, and that events round it came to mold certain traditions of research practice; the most interesting factors, perhaps, were those of quick publication, of a detailed publication in the high inductive style, and of acknowledgment. All three of these factors are relevant to the story.

Francis Bacon is the inventor of the inductive style, advocated in many of his works, especially *Parasceve*.[1] Boyle's "Proëmial Essay" to his *Certain Physiological Essays* of 1661 is a historically important, detailed discussion of the essay form as best suited for the inductive style and for the requirement to publish fast. It heralded the invention of the scientific periodical. Excerpts from this essay were used as a preface to Peter Shaw's immensely popular 18th century edition of Boyle's *Philosophical Works*. Boyle was very successful; the inductive style of writing became the official style of the records of the meetings of the Royal Society and of the scientific writings that it sponsored. It is still today for many periodicals, from anthropology to zoology, obligatory for all of their contributors. Boyle's requirement to publish quickly was his added rationale for the inductive style. The works of Van Helmont, for example, were all published posthumously, but almost all of Boyle's scientific publications went to the printer as soon as the ink dried on them. Similarly, the works on the vacuum of Torricelli, Pascal, and Guericke — the most important ones prior to Boyle's — were published after Boyle's work on it ([Conant] 6n and 9.) Boyle did much to encourage his associates to publish, particularly Townley, Power and Lord Brouncker whom we will meet when discussing his text, as he mentions their works in his: he encouraged them to publish. For, one way of encouraging them (Hooke needed no encouragement, incidentally) was to do so in print, by reference to their intentions to publish, and by the aid of generous expressions of appreciation and, when possible, acknowledgment.

It is hard to speak of the accepted standards of crediting or acknowledgment of a discovery or an idea in the period preceding the year 1660, and for a variety of reasons. Mediaeval and high Renaissance works have often fancy acknowledgements to ancient authorities — and Copernicus still did. Some modern authors, conspicuously Descartes, preferred to make no acknowledgment, partly from excessive anti-authoritarianism, partly from reluctance to admit dependence on others ([Sabra] 100&n, 101&n, 102 and 115.) The very idea of crediting a person with a discovery as a token of gratitude, combined with a reward by posterity, was invented by Bacon, and presented especially forcefully at the end of his *New Atlantis*, where he made the novel suggestion that mechanical inventions should be acknowledged (just as much as, if not more than, philosophical ideas). Bacon even went so far as to suggest an institutionalized mode of crediting, a public acknowledgment by the national college or academy and by society at large by erecting statues to contributors to science.

Before the rise of the Royal Society of London standards were loose or non-existent. Most of the mediaeval inventions are anonymous. It is no accident that we do not quite know who invented the telescope, nor that it has been repeatedly alleged, ever since Bacon, that it was Galileo, or that Galileo plagiarized it. This shows that standards of acknowledgement were in the making. As Galileo explained, he himself did make some claim to the discovery: he said it was no small achievement to make a telescope on the basis of what he had heard. Nowadays, we do not need such arguments, since in order to establish priority for an experimental discovery or invention, this must be presented to the learned world or to the patent office — depending on the purpose at hand — and in a manner clear enough to render it repeatable. It was Boyle who instituted these priority rules, and as means of prompting the advancement of learning (in accord with Bacon's proposals).[2] There is more to this than meets the eye. The idea of quick publication involved the invention of the form of scientific essays and of scientific periodicals.[3] Boyle was the first to advocate this form in his already mentioned "Proëmial Essay", published in 1661 but written over a decade earlier. The suggestions it includes were soon incorporated into the rules of the Royal Society, proposed by its president Lord Brouncker, and seconded by Boyle.

This is the background to hosts of problems. We have legal standards of copyright and priority, there are standards of the community of scholars, and there is commonsense. The legal standards protect ownership and so forbid publication of others' ideas even with full acknowledgment; the tradition aims at incentives for quick publication and so it recognizes, not to say encourages, publications in others' names as establishing others' priority; commonsense recognizes such cases even with no regard to acknowledgment, since the law does not claim to make full restitution; tradition is vague and varied on such points; commonsense may then take over.

All this does not apply to the backward-looking Copernicus but it does apply to Galileo and Kepler. The latter complained (privately [Singer] 189) that that the former made no acknowledgment in his *Starry Messenger* to Bruno and to Kepler himself. His generosity did not prevent him from complaining, presumably because the indebtedness in question was to Bruno. But it was not a case of violation of accepted standards: accepted standards, inasmuch as one could at all discuss them, were hopeless. Galileo even made his own standards as he went along: he was engaged in a controversy about priority concerning the discovery of the sunspots and in the course of the debate he stated that priority must go to the one who adequately explained the discovered phenomenon, namely to Galileo himself. This is quite unacceptable, since many discoveries remain unexplained for generations on end; nor do we think Galileo himself understood the nature of sunspots. (Perhaps Galileo himself saw the weakness of his claim: later in life he offered a somewhat different idea — but only in a private letter.) Yet if we reject

Galileo's rule we must own that others had seen it before Galileo and his competitor; Kepler, for instance, who so misinterpreted one of them (as transit) that he ignored it.[4]

One last example: Lavoisier considered himself a co-discoverer of oxygen, especially since Priestley never thought of it as the cause of combustion but as merely a support for it. His acknowledgment was grudging, and under pressure; was he a plagiarist? I think not, yet I agree that this is an open and difficult question. It is perhaps unpleasant, but it is also interesting ([Kuhn] 7, 53-61, and 117).

An even more complex case could be that of Newton's possible indebtedness to Hooke for the conception of the inverse square law and its possible generation of Keplerian ellipses. Hooke asked Newton a question that he could not answer and that Newton could and did. Supposing the question was new to Newton; did he have to acknowledge it? Should we credit Hooke with having discovered the question? On this standards vary. And to whom do we credit a hypothesis, to its originator or to the one who has empirically confirmed it? We are still troubled by even the question. It receives different standard answers. Eighteenth century researchers, for instance, attributed the inverse square law of electricity to Coulomb; today a few historians attribute it to Franklin or to Priestley. By the same token von Laue's X-ray crystallography and de Broglie's material waves should be credited not to them but to the Braggs and to Davisson and Germer. If we agree with Newton, as I. B. Cohen suggests, that Hooke is the person who first verified Boyle's law, does Hooke thereby gain priority? That depends on the standard of accreditation: the eighteenth century would credit Hooke with the discovery because he verified it; by early twentieth century standards Boyle should be credited since he invented it.

At times the real difficulty is to know how to apply existing standards, be they good or questionable, while these are taking shape or gaining popular assent. We may know what the standards are, yet not know whether to apply them to this person or the other. Take the case of the Abbe Mariotte. Nobody accepts the claim that he discovered the law that in England is called after Boyle, and in France after Mariotte. Did Mariotte fail to acknowledge Boyle, or did he write as he did because he was not yet integrated in the tradition of the New Philosophy? In the 19th century, at least, the prevalent answer was that of P. G. Tait, who quoted Newton's sarcastic remark on Mariotte, and concluded ([b], 75) that Mariotte was a "paper scientist"; which, of course, is a euphemism for plagiarist. Later on, at least one historian of science, Gerland, agrees with Tait that in all probability Mariotte knew of Boyle's work, though he did not mention him. He says clearly ([b] 611),

> … it is hard to understand, how was it possible that Mariotte could remain unfamiliar with the writing of his predecessor …

yet he does not change his previous verdict [a], 351).

> It must indeed be probable, that the second discoverer knew of the work of the first … But it does not follow … that the French abbe must be considered a plagiarist. Likewise one has no right to brand Descartes a plagiarist because he took up in his writings Snell's law without naming its discoverer.

The same doubt was later raised by W. S. James who quotes Tait approvingly and ignores Gerland altogether. Later writers ignore the question, perhaps from not wanting to soil their hands in the dirty question of plagiarism.

Even when standards are established and well-known, problems and difficulties and muddles abound. In recent years this has become commonplace. At least, this is the view of Hellen Berman, who opens her review of Anne Sayre's *Rosalind Franklin and DNA* (*Science*, 170, No. 4125 1975, 665), with the following observation. "It is not really surprising or unusual that credits for some aspects of a discovery as significant as the structure of DNA are often muddled; that often happens in science." Think how more complicated matters looked in the earliest days of the appearance of the standards of crediting and of acknowledgment, and how hard it is to attempt a historical reconstruction of a complicated case!

If standards are difficult to handle, perhaps we should forget them and try to get the narrative straight. Take the two questions: did Newton hear the question from Hooke? If so, should Newton have made an acknowledgement to him? One may suggest that historians should be pleased to discuss the first question and leave the second unanswered. As it happens, Newton took care of the first question too: he said, that question too belonged to him, since he had thought about it before Hooke. No matter: Hooke still asked — even merely repeated — a question, and consequently Newton answered; and so perhaps, hurray for Hooke: perhaps we should credit him with that much! And perhaps not; perhaps Hooke rather pestered Newton and slowed down his progress. Historians of science cannot escape this question, and so they cannot escape the question of priority either. And those who consider the matter of the delay or acceleration of Newton's output too small should notice that he postponed publication of his *Opticks* till after Hooke's death, and in that book he made no acknowledgment to Hooke concerning the discovery of colors of thin plates, or perhaps to both Boyle and Hooke, quite in contrast to an acknowledgment to be found in a private letter from Newton to Hooke concerning that same discovery, now generally known as Newton rings (Sabra, 331 and 321-323]! How shall we judge Newton's action? We cannot call it plagiarism since he explicitly disclaims priority. But how shall we view his action? How did it appear to Newton's contemporaries and immediate successors? We do not even know the answer to this latter factual question, and (strangely or not) because we do not have an answer to the normative question. This is a general cause for frustration. Unfortunately, there are many questions of fact that, because of our present-day standards, we consider important and want to answer before we can set the record straight to our own satisfaction, yet we are frustrated in our inability to answer them, an inability rooted in the fact that they are not answered by the chroniclers of the period who gave them no weight: employing different standards, they skipped them altogether.

Sometimes, the situation is worse. The interests of modern writers may be absent in earlier ones as conflicting aims may interplay: the interests of historical figures and the interests of their intimates may, and often do, conflict with the curiosity of posterity. Sometimes authors reveal more than their intimates permit them to — as was the case with the autobiography of Mill or Darwin, and with Malcolm's life of Wittgenstein. But to take an example nearer home, the inductive style that Bacon advocated and Boyle implemented as the official style of science requires the clear presentation of experiments in chronological order in a manner permitting repetition by readers, without reporting any hypotheses, except — Boyle added — briefly at the end of the report, if one insists. Inductivism, in its original stringent Baconian version, forbids the employment of hypotheses; and so the inductive rules of writing, particularly of the reporting of hypotheses employed during research, seem to agree with the Baconian inductivist philosophy. Hooke, for example, is rather truthful, and confesses this fault. In his address to the Royal Society in the opening pages of his *Micrographia* of 1665, which the Society published at its expense, Hooke says,

> … I have … added … some conjectures of my own. And therefore … I must … beg your pardon. The rules you have prescribed … do seem the best … particularly that of avoiding dogmatizing, and the espousal of any hypothesis not sufficiently grounded and confirmed by experiments … In saying which, I may seem to condemn my own course in this treatise; in which there may perhaps be some expressions, which may seem more positive than your prescriptions will permit: And though I desire to have them understood only as conjectures and queries (which your method does not altogether disallow) yet if even in those I have exceeded, 'tis fit that I should declare that it was not done by your directions.

I hope readers notice how tortuous and tortured this passage is. If from the start recording was as problematic as the above passage indicates, we may well quote cautiously every record of any hypothesis. Otherwise we may even misunderstand the "Queries" at the end of Newton's *Opticks*, and elsewhere, to be queries rather than hypotheses as in Hooke's above quoted remarks. Newton's work belongs to a later period. The above quoted remarks were published in 1665. The story of Boyle's law begins not earlier than 1660, and not later than 1665 — the book just quoted is the latest published primary source on the topic. The part of it concerning Boyle's law is quite complicated and unusually tortured even for Hooke. That fact is not very surprising for such a period of transition.

2. The Gas Law

The gas law is known by various names, as Boyle's, Mariotte's, Gay-Lussac's, or Charles', or sometimes by two or even three or all of these names. It correlates the volume, pressure and absolute temperature of a gram-molecule of any given gas, where a gram-molecule is a certain number of molecules, the number being called after Avogadro or Loschmidt (but hardly ever after both). Perhaps one should say at once that the law and the number so named are named not after their discoverers, but in honor of some students of the field. The formula,

$$PV = RT$$

Says, the product of the pressure P and the volume V of a given gram molecule of any gas is proportional to the absolute temperature T of that gas, with the factor of proportionality being a universal constant. The universal constant is a product

$$R = N\,k$$

of the Avogadro or Loschmidt number and another universal constant called Boltzmann's constant; Planck complained in his *Scientific Autobiography* about this name, claiming to be the person to have introduced that constant (in 1899). With such incongruity about naming, one need not wonder that some historians ignore and others fuss about the fact that on the Continent of Europe Boyle's contribution is called after Mariotte.

The most famous up-to-date variant of the gas law is due to Van der Waals, and seems to fare best, but is not sufficiently unproblematic to be declared the last word by any physicist, least of all by Van der Waals himself. Now all contenders to the status of the gas law are improvements on the most famous version, itself distinctly unsatisfactory but the point of departure nonetheless, *the* gas law, or the *ideal* gas law. The law is applicable only to ideal gases, and there are no ideal gases, strictly speaking; under normal conditions, though, a few gases are fairly nearly ideal: including, say, common air and hydrogen. Let us then assume that the gas law

$$p\,V = R\,T = k\,NT \qquad (1)$$

applies to a gram-molecule, or to 24.4 liters under normal pressure and temperature, of any gas. Now, for our historical study we need not go into so much refinement; we may replace it by its corollary,

$$\text{if } T = \text{Constant, then } P\,V = \text{Constant,} \qquad (2)$$

since prior to Amontons' study of 1699 there were no studies of the variations of temperature beyond what is fairly common knowledge anyway. Moreover, we may restrict (2) to air alone, since, prior to the mid-eighteenth century, there were no other known gases. One may feel that this is a sufficient simplification, but we still have to abolish explicitly the idea of a gram-molecule. Let us say, instead of gram-molecule, any given quantity of air.

This change is an over-simplification: it leads to the loss of some important information, well-known to Boyle himself.

The information thus omitted is rather subtle, and not at all easy to capture, since it is both intuitive and hard to formulate. The more air, we known, the more P·V; for example, two equal quantities of air will normally have the same P·V, and put together should yield 2·P·V. How do you measure two equal quantities of air? Formula (1) says, in effect, they have the same number of molecules. How do we count these? We take a quantity such that for it P·V / T equals R and we know it has N molecules. This is, two volumes with equal P·V / T have equally many molecules. At constant T, two volumes with the same P·V have equally many molecules. At constant T and P, V is proportional to the number of molecules.

All this follows from (1), not from (2). Hence the latter is too weak. To remedy this, we must use another corollary of (1) in addition to (2), which is

> Under constant temperatures and pressures equal volumes of air have equal weights. (3)

It is easy to combine laws (2) and (3) into one. All one has to remember is the ancient (not under) formula which defines density:

density = weight : volume

and we can see that (2) plus (3) is equivalent to the following:

> for any volume of air,
> under fixed temperature,
> pressure is proportional to density (4)

Formula (4) is the one used by Boyle. The following is an interesting empirical observation: historians of science faced with the story of Boyle's discovery that refers to (4) and to (4) alone reject it as faulty. The historian who reports Boyle's presentation of his law as (4) is Webster. Remarkably, he says ([b] 486),

> this particular expression of the law is rarely mentioned by historians of science.

(Boyle speaks of "spring" rather than of pressure; we shall come to that later on.) Now Webster does not say why historians usually avoid formula (4), nor what its merit is. I have explained its merit as compared with (1) and (2). The reason why (4) is seldom stated is quite obvious: when our discussion is meant to be accurate we use (1), and when it is inaccurate we use (2) and unawares assume (3) and even employ it in calculations — check any high-school text with exercises employing Boyle's law. Hence, there is no need to state (4) in preference to (2), and, though (4) is more informative, (2) looks more akin to (1) — quite misleadingly.

The two simplest ways of changing pressure and volume are these: first, using the same quantity of air, one may compress and expand it; second, using the same fixed volume, one may pump air in and out of it. In the first case it is easier to use (2), in the second case (4). Wishing to illustrate (2) we can change the size of a container — say in a tube with a movable piston, or with a tube sliding atop another tube, as in a trombone — and measure both the volume and the pressure in each of these cases. If, however, we wish to illustrate (4) in the "same" manner, we must measure both pressure and density. There are formulae for measuring density, but the earliest is nineteenth century, and they are all operationally quite involved. And so, even nowadays when we speak of high vacuum, we do not speak of very low densities but rather of very low pressures: we employ (4) both for practical purposes and as means of testing other formulas, but (4) was never illustrated in the same "straightforward" manner in which (2) was.

Historically, the case was this: it was easier to use (2) for examining Boyle's law for pressures of more than one atmosphere and (4) for pressures lower than one atmosphere. For high pressures one can use J type tubes, with air captured in the closed shorter leg: when more mercury is poured into the long leg, the more compressed air in the shorter leg gets. To use (2) and perform the same experiment for low pressures, one may use an expandable chamber. like a trombone valve (Hooke). Alternatively, one may use (4), and simply repeat Torricelli's experiment a few times, each time with the tube initially filled with different quantities of mercury before being turned upside-down and dipped into a mercury dish (Mariotte). The correlation of the mercury column heights before and after the turnings of the column upside-down is via (4).

This, too, is of historical interest. Ernst Mach ends his report on the historical development ([Mach, (a)] end of Chapter 1), saying first that the second method — of using a Torricelli tube — is Mariotte's; and second, that today (1893) both high and low pressure experiments are performed with the aid of two glass tubes, each closed and with a stopper, connected to each other by a rubber tube, so that by lowering and raising each relative to the other, all desirable *data* are easily obtainable. This is impressive, as this way Mach expresses appreciation both for past difficulties and for the much derided Mariotte. Somehow this has escaped notice. I suggest that had Mach presented the problems he was solving and had he criticized other historians, his achievement could have had a better chance of meeting the appreciation it deserves. In brief, the history of science may benefit from explicit criticism.

3. Who Designed Boyle's Vacuum Pump?

One result from the previous discussion is that to test Boyle's law we have no use for vacuum-pump (an air pump). And yet, though the logical genesis of Boyle's law can dispense with the pump, both the psychological and historical genesis of the law are both closely linked to it. His studies of the elasticity of the air began with the construction of a vacuum pump, that indeed much of his work centers round vacuum pumps. Thus, the vacuum pump has won an honorable mention in the title of his best known book, his *New Experiment Physico-Mechanical, Touching the Spring of the Air, and its*

Effects, (Made, for the most part, in a New Pneumatic Engine) Written by way of a Letter to etc. As the parenthetic clause indicates, the vacuum pump plays a distinct role in the book. It was a splendid toy and was used for all it was worth by the Royal Society, to whom Boyle made a present of it. The first edition itself, of the year 1660, variably known as *New Experiments*, or as *Spring of the Air*, or even as *Spring and Weight of the Air* (from the title of the third edition), is of much less historical importance than the second edition, in which Boyle's law first appeared. This is the 1662 edition, subtitled *The Second Edition Whereunto is added a Defence of the Author's Explication of the Experiments, Against the Objections of Franciscus Linus, and, Thomas Hobbes*, *etc*. the added part or appendix is usually referred to as the appendix, or the *Defence*; at times it is called *Defence of a Doctrine* (after the title of a separate edit ion of the added part). In 1669 there is *A Continuation of New Experiments, Physico-Mechanical, Touching the Spring and Weight of the Air, etc*., known as the first *Continuation*; the second *Continuation* is of 1680; here, too, a pump — a new one each time — plays a significant role, but these works themselves are not significant.

Though it is well-known that Boyle's pumps have nothing "direct" to do with his law, historians of science usually prefer to speak of the importance of air pumps in general than to point out the facts of the matter. But this in itself does not mean that this way they compliment Boyle: the pump is usually attributed to Hooke.

Boyle had bad luck among historians of science.[5] The nineteenth century almost entirely ignored him. In the twentieth century, though his *Sceptical Chymist* was republished by Everyman in 1911, he was largely ignored by historians of science who hardly noted him except in connection with his law. Nowadays he holds an increasingly important position, and his name came back mainly due to the influence of J. F. Fulton, who published an extensive and impressive bibliography of his works, first in the *Proceedings of the Oxford Bibliographical Society,* 1932-33, and then, a second edition, in 1961. Another influence was that of E. A. Burtt, who gave him an unusually prominent position in his lovely *The Metaphysical Foundations of Modern Physical Science*, 1925. But until the mid-twentieth-century Burtt himself had almost no noticeable influence, and as far as the present study is concerned, this is still regrettably true. All this may be said of R. F. Jones's study as well. So, for the present purpose, it was J. F. Fulton who has put Boyle on the map. And following his lead, later authorities, such as Conant, Webster, and Cohen, accept from him that not Boyle but Hooke designed the first two vacuum pumps. Though to begin with his only source of information is Boyle, Fulton said both that Boyle had made no acknowledgment to Hooke, and yet that Hooke had designed the pump. Later, Fulton said Boyle's omission is quite understandable as it was in those early days when Hooke was more of a mechanic than a research assistant. Let me quote Fulton (11):

> … Boyle himself probably did not design or improve any of the three air pumps which he describes in the first edition of *The Spring and Weight of the Air* and in the successive 'Continuations' …

To continue from Fulton's first edition (1932, 20),

> The first pump was designed by Hooke, although there is no acknowledgment to him in the first edition.

This is corrected in Fulton's second edition (11):

> The first pump was designed by Hooke, who had been taken on by Boyle as a paid assistant about 1655. Regarding him at first as a skilled mechanic, Boyle made only passing reference to him (6-7) in the first edition.

Fulton went on quoting Boyle's claim of 1669 that Hooke and he himself made essential improvements to the design of the second engine. Fulton went on in the first edition (20),

> There is no doubt that he and Hooke were influenced by … Guericke … Schott having published a description …

and in the second edition (12) Fulton clarified,

> There is no doubt that Boyle and Hooke in designing their instrument were influenced by …

All this is not too complimentary to Boyle: the first engine is allegedly thanks to Hooke, yet Boyle makes no or little acknowledgment to him; the second engine allegedly has acknowledgment — rather grudgingly — to Hooke, but not to Guericke and not to Schott. Fulton was familiar with Boyle's works — more than any other modern writer — yet he seems to have overlooked the relevant passages in Boyle's works. How ill founded are his allegations is quite clear from the following quotations from Boyle. In his *Spring of the Air* Boyle clearly says (Boyle, 1, 159).[6]

> … I put both Mr. G. and R. Hook … to contrive some air pump … And after an unsuccessful trial or two … the last named person fitted me with a pump.

This is a clear acknowledgment. Moreover, the paid mechanic, "Mr. G." (Ralf Gratorix; see [Jacob]), is not given the same acknowledgment as "R. Hook". Also, for the record, the paragraph preceding the acknowledgment to "Mr. G. and R. Hook" contains an explicit acknowledgment to Guericke and Schott. Nevertheless, C. Webster's most complete and latest study of the story as a whole ([Webster, (b)]), overlooks this, perhaps following Fulton's error, and says (464),

> It is not certain how Boyle was introduced to Guericke's apparatus … it is probable that he was told about it by one of his correspondents, who might have seen the experiment or

> read the account of the pump in … Schott's *Mechanica hydraulico-pneumatica* which was published in 1657.

Webster is too careful to assume that Boyle read Schott, but carelessly assumes that Boyle received an important letter that somehow escaped Birch's publication of Boyle's correspondence. True or not, it is irrelevant. Boyle made an acknowledgment to Schott and even explained his debt very disarmingly in a period (see above quote from Gerland) when acknowledgment was not quite required:

> I think myself obliged to acknowledge the assistance and encouragement the report [by Schott] of his [Guericke's] performances hath afforded me.

This explanation is somewhat of an exaggeration: Boyle had worked with a vacuum and intended to build a pump even before he had heard of Schott. No matter. Also, there are two "imperfections" in the Guericke pump, one "was in good measure, though not perfectly remedied" by Hooke; "and to supply the second defect it was considered …" — one might suspect that Boyle lapses into an indirect mode of speech because he wishes to avoid acknowledging too much to others. The continuation of that sentence makes it clear that the embarrassment lies in the opposite direction; the innovation stems from another of Boyle's experiments: "because I remembered, that having several years before often made the experiment *de vacuo* with my own hands". The innovation is not important: it is of a hole in the vacuum chamber, with a sleeve and a glove in it, enabling experimenters to use a hand to move things in the vacuum chamber. The point merely is that Boyle uses an impersonal tone rather to avoid self-credit than to avoid credit to others, and that he credited Hooke with as much as he could.

What made Fulton so inaccurate and so ungenerous to Boyle? Perhaps Boyle's cumbersome prolix slow style may be at fault here. For example, Boyle's sentence partly quoted above, is made up of over twelve long lines, and exactly 186 words. But this cannot be the whole story. Fulton quoted Boyle's opening to his own description of the second engine (first *Continuation*). In it Boyle said clearly of the improvement in the design of the second engine that they were partly "suggested by others (especially the ingenious Mr. Hook)" and partly "I added myself, as finding that without them I could not do my work". Clearly, Boyle spoke as the man in charge of the construction of the second engine. He explicitly claimed that Hooke had designed the first engine to which he, Boyle, had contributed one improvement. He implicitly claimed that he himself had designed the second engine while Hooke and others suggested improvements to his own design. After having claimed that the first engine is Hooke's, Fulton listed (12) Boyle's engines thus:

> *The first English air pump* constructed in 1658-9 by Hooke and Boyle … *The Second English air pump* also constructed by Boyle and Hooke … The third air pump used in England

> … Denis Papin had designed … and … brought it with him from France …

It is practically out of the question that Boyle did with his own hands any of the construction (meaning by ‘construction’ the manual labor itself as distinct from ‘design’ that is not manual labor and from ‘improvement’ that is ambiguous). It does not matter who performed the manual labor of construction, whether the two whom Boyle mentions, “Mr. G. and R. Hook”, or people under the supervision of Hooke or of Gratorix or both. The variation between “constructed” in Fulton’s mention of the first two engines, and “designed” in his mention of the third engine is no more than an elegant variation.

Another work of Fulton — a brief life of Boyle — discusses in which all this insignificant affair. It was published in 1960, long after the first edition (1932) of his bibliography, and just before the second edition (1961). He mentioned there the brief life of 1960. Also Fulton makes there use of a private note[7] by Hooke on the pump; says Hooke,

> … in 1658 or 9, I contrived and perfected the air pump for Mr. Boyle, having first seen [one] … which was too gross …

The quote seems characteristic of Hooke in an uneasy match between the love to stake a rich claim and the love of truth (which won: the note is private, not published). Fulton says, Hooke completed the pump in 1658, and starts the above quote with “I contrived”, thus skipping Hooke’s somewhat different dating. This is sad. But to end this rather pointless story on a happy note, in the same life of Boyle ([Fulton, (c)] 124) Fulton says,

> Boyle gives [Hooke] full and generous credit for devising the air pump in *New Experiment* (6-7).

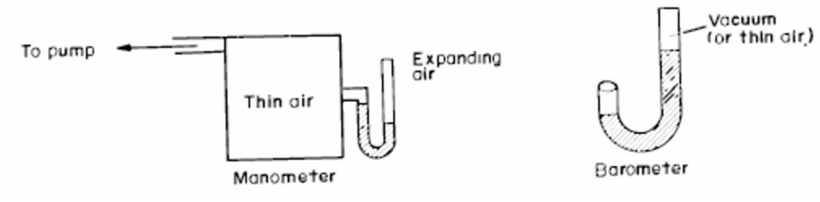
Will it be too much to expect from a historian of science, at least from one of Fulton’s stature and standing, to correct an error explicitly? Why does he say in one place (6-7) that there is only “passing reference to” Hooke, and in another that on the very same pages “Boyle gives him full and generous credit”? Could he not say that he had erred? Did he want the error to be corrected tacitly? It would be a pleasure to oblige so great and gentle a scholar as Fulton; but tacit corrections do not work so well as explicit ones. Webster, for example, now speaks of “Hooke’s pump” as a matter of course ([Webster, (b)] 454), even though he mentions in his references (*loc. cit.*, note 83) not only [Fulton, (b)] but also the correct [Turner]. And it is time to restore to Boyle some of the honor he deserves.

4. The Importance of the Vacuum Pump

The air pump is a means to create vacuum chambers. Its only significance as compared with Guericke’s pump is technical: Guericke pumped out water, whereas Boyle pumped out air. Torricelli, indeed, created vacuum chambers with no pumping at all. Take a glass bottle as large as you please, with a neck over thirty inches long, fill it with mercury (or even, with some dexterity, mercury and water) and turn it upside-down: you have created in the bottle a Torricelli vacuum. Still, this was done by a group of followers of Torricelli in the Florentine *Academia del Cimento* (academy of experiments). They did not publish, but, as Conant says ([Conant] 6n), Boyle "must have heard of them by word of mouth or by letter". We remember from his report of his first air pump, that he had made with his own hands a few years earlier a sleeve to work within the Torricelli vacuum. So, certain vacuum experiments were performed without pumping.

Ernst Mach says that most of Boyle's experiments with the vacuum pump were variants on Guericke's. He notes that Guericke was a believer in *horror vacui*, but adds that he believed in the weight of air, as well as in the variability of its density. He mentions Guericke's experiments about fish blowing up in a vacuum, about sealed barrels that hiss when opened on a mountain, etc.

In brief, though air pumps are not essential to the illustrations of the elasticity and weight of air, they enhanced them directly and indirectly, by raising curiosity, etc. They were useful in other fields, especially in Boyle's demonstration of the increased approximation to Galileo's law in vacuum (where a feather and a marble fall almost together as the vacuum tube is turned upside down) and of Harvey's theory of the heart as a pump. We need not discuss all this. Nowadays, it is hard to notice much difference between a manometer (Greek "manos", thin) and a barometer (Greek "baros", weight), and for two reasons. First, soon after the publication of Boyle's studies the



Manometer Barometer

difference between compressing air and thinning or elating or expanding it, was omitted as irrelevant; but prior to Boyle's researches and until 1665, this was not so clear: perhaps Hooke's contribution was just on this matter. (See below.) Second, barometric measurements depend on two variables: height of the location of the measurement as compared to sea-level, and atmospheric fluctuations. It will be easily understood that for sorting out these two we need a standard of air-pressure — one atmosphere or 76 millimeters of mercury is as good as any — and for the operation of that standard manometry is necessary.

Barometric experiments were performed prior to Boyle's studies to determine mountain-heights. Pascal sent his brother-in-law to climb a mountain with a barometer. Later he discovered barometric fluctuations due to changes in weather conditions. Power and Townley did similar work in England (which will engage our attention a few times below). And so we can raise the question as to their value right away. Were they mere repetitions of Pascal's experiment? If not, what was their value? What problem did they come to solve?

That air pressure was higher at sea level than on a mountain is a result of Torricelli's work and simple hydrostatics. Since hydrostatics was not as obvious in the 1650's as in the 1670's (see for example Boyle's *Hydrostatic Paradoxes*), one might want a simple qualitative proof. Both Pascal and Guericke had them (though Guericke published much later): Pascal showed that a partly inflated ball fully inflates on a mountaintop; Guericke showed that a barrel hermetically sealed on the plain and opened on a mountain will hiss when opened. But Pascal's brother-in-law took quantitative measurements. They were too inaccurate, since they conflated height from sea-level and variations of weather conditions. Power and Townley could have been trying for a higher accuracy; if so, then they failed, and so, the value of their observations is doubtful: when Webster reproduced Power's table (see below) he claims he had to correct some misprints. He says Boyle's law could be read from these tables — which means that he could ignore atmospheric variations. This is an error as Power could not know this. We can add that Boyle conceived of a third factor that may affect the barometer in addition to variations of height and of the weather: he conceived of atmospheric tides akin to ocean tides (26-28). Could Power ignore this factor too? Barometric and manometric experiments were the chief instruments that Boyle used on his way to the discovery of Boyle's law. After all, for Boyle's law the simplest instrument is not at all a pump; and yet such instruments were developed only after the vacuum chamber was used quite extensively. In retrospect we may miss the point: the simple experiments with the barometer were open to the many sorts of fluctuations and led research to different directions. When sufficiently high vacuum was created by the use of the pump, and also high pressures, the scope of the experiments was drastically altered that permitted to ignore many fluctuations. We shall later see Boyle taking regular advantage of all this.

To conclude, the role of the pump is no more than that of an intermediary or a means of creating a lot of variations on some rather simple experiments, and thus as means for determining the upper and lower bound of level of accuracy required in the experiments to insure both stability and repeatability of results. We shall later see that developing the simplest experimental tool was by no means an easy task, when stability and repeatability became the prerequisite. This marks Boyle's experiments as compared with those of his immediate predecessors.

5. The Place of Boyle in History

Robert Boyle was a very important public personality: a plaque on his gate said that on doctor's orders he could not see visitors on Mondays and

Wednesday mornings. He was already very important when, as a teenager, he returned from his European tour and joined the group of scholars that he later privately christened "The Invisible College" and that, as the consensus goes, was the nucleus of the Royal Society of London.[8] The Society was founded in 1660, soon after Boyle published two famous works, his *Seraphick Love* and his *New Experiment Physico-Mechanical, Touching the Spring of the Air*. The title page of *The History of the Royal Society* of 1666 by Bishop Thomas Spratt contains pictures of Francis Bacon and of John Evelyn. Evelyn read *Seraphick Love* with tears in his eyes and wrote a letter to Boyle to say, we must do something, and then Boyle suggested that a meeting be called and so things started rolling. The rules of the Society were introduced by the first president, Lord Brouncker, and seconded by Boyle. They were expounded in the Proëmial Essay to *Certain Physiological Essays*, written a few years earlier. Once he published his *Seraphick Love*, his *Spring of the Air*, and his *Certain Physiological Essays*, he became, as contemporary documents amply testify, the most important intellectual in Europe.

Boyle was held in the greatest esteem during his lifetime and at least until the revolution in chemistry. Dr. Johnson's essay on him (*The Rambler*, No. 106, March 23, 1751) still reflects a very high respect for him. The first somewhat unkind remark about him seems to be Hume's (*History of England*, Appendix to Reign of King Charles II). On the whole, Hume quite admired him. The real collapse of his reputation I attribute to the deep and widespread influence of Sir John Herschel's shallow *Preliminary Discourse to the Study of Natural Philosophy* of 1831.[9] Herschel had a low view of Boyle for two different reasons, that Boyle's philosophy was mistaken, and that he failed to discover Newton's theory of gravity.

In his well-known and once very influential brief *The Excellence of the Mechanical Hypothesis*, Boyle advocated the Cartesian or the mechanical philosophy, according to which the ultimate cause, or the essence, of all changes is collision, or impact, or push, of one kind or another. Newton, who introduced or reintroduced forces into physics, hesitated to view forces as (ultimate) causes — at least until he wrote his third letter to Bentley. Newton's pupil Roger Cotes argued in his famous preface to the second edition of Newton's *Principia* that forces are essential causes. The philosophical world was on the whole very much in two minds about all this during the whole of the eighteenth century, with great exceptions like Boscovitch and Kant who decidedly followed Cotes, and with some more exceptions like Euler, who decidedly clung to Descartes. The majority followed Newton in their vacillation between the Cartesian and the Cotesian views of his theory. A famous example is Franklin who spoke about his own electric forces using the same words with which Newton had spoken of gravitational forces. Another example is the arch-Newtonian Laplace who partly followed Cotes, partly tried to explain Newtonian gravitational forces mechanically, i.e. as results of collisions (by his famous theory of *gravifique*). With the failure of such efforts and with the success of Newtonianism in the fields of electricity, magnetism, elasticity, and chemistry, and perhaps also with the spread of Kant's, Boscovitch's and Lavoisier's theories of matter, Cartesianism or mechanism became unpopular in the nineteenth century, and with it most of Boyle's theoretical writings. Boyle made another serious error of speaking about the particles of fire that he referred to as 'igneous particles'. The question of their affinity to, or identity with, phlogiston, is still under dispute. Consequently, in the anti-phlogiston period Boyle's chemical works lost most of their popularity (as Fulton's bibliography shows).

All this explains why Herschel was disposed to speak ill of Boyle whereas his immediate predecessors spoke well of him. Herschel's attack stems from a quite different position. In the last part of his *System of the World* Laplace states that Newton was not only the greatest but also the most fortunate man in having been born in a time ripe for the greatest generalization. That time, we know, saw also Boyle and Hooke, and Herschel seems to have wondered why Newton and not these other two men of genius exploited this unique opportunity: he explained this — by claiming (115) that Boyle was concerned with so great a multitude of experiments that he had no time for theorizing — being under the bad influence of "remnants of alchemy and natural magic" — while Hooke was too busy with the microscope (no bad influence mentioned here). This is the first appearance of the assertion that Boyle was concerned with facts, not with theory.

In 1809 von Lindenau published his *Tables Barometriques* where he attributes Boyle's law to Townley (p. xx). This was quoted by Gehler in 1829, (283), and perhaps by others. In itself this signifies very little. The error gained significance when it was picked up and interpreted by the famous F. A. Lange. In his classical *History of Materialism*, first published in 1865, he discussed Boyle's metaphysics, perhaps for the first time after a long pause. He noticed the similarity between Boyle's and Newton's metaphysics, but he left the question of a possible influence entirely open (i, 300, 303) despite Newton's famous expression of reverence for him. In spite of his high view of Boyle, Lange's exposition makes him appear as an eclectic Cartesian-cum-Gassendian, who was in addition a strict Baconian. To illustrate Boyle's Baconianism Lange claims in a footnote (302) that Boyle missed the chance of generalizing the *data* he obtained into what is now known as Boyle's law, and left this task to his assistant Richard Townley as an example of his interest in facts but not in theories. That was an ominous footnote.

After Lange's remark, Boyle's reputation was gone; it would be unimportant had the mistake in it not been so typically inductivist (the mistake that science begins with observation). It was restated by the famous German historian of physics, F. Rosenberger who described the story in some detail and more-or-less endorsed Lange's verdict (135):

> The great experimenter Boyle thought to little of drawing conclusions from his observations that he left the discovery of the generalization known as 'Boyle's law' to one of his helpers.

This is Rosenberger's version of the story (138), with my italics:

> In order to convince [his opponent] Linus of the resistance capacity of the air, Boyle used a J shaped glass phial, the shorter leg of which he sealed. When, after this, he poured mercury through the long leg into the phial it pressed the air that occupied the shorter leg in proportion to the amount of mercury that had been poured into it. But the air always managed to hold balance with the larger mercury columns, while contracting respectively. *Subsequently*, Boyle worked out tables for the different amounts of increase of pressure in the long leg and the respective volumes of air in the short leg. But *he did not draw from this any further conclusions about the relations between the magnitudes*. Only after one of his pupils, Richard Townley, *noticed that according to those tables the volumes of the air were inversely proportional to the pressure*, did Boyle take up this law and prove further, that the law holds also for pressures which are *smaller than the pressure of the atmosphere …*

Obviously, Rosenberger read Boyle: many details in the above quotation indicate this. Yet the whole story is a fabrication that runs contrary to explicit statements in the original. In particular, Boyle ascribes to Townley not the first set of experiments on high pressure but the second set of experiments on low pressure.

What of it? Is this perhaps a small error that can easily be patched up? Perhaps. August Heller also noticed that somehow there is the high pressure experiment and the low pressure experiment. He, too, got it wrong, though differently. He says (171),

> Boyle stated the theorem such that air gets denser with the compressing force. His pupil Richard Townely noticed that the height of the mercury in the manometer is the inverse of the volume of the air. Boyle now made experiments both with air made denser and with air made thinner; he found that, indeed, the elasticity stands in the opposite relation to the corresponding volumes of the air.

Heller is perhaps careless: I cannot find in Boyle's work the experiment with the manometer that Heller mentions as described by Boyle. But perhaps I am using the word "manometer" too strictly. No matter. According to Heller, Boyle made the experiments, but only "now", to wit, after Townley pronounced his theory on the basis of facts, and in order to test it. Whereas according to Rosenberger induction is a generalization from tables of *data*, according to Heller it is a generalization from less precise *data*. Each could reconstruct history from his theory of induction, the text being perused only to provide some details, not to check the allegations.

The text may serve in order to refute these two theories of scientific method in action. Before doing so let me observe that Heller clearly asserts that Boyle's law was not formulated by Boyle: in the same volume, in a paragraph devoted to Townley (320), Heller explicitly identifies him as the one who "formulated Boyle's law of the inverse proportionality between pressure and volume of a gas".

Leaving now what other writers (e.g. Poggendorff, 479) have said on the matter, we can close this story with the important contribution of Gerland who, in (a) 1909 and (b) 1913, declared the whole attribution to Townley to rest on the misreading of Boyle. I quote Gerland in full, since he was strangely misquoted in recent years, in oversight of the information in the title of his 1909 paper, in the expression "Boyle (not Townley)". The chief concern of Gerland in 1909 was to refute the attribution of the law to Mariotte; on this he says,

> Since one is finally convinced that the law was expressed by Boyle already in 1662 it is not called Mariotte's law but Boyle's law unless one considers it preferable, for safety's sake, to call it Boyle-Mariotte law. However, this labeling confers equal rights on both discoverers. Yet these do not exist since Boyle beyond doubt discovered it fourteen years earlier than Mariotte and it is not the custom in the history of science to let a discovery be considered new when it has already been communicated by someone else and in such a conspicuous place as one must consider Boyle's works. One must assume as very probable that the second discoverer knew the work of the first.
>
> … It does not follow from this that he did not know Boyle's writings as also it does not follow that we must consider the French Abbe a Plagiarist. …

Is the law, then, due to neither Mariotte, nor Boyle, but Townley? If so, should we label it Townley's law? Gerland raises this question and rejects this suggestion, claiming that all we know of the case

> justifies in no way the presupposition that Townley was the one who gave the law its formulation and thus it carries the name of Boyle with full justice.

And yet Gerland himself notices something puzzling here.

> It is interesting that [Newton] copied out Hooke's table of experimental results relating to "Mr. Townley's hypothesis" though he could have obtained similar data on Boyle's law from Boyle's own second edition of the *Physico-Mechanical Experiments* (1662). This may explain the otherwise odd fact that years later in the semi-popular *De Systemate Mundi* he referred to the relations between the pressure and volume of air as having been "proved by the experiments of Hooke and others".

Nobody has ever taken up this matter or even noticed it.[10] Perhaps at the time Gerland's puzzlement led his contemporaries to disregard his view. But let me first quote from Gerland's history ([b] 501), to clinch matters against the misquotation, in order to show that in spite of his puzzlement, Gerland was convinced it was Boyle and not Townley who had made the discovery.

> But, that the hypothesis which surmises the experimental results of Boyle could originate with Townley, is unthinkable, since Boyle expresses it [the hypothesis] at least with regards to the experiments with compressed air before he mentions Townley at all; after having done this, and speaking now only of experiment in rarefaction of air and producing its results, he emphasizes that these [experiments] were not performed by Townley but by himself. Thus, the only merit which one might attribute to Townley is that he pointed out to Boyle that the law that he [Boyle] had found for compression of air applies also to rarefaction of air; the law itself, also concerning the rarefaction experiments, belongs to Boyle.

This is Gerland's satisfactory solution to the mystery. In Boyle's works Boyle's hypothesis is the name of the law of compression, i.e. for pressures above one atmosphere. And Newton said that Townley's hypothesis was verified by Hooke, viewing Boyle's verification insufficient. This solves the problem posed in the previous quotation from Gerland. Somehow he failed to solve it, and the confusion was not cleared up but further amplified. Let me, then, quote Boyle: in spite of the discovery of additional historical records, published and unpublished, the chief record still is Boyle's own work of 1662 that all his students except Gerland still misread.

6. Boyle's report on his discovery

In the beginning of *Spring of the Air* of 1660 Boyle asserts (Experiment I) that (under constant temperatures)[11] the density of air varies *monotonously* with the pressure exerted on it. Boyle's law is a more precise, quantitative version of this hypothesis, namely that the density of air varies *proportionally* with the pressure exerted on it. The qualitative law is at the basis of the whole of this work (see especially Experiment XVII).

Boyle's law appeared two years later, in his *Defence of the Doctrine Touching the Spring and Weight of the Air ... Against the Objections of Franciscus Linus*. This work is written in a historical manner, something very unusual before Faraday's time. I shall now present a summary of it, preserving the order, adding comments and quoting some relevant passages.

Linus had agreed with Boyle that air was elastic, while vehemently denying that it was as highly elastic as Boyle supposed. Boyle showed that all of Linus's objections are answerable, that Boyle's hypothesis did explain all the facts Linus had referred to. Nonetheless, some comparative assertions regarding the degree of elasticity of air that Linus had made (though only by the way and inconsistently with other of his assertions) started Boyle on a train of thought that led to Boyle's law. Regrettably, it is tradition still refuses to recognize a contribution to the advancement of learning to an Aristotelian like Linus who made an invalid criticism. Boyle, however, explicitly admitted that Linus's objection had led him in stages to search for a quantitative hypothesis.[12]

In spite of his rebuttal of Linus's objections, and in spite of the severity of his own objections to Linus's view, Boyle still feels uneasy: Linus's hypothesis may still be true, unless experiments "render it improbable". To this end he suggests a series of experiments. The first, he says, is even a crucial experiment: it is Pascal's observation that the higher the Torricelli tube is stationed, the lower the mercury column in it. But Linus has questioned the truth of Pascal's report, although it was confirmed, Boyle says (Boyle, 3, 51-2):

> I can confirm these observations of Pascal, by two more, made on distant hills in England: the one of which I procured from that known Virtuoso Mr. J. Ball, whom I desired to make experiments … in Devonshire … ; and the other made in Lancashire by that ingenious gentleman Mr. Rich Townley.

These people were his assistants.[13] Boyle the teacher, it may be noted, encouraged his assistants from the beginning to become independent researchers: he makes a magnanimous acknowledgment to Hooke and he praises Ball and Townley. To return, there are more crucial experiments than Pascal's.

> To all this I shall add two things, that will very much confirm our hypothesis [the *qualitative* law]. The one is, that the freshly named Mr. Townley, and diverse ingenious persons that assisted at the trial, bethought themselves of so making the Torricellian experiment at the top the hill, as to leave a determinate quantity of air in the tube, before the mouth of it was opened under the vesseled mercury; and taking notice how low such a quantity of the air depressed the mercurial cylinder, they likewise observed, that at the mountain's foot the included air was not able to depress the quicksilver so much …

Townley's experiment was clever — Boyle says it was too clever to yield results without simplification. Whereas Roberval added air to Torricelli's vacuum, and Pascal took it up to a high mountain, Townley did both. Neither he nor Boyle could work out the results, but Boyle saw in the experiment a possibility for designing the sought-after crucial experiment between himself and Linus. The pace begins to quicken.

The detailed report of the experiment by "Townley, and diverse ingenious persons" that Boyle views as of crucial importance in the development of his own work is reported by Henry Power in 1663. (It is cited in [Webster, (b)] 473-4.) The report is in first person plural — an intriguing fact that deserves notice (and is used by Webster and Cohen as evidence that Boyle was a bit confused on these matters). Power's report says, the experiment took place in spring 1661; Webster reports (481) that Boyle had access to Power's manuscript in summer 1661 and repeatedly promised to make acknowledgment to Power. Why did Power refrain from publication? Certainly, if Power had discovered Boyle's law, or Townley, the delay in publication looked odd, particularly since Power was fully informed of the pace of Boyle's progress. Boyle used Power's manuscript — and Townley's experiment — in summer 1661; he made his final discovery in the following fall and published in spring 1662 (481 and 486, notes). Contrary to Power's hesitance and Townley's even greater hesitance (his book is still in manuscript, observes Webster; 471). Boyle's research evidently entered that phase of frantic acceleration towards a grand finale and rush to the press that is so often associated with great discoveries.

To return to Boyle's report: he saw something new; he had a flash of an idea about a new crucial test: Townley had designed an ingenious variant of the Torricellian experiment, that could be put to use here.[14]

Boyle's next step is a report is in the first person plural. He saw in Townley's old idea an opportunity of making experiments of Pascal's design without climbing high hills. But for these experiments more sensitive instruments were required.[15] To that end they returned from mercury to water: the preference of mercury over water is due to the inconvenience of working with a high water column (about 12 yards) as compared with a low mercury column (about 30 inches). For the sake of sensitivity they sacrificed convenience and experimented with high water columns (in Westminster Abbey). They found slight deviations from regularity; Boyle ascribes them to variations in temperature. Then the apparatus broke. The experiment was thus a failure and so, I conjecture, the party tried to improve upon it. Though the experiment has led to nothing, Boyle reports it in detail: he obviously considered it important.

Another crucial experiment against Linus, says Boyle, and a qualitative experiment it is again, is Pascal's experiment showing that

> a weakly-blown foot ball … appears as if it were full blown at the top of the mountain.

This did not satisfy Boyle. At this junction he comes to realize that Linus' objection demands a careful quantitative measurement, since Linus has ascribed the phenomena found by Pascal to variation in temperature, rather than of pressure, and there is no denying that the hilly air is cooler. Townley's idea seems to provide the key to the refutation of this view, since in his experiment (with a barometer in which rare air replaces the vacuum) not only the weight of the atmosphere but also the elasticity of the rare air plays an important role.[16]

The reasoning is simple, and Boyle presented it explicitly as his own: In Pascal's experiment, carried out simultaneously with columns of mercury and of water, both columns should fall when ascending to a high hill, and the water column should fall nearly fourteen times as much as the mercury column, being so much lighter. In Townley's experiment this is not so obvious, since the expansion of similar volumes of air play some role in it, but the volume of air over the water expands much more than the volume of air over the mercury, and we do not know exactly [but only qualitatively] the law connecting the pressure and volume of a given quantity of air. By comparing the water and mercury columns at different air pressures, it seems,[17] a quantitative law may perhaps be found.

Here Boyle mentions again that Linus admits air to have weight and elasticity, although less than what Boyle ascribes to them. A report follows on the quantitative experiment with the J tube and the high pressure on the air in the short closed leg. (It is described in the quote from Rosenberger above.) The refutation of Linus must be quantitative and the quantitative experiment for it is a further improvement upon Boyle's improvements on Townley's experiment. Whose it is I do not know: again Boyle uses the first person plural. Note this: whereas Townley's experiment, and Boyle's first improvement on it, relate to the elasticity of air under pressure of less than one atmosphere, the later improvement concerns the elasticity of air under pressure of more than one atmosphere.

Rosenberger first tells about the instrument (the aperture) and then about the tables of measurements; he makes no reference to Boyle's intentions, except to refute Linus; Boyle, however, tells us of the expectations he had when he had built it and experimented with it. The passage in which he describes this is by now well known, but somehow it has failed to inform historians that Boyle reports what he anticipated of the experiment. He describes the aperture and the first experiment with it. They have attached rulers to the two legs of the J-shape tube, one of which has been sealed; they then poured mercury into the open leg and had the mercury leveled equally in both legs; hence, the pressure on the air in the sealed legs was one atmosphere; they then continued to pour mercury without letting any air escape from the closed leg, until its volume decreased to half of what it was when the mercury was leveled in both legs. Then he reports the result:

> we[18] cast our eyes upon the longer leg and we observed, not without delight and satisfaction, that the quicksilver in that part of the tube was 29 inches [one atmosphere] higher than the other… So here the same air being brought to a degree of density about [sic!] twice as great as that it had before, obtains a spring twice as strong as formerly.

Boyle's "delight and satisfaction" is due to his having anticipated the observed result; but here he strays from his earlier reporting style and mentions no anticipation. This is the beginning of the inductive style of suppressing all use of hypothesis prior to and leading to the described experiments. (It seems less dangerous to report a disappointed anticipation than a confirmed one.)

The tube was broken. Boyle decided to continue with the precise measurements. He then invented a simple method of facilitating the experiment by blocking the way of the compressed air by inserting a piece of paper between the air surface and mercury surface. They (again the ambiguity) made careful measurements and recorded in the table that follows. This is the table that historians refer to when they say Townley used it, not Boyle.

Rosenberger tells us of tables that relate observed volumes of compressed air and observed pressures, from which the law could be adduced. Heller tells us of manometric experiments leading to Townley's estimate leading to Boyle's tables. These stories are not in the present text from which they are allegedly borrowed. Boyle introduces his tables after telling us that the estimate had been made and provisionally confirmed. The table presented by Boyle at this point of his report relates observed volumes, observed pressures, and pressures as they should be according to the hypothesis that supposes the pressures and expansions to be in reciprocal proportions. This is the first explicit mention of the quantitative hypothesis (belated in accord with the inductive style) and (as Gerland so rightly insists) with no acknowledgment to Townley or to anyone else. As usual Rosenberger's and Heller's claims that Boyle's observations preceded the formation of his theory are but attempts to write history as (according to their different views) it should have happened, and without even telling us that it is their theoretical interpretations, namely their use of the theory that facts precede scientific theory in time, rather than presentations of mere facts.

So far, Townley's most important function as a collaborator and assistant is definitely not the one that Rosenberger and Heller impute to him. Townley did not provide the hypothesis discussed so far. But he had an ingenious idea that Boyle simplified and that simplification helped Boyle develop his hypothesis; namely, Townley's idea of having a Torricelli column of mercury with a little air added to the vacuum above it.

Townley's hypothesis, however, does exist; it is not Boyle's hypothesis; and it enters our story later. Boyle's quantitative hypothesis (Boyle's law for pressures higher than one atmosphere) had already been found. Now, Boyle thinks, Linus's objection is already satisfactorily refuted (Boyle, 3, 61). The task is now finished, even by the most charitable standards towards one's opponents. Next comes the crucial passage (italics mine), where the acknowledgment of Boyle's law to Townley is allegedly made by Boyle himself and is often quoted out-of-context.

> Now, if to what we have thus delivered concerning the *compression* of the air, we add some observations concerning its *spontaneous expansion*, it will better appear, how much the phenomena … depends upon the differing measures of strength to be met with in the air's spring, according to its various degrees of compression and laxity. But before I enter upon this subject, I shall readily acknowledge that I had not reduced the trials I had made about measuring the expansion of the air to any certain hypothesis, when the ingenious gentleman Mr. Richard Townley was pleased to inform me, that having by

> the perusal of my physico-mechanical experiments been satisfied, that the spring of the air was the cause of it, he had endeavoured, (*and I wish in such attempts other ingenious men would follow his example*) to supply what I had omitted concerning the reducing to a precise estimate, how much air dilated of itself loses of its elastical force, according to the measure of its dilation. He added, that he had begun to set down what occurred to him to this purpose in a short discourse, whereof he afterwards did me the favour to show me the beginning, which gives me a just curiosity to see it perfected. But because I neither know, not (by reason of the great distance betwixt our places of residence) have at present the opportunity to inquire, whether he will think fit to annex his discourse to our appendix, or to publish it by itself or at all, and because he hath not yet … met with fit glasses to make any accurate table of the *decrement* of the force of the *dilated* air; our present design invites us to present the reader with that which follows …

Here come tables that compare observed low pressures (i.e. less than one atmosphere) with what they should be according to Townley's hypothesis: it is *Boyle's test of Townley's extension of Boyle's hypothesis*. (The extension is from pressures above one atmosphere to pressures below one atmosphere.) As Boyle explicitly states, Townley saw no tables concerning low pressures, as he had none to show him as yet. So his guess, like Boyle's, preceded his knowledge of any relevant experiment. The passage shows again how inductivists can create myths, and how hard it is to eradicate them. Boyle reports that he had started experimenting on dilation without having a numerical hypothesis, a "precise estimate" that he then heard and read Townley about. Townley's hypothesis, his "endeavor to supply what I [Boyle] had omitted [sic!] concerning the reducing to a precise estimate", i.e., concerning the move from the qualitative law to some quantitative law for low pressures (below one atmosphere). There are no tables here, just a clear and explicit statement: Boyle — who had started with a qualitative hypothesis concerning compression and rarefaction; who had passed to a quantitative hypothesis concerning compression only; but who made no such a hypothesis concerning rarefaction — first heard about such a hypothesis from Townley. Mr. Townley's theory clearly is about the proportion, wherein air loses its spring by dilation, not wherein it gains spring. This is the hard core of the present essay.[19]

7. The significance of Townley's hypothesis

But are not Boyle's and Townley's hypotheses identical? And if so, is there a problem of priority?

A few points have to be considered concerning this question. There is a point of inaccuracy or misquotation. All those who claim, with D. McKie ([a] 149, italics mine), that

> the 'hypothesis, that supposed the pressure and expansions to be in reciprocal proportion' had been suggested to Boyle by Richard Townley, as *Boyle himself stated at this point*,

is untrue whether Boyle's and Townley's hypotheses are identical or not. When reporting what Boyle "himself stated", he made a clear distinction should be preserved, whatever our comment on it may be. As Boyle claimed the hypothesis quoted by McKie (about both pressure and expansion) to be partly his own, partly Townley's, one cannot say that he attributed it wholly to Townley. Similarly, McKie is insensitive at least to Boyle's terminology, by which, Boyle's law comprises Boyle's hypothesis (high pressures) plus Townley's hypothesis (low pressure).

McKie is not alone in ascribing to Boyle the acknowledgment he never made. Webster, too, says ([a] 227a),

> Both Boyle and Robert Hooke, his [Boyle's] closest associate, referred to the gas law as "Mr. Towneley's hypothesis", and it is clear that it was on Towneley's initiative that they embarked on experiments which confirmed Towneley's suggested law.

To return to Boyle's presentation of the quantitative law in two parts, one for pressures higher than one atmosphere for which he takes credit and one for pressures lower than one atmosphere for which he credits Townley. Do the two differ? They do. If we go far enough in either compressing or rarifying air we find deviations from Boyle's law. And, Van der Waals's equation tells us, the deviations from Boyle's hypothesis are attainable much sooner than those from Townley's.

Suppose we ignore question of accuracy, since the whole business is not too precise anyway. Do Boyle's and Townley's hypotheses still differ? Most historians take Boyle's model seriously, and so they should say, yes. The model describes each air particle as a spring. A spring can dilate and it can compress; it can be strained or stressed. These two are different phenomena. And, historically, another difference was more important before it was ignored: whereas in Boyle's case air is pressed, in Townley's it is 'strained'; we may not notice the difference, but only because we do not notice that we use the by then still unknown Hooke's law — strain equals stress — on which more soon. Now, Hooke's work was first published in 1675, namely, over ten years later. Knowledge of much later ideas about gases, such as Newton's or Clausius', may lead us to the denial of strain in gases; this is an extravagant hindsight.

Let us ignore all later knowledge, assume that Boyle ascribed to air both strain and stress, and take this seriously for a while. A very simple difficulty regarding all this now appears: between the strain and the stress of a spring there is the state of zero displacement, where the spring maintains its natural position, undistorted by force. Boyle tacitly assumes that at the point of one atmosphere the displacement is zero. Here already Boyle's qualitative hypothesis, his model of air particles as springs, makes a serious difference between high and low pressure: under low pressure air does not expand under its own force, as we think since Newton, but it is stretched. This is a remnant of Galileo's theory of the force of the vacuum. Mach ([a] 136) censured Galileo for it. Webster refers to it too ([b] 444 & n), but not in connection with Boyle. This Boylean idea of the strain of air particles, and with it Boyle's model of air particles as springs, is destroyed with the unlimited application of Townley's hypothesis. Hooke discovered this point. He did not openly express criticism of Boyle. Newton is the thinker who abolished Boyle's distinction (by his assumption of repulsive force only) with some (insufficient) acknowledgment to Hooke. His acknowledgment has puzzled both Gerland and Cohen (see above). Cohen takes the acknowledgment as testimony that Newton ascribes Boyle's law to Hooke.

My final observation on Boyle and Townley concerns Boyle's function as a propagator of science and as a public teacher. This function is so evident in the passage in which he makes the acknowledgment to Townley, that it is somewhat odd that it should have been ignored. It is not the problem of Boyle's priority that matters. Nor does it matter whether the difference between Boyle's and Townley's hypotheses is great or small. The important mistake is that Boyle was interested in facts only. Against this views, especially in Rosenberger's wording, I have so far shown (a) that the whole work, experiment and theory, was undertaken in order to refute Linus's objections to Boyle's theory of the elasticity of air, and (b) that Boyle made the original hypothesis. But, in addition, there is this to say about Townley's hypothesis. He made it independently, as did at least Hooke, Power, and Brouncker.

Boyle's fairness to Townley has never been mentioned, as it was taken for granted. This is extravagant: it was the first acknowledgment to an assistant, and Boyle was the first who carefully acknowledged all sources of his information, whether factual or theoretical, published or unpublished (with occasional exceptions).[20] Moreover, in his own time a few people cared at all about priority. At that time the qualitative hypothesis was much more important than the quantitative one (see below). But Boyle, always with an eye on progress and a large scientific society, set the standards that we take for granted. His concern with his own priority in many discoveries is well known, and his well known humility makes it quite clear that this was not motivated only by personal interest.

Boyle's function as a recruiting officer for the new scientific brotherhood misled Lange, Rosenberger, and others. R. F. Jones ([b], Chapter 5) was the first to notice it. In trying to recruit people, Boyle laid double emphasis on experiment and on facts, for the reason that more people could experiment than conjecture. In his attempt to encourage Townley (and others) he gave an exaggerated impression about his debt to them. And the quantitative hypothesis seemed only later to be more important, because it won Boyle the greatest inductivist reward, namely his mention in up-to-date textbooks of science, his secure place in posterity, his inductive surrogate immortality (as Carl Becker would say).

Boyle's exaggeration here was rather excessive. In his zeal to encourage people he made too much of a minor idea, with misleading results. Remembering that the hypothesis of monotonous increase of density with compression is Boyle's, and that the extension of the latter hypothesis to the whole domain for which the first hypothesis was assumed independently by Townley and others (see below), we can hardly deny that he behaved a bit like a schoolmaster.

In the long passage quoted above, in which Boyle makes an acknowledgment to Townley, he also mentions two others whom he similarly wanted to encourage. Townley intends to continue his experiments a bit further first, perhaps to publish his own results, perhaps as an additional appendix to Boyle's book;[21] meanwhile (Boyle, 3, 61-2),

> our present design invites us to present the reader with that which follows, wherein I had the assistance of the same person, that I took notice of in a former chapter, as having written something about rarefaction:

Dr. Henry Power, whose work was published indeed in 1664.[22] And Boyle's intention is to encourage Power (as the immediate continuation of the above quotation makes clear):

> whom I the rather make mention of on this occasion, because when he first heard me speak of Mr. Townley's supposition about the proportion, wherein air losses its spring by dilation, he told me he had the year before (and not long after the publication of my pneumatical treatise) made observations to the same purpose [sic], which he acknowledges to agree well enough with Mr. Townley's theory: and so did (as their author was pleased to tell me) some trials made about the same time [sic!] by the noble virtuoso and eminent mathematician the Lord Brouncker, from whose further enquiries into this matter, if his occasions will allow him to make them, the curious may hope for something very accurate

that, need one say, is flattery, cajoling, and wishful thinking. It all amounted to very little unless it helped establish science as an amateur occupation.

8. The history of the study of elasticity

The question that intrigues seventeenth century thinkers is, can there be a vacuum? Boyle gave this question two distinct versions, scientific and metaphysical. Scientifically, the question was, is space without gross matter possible? To this, he said, the empirical answer is evidently yes. Metaphysically, the question was, is there non-gross matter in the vacuum? To this he said, not necessarily. He thus was a vacuist both in physics and in metaphysics; he took pain to distinguish his two positions and stress that he was demolishing plenism in physics, leaving the metaphysical question open.

This was no news to either Galileo or Torricelli, not to mention Descartes and Mersenne. Still, it took Boyle to formulate and explain it. Once this was done, the initial interest in the vacuum and related phenomena declined. Perhaps Boyle noticed this from the start, when he labeled his original book with reference to the "spring and weight of the air", not to the vacuum. But perhaps he thereby merely showed tact in the face of the immense popularity of Cartesianism.

Boyle raised interest in the elasticity of air; and it proved to be less interesting than he had hoped when he suggested that his readers would take his broad hints and repeat his experiments while varying the temperature of the air; his hopes led to a deep disappointment. The study of this field, as of any other field, must be connected with some interesting problems.

Descartes hoped that his theory of matter as extension explains impenetrability (inter-penetrability of the pieces of matter would diminish their total volume, and on the assumption of the identity of matter with extension this would violate the geometrical law of invariance of volume.) Strangely, the vacuists accepted the thesis of impenetrability of matter just as much as the Cartesians; among those was Locke, who denied the validity of Descartes' deduction for empirical reasons, as well as Leibniz who denied it for *a priori* reasons — they all accepted impenetrability of matter quite axiomatically. So did Laplace. So did even Kant, — in his own peculiar way, need one add — at least in his *Critique of Pure Reason*.[23] Perhaps they all did so on Democritus' or Plato's authority, but I have no evidence for this. There exist many known cases of matter seemingly penetrating matter, of course: these cases, as Kant has noted,[24] are not allowed to refute the thesis of impenetrability. A simple and surprising example of penetration confirms it: a ball of metal full of water can be compressed with a hammer, with the result that water penetrates its invisible (hypothetical) pores and appears on the surface. What is conserved here is the total volume of matter, as the law requires. Similarly, a sponge absorbs water but expels air from its pores, as immersing the sponge in a bucket of water shows. But the case of a football that is blown to its maximal size, into which it is still possible to push air refutes the thesis of impenetrability. This last example is Boyle's ("Proëmial Essay"). He was the first to notice this refutation; the thesis of impenetrability remained popular in spite of this and of Leibniz's criticism and his claim that impenetrability is the result of repulsive forces. Boscovitch and Kant presented, each in his own way, a Leibnizian theory of the expansion of matter as due to repulsive forces.

Similarly, plenists stuck to their view in spite of the discovery of the vacuum: it was quite possible that space is filled partly by air and partly by another matter — the aether — that can slip through pores of glass containers and of skins of footballs. This reconciles impenetrability and plenism with *vacui* and with air compression in footballs. This is, again Boyle's reasoning.

Why, then, did Boyle keep his interest in the football? What was its significance? Will this shed light on the great value placed by contemporaries on Boyle's law? He had a particular dislike for the dogmatism with which Descartes' view on matter was advocated. He successfully advocated a liberal attitude to scientific and metaphysical ─ an attitude that was essential to the success of Newton's deviation from Descartes. The beginning of the story of Boyle's advocacy of intellectual toleration lies in a sad incident in his own life. In 1655 he published anonymously a medical collection plus a plea to publish all medical secrets. It was a complete failure. (See Margaret E. Rowbottom, 1950. See also Fulton [b] 1.) He linked his plea for openness with his demand for the admission of all and only repeatable experiments. He explained this at great length in his *Sceptical Chymist*, especially in his preface to it.) In his "Proëmial Essay" to *Certain Physiological Essays*, published in 1661 and written a short time before he started the work on the elasticity of air, he complains that works of "learned men, especially physicians" were often undervalued because they were not cast in the Cartesian system, as if it was a basic requirement for rational thinking. In opposition to this dogmatism he suggests that even if Cartesianism is true, there is no reason to oppose the presentation of a non-Cartesian explanation of facts, as it may always be hoped that researchers will find a Cartesian explanation of that explanation. This idea is a drastic deviation from those of Galileo and Descartes. Newton relied on it as he introduced forces in the hope of finding a Cartesian explanation of them.[25] suffice it here to notice that that Boyle saw an opportunity to study the elasticity of air in this fashion, and that Pascal's loosely blown football that expanded fully blown, was a chief reason attracting him to the study of the vacuum. This explains why his *Spring of the Air* concerns elasticity, not vacuity. It introduced his non-Cartesian hypothesis, his non-Cartesian model of the elasticity of air as a heap of minute springs. If Cartesianism will include a model to explain the behavior of ordinary spring, then it will also thereby incorporate an explanation of Boyle's (seemingly) non-Cartesian model. This then is Boyle's methodological principle. The example of a football occurs in the "Proëmial Essay" and was thus written as an example of Boyle's methodology before his reading of Pascal's football, and his experiments with the vacuum pump that led to Boyle's law. The *Spring of the Air* contains the qualitative law of monotony between elasticity and pressure. It justifies his spring model of the air. The quantitative law is but a part of the *Defence of a Doctrine Touching the Spring ... of the Air* that had no particular significance for Boyle. So he never laid too much stress on it. The history of the study of elasticity changed radically with the introduction of Newtonian mechanics. Prior to that there are Hooke's studies[25] and young Newton's private notes that are relevant to this discussion.

Truesdell reads [Truesdell, 53-58] Hooke's law to say, elastic force is proportional to displacement, where displacement can be caused by a weight hanging from a spring, a wire, or even a string (54). This sounds very much like Hooke's law out of the standard elementary physics textbook, and that is evidently a-historical. Later on (56) he makes observations that prevent this gross a reading: "it was not yet customary" in Hooke's times and before Newton's *Principia* was absorbed, he says, "to think of motions as determined directly by assigned forces." Still later he adds (57) that the standard treatment of Hooke's law as the law of harmonic motions "seems first to have been given many years later by John Bernoulli." No matter how well-grounded is Truesdell's reading of Hooke to say force is proportional to displacement, it still faces quite a few difficulties.

Hooke says, "*ut tensio sic vis*", that is, "the power of any spring is in the same proportion with the tension thereof" (54) where both "*tensio*" and "*tension*" should mean displacement, of course. Hooke continues, concluding or expanding, to say that the force or power of an elastic body "to restore itself to its natural position is always proportionate to the distance or space it is removed therefrom, whether it be by rarefaction … or by condensation", i.e., whether by strain or by stress (55). One may wonder, what strain or removal from "natural position … by rarefaction" air can suffer: does Hooke's Boylean analogy of air particles with springs breaks down? How? Why does Hooke translate "*tensio*" to "*tension*" but in his examples (55) he uses "*extension*" as usual? He means, first, force is proportional to tension and tension to extension; second, force is proportional to stress, and, third, stress to compression. He may also mean to say that the proportionality in extension is the same as that in compression. Truesdell says (55),

> While Hooke does not say explicitly that the *moduli* [i.e. the factors of proportionality] of extension and contraction are the same, this seems to be his opinion; in the case of air, the only material for which he says he has measured condensation, this is true.

It is surprising that of all materials to which Hooke generalizes Boyle's law, he only tested his view on the original material, namely air. The experiment is not hard to perform on springs: a spring may be both stretched and compressed by the same weight, once hanging from the ceiling with weight hanging on it, once resting on the floor with the weight resting on it. So it is easy to measure the expansion and the compression of a spring and see whether they are equal or not. For displacements small enough the results are nearly the same, of course.

In the two experiments about springs just described the displacements are from the "natural position" of the spring, from the equilibrium position it has when not under external force so called. (This is only nearly so, as the spring's own gravity is also external to it.) And, to repeat, air has no such "natural position". Air particles do vibrate in sound around equilibrium positions, but these equilibrium positions are not "natural" — not in the absence of external force. Hooke's alleged claim that strain equals stress was replaced by Young's modulus so-called, which says, strain is proportional to stress. (Truesdell says, we should credit Euler for it, nor Young.) Young's modulus is different when the equilibrium is "natural" than otherwise.

The straight reading of Hooke is different. It is to take him to assume Boyle's model of the air particle as a spring, and one atmosphere as the condition of the "natural position" of air particles. We can then read Boyle's law to hold for all elastic bodies,[26] and thus read Hooke to say, force equals tension equals stress, and tension equals displacement in one direction and stress in the opposite direction. With this reading all the difficulties mentioned above disappear — on the condition that we allow Hooke to have confirmed his false view with somewhat inaccurate experiments.

The *Encyclopedia Britannica* Article "Elasticity" translates "*tensio*" to mean at times, tension, at time displacement; so do other writers, particularly Andrade [b] — unless he interprets '*tensio*" to mean systematically both tension and extension. This puts Hooke in the right at the expense of destroying our ability to understand the history of elasticity.

Interest in elasticity in the eighteenth century was limited. The one somewhat pressing empirical case was the study of water that, acoustics informs us, is elastic, yet we know from experiment to be incompressible. (Beating a metal chamber full of water to reduce its volume, we remember, forces water out of it, presumably out of its pores.) This was not a very interesting, and not within the range of available experimental accuracy. Euler's interest was rooted in his plenism. The interest was given new life in the early nineteenth century when Young imposed the aether theory on all Newtonians, for, in order to test it, more knowledge of elasticity was needed ([Love] 7). Dalton revived interest in the pressure of airs then by raising the problem of diffusion that arose from Lavoisier's theory of air as a mixture. He tried in vain to solve it with the aid of Newton's explanation of Boyle's law. Earlier, Boscovitch's study of elastic collisions and his consequent atomic theory changed the whole scene drastically; the connection between gases and elasticity was soon lost. Boyle's law was generalized and used as basis for the kinetic theory of gases in which atoms of gases were assumed to be perfectly elastic with no further ado.

9. Webster's defense of Townley's priority

The first paper on Boyle's law after Gerland's (1909, 1913) is by W. S. James (1928). It is enjoyable; it is clear and refreshing in that it contains an explicit statement of quite a few difficulties; the author was also very clear as to what he was asking and why he used which documents, and when he used his own judgment. He also brought together most of the then available documents, including records of the Royal Society of London and Hooke's testimony. There is no need to discuss James' view in detail, since it is almost fully reflected in those of his successors that I will discuss soon; I shall use details from his presentation while discussing these. Let me merely present here a few general points of information. James identifies Boyle's law with Townley's hypothesis. He identifies the experiments reported in the *Defence* as the ones that Boyle reported to the Royal Society on September 11th, 1661 (see below). He notices that Hooke claims having performed them on August 2nd, 1661, and he dismisses Hooke's testimony, first, since Hooke was unreliable, especially when staking a claim, and second, since Hooke was an assistant anyway. He dismisses Mariotte as a plagiarist.

Next comes the paper by D. McKie, 1948, that, for all I can judge, says nothing more than James, but, for reasons I am unable to discover, has fared better than James' paper: it is cited approvingly, one way or another, by Sarton, Fulton (who calls it "important", ([b] 11n), I. B. Cohen (who calls it "convincing", 618), and others.

McKie claims that the law should not be called Mariotte's but Boyle's — since it was found by Townley. In 1950 Andrade claimed ([b], especially 459) priority for Boyle's law to Hooke, since, he says, Hooke claims to have anticipated Townley's hypothesis. Andrade's claim has no leg to stand on, since Townley's hypothesis was published in Townley's name prior to Hooke's publication, and priority goes to the first published. The final touch was given by C. Webster and by I. B. Cohen, especially since Webster's second paper is very detailed, and covers all known material and some new material that he is the first to discuss in print. I shall report this study in detail now since it contains abundant historical material misread in a complex manner.

Webster's general thesis is one that is becoming increasingly popular these days, and it is the thesis of multiple discovery of Usher, Merton and Kuhn: every discovery is made by a few individuals. This thesis is very easy to support by multiple evidence: all one has to do is ignore differences between contemporary researchers, and they look identical. In other words, unless a historian provides a criterion by which to identify or differentiate works of different writers, his study may be safely ignored. Of course, multiple discovery is possible, especially when a problem hits the public fancy — even when an experiment, or even an instrument, does. And, in order to explain a specific multiple discovery, then, we must say what problem, or other factor, became a focus of interest and why.

Webster merely states that the elasticity of air did take public fancy, and illustrates this by the number of students of the topic from Torricelli to Boyle and his associates. But he does not explain this phenomenon; although he notes for the first time the Cartesianism of Townley and discusses its relations to vacuism, he does not relate it to spring or elasticity. On the whole, he barely refers to theories of elasticity except in the case of Boyle, where he links Boyle's view with scholasticism in order to belittle him so as to make room for Townley, as I shall soon explain.

Webster finds ideas similar to, and reminiscent of, Boyle's law in Roberval (1648) and a more succinct version of it in J. Pecquet (1651); Boyle's progress (1660), Webster says, was in his groping — perhaps towards more precision, perhaps towards more clarity (see below). Also Webster indicates the difficulties on the way towards Boyle's law, particularly as seen from the failures of Power and Townley.

Webster also quotes (468) Boyle's summary of his unsuccessful attempt to record quantitative observations, made in 1660, which ends with a plea to others to try it out again, and he quotes (469) Boyle's report of his plea to mathematicians to take up matters as well — adding that commentators had been in error when they took this to mean that Boyle was weak in mathematics. Webster notices as well Power's and Townley's reluctance to publish, and he also notices that Boyle's reference to his colleagues was often both highly encouraging and quite cursory — so as to let them do their own publishing.

All this is very interesting, and also very charming; from now on, however, things start downhill.

Webster's attitude towards Boyle is ambivalent. Already when discussing Boyle's debt to Guericke, though he is careful in his report of the degree of Boyle's dependence on his predecessors, he is unfair to Boyle in his over-generosity to Hooke (see above). He then simply speaks of "Hooke's pump" or "Boyle's pump" in a rather indiscriminate fashion.

Webster wishes to ascribe the law to Townley; but he ascribes the law, in effect, to Power and Townley. For, he claims (see below) that Townley's hypothesis comes no earlier than September 1661, yet he reads the description of the experiment of April 1661, performed by Power and Townley to be an expression of Boyle's law (482, also [a]227a)! He admits that Power is rather "cryptic", but he still reads the law there. Also, he does not mention the possibility that though the Power-Townley experiments were concluded in April, 1661, the tables may have been included in the manuscript later on, and even the text may have been updated later on to show the influence of Boyle's writing of late 1661 and early 1662.[27] Yet, on Webster's own claims, one has to conclude, as I. B. Cohen concludes, that the law was discovered in April 1661 by Power and Townley, not in September 1661 or later by Townley! This, incidentally, covers entirely Hooke's claim to have discovered the law on August 2nd, 1661.

But even on Webster's understanding we must ignore Power's manuscript — for a while at least — as quite puzzling. Webster's most important section is his tenth, on "Boyle's experiments on the compression and dilation of air"; his major point (481-482) is contrary to his claim for Townley and Power; it is one that I would gladly endorse, and can only regret that later he rejects:

> It is probable that Boyle derived the law from his experiments on the compression of air, whereas Townley pointed out that it also applied to the experiment on expansion.

I agree, except for the expression "derived the law from his experiments", which at worst merely reflects a widespread prejudice (which on page 492 Webster accepts in part and rejects in part, but with no discussion), and can easily be translated into "confirmed his own law by experiment", or "conjectured and confirmed", or some such. But it seems that Webster himself cannot accept his own view, as it amounts to saying that it was Boyle who both first stated Boyle's law, and confirmed it for pressures above one atmosphere.

Webster ascribes the law to Townley on three conflicting grounds: first, that (Power and) Townley deduced the law from his (their) experiment ([a]227(a), [b]488, lines 6-8), second, that he made the hypothesis on the extension of Boyle's law to expansion and coaxed Boyle and Hooke to confirm it ([b]488, lines I and 2), and third, that he clarified the meaning of Boyle's results, ([b]487, line 19). There is no hint of a clarification anywhere, and no one before Webster ever made such a claim. But Webster even thinks "in Boyle's mind there still lingered the scholastic notion that condensation and rarefaction were qualitatively different" (lines 1 and 2 of the same page); needless to say, this scholastic view is still upheld in all standard texts on elasticity; what is of more concern here is that both Boyle and Power ascribe the same "scholastic" notion to everybody, including Townley. Webster quotes ([b]484), from the records of the Royal Society (Birch, ed.), the passage referred to by James and McKie; and quite a remarkable passage it is: it begins with a report of two experimental demonstrations of a weekly meeting of the Society on September 11, 1661, and ends thus:

> Mr. Boyle gave an account of his having made the former of these experiments by compressing twelve inches of air to three inches with about a hundred inches of quicksilver.

In other words, on September I, 1661, Boyle already knew that the pressure of three atmospheres reduces volume to one third; hence, one might assume . with James (226) that the experiment, reported in Boyle's *Defence*, when Boyle reports his first "delight and satisfaction" in confirming his hypothesis, came prior to September, 1661. I assume that Boyle had put together the results of his two experiments; if so, he had before him something like the following table:

Observations:	First	Second	Third
pressure, approx.	1 atm.	2 atm.	3½ atm
volume, approx.	V_o	½V_o	¼ V_o

(where observed pressure is the differential of height between the two columns, and observed volume is the height of air column in the closed tube). (The inaccuracy in one direction may be explicable by. reference to the escape of air which we know Boyle was sensitive to, but not in the other direction; naturally, the inaccuracy is in the right direction.) The table looks suspiciously like a confirmation of Boyle's law. One might ask, however, why didn't Boyle scoop and pronounce that hypothesis there and then?

Clearly, we have no evidence that he did not. And no evidence has been misread as evidence of absence of such a pronouncement — by James or anyone else. The records of the Royal Society, according to the statutes of 1663, clearly indicate preference for facts over theory and a taboo on reporting fact and theory in juxtaposition — indeed, they were put into two different books by 1663 (Weld, 527). It is very likely that this is a compromise, that in 1661 they recorded facts alone.

Webster reports the three-atmosphere experiment of September 1661, adding (484), "Already Boyle had an intuitive understanding of the nature of the relationship between the elasticity of air and its pressure …"; he continues with the experiment with the tables of October 1661; he digresses to the details of the apparatus, and then plunges into the two-atmosphere experiment with "The observation which Boyle notes with the greatest pleasure", concluding with "He now arrives at the following hypothesis". There is about one page between "Already Boyle …" and "He now …", and I do not quite know if and how they connect.[28] If they do, Webster is mistaken; if they do not, he is bizarre. One way or another, it cannot be said that he shares his difficulties with his readers frankly, which is a conduct not uncommon amongst historians of science, and of which I have already complained extensively (Agassi, [a] section 3 and notes; where examples for this very misuse of "now" are given). Before leaving the chronological difficulty, let me only mention in haste, that on Webster's chronology, but not on mine, there is the problem of priority of Power and Townley of April 1661. Also, on Webster's chronology, and on Cohen's ascription of priority to Power and Townley, Hooke's claim (see below) that he worked independently on Townley's hypothesis in August 1661 is problematic, but not on my chronology. I shall come to this later.

Webster now arrives at Boyle's hypothesis following his experiment with the pressure of two atmospheres. The hypothesis is, the spring of air is proportional to its density. In Section 2 I have discussed in detail the relation between

$$\text{Pressure} \cdot \text{Volume} = \text{constant} \tag{2}$$

and

$$\text{Pressure} = \text{constant} \cdot \text{density} \tag{4}$$

and shown (4) to be slightly more general than (2). Since I consider my discussion on that point quite a trivial discussion of very pedantic elementary physics, containing only an elementary deduction and a trivial law

the weights of equal volumes of air under equal pressures are equal (3)

I naturally did expect Webster at this junction of his discussion to congratulate Boyle on his statement (4) in preference to (2), especially since Boyle was — beyond any measure of doubt ever entertained by a historian of science — quite familiar with the definition of density, and since Boyle asserted (3) quite explicitly. Instead, Webster claims (486) that

> it is by no means certain that Boyle realized at this time that (4) implied (2) …

and after some further exposition Webster concludes (486),

> Perhaps in Boyle's mind still lingered the scholastic notion that condensation and rarefaction were qualitatively different.

This is on the right track but nonetheless a howler and an injustice unusual even in the annals of the history of science. It is not a mere scholastic notion that strain and stress are different qualities. Moreover, Boyle did not have to assume difference, but merely avoid assuming identity! Strangely, it is because the above quotation is so unjust to Boyle that Webster who is usually well-disposed towards him, does not pursue the idea more seriously. He almost arrives at the idea that Boyle speaks of high pressures (above one atmosphere) and Townley of low pressures. He takes Boyle's *Defence* to be chronological and puzzles that Boyle uses "expansion" only in the later part — yet he still claims (486) that "expansion" means volume (rather than the opposite of condensation), even though in the passage in which Boyle first introduces the word (see above), he introduces the contrast between . "compression" and "spontaneous expansion", inserting Townley's contribution between them). All this Webster does merely in order to identify Townley's hypothesis with Boyle's law. He even notices (487-8) that

> Boyle derived the law from his experiments on the compression of air, whereas Townley pointed out that it also applies to the experiments of expansion.

And he corroborates Boyle's testimony by Townley's manuscript. Yet he denies that it was Boyle who discovered Boyle's law. I must admit, however, that he has one piece of evidence: he quotes (498n) a letter from Townley to Oldenburg, of 1672, where Townley too misreads Boyle.

> It was some satisfaction to me [says Townley] to find in the Transactions of July that the hypothesis (which Mr. Boyle was pleased to own as mine) about the force of air, condensed and [sic!] rarified, both succeed as well in deep immersions, as in those I made trial, and that it both administer now to the learned matter of further speculation, as formerly it did to me of writing some few things, (of which I then showed Mr. Boyle) …

Clearly, however, this letter is a moving nostalgic reminiscence of a person who is already out of it all, not any careful testimony; no court, and no political historian, would accept such evidence; but historians of science often do, and show even worse credulity. Whatever is the case and the value of this letter, doubtlessly it must have been soon forgotten.

Finally, Webster views Townley's letter just re-quoted as confirmatory of his own "interpretation of Towneley's part in Boyle's experiments". And yet Webster rightly concludes (492) (italics mine) that

> Boyle [said] that *the spring of air was proportional to its density*

whereas later on

> Towneley [said] that the pressure of air was reciprocally proportional to its expansion

which two quotes are inconsistent with Townley's claim in his letter to Oldenburg. But I suppose I do injustice to Webster, because, finally, he attributes the law neither to Boyle, nor to Townley, much less to Townley and Power. He closes by saying that this summery

> leaves unanswered the question of priority of discovery and the correct title of the law, problems which are of limited importance compared with that of obtaining an accurate historical account …

with which I fully concur.

Thus, Webster claims to have shown with his massive erudition that Boyle's achievement was "the climax of a cooperative enterprise" (490); but I am afraid collaboration is not the same as multiple discovery; and "cooperation" is here ambiguous. Even with obviously collaborative and other interdependent enterprises going on in front of our own eyes, we go on attributing priorities and rewards for them of all sorts. To argue for the priority of one and then credit another's contribution is not to credit and then withdraw credit.

10. Cohen's defense of Hooke's priority

Let us now revert to I. B. Cohen, the contributor of the latest weighty comments on the situation. His paper. "Newton, Hooke, and 'Boyle's law' (Discovered by Power and Towneley)", is probably the last word on the present topic. Cohen ascribes to Webster the establishment of the following facts. Power and Townley postulated the reciprocity of pressure and volume and confirmed their postulate and had their results communicated to Boyle prior to Boyle's postulation of any reciprocity, let alone confirming it (618). Cohen next accepts Webster's explanation of the alleged fact that Boyle omits reference to Power.

From this Cohen moves on to a discussion of Hooke's activities and Newton's comments on them. As it turns out, however, Cohen is in a similar predicament to Webster: whereas Webster wishes to defend the priority of Townley, and does defend that of Power and Townley, Cohen wishes to defend that of Power and Townely and does defend that of Hooke. To Hooke, then.

Cohen quotes Hooke to say that

> the Elater of the Air is reciprocal to its extention or at least very neer

which can be interpreted (correctly) as Townley's hypothesis, or (incorrectly) as Boyle's hypothesis, or as Boyle's law. Cohen interprets it as Boyle's law without hesitation: he goes on quoting Hooke to confirm the above quoted hypotheses by pressures higher than one atmosphere: without hesitation he simply reads both "elater" and "elastic power" as associated with "expantion" as well as with "compression", even though in Hooke "elater" is systematically coupled only with "expantion".[29]

Hooke's essay is the penultimate contribution to his *Micrographia*, 1665. His purpose in that essay is to aid astronomy: to explain the apparent disfiguration of the sun's and moon's figure at the horizon (discovered thanks to the telescope) and the observational errors of stellar locations due to atmospheric refraction. Briefly. the index of refraction of the air is a function of its density that diminishes with height, so that rays of light travel in curved lines — except for the zenith, of course. The other interesting aspect of Hooke's penultimate essay is relevant to astronomy and to other topics, but not very much to the present essay: it is the estimate, with the aid of Townley's hypothesis, of the height of the atmosphere.

Now, Hooke introduces his problem in a very straightforward manner; but from his very assault on it, he is strangely devious. To begin with, a definition ([Hooke]219):

> By density and rarity I understand the property of a transparent body, that does either more or less refract a ray of light … I call glass a more dense body than water … because it refracts light more … So to the business of refraction, spirit of wine is a more dense body than water …

This is Hooke's definition of density. The expression "I understand" signifies definitions in the purely verbal and arbitrary sense — at least in the works of Boyle and his contemporaries. When definitions are introduced as essential (and hence non-arbitrary), whether earlier (say, by Descartes) or later (say, by Newton) there is no use of such expressions as "I mean" or "I understand". And so, Hooke may define any word any way he likes. Yet, his definition is a bit odd, to say the least. After all the words "density" and "rarity" follow a standard use that Hooke employs even in these pages. His conduct may be explained, however, as an expression of his ambivalence towards expressing a hypothesis. For, evidently, his unstated but clearly indicated hypothesis is this: the refractive quality of air is a monotonic function of rarity in the ordinary sense; or perhaps even rarity is proportional to rarity. As it is a mere matter of choice of proper units to convert proportionality to equality, Hooke's hypothesis may perhaps be put as, "rarity is rarity" that sounds like a tautology but is not since the word is employed in two senses. Hence, the hypothesis needs a test. The test requires an estimate of the air's rarity. Hence, the interest in Townley's hypothesis that deals with rarity, not Boyle's that deals with density.

Before the detailed discussion, however, Hooke offers qualitative experiments — like Boyle before him, and like Faraday after him.[30] First, Hooke shows empirically that a solution whose concentration at bottom is higher than at top yields a curved path for a light-ray passing through it. Then he shows that a change in the density of air changes the air's refrangibility: he heats a glass ball, seals it, lets it cool, and uses it as a lens.

The stage is now set for the study of the employment of Townley's hypothesis for the estimate of the distortion of astronomical observations. We are now coming to Hooke's barometric experiments (222),

> which experiments, because they may be useful to illustrate the present inquiry [sic!], I shall briefly describe.

Hooke's relevant texts (222-6) contain over one page (222-3) describing his experimental arrangement for the illustration or test of Townley's hypothesis, a page (224) offering the first and poor table, concerning *low* pressures, a page (225) of about three paragraphs that I shall soon discuss in detail, a page (226) of precise *data* concerning low and high pressures, and a little more — including one sentence that Cohen quotes and that has been re-quoted in the beginning of this section.

Hooke's first table of experiments relates to Townley's hypothesis, but it is problematic: when the pressure on a given quantity of gas decreased from 30 inches to 3 inches its volume increased not by 10 but by 15 !fl. From today's stand-point, with all the hindsight we normally amass, we can say for sure that the discrepancy was a result of a leak. But if we remember that Hooke wanted to publish responsibly — as he says (see below) — we may well understand his unease about not gaining enough recognition because of responsibility (as Galileo did before him, see note 2 and note 4 above).

One must, in simple human sympathy, notice the unease Hooke felt when he wrote his report.

> I had several other tables of my observations, and calculations which I then made; but it being a twelve month since I made them; and by that means having forgot many circumstances and particulars, I was resolved to make them over once again, which I did August the second 1661. [*sic*] with the very same tube which I used the year before, when I first made the experiment (for it being a very good one I had carefully preserved it:) And after having tried it over and over again …

What does Hooke report in so much meticulous detail? Perhaps he means this. The experiments I am here reporting are of August 1662, and they are a repeat of experiments made twelve months earlier, i.e. August 1661. This is James's and Andrade's and Cohen's reading of the above quotation. Now, the above quotation follows a table of low pressure experiments. All low pressure experiments have been introduced previously (222) as merely variants on Torricelli's experiment, but immediately after the above quotation there is a brief paragraph opening with "the other experiment", following with one paragraph describing the apparatus, one final paragraph of explanation, and a table of high and low pressures. The final paragraph begins:

> But having (by reason it was a good while since I first made) forgotten many particulars, and being much unsatisfied in others, I made the experiment over again …

And so, it seems, in August 1661, Robert Hooke made sophisticated quantitative observations of both high and low pressures and kept quiet until in step after small step Boyle, Townley, Power, and perhaps others improved their experimental techniques to cover the same ground. So say James and Cohen. Let it be so. Why do they express no puzzlement at such a silence? Is it because they are not puzzled? This is hardly credible.

There are a few points at issue here. First, the two experiments reported by Hooke, second the dating of them, third, their relations to theory, and fourth Hooke's relation to Boyle. There are two experiments: first a variant of Torricelli's, illustrating Townley's hypothesis and reported in the first and poor table and in the second part of the second and good table. The second experiment is referred to as "the other experiment" and is reported in the first part of the second table:

> I made the experiment over again and, from the several trials, collected the former part of the following table …

So much for the two experiments. This is amply clear from the above quotation: Hooke has performed both experiments twelve months earlier! The question is, earlier than when? Twelve months between the two experimental sessions, or twelve months between the first experimental session and the writing of the first draft of a text published over two years after the second experimental session? There is even a wrong fullstop in the crucial passage.[31]

In my view, it was the first experimental session, not the second, that took place on "August second 1661.", and the second took place twelve months later. This becomes clear from the end of the paragraph concerning, and immediately following, the first and poor table of high-pressure experiments:

> And after having tried it over and over again; and being not well satisfied of some particulars, l, at last, having put all things in very good order, and being as attentive, and observant, as possibly I could, of every circumstance requisite to be taken notice of, did register my several observations in this following [second] table. In the making of which, I did not exactly follow the method that I had used at first; but having lately [sic!] heard of Mr. Townley's hypothesis, I shaped my course in such sort, as would be most convenient for the examination of that hypothesis; the event of which you have in the latter part of this last table.

Hooke relates quite a few interesting facts here. First he relates Townley's hypothesis concerning low pressures. Note, however, that in "this last table" Hooke refers not to the previous table, of course, but to the following table, namely to the next and last one. Yet, even though he is ultimately clear, his presentation is not straightforward and easy to follow. Now, Hooke says he has “lately” heard of Townley’s hypothesis. This “lately” must be between September or October of 1661 and the publication of the *Defence* of 1662. But when, more exactly? There is one indication that indeed Hooke’s “lately” is in 1662: We know that Hooke performed experiments before the Royal Society on December 10th, 1662 on low pressures and on January 28th, 1662/3, on high pressures. It is quite possible that “lately” then means before Hooke was writing the details for the demonstration, i.e. not long before December, indeed not long after he was making his second set of experiments — in August 1662, twelve months after the first set of August 1661.

Cohen offers one clear-cut piece of evidence against this: he quotes (618) Boyle’s passage already quoted here, and adds (619a):

> Boyle refers explicitly to Hooke’s claim that when he had first heard Boyle speak of the “proportion” supposed by Towneley, he had stated unequivocally that he himself “had the year before (and not long after my publication of my pneumatical treatise) made observations to the same purpose which he acknowledged to agree well with Mr. Townley’s theory”.

In other words, Cohen thinks that here Boyle repeats Hooke’s claim that in August 1661 Hooke had invented and tested Townley’s hypothesis! Cohen even says “explicitly” and “unequivocally”. This is not fully documented to everyone’s satisfaction. And he accepts the “agree well” that Hooke does not accept about his own first set. Between James’s calling Hooke a liar and Cohen’s trusting a vague report as “explicit” and “unequivocal”, there is a lot of room for maneuver. Why does Cohen stress this so much? How could it escape his notice that Boyle was talking, not about Hooke, but about Dr. Power, and that Gerland said so explicitly (see note 22). Perhaps, his misreading fits the generally accepted misreading so well that he could not doubt it; but I really do not know.

Finally, as James notes, Hooke says clearly that the first table is not presented as a verification of any hypothesis, that it was performed before he had heard Townley’s hypothesis. Hooke also says that his second table is a test of that and of another hypothesis — the other hypothesis remaining both unstated and unnamed! Also Hooke tells us that his oldest *data* concerning the other hypothesis — “the other experiment” — were good from the start, yet he repeated them now! Moreover, Hooke presents the experiment to test Townley’s hypothesis as a mere variant on Torricelli’s but stresses that “the other experiment” stands by itself. That is where the action lies.

To put it differently, I think that in August 1661 Hooke had both Boyle’s and Townley’s hypotheses; that his test of Townley’s hypothesis was very unsuccessful and so he claims no priority for it, but that his test of

Boyle's hypothesis was successful and so he claims priority for it. In August 1662 or so he repeats his test of Townley's hypothesis and now gets excellent results.

In checking Hooke's text carefully, I find very few unclear or inconsistent expressions; the rest clearly disagrees with Cohen's reading and agrees with mine, except for one passage that seems to go the other way. Between the poor low pressure table and the good high and low pressure table there is the page (225) containing one paragraph on low pressure in both tables, ending with Townley's hypothesis, and two paragraphs or so on the high pressure part of the second table, one of these describing the instrument and such, and the other commenting on Hooke's experience and on the table. The first of these two paragraphs (225) ends with

> and by making several other trials, in several other degrees of condensation of the air, I found them to exactly answer the former hypothesis.

At least *prima facie*, the words "answer the former *hypothesis*" refer to "Townley's hypothesis" in the previous paragraph, the word "answer" means agree with or confirm; meaning that Hooke uses this descriptive phrase to name the law "pressure x volume = constant for all pressures, above or below one atmosphere".

It is not incumbent on an interpreter to offer a view in accord with every word in every document. This may be impossible, at least since records are not all reliable to the exclusion of all slips and errors — not to mention confusions and ambiguities. The famous economist and philosopher of science, J. M. Keynes, has declared the proper criterion in a very well-known passage (*Treatise on Probability*, Part III, Chapter 23), in which he wholeheartedly endorsed R. L. Ellis's reading of Bacon's works, in spite of its having left many passages obscure or inconsistent with itself: that interpretation is to be preferred which makes better sense of more available passages. Possibly, however, one may resolve the above difficulty by reading the word "answer" not to mean "confirm", but to mean "in accord with" or "in harmony with", similar to the use of the word "answer" in music, in the analysis of a melody. This, I suppose, is a reference not to the expression "Townley's hypothesis" of pressure, which reads:

> The other experiment was, to find what degrees of force were requisite to compress, or condense the air into such or such a bulk.

It may sound strange to say that the table answers this hypothesis rather than this question by an hypothesis, but this is true to Hooke's style;[32] other writers of the same group used even worse styles; see Isaac Disraeli's delightful and thoughtful "Calamities and Quarrels In the Royal Society".

One more discomfort: during the whole discussion there is no mention of Boyle. James asks (269) why, and answers, because no one thought much of it all. As evidence he lists Hooke's flimsy manner of claim staking.

One might indeed disagree: the importance of Boyle's work was noticed almost at once. Besides, James is partial in viewing Mariotte, but not Hooke, a plagiarist; all points on which he claims ignorance for Hooke are valid for Mariotte as well. Moreover, Hooke mentions Townley's hypothesis by name, but does not name Boyle's hypothesis. Surely he did not think Townley's work more important than Boyle's. James mentions Boyle (55) in reference to an insignificant contribution he made in his *Discourse on Colours*, and immediately after the discussion just reported (227), in reference to an even less significant contribution of his, in his (by no means original or interesting) estimate of the relative densities of mercury and air,

> the most accurate Trials of the most illustrious and incomparable Mr. Boyle published in his deservedly famous pneumatic book … .

In my opinion Hooke does not label the counterpart to Townley's hypothesis, hoping it be labeled Hooke's hypothesis or law. He hopes so, both because he establishes it empirically so much better than anyone else, because he thought about it first, and because his delayed publication was rooted in his seriousness and pedantry. Note that the rule, the first to publish is credited with priority, was not yet entrenched, and even if it were, it was supposed to pertain to the verification of a hypothesis. For example, we call the law Coulomb's that was first announced hypothetically by Franklin and Priestley! And Hooke could claim to be the first careful, and hence proper, verifier! Indeed, Cohen suggests (619b) that this is Hooke's priority.

As to the homage, it shows uneasy feelings; as if to say, I do not wish to belittle Boyle, and his name is secure anyway but mine is not yet, etc. The unease about colors, incidentally (55) is smaller, because it is less clear that Boyle deserves mention or Hooke claims priority ([Sabra] 321-3, 328).

The very fact that Boyle's hypothesis is not stated means little: Townley's is not stated either; I have explained here, and elsewhere, how the tradition reigned of alluding to but not stating hypotheses. The very fact that Hooke fails to name Boyle's law because it ought to be named after Hooke himself indicates humility — a humility first charmingly shattered by Parkinson who called his own law Parkinson's. Before that, writers who wish their names immortalized were left to find a device to indicate their pleasure, and staking claims was no exception. Thus Planck staked his claim in his scientific autobiography, not in his original and trailblazing papers.

Perhaps I should not make much fuss about such matters. But perhaps I shall be excused as providing some counter-balance to what Andrade says, and Cohen quotes approvingly,

> Hooke, who always expressed the greatest veneration for Boyle, would never have published his [priority claim] if it was likely to give pain to, or be disputed by, Boyle.

Cohen quotes two arguments from Andrade, one which Andrade quotes from L. T. More (the biographer of Boyle), and one of his own. Andrade's first argument is that Hooke would not hurt Boyle. The other is that Boyle gives priority to Townley and quotes Hooke to say he had had Townley's hypothesis before Townley. My view accords with this, but not with identification of Townley's hypothesis with Boyle's, of course. L. T. More's argument is that Boyle's law is the only quantitative law Boyle ever studied, so it is not in his character. First, this is false: Boyle had the quantitative law of proportionality of heat increment to the increment of pressure times volume — quite a quantitative idea — and he begged people to work on it. Second, there are few quantitative laws anyway. How many quantitative laws did Ohm produce? Or even the mathematical Fourier? Or even Van der Waals? And was not Edison's discovery of thermionics out of character? And Faraday's metallurgy? Galileo's astronomy or Pasteur's biology were out of character, the one having had a mechanical and mathematical interest, the other chemical. What do we know about character! Not only did Boyle have exceedingly many scientific commitments, but some of these were pressing and he could not even discharge them. Cohen's own argument is the best of the lot: Hooke's greatest enemy, Newton himself, acknowledged Hooke's priority. But though what Cohen says (620),

> any statement of Newton's giving credit to Hooke for any discovery, is to be taken very seriously,

is unquestionable, he shows not that Newton gives "credit to Hooke for any discovery", but that he credits Hooke with the table of pressures and volumes representing empirical support for Boyle's law. Indeed, I think Newton is correct in crediting Hooke both for unusual precision and for the combination of two tables in one: he, Newton, says ([Cohen] 620),

> and Hooke proved by experiment that the double and treble weight compresses air into the half or third of its space, and conversely,

and nobody before Hooke, I agree, did just this just as neatly. But this is a matter of a neat experimental proof.

And yet, the fact is, Newton does not mention Boyle. It is hard to discuss such matters. Clearly, as the quote is from a manuscript one cannot take it as definite. Cohen quotes an even less definite, but more decisive, passage from a manuscript of Newton — less definite because "juvenile", and more decisive as it does "credit … for any discovery". Except that it credits Townley: he, young Newton, says (*loc. cit.*),

> Mr. Townley's hypothesis is the dimension (or expansion) of air is reciprocally proportional to its spring (or force required to compress it). By Mr. Hooke's experience …

(Why not refer to Newton's mature passage that Gerland mentions?)

So much for Cohen's argument. He offers Newton's remark with his own interpretation. I shall now offer a few alternative interpretations of the same passages. First, one might say, Newton read Hooke here the way Cohen reads him, and entirely obliterates any difference between high pressures and low pressures. In which case Newton is scientifically correct, but historically overlooks a difficulty that once existed but had been overcome. This happens all the time (see [Agassi (a)] Section 16, *The Difficulty of Avoiding being Wise After the Event*), and so its being exemplified in a private manuscript of a young man of scientific genius and little historical interest is quite understandable.

Another interpretation rests on the fact that Newton mentions no name, as Cohen notes when discussing Boyle's law in the *Principia* (Bk. II, Prop. 23 and *Scholium*) — probably from reluctance to name Hooke. This reluctance Cohen explains as reluctance to credit Hooke with priority. It may also be interpreted as reluctance to enter other people's priority disputes. Still the absence of Boyle's name is puzzling and consistent with the previous manuscript passages. It is quite possible, indeed, that Hooke's second table did so impress Newton that he felt the need to acknowledge to Hooke priority over Boyle's hypothesis (not Townley's) that is more than public opinion would permit; that Newton would not launch a campaign for Hooke's name is quite understandable: much as he wanted public acknowledgment for himself, he was very reluctant to campaign even for his own priority.

One may offer quite a different interpretation, taking the following very seriously. With the discovery of Torricelli and with Pascal's barometry it became clear that the atmosphere is finite. Boyle compares it to a sea, we remember, assuming that it has a surface with waves and ripples and tides. Hooke was the first to use Townley's hypothesis to estimate the height of the atmosphere. He concludes, from simple calculations, that the atmosphere is infinitely high. This, of course, utilizes Galileo's theory of gravity; it assumes pressure to be a constant with respect to height. Newton assumed gravity to diminish according to the inverse square of the distance and elasticity according to the inverse of the distance, thus achieving a theoretical cut-off point, a theoretical limit to the atmosphere, where the two forces balance.

Now, consider what Newton should have acknowledged to Hooke. Today we acknowledge good first shots even when they are misses. Up to the end of last century, this was not acceptable, except for some very eccentric writers. But one could say that Hooke was the first to attempt to apply Townley's hypothesis to the estimate of the height of the atmosphere. Newton did not. His acknowledgment to Hooke, then, is grudging and far from generous. Is it also over-generous in the wrong direction as a compensation? I do not know if and how one can study all this. Nor is it clear how much Hooke's application of Galileo's gravitational theory to any distance provoked Newton to think that the theory needs modification, that the moon may be constantly falling towards the earth, and so forth. Did Newton owe anything to his reading of Hooke? If so, did he know about this debt? Was he then ambivalent about it? This is all open to further exploration.

The last interpretation I wish to mention is perhaps the sharpest — it attributes to Newton high sensitivity, intellectual and psychological. Boyle, we remember (see above) saw in air both strain and stress — extension causing stress. Now Hooke's extension of the atmosphere indefinitely puts an end to this and views all extension as expansion releasing prior compression. If so, then there is room only for Townley's hypothesis, none for Boyle's! And Hooke, indeed, has in his second table pressures below and above one atmosphere!

And so, perhaps it was the desire to avoid mentioning Boyle's error and a subsequent absurdity concerning priority that has further complicated the picture!

Newton, then, assuming the elasticity of air to be a result of sheer expansive force, and assuming gravity to be variable, tried to estimate the height of the atmosphere. Here he solved a few interdependent problems quite satisfactorily, as if by miracle. The weakest point in all this complex reasoning is the application of Townley's law to very low pressures, particularly since it leads presumably to the difficult conclusion of unlimited atmospheric height. Here Newton accepts Hooke's experimental evidence as a crucial factor.

Hence Newton's ascription is even historically very accurate and uncontested. Here is the relevant quotation in full:

> In just the same remarkable manner [air] rarefies and is condensed according to the degree of pressure. The whole weight of the incumbent atmosphere by which the air here close to the Earth is compressed is known to philosophers from the Torricellian experiment, and Hooke proved by experiment that the double or treble weight compressed air into the half or third of its space, and conversely that under a half or a third or even a hundredth or a thousandth part of that [normal] weight [the air] is expanded to double or treble or even a hundred or a thousand times its normal space, which would hardly seem to be possible if the particles of air were in mutual contact; but if by some principle acting at a distance [the particles] tend to recede mutually from each other, reason persuades us that when the distance between their centres is doubled the force of recession will be halved, when trebled the force is reduced to a third and so on, and thus by an easy computation it is discovered that the expansion of the air is reciprocal to the compressive force.

This discussion is particularly enlightening when one considers the following facts. If Newton's theory of inverse distance force is true, then Boyle's law is absolutely true for any degree of rarefaction and for any degree of compression short of the ones involving sub-atomic distances (which are *a priori* excluded anyway). Was Boyle's law considered absolutely true? Newton seems to ascribe to Hooke the affirmative answer. E. Mach, ([b] 14) says "already Boyle himself viewed [Boyle's law] as not entirely correct". If both Newton and Mach are historically right, and I think they are, then, doubtless, Newton's ascription to Hooke becomes very powerful, especially in an era when an error was to be overlooked and certainly not to be used for credit.

11. Conclusion

My interpretation, then, leaves a few gaps, particularly concerning a sentence or two in Hooke's *Micrographia*, and perhaps young Newton's understanding of Hooke. But note: canons of historical interpretation cannot be as strict as those of natural science: there are any number of reasons why Boyle, Hooke, or anyone else may have been unclear. How then should one piece together an interpretation? The usual rule is, one takes the interpretation that makes better sense of more historical material. The question remains, what do we mean by better sense? I have complained before that unlike all other historians, historians of science too often fall prey to the misconception of modernization: the idea that bringing an old text to be in better accord with today's views is alluring but is highly un-historical.

This misconception makes one ignore the difference between pressures above and below one atmosphere. It leads one to conclude with Webster and Cohen that priority goes to Power and Townley. Their view is marred by a few difficulties, some of which Webster glosses over, some of which Cohen discusses. Indeed, I understand that Webster may endorse the criterion in question, and refuse to see the theoretical difference between high pressure and low as anything other than a remnant of scholasticism, and leave it at that. But Webster himself stresses that experimentally high pressures were easier to measure. It is this difficulty that Cohen tries to remove.

Cohen's last paragraphs indicate that he is still not satisfied. He claims there that Boyle had extended the Power-Townley experiments of low pressures to high pressures — which is a valid though false conclusion from Webster's faulty chronology, and which solves some difficulties for Webster. But this is not all; for, Cohen parenthetically adds (620b):

> The question whether such an extension [as Boyle's] be significant or not depends on whether the limitation in the first instance to pressures less than one atmosphere resulted from a failure to devise an instrument to make a test of this additional range or from a psychological inability to recognize that the law for the rarefaction of gases might equally be a law for compression of gases.

Cohen kindly refers to the present work in an earlier draft (actually an appendix to my doctoral dissertation) and reports that I discuss this question. I do (except that I would omit the word "psychological"). But the question as Cohen puts it, implies my view, not his. He is here inconsistent — seemingly without notice. His seeming inconsistency is easy to repair, but at a very high cost. For, my view rests not only on the historical supposition — which even Webster endorses — that Boyle's *Defence* is chronological, but also on the historical claim that high pressures were easier to examine than low pressures — a fact which Webster stresses and explains at length. If Cohen wishes to stick to his view, if he would retain the question he kindly attributes to me, and if he would rectify the impression that he is inconsistent, then he has to deny that the low pressure experiments were harder to observe than the high.

Moreover, as no previous writer has noted, Townley's first idea concerning low pressures, of which Webster speaks at length, is the one that Boyle converted to an idea concerning high pressures so as to examine the two atmosphere cases — which is the easiest, simplest, most obvious, and the first real success. Then Townley (and probably others) reverted to Townley's original idea, and applied to it Boyle's improvement. This inner logic of events is my chief argument. Finally, may I stress, with Mariotte's contribution one major factor in this logic has disappeared: Mariotte's measurement of high pressures and low pressure is of practically the same case.

And so, my view is of collaboration rather than of simultaneous discovery. I do agree that some measure of simultaneous discovery is necessary. We all rediscover constantly parts of our heritage that have not been articulated to us, and what young learners do in later generations, contemporaries often have to do in the course of their research. But it is the act of collaboration that is both more important to notice, and more difficult to sort out. For example, Mariotte's contribution has thus far not been appreciated by most historians of science.

Boyle, following Bacon, has instituted rules of crediting priority and public recognition of other contributions to science as a reward and an incentive. The infancy of modern science was much more problem-ridden than many would acknowledge, and Boyle's rules may have made all the difference between its survival and infant-mortality. The success of Newton, and his middle-class attitudes (so well discussed by Augustus deMorgan), has led to an exaggeration and a perpetuation beyond reason.[33]

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NOTES

* The present chapter was initially a brief appendix to my doctoral dissertation, University of London, 1956, unpublished. This is no priority claim, since my main task here is corrective and since the correction is due to Ernst Gerland (1909 and 1913; see Bibliography). It received many rejections before it appeared in the learned press in 1977. During that time it grew to become 30 times longer — in attempts to cope with comments by friends and colleagues and much more so by editors and referees. Most editors who have rejected early versions have claimed that the error I correct is too obvious to be rectified in the learned press. It still persists and I have found it repeated in respectable histories of science of the twenty-first century (although happily not everywhere). Some of the comments I received saved me much embarrassment by correcting some of my worst errors; other comments were very good suggestions and I tried to use them as best I could (especially those of Daniel Greenberg and of Bernard Cohen); still other comments were of the referees. Some referees' comments rested on errors in elementary physics. So I added a brief section that features some elementary physics. A scholar who was a learned historian of science and an editor of a leading journal in the field declared my presentation most unsatisfactory, as it stands somewhere between the way it looked in the seventeenth century and the really up-to-date way. I agree, but I leave my presentation as it is, since it is a simplified, serviceable and not misleading. That editor was more displeased with my criticism: he was willing to publish my presentation without my critical comments: he found all polemic pointless. I thankfully declined.

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The translations from Gerland, Rosenberger, and Heller are mine; the beginning of the translation from Rosenberger, however, is from Ornstein (52).

1. The English version of Bacon's *Parasceve*, his *Prescriptive Toward a Natural and Experimental History* is appended to Fulton H. Anderson's edition of Bacon's *Novum Organum*, 1960. R. F. Jones [b], is the only historian who has published extensive studies of the rise of the inductive style. Unfortunately he omits mention of Bacon's prescriptions, and their role in the development of the inductive style.

   For the history of the inductive style see [Jones, (b)] 19, 21, 33, 335 and [Agassi, (a)] 93, 97, where Maxwell is quoted praising Faraday's frank speculative style but recommending Ampère's inductive style. See also [Agassi (c)], and consult Index, Art. Style.
2. For Galileo on the telescope see his *Opere*, (Favaro edition), 3:60 *(Sidereus*); 6:258 (*Saggiatore*); Bacon's view is expressed in his *New Atlantis* and *Parasceve*; Boyle's in his "Proëmial Essay" to his *Certain Physiological Essays*, 1661 and elsewhere; his proposal was translated into a rule proposed by the president of the Royal Society, Lord Brouncker, and seconded by Boyle and adopted early in the day. I have discussed all this in detail in my unpublished doctoral dissertation (University of London, 1956).

   A curious case of a priority claim published as a patent application rather than as a paper in a learned periodical is Edison's invention of the diode-tube. See Matthew Josephson's life of Edison.
3. It is amazing that historians take both scientific essays and scientific publications for granted; see e.g. [Kuhn] 20:

   > Given a textbook, however, the creative scientist can begin his research where it leaves off and thus concentrate upon the subtlest and most esoteric aspects of the natural phenomena that concerns his group. And as he does this, his

> research *communiques* will begin to change in ways whose evolution has been too little studied but whose modern end products are obvious to all and oppressive to many. No longer will his researches be embodied in books addressed … to anyone who might be interested in the subject matter of the field. Instead, they will usually appear as brief articles addressed only to professional colleagues, the men whose knowledge of a [shared paradigm] i.e. textbook can be assumed and who prove to be the only ones able to read the papers addressed to them.

Kuhn, too, takes all this for granted; only he wishes to have the evolution of periodicals studied.

4. See Galileo, *Opera,* 5:95, that is his open publication, and 17:296-7, that is a much later private letter. In the former he declares that he should not be credited because instead of rushing to the press he wanted to prepare correct results for publication. (This problem, we shall see later, occurred to Hooke. Cavendish has priority for the discovery of the decomposition of water because James Watt took his time in a similar manner. The problem is still unsolved.) In his letter and private communication he declared that the person who published first heard it from Galileo himself *via* an intermediary — that raises a still more difficult problem.

   Kepler's oversight is discussed in Koestler, 1959.
5. For details see Section 5 below and references there. The wealth or poverty of the literature on Boyle can be read off from Fulton's bibliography. Readers may not easily learn from that, however, what incredibly bad luck Boyle had with biographers. Why William Wotton never wrote his intended life of Boyle is unclear; that Thomas Birch was too busy to do him justice is obvious, and at least he published his letters; of the rest of the Boyle biographies the less said here, the better. The most respected one is L. T. More's and I have failed thus far to publish my views on More because my paper in which they are discussed contains historical conjectures, and I found editors of history of science journals still reluctant to publish conjectures. Indeed, L. Pearce Williams, "Should Philosophers Be Allowed to Write History?", *Brit. J. Phil. Sci.* 26 (1975),241-253, which is a review of my *Faraday As a Natural Philosopher* (1971), blasts at me for daring to publish historical conjectures. I concede, however, that since historians of science more often than not study the classical period, that is also the inductivist period, they may easily fall prey to the demands of the inductive style.
6. Michael Hunter and Edward B. Davis, *The Works of Robert Boyle*, 14 volumes, 1999-2000.
7. Hooke's *Posthumous Works,* page iii; McKie [b] 28; Fulton [c] 123.
8. For details see my review of Margery Purver's book, "The Origins of the Royal Society", *Organon,* 7, 1970, 117-135, reissued in my *Science and Society,* 1981, as Chapter 25.
9. See my "Sir John Herschel's Philosophy of Success", in *Historical Studies in the Physical Sciences,* 1, 1969, 1-36, reissued in my *Science and Society,* 1981, as Chapter 27
10. Cohen says (621b, note 18), "Indeed, Hall, A. R., and Hall, M. B. (399) were the first to call attention to Newton's citation of Hooke in relation to Boyle's law." Cohen's reference to Gerland (618b) is "The Towneley-Boyle relation was studied carefully in 1909 by Prof. E. Gerland, who concluded that, despite the suggestion made by Towneley, the credit for the law should be assigned to Boyle (and surely not to Mariotte)".
11. The proviso, qualifying Boyle's law to constant temperatures alone: is often omitted in contemporary works, but it is never ignored. Boyle even worries often about unnoticed temperature variations, that Linus often blames on Boyle's results.

12. Notice that in effect Rosenberger says the following: Boyle's experiment with compressed air was not initially connected with a quantitative hypothesis, as evidenced from the fact that it was meant as a reply to Linus. He tacitly agrees with all inductive philosophers that criticism is a preliminary to constructive scientific work, not itself constructive. A similar opinion is expressed in Webster (b) 467 although Boyle came to the view that his reply had to be quantitative as Huygens noted in his comment on this point, [Rigaud, 1841] 93:

    > "I was at first astonished to see that he has taken the pain to write so big a book against objections so frivolous as those of his two adversaries. But having begun to peruse it and seeing that among his refutations he has inserted many new discoveries and observations not yet seen I wished it had been bigger."

13. Both Webster (a, 227) and Cohen (6l9), wonder why Boyle refrains from making an acknowledgment to Power here. Webster explains (and Cohen enthusiastically agrees) the omission as an oversight as the result of a cumbersome title to Power's section on the Power-Townley experiment on rarefaction. This is too much of an insult to Boyle, who had read many a cumbersome title and a cumbersome report very carefully, and who may have had a few conversations with both Townley and Power now and then while they functioned as his assistants, and who further did refer to Power's tables of rarefaction in a passage that Cohen misread and Webster ignored. For Boyle's reference to Power see note 22 below.
14. Hooke has described all experiments with pressures below one atmosphere as a variant on Torricelli's experiment, but of high pressure as "another experiment". See below.
15. These two sentences constitute ample reply to Webster's and Cohen's attribution of the law to Townley or to Power and Townley. See note 10 above; and see note 27 below for Webster's reading of Boyle's law into the experiment that Boyle here simplifies to obtain the quantitative law.
16. Webster and Cohen read Boyle's law into Power's measurements of high altitude experiments performed prior to Boyle's vacuum-pump experiments. Reading Boyle's text carefully shows Webster's reading to be hindsight with an element of truth in it that Boyle fully stressed. Note also Boyle's uneasy vacillations between the quantitative and the qualitative. Psychologically it indicates a reluctance to use exact measurements as an argument and intellectually it indicates reservations.

    These may rest on Boyle's skepticism about any exact measure at such an early stage of crude experimentation. Or they may rest on his fear that too much calculation may drive amateurs away. (After all, his attraction to chemistry and pneumatics rather than the more traditional astronomy and rational mechanics is partly due to the openness of these fields to amateurs.)

    Not so: there was an excess of quantitative *data* available, and so he had to discard some, and to decide on the limits of accuracy recommendable. This is standard practice, even though most philosophers of science think of precision as something limited only by the grossness of our experiments [Agassi (e)]. Even were the mistakes in Power's tables mere misprints, as Webster insists (b, 475), the difficulty Boyle faced was real enough. This kind of difficulty is ever present, and they are especially hard one for pioneers. Even if he saw a better manuscript, he was not freed of it. Modern writers on the subject find it hard to see what the fuss is about. The qualitative hypothesis was known to diverse writers even before Boyle began his researches, and young Boyle knew it too [Webster, (b)] 467); the difference between the qualitative and the quantitative versions of the law ("a increases together with b" and "a increases proportionally to b") is so small, that one may confuse them in a careless formulation of the law. One may well remember how few quantitative laws physics had at the time — Archimedes', Snell's, Galileo's. It is time to notice the problems anyone faces when attempting a quantitative law, especially before the development of approximation methods (by Newton and his followers).

An example illustrating the difficulty is a passage from Roberval quoted in the Latin that, Webster rightly says (Webster, (b) 450), is reminiscent of Boyle's law. On the same page Pascal's half-blown football that expands on a mountain is quoted, but with no reference to Boyle — even though Webster knows (467) that Boyle knew of it early in his career. Webster expects to see in Roberval more than there is in Pascal. He is in error. What Roberval was commenting on is his own experiment of adding equal quantities of air to the top of a Torricelli tube and seeing the mercury drop. This experiment of Roberval is a predecessor to Townley's that is a predecessor to Boyle's experiments that culminated with Boyle's law.

17. Boyle's own rather contrary view ("Proëmial Essay") notwithstanding, he did hope to read a quantitative hypothesis off the *data*. After he performed the experiment, he indicates (see below), he had a definite quantitative estimate in mind; but a few steps (and a few days) earlier he was still groping, hoping that the *data* would provide the exact quantitative hypothesis. This is the only strictly Baconian passage that I have found in Boyle's works. But I am possibly reading too much philosophy into a casual narrative.

18. Gunther's conjecture [Gunther, p 731 is that "we" designates Hooke. It rests on Boyle's near-blindness. The question whose eyes Boyle used is not too important. Still, we know that Townley assisted Boyle in the quantitative experiments; we have no evidence that Hooke did.

19. Gerland quotes ([b] 499, 500) the above passage, and its continuation, quoted here, as sufficient evidence against the claim that Boyle's law is Townley's. Gerland failed to get his point across. Anyone who still insists that Boyle admitted that Townley was the discoverer of Boyle's law should, in deference to Gerland, read Boyle's passage carefully and offer a detailed and different reading of it. One should, in particular, explain the systematic use, shared by Boyle, of "compression", "condensation", and "elasticity" for high pressures, and "rarefaction", "dilation", and "elater" for low pressures. See also [Tait (b)], 73 and 75.

20. My doctoral dissertation, *The Function of Interpretations in Physics,* Pt. II, Ch. IV, sec. 8, (University of London, 1956, unpublished) discusses Boyle's (seemingly unconscious) plagiarism from Browne, *Pseudodoxia Epidemica,* 1646, Bk. 2, Ch. 2, 59 in his *Usefulness of Natural Philosophy,* written in 1648 or 49 and published much later.

21. Webster noticed ([Webster, (b)] 482) that Boyle expressed his wish to add an appendix with Townley's result as if it was performed. Townley was both attracted to the idea of publishing and inhibited about it, it seems, and Boyle tried to help but failed.

22. Webster studied Power's manuscripts. He says, "Although the manuscript emanated from Power, it is quite possible that Boyle overlooked Power's part in the work … Examination of … Boyle's appendix i.e. *Defense* shows [sic!] that the author leant heavily on the information from … Power's manuscript. Power himself is mentioned by Boyle …" He later suggests ([a] 227a) that Boyle used Power's tables ([b] 483) — an allegation contradicted by Boyle's narrative quoted here — but "did not understand the significance of the hypothesis which was suggested at the end of the experiment."

Oddly, Boyle wishes to "make mention" of Power but only refers to him as "the same person I took notice of in the former Chapter, as having written something about rarefaction". We have here reference to two manuscripts by Power ([Webster, (b) 3, 481].

Cohen says (6l9a), "Boyle mentions Townley but not Power. This is explained by Webster" as an oversight and as a result of the fact that the title of Power's manuscript includes Townley's name, and others', but not Power's. Cohen takes it for granted, and he is in error, that Boyle's reference to "the same person I took notice of in the former Chapter" is Robert Hooke. Gerland says ([b] 500), that person is presumably Power. Since Cohen views Gerland's study as careful, he might have explained why he said that it was Hooke and not Power.

23. Kant says, *Critique of Pure Reason,* 8278, 247 of Norman Kemp Smith translation, 1929, 1961, " ... *impenetrability* serves in our *empirical* intuition of matter"; and, again, A618, B646, 516:

    > In fact extension and impenetrability (which between them make up the concept of matter) constitute the supreme empirical principle of the unity of appearance; and this principle. so far as it is empirically unconditioned, has the character of a regulative principle.

    Kant's "empirically unconditioned" means, neither verifiable nor refutable. His claim that impenetrability is a regulative principle conflicts with his *Metaphysical Principles of Natural Philosophy* [Agassi (d)].

24. For more detail, see my doctoral dissertation, mentioned in note 20 above.
25. See [Agassi (e)] chapters 8-10.
26. Hooke says in 1678, "It is now about eighteen years since I first found it, but designing to apply it to some particular use, I omitted the publishing thereof" ([Truesdell] 54); I suppose sixteen or seventeen is more accurate than "about eighteen".
27. Webster cites ([a], 227b) from Power's book a wording of Boyle's law that amounts to saying, $p_1 \cdot v_1 = p_2 \cdot v_2$,. He takes it for granted that the tables and formulae from the book occur in the 1661 manuscript that Boyle saw. He has the choice of accusing Boyle of plagiarism, stupidity, or oversight, and suggests mild doses of each of these remedies. Evidence clearly indicates the opposite, no less Webster's own crediting Townley with incompatible contributions. Webster uses tables in which he first streamlines and corrects some "profound typographical errors" in them ([Webster. (a)1 227b), last paragraph).
28. Webster explicitly and rightly assumes that the relevant part of Boyle's narrative in his appendix is chronological ([Webster, (b)1 486, note).
29. Webster (b) has an interesting discussion on the history of "elater" and "elasticity"; section IV and Appendices II and IV. His discussion amply shows that "elater" refers to elastic behavior under low pressures, unlike "elasticity"; this is true for the later period, not for the period when it was introduced and used.
30. See Faraday, *Exp. Res. Electy.,* for example, Volume I, §704, where Faraday explains his design of a Voltaic electro-meter as rooted in his desire for increased accuracy, and §738ff., where qualitative experiments are discussed — and then reported — before quantitative details are introduced.
31. Note that Cohen corrects the misprint in Hooke's text.
32. Similarly Hooke's use of "hypothesis" in *Micrographia,* 67, is problematic — see Sabra, 328. Note also that the statement of Descartes' rejected hypothesis (*Micrographia* 60, 62) is very clear; Hooke's hypothesis (64) is labeled unabashedly "short definitions"; the "hypothesis" (67) may well be this "short definitions", but the clarity of the matter is not exactly perfect.
33. See also my "Fighting the Philistines" *Philosophia*, 4 (1974) 163-201, section II and final paragraph, reprinted in my *The Gentle Art of Philosophical Polemics*, 1988.

5. Theoretical Bias in Evidence: A Historical Sketch

0. An Introductory Apologia

All my efforts to present the following historical material without any complaint made friends and colleagues misread and express puzzlement at what I intended to say. The kind comments from the editor on the final draft finally made me decide to declare my hand clearly as follows.

The studies of theoretical bias in evidence are these days developed by many clever psychologists, social psychologists, and philosophers. It therefore comes as a surprise to realize that most of the material one can find in the up-to-date literature repeats discoveries due to the heroes of the present sketch, namely Galileo Galilei, Sir Francis Bacon, and Robert Boyle; William Whewell, Pierre Duhem, and Karl Popper. We may try to raise scholarly standards by familiarizing ourselves with their ideas and studying them with a little appreciation.

A little familiarity and a little appreciation, not consent or assent or agreement, is what I seek. My disagreements with each and all of these writers are to be found in other writings of mine. Here I wish to direct the attention of the learned readers to the overlooked classical writings and invite them to throw a new glance at them (see bibliographic note at the end).

The main hero of this sketch, however, is Sir Francis Bacon. In the eighteenth century his status as a leading thinker was quite exaggerated and invited the debunking he received in the nineteenth century. The chief editor of his works, Robert Leslie Ellis, began his work as an act of hero-worship and ended by condemning him as an unoriginal thinker, a plagiarist, and an author who violated his own principles when he described the process of induction (since he permitted the formation of hypotheses). Justus von Liebig exposed his plagiarism, ignorance, gullibility, and scientific incompetence. Severe as Liebig's judgment was, his strictures were just and unanswered, and so his is the last word, all the many later works on Bacon notwithstanding. It is admittedly dangerous to cite Bacon to support any interpretation of his philosophy—since he was so often flagrantly inconsistent. Nevertheless, a person considered a leading thinker by both Immanuel Kant and Solomon Maimon cannot be dismissed. I have discussed his enormous importance elsewhere. Here I should observe that he doubtless made vital discoveries concerning perception. In particular, he knew the difference between sense illusion and theory-laden observation whose error is theory-based; he knew the difference between theory-ladenness on account of some very general features of our faculties (or our perceptual-cum-cognitive apparatus) and theory-ladenness on account of a specific theory, be it Aristotle's or Gilbert's. And he observed both the impact of a specific theory that is metaphysical, which makes one observe everything in its terms, and the impact of specific local hypotheses that refer to a small sector of our experience. Each hypothesis make one see only the evidence that corroborates it, he observed, and ignore or dismiss all evidence to the contrary. When one notices that these facts still occupy the writings of the latest commentators on the matter, one cannot but gasp in admiration.

Nor is it a matter of sheer historical curiosity. Whewell refuted Bacon's hypothesis that we are captives of our hypotheses, by arguing that critically minded science is the critical test of theory, so that we can employ hypotheses without being imprisoned within their frameworks. This way a new vista opened for philosophy. . And, I surmise, Whewell's philosophy helped Duhem develop his justly admired conventionalist-instrumentalist philosophy of science. This included his claim that a new framework does not supersede the old one. This claim is these days hotly debated and is known by an oxymoron anachronistic label, as the Kuhn-Feyerabend incommensurability thesis.

Perfectionists know that seeking high appraisal while refusing or resenting low appraisals is improper. They often try to be fair, however reluctantly, and even though only after they pour unjust wrath on their critics. They are thus able to face critical appraisals in a somewhat adult fashion even though only despite themselves. This takes them much effort, as their initial reaction is distinctly not adult. Only after they go into lengthy processes of denial and rejection can they face the criticism in a more disinterested manner and appraise it in a balanced way, with some measure of a sense of proportion. This is more expensive than they will admit. It would be nicer had they noticed that they should be a bit more perfectionists in their attitude to criticism and to the low appraisals of their output, had they tried more sincerely to behave like adults. This would kill their perfectionism, and they will then decide either to cease producing or to approach their output more judiciously.

The rest of my complaints are not important for the avoidance of confusion, so I will drop them. Let me repeat, my aim is to present the still-topical material with a historical perspective; complaints are better overlooked whenever possible.

1. The Legitimization of Science: Bacon versus Galileo

Bacon and Galileo published, more or less simultaneously, the claim that empirical evidence carries with it theoretical bias. Priority should presumably go to Bacon, for whom it was a very central point that he elaborated upon in all of his writings. He made the claim for the purpose of debunking the inductive basis of traditional theories. Every theory can be inductively based on evidence that is biased in its favor. The bias in favor of a theory is given both in the choice of evidence as significant and in the interpretation of the evidence in the light of the theory. This claim is dual: we use a theory both to decide on the significance and the interpretation of facts, and, presenting a series of such interpreted facts amounts to neither more nor less than a round-and-about way of presenting that theory. It is intuitively obvious that this can be done and it is both intuitively and logically clear that the support a theory received from such evidence is invalid: it is circular and unconvincing. Given two competing theories and a given pool of information, some of the information can be used twice, once to support the one theory and undermine the other, and once the other way around. The advocates of the competing theories disagree about the facts: what facts are significant and what is their verdict. This kind of disagreement is rooted in the erroneous theory that they support their respective theories by reference to facts. This way they achieve a stalemate. This illustrates the truth of the Bacon-Galileo thesis that all information is theory-laden, so that factual testimony is biased, so that it is invalid.

The Bacon-Galileo thesis is repeatedly discovered by a number of philosophers and social scientists from different disciplines. Each generation sees the thesis ascribed to some different thinkers. These days it is most often ascribed to Maurice Ginsberg or to Gordon Alport or to Leon Festinger, but things are changing. The ascription is often to slight variants of the Bacon-Galileo thesis. We may therefore prefer to leave the thesis and look at the facts of the matter, as was done in the end of the previous paragraph. Except that the presentation of the facts in the previous paragraph is also theory-laden. Hence, we may have to live with the existence of different variants of the Bacon-Galileo thesis and only attempt to observe the significance of the differences, so as to be able to ignore variants whose difference do not make much of a difference, to echo a wise dictum by William James.

The major difference in variants of the Bacon-Galileo thesis is the one between Bacon and Galileo. Bacon and Galileo said, if one has a theory it biases one's perception; hence, they said, one should take care to approach the facts with the right theory. But Bacon was convinced that the right theory must be properly based on facts. He therefore claimed that one's very first scientific act should be the observation of facts with no theory in mind, the unbiased observations, namely, the uninterpreted ones. These, of course, would be unordered as to their significance and unclassified—just a heap of observations. This looked to Galileo to be a monstrosity. He was convinced that without geometry one cannot observe facts—one might as well see the moon jump from one roof-top to another like a cat while one walks in a moonlit city street. Geometry must, therefore, precede observations, and thus it is not founded on them, but on *a priori* intuition. Intuitions about space, time, and causality comprise the framework preceding all experience, he suggested, as did Kant; and both took this to be the strongest case against empiricism.

The discussion of science that took place between the early seventeenth century and the early nineteenth century was very general and limited to more or less this point. The center of debate was epistemo-logical: how is knowledge justified. The *a priorists* began with the justification of the most universal intuitions and the empiricists with sensations as the most basic observations. These basic observations—sensations or sense *data*—were deemed not biased, resting on no theoretical basis. In particular John Locke and his followers attempted to present sensations as not dependent in any way on the validity of Euclidean geometry. George Berkeley and David Hume even questioned this validity. The *a priorists*, on the contrary, insisted on the need for an *a priori* valid framework to insure that the theoretical bias of our observations is innocuous. Science, as usual, lies in between the two extremes. In empirical science sensations are hardly ever mentioned and its framework is taken for granted when experiments and observations are reported in its literature.

2. The Scientific Tradition Since Robert Boyle

The tradition that was most strongly represented in the literature of empirical science was based on opinions of neither empiricist Bacon nor *a priorist* Galileo, but skeptical Boyle: his philosophy was elaborate, detailed, eclectic, and incredibly famous. Most of it is intentionally not relevant to the point at hand, which concerns techniques of reporting scientific information in the learned press.

Boyle decreed a few very simple rules. They were endorsed by the Royal Society of London and its daughter societies and so were absorbed into the ideology and the practice of the scientific tradition—though the application of the traditional standards is not always strict. (The result of this laxity is at times happy and at times regrettable.)

The first claim of Boyle was simple. It is only dogmatism to ignore information only because it is interpreted in the light of an objectionable theory, and the dogmatist is the loser. It is a challenge for one who deems information biased to couch it differently. This is Boyle's principle of methodological tolerance. In particular, said Boyle, when he interpreted the elasticity of air as caused by springs, he was not using the established theoretical framework. But since from the established theoretical framework one has to explain the elasticity of springs, the reduction of the elasticity of air to that of springs is progress even from the viewpoint of the establishment, as it is the reduction of two difficulties into one.

Once theoretical bias is so legitimized, the problem arose, what is theory and what is fact? To emphasize the importance of this question, let us notice that to Laplace the certitude attained by Newtonian mechanics seemed so perfect that he unhesitatingly ascribed to it the status of a fact of nature. True or false, certain or doubtful, we do not share his view and consider it a theory proper, not an observed fact. If we insist it is a fact, then we still wish to know what fact is observed, what not.

The default tendency is to consider sense *data* observed facts. Let that be so. It is irrelevant to our purpose. Sense *data* may be the ultimate basis of all scientific theory. If theory is based on information, and information is interpreted, we may wish to distinguish between its theoretical part and its uninterpreted part. The theoretical part then is based on information that is either uninterpreted or partly interpreted and so in need of further foundations. If all theory is well founded, then ultimately it must be founded on unbiased information and so on sense *data*. This should be the analysis that empiricists should declare possible. True or false, the view in question is the result of an analysis, not a straightforward report. Once we agree that the scientific empirical literature reports interpreted observations but not theories and not sense *data*, we want to have a clear demarcation between information and theory.

Boyle demarcated them as follows.

(B1) Observation reports are statements that eyewitnesses can report on the stand.

(B2) To count as scientific they must be reported at least in two independent reports and must be declared repeatable.

(B3) The advantage of an observation that has scientific status is that in any conflict with a theory it always has the upper hand.

It may be observed that Galileo, Bacon, Descartes and Boyle all made the demand for repeatability as a mark of the credibility for science—as an expression of exotericism, as a part of the opposition to esotericism (especially to alchemy). Yet Galileo explicitly rejected Boyle's Rule (B3) as he expressed profound admiration for Copernicus for his refusal to accept the evidence from Mars's brightness that failed to fit into his system. Clearly, contrary to Galileo's reservations, Boyle's Rule (B3) was essential as an expression of empiricism: hypotheses are doubtful but observations are not. Yet Boyle knew that this status of exemption from all doubt holds at most only for theoretically unbiased observations, not for ordinary scientific observations. So he granted these no more than moral certainty and he characterized them morally, not philosophically, by relying on court procedures. He also knew that an eyewitness can never make a claim for repeatability, but at most a claim for successful repetition.

Court procedures in Boyle's time were not sufficiently clear to warrant Boyle's reliance on them, since in his days witch-hunts were quite common and he opposed them as a matter of course. Yet his idea was quickly adopted by courts all over the civilized world, so that eye-witness reports were supposed to be not theory-free but as straightforward as to count as unproblematic. Courts also demand, to this day, that when emphasis on repeatability is essential, witnesses count as expert witnesses, not as eye-witnesses, so that their status is different. (They can be countered by contrary expert testimony.) This seems to settle matters for most court procedures, but not for science. At least the generality of a generalized observation must remain clearly hypothetical. Hence, Newton felt the need to add to Boyle's rules one more:

(N) When refuted, a generalization of an observation should be qualified and endorsed in its new qualified form.

This is a very important rule that does indeed give a sense of completeness to scientific procedure. Yet, like Boyle's rules, it was hardly noticed by philosophers. The reason is apparently no more than a historical accident. As long as the controversy between philosophers centered on the means of justification of science in general, neither Boyle's nor Newton's practical legislation mattered much, since the debate was on a general matter of principle whereas the rule came to distinguish in practical scientific affairs between the admissible and the inadmissible. For a simple instance, Boyle demanded that every new fact be published with no further ado—if it passes his criteria, of course. As to theoretical papers, how much they had to be based on fact was never determined, but which facts may be used for or against a theory was determined by Boyle and Newton.

3. The Rise of Modern Methodology: William Whewell

The picture altered when Newton's theories received the status of established unalterable truths. And with that came their empirical justification and thus, as Laplace observed, empiricism won over *a priorism*. The picture altered again when Newton's optical theory, his corpuscularian theory of light, was deemed superseded. The date for this event is usually declared to be 1818, though it is hard to see how at all this can be precisely determined since throughout modern history some significant thinkers sided with waves and some with particles.

When the Newtonian optical theory was deemed rejected and the Newtonian mechanical theory, especially his theory of gravity, was upheld, better criteria than either empiricism or *a priorism* were urgently required and had to be devised; the old ones were too general. In 1830 Sir John Herschel tried to sharpen Bacon's ideas so as to be able to show that the *data* on which one of Newton's theories rested were uninterpreted and those on which the other did were interpreted: and, we remember, according to Bacon, only uninterpreted *data* were kosher. Herschel's work was not taken to be a success.

Enter Dr. William Whewell. Under the influence of Immanuel Kant he declared all *data* interpreted, since they are couched in the language of space, time, and causality. Also, Whewell himself performed observations to test Newton's theory of gravity on earth, and he knew how sensitive the outcome of an experiment is to the assessment of space-time coordinates. Nothing is easier than to secure success in such experiments than by the use of the tested theory in order to assess coordinates. Hence, Bacon's strictures were certainly valid.

How then do we distinguish valid and invalid *data*? Why was only the empirical support of Newtonian optics invalid but that of Newtonian mechanics? This was Whewell's chief question.

Given that in every stage of scientific progress there are facts and theories, Whewell claimed the following.

All the facts are theory-biased, but not all are deductively explained

Science comprises attempts to invent new theories that explain some facts and some theories.

Tests subject theories to risk of refutation, and usually they refute them.

A theory is verified when it withstands a test. The benefit then is both of new *data*—the result of the test—and of the validity of their interpretation.

Theory-bias is here a matter of degree. It is one thing to say that no observation is free of theoretical bias, and another thing to say that an observation is generated by a theory. In an unscientific context things are relatively simple. Even then we may be using a theory as we observe a fact; and this may well render our observation invalid. But we do not usually attempt to observe the facts we see; least of all do we make intellectual efforts when observing. Nor are we aware of the theoretical bias we employ (unless it is pointed out to us). In the contest of science things are different. The stars we normally see with no effort are described differently in the scientific context: in a star catalogue they appear in a manner not available to the scientifically untrained. The more advanced scientific observations invite more intellectual effort. The claim that our observations are involved with interpretations (whether we like it or not) is important just because we use them as empirical foundations of theories. This exactly is what makes them suspect. The claim that the more advanced theories are, the more interpretative their empirical foundations are, is what makes these empirical foundations all the more suspect. According to Whewell, only by severe tests leading to new facts allay this suspicion.

The crowning success of Whewell was his ability to contrast the foundations of Newtonian optics with those of Newtonian mechanics. Newtonian optics was never risked by tests: it was repeatedly modified ad hoc in order to accommodate new facts. By contrast, Newtonian mechanics was severely tested and came out of the tests most successfully, thereby enriching the stock of empirical knowledge.

4. The End of Finality in Science: Pierre Duhem

Whewell's marvelous edifice collapsed when Newtonian mechanics was superseded. Before that it was found wanting. Before the end of the nineteenth century Duhem argued that all scientific evidence is theory-laden and that therefore the confirmation it offers to theories is useless. Duhem inverted every point Whewell had made.

(D1) Theories serve as classifications of diverse items of information by deductively incorporating them; but they do not explain, since explanations are realistic and thus have metaphysical import and thus ruin the unanimity that characterizes science.

(D2) Classifications are improved so as to accommodate ever increasing numbers of items of factual information.

(D3) Classifications are not risked by tests and so cannot be confirmed.

(D4) The incorporation of a new prediction into an old classification is done tentatively, to be reaffirmed only after the prediction is verified.

Otherwise the incorporation the new recalcitrant item of information is deleted. Instead, a limit to the applicability of the classification is recorded. A modification is invited to existing classifications with the aim of incorporating into them new item of information, including the recalcitrant ones.

The fact that a piece of scientific evidence is theory-laden and that the theory is open to modification meant, according to Duhem, that scientific evidence, too, is open to modification. This naturally incorporated and extended Newton's rule (N): a refuted generalization is not rejected but modified. Since evidence is theory-laden, diverse theories are operative in new predictions. When a prediction is refuted, there is no telling which of the various theoretical items employed in the prediction is at fault. There is then no telling which of them invites modification.

According to Duhem the refutation of a prediction does not refute a theory but only its application to new cases. The refuted application is of the set of theories, not of any single theory. Hence no single theory can be confirmed. The experiment that refutes a given theory and confirms another is known as a crucial experiment. Whewell taught that by proper confirmation we verify a theory. Duhem denied that. Hence a crucial experiment—as a verifier—is impossible.

(Duhem was aware of the fact that crucial experiments were performed repeatedly; what he denied is not the fact but its theoretical bias in favor of verification and refutation. He rejected both. This is regrettably often ignored these days.)

Another defect in Whewell's theory was bridged by Duhem. Whewell never explained the presence of unexplained facts. He well accounted for the ability to discover facts by tests, and he emphasized this. But for these theories, these facts would remain undiscovered. But how can there be facts not due to tests? Whewell assumed that they exist, but he could not account for their existence. Duhem could. He spoke of two kinds of facts, the ones given to commonsense, and the ones that are part-and-parcel of science. Commonsense facts are crude, free of theory, and final. They are forever extra-scientific, he said. Scientific facts are precise, theory-laden, and modifiable. This sounds convincing but it is highly problematic: is it theory or common-sense? Duhem's view of commonsense is not commonsense: commonsense is never final. Duhem's view is a theory, and it cannot stand as it is.

The hardest aspect of Duhem's theory, however, is its place along with classical empiricism and *a priorism*. Whewell, we remember, was an empiricist of sorts: his chief merit is that by stressing hypothetico-deductivism he moved from the generality of the empiricist philosophy of science to specific historical examples of progress in the empirical sciences. His major modification of empiricism was his rejection of the standard empiricist search for empirical evidence not theoretically biased. He thus sounded problematic, and, indeed, following him Duhem declared no empirical foundation of science possible. Nor was Duhem ready to permit *a priori* justification to any scientific theory, viewing the domain of *a priori* thinking to be logic and mathematics alone. How, then, did he think science could be justified?

Duhem denied total justification, as he demanded that both theory and evidence be regularly modifiable. But he felt that as modification improves a theory, and then it deserves an increased justification. Modification improves not the theory but its domain of applicability—either by increasing it or by clearly delimiting it. So this is its partial justification. Duhem saw the justification of a theory in the scope of facts it covers and in its simplicity. Both these factors are theory-laden, of course, yet we can easily see if and when a modification is an improvement or not. Once we omit commonsense from Duhem's theory, its consistency and success are truly imposing.

The weakness of Duhem's philosophy is in the difficulty one has in viewing science in its light. In addition, we may observe that it was empirically refuted by evidence that Duhem had only a glimpse of—the scientific revolution of the early twentieth century.

5. The Duhem-Quine thesis

The weakness of Duhem's view can best be illustrated by contrasting his image of science with that of the contemporary empiricist followers of F. P. Ramsey. He viewed science as a set of statements of three or four kinds: logic and mathematics, theories, theory-free observations, and a few correspondence rules to link theory to observation. These rules are necessary because to be theory-free the observation statements in Ramsey's system should not include theoretical terms, and *vice versa*. Duhem, on the contrary, declared that scientific observation reports always include theoretical terms and so the revision of theory immediately revises also observation statements couched in its language. Also, when an observation statement clashes with a theory, then in Ramsey's system it is possible to present a complete set of theoretical statements that the standard correspondence rules make conflict with the observation statement. Quine goes so far as to claim that in each case of conflict our whole theoretical system was tested as a unit and then we cannot know which part of the premises is refuted when an empirical conclusion based on it is refuted. We do not, therefore, know *a priori* which part of our theoretical system invites modification. Duhem saw a greater difficulty in the situation than Quine. He considered the fact—and it is a fact—that only a part of the theory is explicitly stated, whereas another part may well be expressed as the theoretical bias of the observation, not as a premise.

To take an example, a researcher tries to extend an astronomical theory to a new prediction. Suppose the venture turns out unsuccessful. There will be then a straightforward contradiction between the astronomical theory and the observation report. Nothing can make us ignore this contradiction and stay scientific. Yet it will be rash to conclude that either theory or observation is false, since the error was in the excessive application. It will also be rash to conclude that the elimination of the contradiction from the application to this new case necessarily requires the modification of the astronomical theory. Since the observation was attained with the aid of optical theory and with the aid of optical instruments whose design embodies optical theory, there is a wider choice here.

The label Duhem-Quine argument is not in itself objectionable, but the two variants are better not confused. According to Duhem's variant some theory is declared implicit in the situation. According to Quine's variant there is no need for an implicit hypothesis. Or perhaps it is not Quine but Rudolf Carnap and other followers of Ramsey who would not put the argument the way Duhem has put it.

In Ramsey's system, at least in Carnap's version of it, each observation report has a fully determined meaning, whereas a theory has only as much meaning as experience warrants. In this way Carnap too, as Duhem before him, could deny theory the status of hypotheses, and he too could grant this status only to every new application of an established theory. And that application could then be tested and either be fully verified and then added to the theory by the extension of its meaning, or else it will be fully refuted and it should then be noted that the applicability of the theory is limited. In Duhem's system, however, there is a slight problem here: theory gets its meaning from experience and *vice versa*, which is somewhat most unpleasant, since it looks as if meaning is thereby totally absent from the system.

6. Poincaré's modification of Duhem's Philosophy

At this junction Henri Poincaré; steps in: what he adds to Duhem's system has to do with meaning. The meaning of the axioms of the system, he said, is left open, à la Duhem, by viewing them as implicit definitions. This idea is very important in the history of mathematics, particularly in the theory of the foundation of mathematics. It is of no concern for us here, except to observe that this entrenches Duhem's idea that informative meanings of theories are endowed in them by the empirical information that they are supposed to incorporate. As to that information, Poincaré said, it must be theory-independent. Duhem criticized this point sharply by showing that it does not apply to real science as we know it.

To take a simple modern example, it was deemed highly accurate and reliable that the atomic weight of chlorine is 35.55. This, of course, is a highly theoretically biased statement, a theory-laden observation report. It looks as if it is rejected by physics less than a century after it was very well established. Yet, according to Duhem, the content of observations is certain, only the wording they receive needs alteration when theory is modified. Today the same information is put in modern language in a modified version: the terrestrial average atomic weight of chlorine is 35.55.

Poincaré could not elicit instances of observation statements not theory-laden. Hence his defense of Duhem's system failed. Duhem's system is defective.

7. Popper's theory o f science as criticism

The final stage in this history is the system of Popper. All statements of science, he says, are revisable, and hence they are hypothetical. What makes hypotheses scientific is their very revocability, namely their refutability.

One may take Duhem's system, practically as it is, but reads it realistically, contrary to Duhem's expressed demand to deny theory all content. In that case one gets the result that when observation contradicts a hypothesis we cannot declare both true, and so they compete for the status of truth, a status that anyway cannot be granted except tentatively, until the next examination. What, then, is the practical methodological difference between Duhem and Popper? Both recommend deduction of old *data* and theories à la Whewell; both recommend tests à la Whewell, both reject finality of any statement in science quite contrary to Whewell; both recommend repeated modification of both theory and observation reports. Granted that Duhem is an anti-realist and Popper is a realist, does it make a difference in practical matters?

Yes. Very much so. Duhem was aware of all this, as was Poincaré. They both stressed that upon a realistic reading of a scientific theory, upon giving it a truth-value straight-forwardly, it is most likely to turn up false. This is what they attempted to prevent, on the ground that some theories are too valuable to forget. Popper, on the contrary, attempts to present this probable falsehood as unavoidable. He denied, however, that false theories are to be forgotten: the precious stock of human knowledge comprises great ideas, most of which are refuted.

Why, then, the wish to avoid falsehood in science? Why do we speak of superseded theories as either false and rejected, or as not quite false? The average science teacher, high school or university, insists that Aristotle's theory of gravity, Phlogistonism, and other theories are false and so to be rejected, whereas Galileo's theory of gravity, or Newton's, is not quite false, i. e. true for its domain of applicability. This way they apply a Baconian standard to some theories and a Duhemian standard to other theories. The reasonable competition, however, is between Duhem and Popper, since the Baconian demand for the absolute truth is out and a compromise between Bacon and Duhem makes no sense and is but a confusion to be explained historically.

Once it is admitted that false theories are not rejected but taught in universities, then it can also be seen that in university courses false observations are also taught, as they are presented in the light of refuted theories. Thus, nineteenth-century atomism is described as including atomic weights that are today declared false. Likewise Lavoisier's theory and the facts that fit it are taught in high schools, and only later do students learn that, contrary to Lavoisier's theory, not all oxidizers contain oxygen. This practice is in accord with Popper's theory. Hence, our teaching is a mixture of Popper, Duhem and Bacon, with Popper dominating the highest echelons, Duhem the middle stages of classical science, and Bacon the early stages of science and its struggle for survival. Is that necessary? What does Popper offer that Duhem denies?

The answer, in one word, is boldness. Duhem required that modifications be small so as to retain continuity and assure that empirical information is modified with the same continuity as theory. He denied that there ever was a scientific revolution. And when Einstein pronounced his revolution, Duhem held him in contempt because of his revolutionary attitude. There is much to discuss in this context, especially the impact of a change in metaphysics on science as revolutionary (as Duhem knew very well when he demanded that science have no metaphysical import). But this takes us away from theory-ladenness.

8. Popper on Observations in Science

Since Duhem argued that clear-cut refutation is impossible (so that clear-cut verification is impossible too), the question is repeatedly raised these days, how did Popper handle Duhem's argument? Or rather, the Duhem-Quine argument. And the question is often put in a quasi-Ramseyan way: if we put theory in the premises and a statement regarding observation as the valid conclusion, then the premises include all sorts of hypotheses so that we are never sure any of them is refuted along with the observation. But Popper presents things not in line with Ramsey, Carnap or Quine. Rather, his presentation accords with Duhem's: the inference includes only one theory and one observation statement, and we use all sorts of theories to decide that the prediction is false. Once we have done so, we are in a position of having already decided that the theory on which it rests is false. The question, then, is, how do we decide that the prediction is false when we cannot be sure of it?

This question is absurd: when we are sure we neither can nor wish to decide. Decision is a matter for cases of uncertainty. Query: is there a decision procedure? Yes, Boyle's. An observation report made twice with the claim for repeatability is generalized, and the generalized observation report has to be admitted—until refuted, Newton and Popper have added. Popper has slightly altered Newton's rule to read as follows.

(P) An observation report can be rejected only when properly replaced by its refutation.

Popper endorsed Boyle's rules and was reticent on Newton's rule (N) that demands to reinstate the refuted generalization after it is duly modified. But clearly he could endorse Boyle's as well as Newton's rules and add his own: the refutation of an observation report is its modified version! All this is quite in accord with widespread scientific practice. (This is not to endorse Popper's theory. I have criticized it elsewhere.)

Popper's system clearly overcame the difficulty that Whewell's system encounters: new facts are refutations of old theories. Old facts are either refutations of older theories (often in new interpretations) or survivals from prescience. The facts one observes daily that in a sense are new but not related to new theories are thus, according to Popper, outside the domain or empirical science. This is a questionable situation, since we may wish to incorporate them within science. The blueness of the sky or the greenness of grass were inherited from prescience. They were explained by modern physics. There are also new facts not scientifically discovered—not discovered as refutations—such as the mountains on the back of the moon and the atomic weights of new elements that we regularly incorporate into science. This makes science more than the mere acts of conjectures and refutations since it is also the incorporations of two kinds of facts, refutations of old conjectures and non-scientific facts. How exactly the refutations are theory-laden is clarified by Duhem and more so by Popper in a very satisfactory way. The rest is less clearly explained.

The state of the art today seems as follows. Many philosophers are using Ramsey's idea about scientific explanation in the hope of establishing the possibility of theory-free or theoretical-bias-free observations and many empirical psychologists are searching for instances of such observations. Yet these ventures are *a priori* doomed to failure, at least as long as arguments discouraging them are not answered. Whewell, Duhem, and Popper explain the fact that advanced empirical information is theory-laden by the observation that such information is the result of tests of new theories. Popper's claim that they are refutations of previous theories makes their value independent of further developments, whereas Whewell's claim that they verify new theories risks their value since allegedly verified theories may be refuted. Yet the theory-ladenness of everyday observations and the novelty of observations not relevant to any known theory—these are subject to further studies, whether of within empirical psychology (perception theory) or of methodology.

9. A historiographic note.

Were the modern thinkers discussed here aware of their important predecessors? Whewell was certainly aware of all of his predecessors. Duhem was most probably not aware of Boyle's procedure, or even of Newton's—he dismissed their empiricism. He was probably fully aware of Whewell's ideas and works; if not he must have absorbed them from secondary sources—Claude Bernard is a likely candidate. Poincaré's indebtedness to Duhem is a known fact. Popper was familiar with their works that he mentions in his own works. He was familiar with Whewell's ideas. To what extent I cannot say.

And he probably knew them only from secondary sources. Whewell is now slowly gaining a revival and a very welcome one, but even when his name was utterly forgotten his ideas were in the air. Presumably Popper had no knowledge of Boyle's rules, which he learned from the tradition of scientific practice. This is no small matter. Except for Boyle and Popper hardly any author about science has noticed that though scientific evidence must contain factual information that makes it *bona fide* testimony of a *bona fide* eyewitness, and though it must be stated at least twice, the established body of scientific knowledge and of methodology ignores this. Soon after the discovery of the existence of non-parity, Jacob Bronowski, a follower of Popper, noted the following with satisfaction. Whereas so many philosophers of science are still concerned with the grounds for generalizations in empirical observation and in the reinforcement that repetition lends to this process, within science only one repetition is required, and the generalization is fully established at once and with no further ado—until it is successfully questioned anew. This, of course, cannot make Popper's victory over his Ramseyan opponents final. Moreover, some doubts have been thrown on Popper's theory already. But this is another story.

10. A Bibliographic Note

Since the literature surveyed here is classical, one needs hardly mention even names of books. And rather than give page numbers, let me remind readers that the subject indices to the standard editions of the classical works are often excellent. The following observations, then, have only a limited function.

The works of Galileo are, of course, collected in his impressive *Opere*, but the English-reading scholar may be satisfied to begin even with Stillman Drake's small, popular collection, *Discoveries and Opinions of Galileo*, not to mention the two translations of Galileo's major dialogue and his *On Floating Bodies*. I should also draw attention to Michael Segre's study of the role of experiment in Galileo's physics in the *Archives of the History of the Exact Sciences*, 1980, as well as his superb *In the Wake of Galileo*.

Sir Francis Bacon's standard *Works*, including the prefaces by James Spedding and Robert Leslie Ellis, are breath-taking; *Novum Organum*, Book I and *Valerius Terminus*—a fragment—will do.

Robert Boyle's monumental *Works* have a wonderful, detailed index. His very early *Certain Physiological Essays*, first two essays, and his posthumous *Experimenta et Obseruationes Physicae*, Preface, should do for a start.

Newton's rule appears in the end of his *Opticks*, in the last "Query", Query 39, in the book's powerful penultimate paragraph.

William Whewell's philosophical works comprise a few volumes; his *Novum Organum Renovatum* that emulates Bacon's aphoristic style, will do amply. But all of them, plus his three volumes of the history of science, are just delightful. All these works are still better than most of their up-to-date upgrades and rivals.

I should not skip Claude Bernard, *Introductory to the Study of Experimental Medicine*, even though its English translation is rather free, and even though it is not discussed here.

Pierre Duhem's *The Aim and Structure of Physical Theory* suffices to introduce him in all his glory, and the book is certainly superb. Also his *To Save the Phenomena*. But his historical studies also deserve mention here, and I should observe that Floris Cohen notices a variant of Duhem's views presented in the introduction to his *Etude Leonardo da Vinci*, Volume 3. Readers interested in the background to this variation should consult Stanley Jaki's comprehensive biography that is impressive despite his naïve hero-worship.

Henri Poincaré's *Science and Hypothesis* and *Science and Method* do not need any recommendation. With all their deserved popularity they are still unknown: his proof of the metaphysical, unempirical nature of the law of conservation of energy, for example, is still simply unknown. Little learning should suffice to prevent much verbiage.

Karl Popper's *The Logic of Scientific Discovery* is not as much to my liking as his original *Logik der Forschung*, of which it is an extended translation, but as a start it will do amply. His best on the topic, however, is his 'Philosophy of Science: A Personal Report' issued as the first chapter of his *Conjectures and Refutations*; also his 'The Aims of Science' reissued in his two latest books, *Objective Knowledge* and his *Postscript*, volume one.

This bibliography is only of the topmost classics of the field. Much more fun awaits the curious. But one has to take good care to avoid the countless studies that at best add nothing. For more details see my *Towards an Historiography of Science* and my *Science in Flux*.

6. Field Theory in De la Rive's Treatise on Electricity

This chapter concerns one question: why did Faraday approve of this single book on electricity and magnetism? The presentation of the problem requires explanation of the significance of this book and some background material. The significance of the book is explained below tangentially. As to the pertinent background material, it contains information on Faraday and on his character. Since I have a book devoted to him (*Faraday as a Natural Philosopher*, 1971), my discussion of this aspect of the problem is rather brief. Another aspect of the problem pertains to the standard treatment of Faraday in contemporary literature. This is not easy to document, as the custom was to praise him as an experimenter and to ignore his ideas, especially his field theory. (I discussed this in my book.) After some search I found a presentation by a physicist who is now forgotten but who at the time was respected as an expert presenter. Viewing De la Rive's unfriendly treatment of Faraday against the background of that fellow's presentation reveals De la Rive as relatively friendly. This covers the background to my study. Let me add my own background too, and in two short paragraphs.

Educated as a physicist in the mid-twentieth century I mastered field theory with not much difficulty yet it troubled me, inexplicably. The same teacher who taught me field theory taught me earlier analytic mechanics. He made no excuse for the juxtaposition of these two systems and no comparison between them. His attitude to quantum mechanics was different: he presented its background and spoke of a crisis, of a break between the classical and the modern views (with relativity as classical). It did not occur to him that there may be a break between classical mechanics and classical field theory. This is what was troubling me, as I found much later. I found that this conflation of Newton and Faraday is standard. Henri Poincaré presented classical mechanics as a field theory, and whenever Niels Bohr spoke of the classical theories he included field theories.

Historians of science noticed the effort it took continental physicists to recognize Maxwell's field theory. They usually do not explain, and they ignore Faraday (although Einstein, for one, usually expressed his personal gratitude to both). This was a gap. It was closed by L. Pearce Williams who wrote a biography of Faraday (1965), a book on field theory, and more. He systematically presents Faraday as a follower of Roger Joseph Boscovitch. Admirable as both Boscovitch and Faraday are, there is no excuse for mixing their views or for bridging them; the former took action-at-a-distance as basic and the latter took as basic fields and the impossibility of such action. This was a great scientific revolution that was not heralded and that was never declared victorious. But whatever one may say about field theory theoretically, historically one has to see that leading nineteenth-century physicists found it deeply troubling. De la Rive was one of them.

Auguste De la Rive, 1801-1873, is nowadays scarcely remembered even amongst historians specializing in 19th century electricity. The major work on this is the revised and enlarged 1951 edition of E. T. Whittaker, *A History of the Theories of Aether and Electricity*, *The Classical Theories*. It mentions De la Rive a few times as a supporter of the chemical theory of the voltaic pile with no mention of this *Treatise*, perhaps because Whittaker discussed only original results; the *Treatise* claims no priority.

A glance at Jean-Baptiste Dumas' *Eloge Historique D'Arthur-Auguste De la Rive* (*Institut de France, Academie des Sciences*, 1874) offers a different picture. Let us note only the following. Dumas considered De la Rive's contribution to electrochemistry sufficiently important, though secondary to those of Faraday's (p. 19). He mentions other researches in his obituary (p. 20) and in his notes (pp. 47-48); he mentions other works of De la Rive, including some literary essays, as of some significance; yet he declares the *Treatise* to be his major work (*l'oevre capitale de sa vie*, p. 48), where both his own work is summed up and at the same time work of all researchers were analyzed.

The book is indeed fairly comprehensive. It describes innumerable experiments, offers some background, sketches and contrasts scientific opinions; it includes little by the way of mathematics (consigned to appendices). The work is declared to be aimed at the knowledgeable rather than the dilettante; these days it does not look too hard to read. Let me discuss my minor interest in it.

First is a historiographic point: the little history offered by De la Rive has become extremely influential as it greatly influenced Whittaker. I shall mention only two points of similarity between them. First, they both mention the experiment of Desormes and Hachette of 1805 as a prelude to Ørsted's. Neither explains. Second, the fusion of Faraday's work into the pattern of the continental theory of action-at-a-distance, with works of Ampère, Weber, and Neumann, treated as the evolutionary stages in the development of one idea. In truth most of the work of the school of electric action-at-a-distance after Ampère was futile, up to and including the contributions of Duhem and of Ritz. And even within this school there was less continuity than historians claim. In particular, Weber did not consider his own work the mere elaboration and corroboration of Ampère's work; yet De la Rive and Whittaker (and between them Duhem) did.

My second minor interest is in Faraday's own attitude to De la Rive. Faraday was a decade senior to Arthur-Auguste De la Rive, and two decades junior to his father Charles Gaspard. Old De la Rive, a Swiss aristocrat, had been a refugee in Britain. He studies medicine in Edinburgh and practiced it in London, where he befriended another refugee, Dr. Marcet, whose wife, Jane, wrote the *Conversations on Chemistry* that helped Faraday as a lad to teach himself chemistry. (Auguste De la Rive wrote essays on both Faraday and Jane Marcet, among other eminent scientists.) When Davy came to Europe, Faraday accompanied him as a servant and was handicapped by his ignorance of any foreign language. Old De la Rive befriended him. Later, when Davy visited Geneva, Faraday was treated as an equal. The friendship grew. They corresponded; old De la Rive published a letter of Faraday on metallurgy. As Dumas notes in his eulogy, when Gaspard De la Rive died many of his functions were naturally passed on to his son Auguste. The friendship and correspondence with Faraday was one of them. The house in Geneva was perhaps the only private place where Faraday would relax and feel at home. He mentions his visits in a few of his letters. But I have in mind his remarks to De la Rive on his *Treatise*. It is mentioned in his letters of March 11, 1854, May, 29, 1854, and March 21, 1856 (H. Bence-Jones, *The Life and Letters of Faraday*, 1870, Volume 2, pp. 328, 344 and 375 of extended edition). Let me quote only from the last one: "I rejoice" is his general response, "for now, when asked for a good book' on electricity, I know what to say." Is this a friendly note of encouragement or a sincere appraisal?

Either of these hypotheses is hard to uphold. Faraday followed a very strict code of conduct: he spoke his mind diffidently and politely but very candidly; or else he frankly and firmly declined comments. Yet it is hard to see how he could be satisfied with a book that so maltreated him, as we shall see, particularly as he was very sensitive about his being maltreated. All this is partly resolved by Faraday's praise of the book as a well of information, especially about German sources. But this is hardly the whole story. My own hypothesis is comparative: a misrepresentation as De la Rive's work was, it was far better a presentation than the average.

This, indeed, is the chief interest I find in De la Rive's work. There is a literature about the penetration of Maxwell into the Continent; as long as Faraday's revolutionary ideas were not sufficiently appreciated — and prior to recent studies, particularly L. Pearce Williams' *Faraday* of 1965, he was considered an aetherist — there was little reason to study the penetration of his ideas into the Continent. Now, however, it seems obvious that even in converting the Continental scientists — for in the nineteenth century science was still much a matter of creed, and for many it still is even today — Faraday was the trail-blazer.

An attitude that may well be very characteristic of the time in Europe is that exhibited in works of Johannes Mueller of the University of Freiburg, author of a textbook on electricity and of reports translated and published by the Smithsonian Institution. In the *Annual Report* of 1856 there is a Report on the recent progress in physics — galvanism, by Mueller, pp. 311-423, and in 1857 a report on static electricity, pp. 357-456. There is another report in the 1858 volume on electricity and galvanism, pp. 333-431, and the next year, pp. 372-415, too. Today, armed with full versions of electromagnetic field theory, it is easy to spot Mueller's errors, and shooting clay pigeons is not interesting. My point is to illustrate the hostility to field theory not in order to settle stale accounts but to combat the historians who conflate action-at-a-distance theories (of Boscovitch or anyone else) with field theories (of Faraday, Maxwell or Einstein). Let me note a few general points on this report, especially its attitude towards Faraday. Mueller's report is much more analytic than that of De la Rive, but otherwise fairly similar. It refers to Faraday's *data* as true almost invariably. (The exception is in the one in the Report for 1857 of 1858 where, on p. 373, a seemingly continuous spark breaks down into a rapid succession of sparks by moving the eye rapidly, a technique all too obvious in the days of the flicks and fluorescent light; Mueller reports that it "has not succeeded perfectly in my trials." He does not even mention that he only contests Faraday's technique — since the fact was also established by Wheatstone's revolving mirrors.) But, not only Mueller dissents from all of Faraday's views; he does so condescendingly and inaccurately.

Even when Mueller has no special reason to be condescending, he is. Thus, when he reports on the debate on the cause of electrochemistry, he sides with the chemical theory (which identifies the pile's action with chemical action, by identifying chemical forces as a kind of electric force) as against the contact theory (which asserts, with Volta, that the contact points between the electrodes and the solutions are poles that act at a distance). To be broadminded, perhaps, he makes a concession to the contact theorist. He puts it thus (p. 314): "Even Faraday", he says, and I draw attention to the word "even", "who is prominent in maintaining the chemical [theory] ... concedes that decomposition is preceded by a state of tension ..." He quotes Faraday and repeats: "Thus Faraday himself concedes".

This is incredibly crude. The idea is this. Since Faraday admits the existence of tension, he concedes that there are centers of force causing the tension; or, since there is polarity, there are poles. And, the contact I theory is a theory of poles. Yet Faraday was at pain to stress his dissent on this point. He renamed the poles "electrodes" just for this reason. In a letter to William Whewell, the person whom he had consulted and who had suggested electrode, anode, cathode and ion, anion, cation, Faraday relates the enormous opposition to his renaming that he crushed on Whewell's authority. Of course, his audience was as aware as he that names are not theories, but they were clear about the purpose behind his renaming. Indeed, Faraday's very approach to the pile was an attempt to look for an electric phenomenon where the medium plays an undeniable role, and he tried to abolish the electric poles as causes of polarity, similarly to his prior investigation into magneto-electricity where he showed that cutting the lines of force is the cause of the phenomenon, and that the lines of force, i.e., of polarity, do not depend on the magnetic poles. Faraday began his researches with the pile because he deemed that the medium of electrolysis least susceptible to be ignored by his opponents; for his own part, he saw empty space as the medium just as much.

This came up sharply with Faraday's study of electrostatic induction. To explain this phenomenon, most physicists assumed the existence of latent electricity — the existence of positive and negative electricity in equal amounts, to use the two-fluid language, but the same holds within the one-fluid system — and normally the existence of electricity is assumed to be undetectable until some electric transfer takes place. Faraday rejected this theory because it assumes that polarization is caused by poles; rather, he identified electricity not with the electrified body or its content but with the polarization itself. He argued, first, that the medium cannot be ignored when it is filled with a dielectric material, especially inhomogeneous. But he then argued that even a single body in the vacuum, when electrified, so-called, is merely a center of induction, homogeneous or not, as the case may be.

Mueller speaks of Faraday's researches on latent electricity. Though one can understand it, one cannot avoid the impression that it is an insult. In our own century, by distinction, even those who considered Schrödinger's equation as good for diagonalizing matrices were not so rude as to speak of his method of diagonalizing matrices, and almost every writer does him the courtesy of giving his own reading of the meaning of the wave function, heretical though it is. Mueller does not have any criticism of Faraday's electrostatic doctrine. He puts this fact nastily thus (p. 393): "Faraday's experiments are perfectly correct, but it appears to me that he has erroneously interpreted these experiments and drawn conclusions from them which he is not justified" and he goes to say what Faraday should have proven them empirically if he were to convince him (Mueller). He goes on to dismiss Faraday by declaring (p. 397) Faraday's view of insulators as poor conductors "a truth which no one, to my knowledge, has disputed" — whereas everyone before Faraday followed Stephen Gray in denying this truth — and by scolding Faraday for not noticing that electrostatic induction in the vacuum must be an action-at-a-distance.

One must be indulgent toward Mueller here. Faraday's empty space as a medium was very hard to comprehend, and at the same time Tyndall said so in an open letter to Faraday ("On the Existence of a Magnetic Medium in Space", *Phil. Mag.*, Vol. 1, 1855, 205-209). Faraday himself could only clarify the difference between the action-at-a-distance theory and the medium theory when applied to empty space only a little later, in his lecture on the conservation of force of 1857 (*Exp. Res. Chem. Phys.*) where he said, all action takes time. Hence, if you abolish the center of force, the theory of action-at-a-distance will tell you that the action will there and then disappear, whereas the medium theory will tell you that the medium will be able to act for a while without it. But this Mueller did not know as yet.

One cannot, however, be as indulgent regarding the following remark of Mueller's (p. 400). "Faraday's views on electrical induction must necessarily have forced upon him the question, whether magnetic attraction and repulsion ..." act through the intervening medium as well. "The experiments which he made for the solution of this question gave invariably negative results ... No sign of the influence of intermediate particles could be obtained." This is astonishing. Not only did Faraday start with the magnetic medium and then move to the electric medium. Not only did he deny that magnetic action was "attraction and repulsion." At the time when this was written diamagnetism and magneto-crystallism were the hottest topics, and due to Faraday's efforts to find a magnetic "influence on the intermediate particles."

I shall leave Johannes Mueller now and also the 1858 *Reports* of the Smithsonian Institution after noting that a very learned paper on atmospheric electricity by M. F. Duprez appears there on pp. 290-371 that refers to Faraday only once, a propos of his theory of lightening discharge, that he dismisses offhand (p. 361). Let me also note that a similar, though less detailed, paper on the same topic occurs in the *Britannica* 1842 edition, where various theories are listed, but where Faraday is not mentioned, not even his 1841 theory of the lightening discharge, not to mention his ionization theory of its source. Even the later, eighth edition of the *Britannica* of the 1850's is unkind to him, reticent and by implication unfriendly. The ninth edition, however, has Maxwell's essay on him.

We can now revert to Auguste De la Rive and his treatment of Faraday. Against the background I have tried to illustrate he stands out as a fairly honorable opponent.

De la Rive's *Treatise* was meant to be published in a complete version of two volumes, one pure, one applied, in both French and English. The work was interrupted by private misfortunes and the first volume appeared alone, the English translation in 1853 and the French original in 1854. Volume 2 appeared in 1856 also on pure electricity and Volume 3, on applied electricity in 1858 — in both languages. The French edition of the first volume is slightly corrected, and the corrections occur as additions in the opening of volume two of the English version of 1856. I shall refer to one of these later on.

The opening of the first volume is dominated by Coulomb. The theories of action-at-a-distance of electric fluids up to Chapter 2, on the distribution of electricity on conductors' surfaces only. The principle is that electricity is distributed on surfaces only. On p. 71 Faraday appears first, or rather his "experiments, which of an elegant manner demonstrate the same principle." We return to Coulomb fast. Chapter 3 is on electrostatic induction. Induction is action-at-a-distance. Chapter 4 explains it as the result of splitting the two fluids hidden in a matter. Chapter 5 is on dielectricity. On page 126 Poisson's authority is invoked. On p. 133 Faraday comes in again. The theories here advocated, De la Rive admits, "are now attacked by ... facts, which tend to nothing less than overthrow them entirely by leading to the denial of action-at-a-distance, and replacing them by molecular action." For his own part, he thinks "they do not entirely overthrow the theories founded upon labours of Coulomb and Poisson" and he only looks for "the degree in which they must modify" these theories. On pp. 140-141 Faraday postulates the action of intervening matter; "there is no action-at-a-distance, or at least at a distance greater than that which separates two adjacent molecules." On page 143: "According to M. Faraday [distributions cannot] be explained but by admitting that … induction ... is necessarily more feeble ... along curved lines than along straight lines ...” This is hardly clear even to readers of Faraday.

Faraday’s general theory of static electricity then appears (pp. 144-146). De la Rive uses Faraday’s last paper in the high inductive style, written just before he began to publish his speculations boldly. Quite clearly, other historians, notably Whittaker, heavily depend on De la Rive, though without being very eloquent about their debt (a point-by-point study might prove amusing). When De la Rive comes to his conclusions from Faraday’s work (p. 147), he does so in a rather unfriendly manner: “Faraday is led to admit that the tendency of electricity to distribute itself on the surface of a conducting body is more apparent than real”, and later (p. 148), “Faraday was not contented to follow out the consequences of his theory as far as the phenomena of static electricity alone are concerned,” concluding with the judgment (p. 149) on Faraday’s electrostatic theory: “Although it still has need of being more precise, it deserves, however, even in its present state, to draw the serious attention of a philosopher.” But he appends a promising coda to this passage, continuing it thus. “It has in its favour, as we shall see, the establishing a more intimate connection between the phenomena of static and those of dynamic electricity.” He continues (p. 150), “we cannot yet completely admit” Faraday’s (and Mossotti’s) theory, as Faraday’s facts may yet be explained in a traditional way! He speaks of “a difficulty of conceiving” of electrostatic induction in a manner postulated by Faraday ─ that he considers an objection. “It is true that Faraday and the partisans of his theory reply … But we do not believe, not withstanding these replies that the principle ... is demonstrated.” He suggests that electrostatic phenomena in the vacuum seriously conflict with Faraday’s view. (This, to repeat, was then a common objection to Faraday’s view.) Moreover, continues De la Rive, Matteucci has refuted Faraday empirically. This, of course, is untrue.

Let us not go into the poor logic of this discussion. Let me only quote the final sentence of the chapter on the theory of static electricity (p. 155): “We shall see that electrical phenomena very probably depend upon the combined action of the particles of matter and of the etherical fluid which fills the universe; and, by thus approaching to Faraday’s molecular theory, we shall be nearer to the truth than with the hypothesis of two imponderable fluids, existing of themselves, and in a manner independent of bodies.” Action-at-a-distance, again.

Magnetic curves are rather prominent, but still as indicators of action-at-a-distance. De la Rive notes a significant paper by P. M. Roget, published by the journal of the Royal Institution in 1831, on the mathematics of magnetic curves; Roget viewed magnetism as action-at-a-distance, and the magnetic curves were purely mathematical, with no independent physical existence (p. 185 and 542-545). Electromagnetics. Hachette and Desormes, Ørsted, Ampère. We are told definitely (p. 239) that Ampère answered all objections “and established this theory upon such a solid basis that it is at the present time generally admitted.” This statement is puzzling, unless we realized that it is not at all clear what De la Rive designates as Ampère’s theory except that it includes at least and perhaps at most his molecular currents. (Current-current interactions are phenomena unless their magnitudes, etc., are specified and Weber, for example, had severely objected to Ampère’s specifications.) The lack of clarify becomes stronger when discussing Faraday’s early electromagnetic work, (p. 251). Faraday’s rotations of 1821 (his electric motor) looked irreconcilable with Ampère’s theory, particularly since at the time Ampère “had not at that period made known his law of angular currents, by means of which he was succeeded in easily explaining ...” Faraday’s rotation. “Then, in order to add an experimental proof to the theoretic demonstration ... that [Faraday’s] facts were not contrary to his hypothesis of the nature of the magnet ...” we may remember that Faraday never attacked Ampère’s molecular hypothesis, yet De la Rive defends it vehemently over a few pages. The defense has certain validity by stressing that Ampère’s view holds only for currents that are closed. In 1856, soon after, Maxwell argued that Ampère’s theory holds for stationary (closed) currents and leads to the same results as Maxwell’s reading of Faraday for the same cases.

Arago’s experiment on the magnetism of rotation of metallic discs of 1825. De la Rive notes (p. 356) that Poisson’s explanation of it “was overthrown by the subsequent discoveries of M. Faraday” of 1831-1832. The discovery of magneto-electricity (p. 356). “In 1832” we are told (this is puzzling inaccuracy, very uncharacteristic of De la Rive), “Faraday made his discovery, of electromagnetic induction.” The two experiments (magnetically induced currents and current induced currents), are presented and shown to be one — and no mention of the magnetic curves that are cut when the currents are created. For an unstated reason Del la Rive introduces (p. 358) Faraday’s electronic state and his withdrawal of it. Perhaps he wished to tell the reader that he may follow Faraday and then be left by him high and dry. This is a prelude to an embarrassment (p. 358).

“The intensity of the induced current depends on many circumstances ... We can give no precise rule ...” And we soon move to self-induction. We move on. “Faraday, in his beautiful researches on induction” we are told (pp. 360-361), “was the first to demonstrate that induced current, as we might have expected, may be” caused by terrestrial magnetism. While he compliments Faraday’s beautiful researches he tells us of a result that is expected anyway. It is understandable, but not too pleasant.

The greatest insult comes not long after (p. 365): “The learned English philosopher”, this is just a buffer, one gets used to it by now, “endeavoured to establish a relation between the direction of the currents that he obtained in his experiments, and the direction of the lines of magnetic force or magnetic curves...” There is no hint at any cutting of any lines of force that Faraday viewed as the cause of the current and as the measure of the current’s strength. The direction business, by the way, has precedence in Ampère's work, yet De la Rive does not like it. "All the effects" related, he says (p. 635), "appear to me explicable in a more simple manner by tracing them to the primitive law of induction discovered by Faraday himself and" by Ampère's hypothesis about magnets.

This is obscure. The facts are explicable more simply — more simply as compared to what? What does he reject? Clearly he sees no need for magnetic curves round a conducting wire and offers Faraday's own "primitive law" that says that electricity flows in a closed conductor when a current is made or broken in the vicinity. But this is a guess on my part. After all, we remember, De la Rive admits (p. 358) inability to express this law precisely.

On page 391 we are told of "the important principle which Faraday had already glanced at but which [others] ... have verified and established more conclusive[ly] ..." namely that electricity produced by a dynamo shares all properties with friction electricity All this is trite; Whittaker has played the same game as De la Rive; Faraday's own point is meant to say more, but says explicitly just this trite point, since he was still using the inductive style.

On page 409 Wollaston and Faraday prove "that an electric discharge of feeble tension is able to produce chemical decomposition; but", etc. Always but, always a sense of irritation at Faraday.

On pp. 417-418 we are told that "induced discharge ... is a very complex phenomenon ... determination is very difficult". A theory is nonetheless given: two other thinkers are mentioned as having alternative theories that are not described; Faraday is not mentioned.

Page 433. "General Considerations on Induction Weber and Neumann ... both by means of experiment as well as by calculation ... connect the phenomena of induced currents with the laws by which electrodynamic actions in general are governed. M. Weber, in an important work ... very profound ... interesting approximations ... we shall quote, as an example, the following experiment, which is a modification of one of Faraday's: — The English philosopher, as the result of series of experiments, had been led to observe ..."

We have here Weber thinking and experimenting, the example is an experiment that is a modification of Faraday's, and then we land in Faraday's experiment plain and simple. It relates to a magnet cutting its own lines of force and thus causing a current. Faraday ascribed to it a great importance, since it showed, as he had suspected all along, the independence of the lines of magnetic force from the magnet, and that the magnet is a mere locus; i.e., it convinced Faraday personally that lines of force are more primary than ordinary matter. De la Rive mentions none of this, and only says that this experiment results from series of other experiments. I cannot say in which respect Weber's experiment differs from Faraday's. It seems to me to amount to precisely the same thing, except that it employs a more up to date arrangement for the selection of currents. "It is difficult for us to admit, with Weber and Faraday..." Never mind; the debate is directed against Weber. De la Rive I nevertheless notes that much of Weber agrees with Neumann's and his own ideas.

All the same, something made De la Rive withdraw all this before the French edition appeared. In the *Traite*, Volume I, page 439, we are told that Weber's experiment is but a variant of Faraday: "*Weber avait egalement decrit ... une experience qui ne differe de celle de Faraday* ..." Preceding this, there is an insertion (p. 436 ff; it appears in the beginning of the second volume of the English version. In it De la Rive does two remarkable things at once: he declares — twice (pp. 13 and 16) — Lenz's theory utterly satisfactory, explanatory of all known facts and highly confirmed, and introduces Faraday's field theory, a field and lines of force as well. This raises the suspicion that he would not mention fields as long as he feared that the field theory is unrivalled. Let me postpone this point, however, and continue with Volume 1, so as to see how, in steps, De la Rive relaxes his own taboo on explaining Faraday's view. For the gradual relaxation may be better explained as a success to overcome some reluctance rather than a decision not to give Faraday a chance to appear as the leading thinker in the field.

Back to volume one, then. The next topic is diamagnetism; p. 446. "The facts that we have been relating, would seem to prove ... But these were isolated facts ... and it is to Faraday that we are indebted for having established ... The learned English philosopher ..." Still no lines of force. De la Rive introduces Faraday's terms "equatorial" and "axial" which are more descriptive then "lines of force". Though this terminology forces him to confine his descriptions to phenomena in a fairly homogeneous field, such as between two poles of a horseshoe (electro) magnet with two blocks of soft iron attached to it. Next we are told of repulsion between magnets and diamagnetic substances (p. 488), though Faraday had disproved this idea.

De la Rive manages to skate quickly over the point at which Faraday, "this clever philosopher", decides that both air and the vacuum are neither diamagnetic nor paramagnetic (p. 452) — by promising to return to the topic of the diamagnetism of gases. On page 455 we are told that according to Becquerel the "vacuum, or rather the etherial medium by the aid of which the magnetic actions are transmitted, is itself magnetic." All of a sudden magnetism is not due to action at a distance, and the magnetic ether is introduced via a qualifying clause. This is not fatal: Becquerel's view is at once rejected. It only indicates how absurd it looked to De la Rive to talk of the action of empty space even for one tentative paragraph.

Theories of diamagnetism (p. 458, my italics). Faraday, "who discovered, and who so carefully analyzed, the phenomena of diamagnetism was content with putting forth the law with which experiment had furnished him, namely; that diamagnetic substances are those which, in the field of magnetic forces, direct themselves... We must not forget that Mr. Faraday distinguishes, by *field of magnetic force*, the ... space within which the poles of an electromagnet cause their influence to be felt ... of which the curves marked out by iron filings give, to a certain degree, a very exact idea."

This is the first time fields enter De la Rive's work; the two qualifications ─ speaking of poles, and of those of an electromagnet ─ are strange but unimportant. The field comes in again, with Thomson's [Kelvin's] work, on p. 462. Faraday now comes more frequently, a propos of a mistake of Weber that was very hard to correct and that De 1a Rive first spotted (pp. 464, 466), and the diamagnetism of gases (pp. 468-471).

Magne-crystallic action. Faraday introduces "*magne-crystalline line* in order to distinguish it from the force that he calls magneto-crystalline" (p. 483), but again the phenomenon (discovered by Plücker) comes with no lines of force the way diamagnetism comes, again with a description of the phenomenon restricted to a homogeneous field (pp. 481-482). On p. 485 we are told, "it is easy to see that Faraday's experiments are altogether of the same order as those of Plücker." This is false as Faraday did not confine himself to homogeneous fields. Indeed, already a page earlier we were told (p. 486), "The surrounding media exercise no influence over the magne-crystalline property of bismuth, which establishes a further difference between this action and diamagnetic action. M. Faraday only..." etc.

It is quite clear that in De la Rive's version Faraday holds to the theory of action-at-a-distance: "Mr. Faraday was struck" we read (p. 489), "with what is so extraordinary a force which, emanating from the poles of a magnet, directs from afar" all sorts of crystals. Needless to say, Faraday explicitly rejected the idea that magnetic forces emanate from magnetic poles; indeed, even Coulomb and Poisson, whom De la Rive follows, had rejected this idea; and so, clearly De la Rive did not mean to be taken literally; indeed, it is quite possible that because this cannot be taken literally it can be used as a mere hint, but to something not specified. My impression is that the hint is to the claim that Faraday had accepted the common doctrines of magnetostatics.

This impression is strengthened by the sentence that follows the one just quoted. "He had consequently admitted that this force is neither attractive nor repulsive, but a simple directive force due to a species of radiation, which, emanating from the magnetic poles, traverses the interposed crystal, and compels it ... to place itself so that its axis is parallel or perpendicular to the line according to which this radiation operates." Here we have explicitly action-at-a-distance, emanating from poles, after all, traversing interposed bodies, and as a kind of radiation! Needless to say, all this is the mere attempt to avoid field language, yet after fields had already been introduced! "This manner of regarding the action has been suggested to Faraday by the phenomena presented by polarized light", namely that of magneto-optics. In other words, the peculiar radiation is just the lines of magnetic forces when illuminated, to use Faraday's language. But magne-crystallic action is independent of illumination, and so the whole presentation is a mere apologetic wriggling. No sooner De la Rive presents magneto-optics, and he distorts Faraday's view on it again.

That De la Rive is uncomfortable is quite obvious. The paragraph that starts with “Faraday was struck”, and continues with “as species of radiation” and all that, ends (p. 490) with “Observation... would become inexplicable without this move regarding the phenomena.” Here Faraday is reluctantly introduced as unrivalled. And in the only field in which he felt he was justly rivaled! For magne-crystallism is the only field where Faraday ever acknowledged that an action at a distance theory adequately explains all known phenomenon ─ the Tyndall-Knoblauch theory. Doesn’t De la Rive know of this theory?

Faraday’s theory, just declared necessitated by observation, immediately comes under attack (p. 490): Faraday is “Constrained... to admit that magnetic action may be exercised independently of ponderable matter” which is a slur on Faraday since this was his point again and again, especially in magneto-crystallic action ─ and he is anyway superseded in the next paragraph by Tyndall and Knoblauch (pp. 490 ff). So Faraday is excused for having introduced a theory when there was none better, but now, thanks to others, etc.

Magneto-optics, p. 497. “We have arrived at an important discovery, by which Faraday prefaced his researches upon diamagnetism, which, however, are so independent that we have been able to explain them first, as indeed the logical connection of the facts required of it.” Here is a compact wealth of puzzles. How did Faraday “preface” his diamagnetism with his magneto-optics? Two fields are either independent, or logically the one comes before the other, but not both. Yet De la Rive claims that Faraday claims that magneto-optics precedes diamagnetism whereas both diamagnetism precedes magneto-optics and they are independent of each other. De la Rive manages both a historical error and a logical error in one short paragraph!

What De la Rive seems to say is this. Magneto-optics proves for Faraday the theory of fields of force, and he uses it in his diamagnetic investigations; but he is in error; diamagnetism can be presented without fields, with the geometric image of elongated objects lying between poles in a transversal or a longitudinal position, (equatorial or axial position); and the idea can be used to introduce magneto-optics, too. This reading resolves the difficulty by removing both the historical and the logical error. It leaves De la Rive with two other errors. First, his description is not as general as Faraday’s, since it holds only for homogeneous fields (or at least fairly homogeneous ones). And it assumes that De la Rive was tongue-tied when discussing Faraday’s heresies. Yet these two allegations are a running theme throughout my reading of De la Rive’s first volume.

As to De la Rive’s view, he ascribes (p. 524) Faraday’s magneto-optic effect “to an action exercized neither on the [ponderable] particles alone nor on the [particles of the] ether alone, but in the manner of the existence of the particles in respect to the ether.” Again, De la Rive introduces the ether in desperation and again it is not clear how; but here, at least, it is his final word. Finally, he must admit the existence of the medium, even if he considers it an ether. As usual, after Faraday takes all the abuses, he wins. He was as sensitive to this as to other points, and it must have cheered him up in a small way.

The remark on the ether comes at the close of Volume 1 of the English edition. The French edition has an additional section on the general theory of magnetism that appears at the opening of the English edition of Volume 2, beginning with the additions to Volume 1.

De la Rive had intended to publish two volumes — one pure, one applied — and he published two pure volumes and one applied. The first volume contains less than 600 pages; the second, unintended one, contains 900. This happens to all who deceive themselves about the possibility of completeness. De la Rive underestimated his tasks, and this indicates clearly that his injustices to Faraday were rooted largely in the naive optimism of the age.

In his advertisement to the second volume he explains his delay in publishing it as due to his work on the pile. "I hope to have solved this difficult and contested questions in a manner that will be accepted by all who have turned attention to it", he says. First, let us glance at the supplements to Volume 1.

On page 2 we read, "Ampère's theory, however, failed in certain points of direct experimental demonstration. M. Weber succeeded in filling up this gap, demonstrating by certain experiments ... the complete identity between the laws of electromagnets and those of natural magnets." This important result has been the means of removing all doubts that might still have remained, as to the accuracy of Ampère's theory; consequently it gave it a degree of probability that approaches certainty.

Those who wish to snigger at this may be reminded that Max Born has said almost the same about quantum mechanics. Unfortunately, De la Rive erred not only about probability and certainty; he was ambiguous as to whether Weber corrected Ampère's formula or whether he verified the same old formula by new experiments. Of course, Weber explicitly rejected Ampère's formula in a rather unfriendly way and replaced it with his own. But since both formulas are of currents acting at a distance, the later may be a modification of its predecessor. De la Rive could have said so; perhaps he would if he were not so uptight about the whole matter.

On page 13 we return to Faraday's rotating magnet. Faraday's and Weber's views on the matter are rejected. The general theory of Lenz is endorsed as one that connects all the phenomena involved! It is a bit strange to encounter such a sweeping statement, especially since Lenz's theory is entirely qualitative, and thus *a priori* unsatisfactory. We may remember that when Faraday's (quantitative) theory of electromagnetic induction was introduced qualitatively only, De la Rive admitted his inability to specify the law well enough and he found this an objection to Faraday's theory. His attitude to the theory of Lenz is different: he is at pains to show that Lenz's law covers well enough the case in point, namely, that of the rotating magnet.

It does, but only when not viewed within the action-at-a-distance framework. De la Rive hints at this (p. 1.6), "whenever the mutual action ... gives rise to ... an attraction or a repulsion, or a deviation in one direction or another ..."; but he does not allow himself to conclude that this refutes both Ampère and Weber. He pushes on bravely (p. 16). "Still more recently, Faraday, with a view of studying the magnetic field," (incidentally, the word "field" occurs in Faraday's work only sparsely, in 1846 and later, and here in 1856, yet *Oxford English Dictionary* quotes Tyndall, 1860) "namely, the distribution of the forces that emanate exteriorly from the poles of a magnet" this is too inaccurate, " ... obtained induction effects, that are remarkable confirmation to Lenz's law. We shall return to these experiments further on, when we are speaking of Faraday's lines of magnetic force."

Let us quickly skip a lot, including an interesting presentation and discussion of Weber's theory of diamagnetic polarity (pp. 41-44) and return to Faraday and the unqualified, open recognition of his field theory (pp. 44-47).

> Mr. Faraday does not admit of diamagnetic polarity; we have already said that he regards the action exercised by magnets upon magnetic and diamagnetic bodies as the results of forces emanating from the poles of magnets, according to certain directions, and which he calls 'lines of force', and the whole of which constitute the magnetic field. The presence of a body in this magnetic field modifies the directions of the lines of force: if the body is magnetic, it concentrates the lines of force; if diamagnetic, it makes them diverge. This modification, brought about in the distribution previously uniform of these lines of force~ gives rise to attractive movements for magnetic bodies, and repulsive for diamagnetic. Mr. Faraday entered into a detailed study of the magnetic field, and the direction of the lines of force, a very exact idea of which is given by the distribution of iron filings around and between the poles of magnets. We have already seen that he succeeded in employing induction to demonstrate the equality and the distribution of these lines of force in the magnetic field. It follows indeed from the experiments to which we have referred in the chapter on induction that, at whatever distance from the magnet these lines are cut, the induction current, collected by the movable wire by which they are cut, possesses the same intensity; which proves that magnetic force has a definite value, and that for the same lines of force, this value remains the same at all distances from the magnet: neither the convergence or divergence of the lines, nor yet the greater or less obliquity of the intersection, introduces any difference into the sum of their power. The study of the internal part of the magnet leads us to recognise that the lines of force have there also a definite power, and perfectly equal to that of the exterior lines, which are only the continuation of the others; and this whatever the distance may be, which may be infinite, to which they are prolonged.
>
> We must not forget that Mr. Faraday, by the term lines of magnetic force, expresses the power of the force of magnetic polarity, and the direction according to which it is exercised. If the magnetic field is composed of equal forces equally distributed, as may easily be obtained with a horseshoe electro-magnet, we have merely to place a sphere of iron or nickel in this field, to cause an immediate disturbance in the direction of the lines of force ...
>
> The few words that we have been devoting to Faraday's theoretic ideas suffice to make them understood: the fundamental idea of the illustrious philosopher is in the main the negation of all action at a distance, and the explanation of the

> phenomena by continuous force, forming what he calls lines. of force. Bodies, by their presence, modify these lines of force; and there arise directive motions, which are manifested by the disposition of these bodies to place themselves according to their nature, either axially or equatorially, namely, in the places where the force is at its maximum, or in those where it is at its minimum. A learned English philosopher, Mr. Thomson, on applying calculation and notions of mechanics to Faraday's ideas, found that they represented, in a remarkably exact manner, what takes place in this order of phenomena, providing we take into account the mutual action of the parts of which the bodies are composed that are submitted to magnetic influence ...
>
> ... We cannot altogether acquiesce in Faraday's ideas, however ingenious they may be. Does the magnetic field really exist, as the learned philosopher conceives it to be, namely, independently of the bodies by which its existence is made manifest? This is the point upon which I have some doubts. I am rather disposed to admit that magnetic forces are exercised only so long as there is a body which determines their manifestation...
>
> ... Finally, we may remark further, that if the lines of force are sufficient, as Faraday admits they are, to explain all the phenomena, why have these lines need of the intervention of a body in order to act upon the polarised ray, and cannot they act directly upon this ray *in vacuo*? — a result which we have not been able to succeed in obtaining although employing even a very considerable magnetic power.

I have quoted De la Rive in full here because this passage is perhaps the only fairly adequate representation of Faraday's ideas made in his lifetime, and indeed one of the few Faraday could even find, even if we count Snow-Harris (whom he overlooked, though he referred to his observations of discharge patterns and though they were fairly close friends); in part the accuracy of De la Rive's description is the result of a disagreement, just as the inaccuracy of Kelvin's description — his ascribing an aether doctrine to Faraday — is the result of an agreement (in the patronizing manner of the age). I have omitted the objection that De la Rive makes, as it is question begging, and left a very good one that was surprisingly answered by Maxwell's theory, or rather by the gauge invariance of the vector potential in it.

To return to Faraday, no doubt he was pleased with this presentation, no less because it was fair but not in agreement with him.

There are only two further points for me to make. First, De la Rive's presentation of the theory of the pile is greatly influenced by Faraday and is very sympathetic to him (pp. 353-354, 446-450, 664 ff, and 694 ff), although he refuses to adopt Faraday's terminology (note p. 354). Similarly, many of the experimental details of conduction derive from Faraday or from those who followed his experiments. In particular De la Rive is lucid about the subtlety and importance of Faraday's corrections of experiments determining speeds of currents (pp. 196 ff.). Yet even here De la Rive is not accurate, for example, when declaring (p. 376) that according to Faraday chemical forces act at a distance.

Second, De la Rive does not explain sufficiently why according to Faraday there is a complete symmetry between positive and negative electricity. Yet he does, correctly, record Faraday's own admissions of cases of asymmetry, in the positive dark discharge (pp. 276-277), in the difference of potential level between the negative and positive surfaces of the condenser (p. 166), and in the negative spark. Yet, somehow, he manages to ruin the effect of this point. On the one hand, he does not say that Faraday himself did not consider this criticism sufficiently strong. On the other hand, it De la Rive did not say whether he shared this or not.

On the whole, and in conclusion, what is missing in the two thick volumes is a focal point, and this is clearly seen in the author's wavering attitude toward fields. In his *Notice sur Michael Faraday* of 1867 De la Rive says explicitly that he is suspicious of Faraday's immaterialism as it seems to him to be idealistic and thus anti-scientific. This, at least, is a clear position. In the *Treatise* he says that Faraday's theory is pretty coherent; but he does not explain it beyond the two or three pages here quoted almost in full. Clearly, De la Rive would have liked Ampère, Weber, and Lenz to win, but he also thought the world of Faraday; clearly he was greatly ambivalent. Beyond this, it is hard to say.

Perhaps, then, in his very ambivalence he presented himself as open minded *malgré lui* and he thus won afresh Faraday's fondness and appreciation.

For the sake of completeness, may I add the following. There is little material added from volume three of over 800 pages on applied electricity regarding Faraday and nothing regarding fields. Faraday makes a small appearance when the electric fish is analyzed; he is conspicuous in his absence from the long (over one hundred pages) chapter on atmospheric electricity; he appears with his theory of the atmospheric causes of the variation of terrestrial magnetism and its refutation by solar influences on these variations (p. 274); his contribution to conduction in telegraphy (entirely superseded, incidentally, by the work of Kelvin on the matter) is fully acknowledged (pp. 442-443, 446, 468); and, in conclusion of the physiological part, on the last page of the text, Faraday's experiment showing that air may act as an electrode, opens the possibility of viewing a plant as a pile (p. 702). The last 100 pages or so of the last volume constitute series of appendices and notes. First electrostatics, culminating with the debate between Riess and Faraday, and the reaffirmation that Coulomb's force plus dielectric polarization explain all electrostatic facts well enough. A few fleeting references to Faraday are there, including a minor disagreement concerning the pile (p. 753). The chief significance of this volume seems to be that in it attempts to encompass technology within a scientific treatise make the enterprise burst to the seams. A few decades later such a venture would be quite encyclopedic.

7. Anthropomorphism in Science

Anthropomorphism is an inveterate tendency to project human qualities into nature — consciously or not. The standard and most important variant of anthropomorphism is animism that ascribes souls to all things. Before entering into its role in the history of science, let us consider a few important and usually neglected logical aspects of it.

First, when we draw analogy from humans to non-humans, we assume that we know humans: we conclude from known human qualities to unknown natural qualities. Yet it is not what we know, but what we assume that we read into non-humans. For all we know, the analogy may go the other way: perhaps like sticks and stones humans have no souls. At the very least, we may speak of animism as an analogy without deciding about souls — not so much from known human qualities but from assumed human qualities.

The second characteristic of anthropomorphism in need of critical attention is a "genetic fallacy." An anthropomorphic assumption may be true or false; it is not decisive to show that it is anthropomorphic, as it is no criticism of any idea to point to its origins. Some anthropomorphic assumptions are known to be false, but not simply because they are anthropomorphic, since other assumptions, e.g., that animals behave like humans in certain specified respects, may indeed be anthropomorphic and yet true. Nevertheless, received opinion is that anthropomorphic assumptions are not likely to be true. This, however, may rest on a more general situation, in which any guess — whether based on analogy or not — is not very likely to be true simply as a guess. If we want our guesses to be more likely than wild fancies, we may suggest a theory concerning the increase of the likelihood of *a priori* guesses. But then, this theory may be false as well. And therefore we have, at least for the time being, to leave open the question, are any anthropomorphic assumptions true? Nevertheless, on different grounds we may suggest that practically all anthropomorphic assumptions are likely to be false. The reason is very simple. Looking at the history of culture, we can see that the deeper we go into the past, the more likely we are to find anthropomorphisms; and the nearer we come to our era, the less anthropomorphic our theories become. We also know that the deeper we go into the past, the more likely we are to find erroneous views, or at least, views we consider erroneous today. For this historical reason, we may claim that by and large anthropomorphism is "out." The question that this approach raises, of course, is, is there some fundamental defect in anthropomorphism?

This leads us to the third point. We know that some anthropomorphic ideas rest on false assumptions (or at least on views that we find unacceptable) — indeed, often one false assumption may generate quite a few analogies. We speak pejoratively of anthropomorphic analogies that present no problems to us because they depend on unacceptable assumptions. The most prominent example is anthropocentrism, namely, the idea that the universe is created for the benefit of the human race and, therefore, may be judged from the viewpoint of its utility. For instance, the essence of wood, Aristotle suggests in his *Physics*, is that it is floatable and combustible, for the obvious reason that the most important functions that he ascribed to wood were in its use as material for ship-building and as fuel. One may wonder, were Aristotle living today, whether he would make the essence of wood reside in its use as raw material for making paper. A similar criticism of Aristotle is found in the late Renaissance and the seventeenth century; for instance, in the works of Robert Boyle, who suggested the following observation: for many people the essence of ice is that it can melt into water, and thus, its essence is water; whereas, for doctors, who use ice for lowering temperatures, the essence of water may be that it is freezable into ice.

This criticism of anthropocentrism is not decisive, of course. It is quite possible to claim that though it is an error to judge wood and ice on the basis of their present usefulness, we should judge the essence of wood or ice from the viewpoint of humanity throughout its whole history. Perhaps it is very difficult to find out the total possible uses of wood or ice from the beginning to the end of human history; but anthropocentrists might claim that this is what science should be about — that science is more difficult than Aristotle thought, precisely because scientific knowledge grows by attempting to find new uses of different natural things. It looks as if this generalized anthropocentrism is merely an intellectual exercise, but one may interpret instrumentalism in science as just that. Instrumentalists, however, will object. Somehow, the evidence that anthropocentrism happened to be parochial in the past is deemed evidence that anthropocentrism in any form must be parochial; and parochialism, of course, is “out”.

We come, finally, to the fourth and last point about anthropomorphism. It may be viewed (rightly or wrongly) as a version of the parochialism that Sir Francis Bacon designated as the Idols of the Tribe and of the Cave. Parochialism is the projection of our present knowledge of our limited environment into the whole universe. It is the idea the worm in the apple has, that the whole world is an apple. And, of course, anthropomorphism may be viewed as a version of parochialism in the sense that we are very close to ourselves, and having some notions of our human traits, we generalize and project hem into the universe at large.

This seems to be the final condemnation of anthropomorphism. Somehow, we all condemn parochialism and we feel that, viewed historically, science on the whole aims to break down parochial barriers, to give us a better view of the universe, rather than to reinforce the views into which we are born or which are due to space-time accidents of birth. And inasmuch as anthropomorphism is historically parochial, or has its roots historically in parochial philosophy, this makes anthropomorphism run against the spirit of science, and as such it is “out”.

There is also quite a different aspect or positive value of anthropomorphism in the history of science, which cannot be condemned as parochialism, namely, the human uses of science. To take very simple and obvious examples, scientists have devised many sorts of machines that imitate human operations. This, at least in part, is a technological matter of purely practical significance, interest, or value. We all want to jettison as many of our human burdens as possible with impunity; so we try to dump them on machines. Thus engineers will apply science to the designing of machines to perform as accurately as possible as many human functions as possible. One might say all this technology is devoid of intellectual value. But this is only partly true. There is much to be gained scientifically in the theories of servo-mechanisms and "thinking machines" as they are half-jokingly called: we do want to embody part of our views of our functions and of our thought-processes in the observable operations of models, and thus form generalizations in a more scientific and interesting manner. What we learn from these mechanical models may then be used in research — say in biology.

Whether we try to apply our knowledge of machines to humans, or *vice versa*, there is in each case an intellectual — even philosophic — interest. We can give examples of both cases, and show thereby that there are certain interactions between the human and the nonhuman sciences, as well as between sciences and technologies. These are very stimulating, very suggestive, intellectually very fruitful — and thereby justifiable. Take examples of the applications of scientific knowledge of the inanimate world to the animate world, to humans in particular. Not only have scientists claimed in a succession of hypotheses that the eye is the *camera obscura*, that the eye is a camera (with a lens), but also that the eye is a television camera of some sort. These are various physiological views of the function of the eyes. We also attempt the opposite when we apply the theories that were first created for explaining human phenomena to the explanation of nonhuman phenomena; there is no reason to discard such hypotheses just because of their anthropomorphic origin. To give a simple example, and a well-known one indeed, Darwin was influenced by Malthus. Malthus wrote on economic competition and struggle for food as limiting population growth, and Darwin wrote on the origin of species and of biological ecology; nobody ever dreamt of censuring Darwin just because he was indebted to Malthus. For more see I. Bernard Cohen *Interactions: Some Contacts between the Natural Sciences and the Social Sciences*, 1994.

To give another simple example, perhaps more intricate but more important in history, there is nothing more evidently anthropomorphic than the ideas of attraction and repulsion, of love and hate. The introduction of the ideas of love and hate into physics by the Stoics, and in modern times by William Gilbert in his *De Magnete* (1600) and by Sir Isaac Newton, is certainly not in itself condemnable. There is even something very interesting in the further development of the theory of love and hate, or attraction and repulsion, in the history of physics. When attraction and repulsion appear together in Newton's *Principia* (1687), they are put together as a theory of force, and the idea of force was considered at that time to be highly animistic.

Newton was criticized for his animism and for his occult qualities. He insists in his *Opticks* (1704) that his theories are proper explanations rather than *ad hoc*, and true (as they provide precise predictions), so that there is no room for complaints about them even if they may need further explanation to fit them into the Cartesian philosophy that does not recognize forces.

Newton's theory of force was abstract — at least as compared to ideas of force we employ when we speak of applying force to break through locked doors, etc. — the force of the muscles, the actions of the muscles, the disposition of the muscles to act. James Clerk Maxwell, in his *Treatise on Electricity and Magnetism* (1873), compared Faraday's tubes of force to muscles. The tubes of force by which Faraday operated, however abstract they were, had two qualities. They tend to shorten and to become wider, in a manner very similar to that of a tube of a muscle. So the criticism that the Newtonians of the day launched against Faraday, namely, that his theory was distinctly anthropomorphic and less abstract than the Newton's, is quite understandable. Those in the Newtonian camp who were indulgent towards Faraday, such as John Tyndall and Hermann von Helmholtz, stressed that they had no quarrel with Faraday's use of those concrete images because of his "want of mathematical culture": people who were better versed in mathematics than Faraday, then, can do without his anthropomorphic analogy. Historically, this is why Maxwell's work was so important: he translated Faraday's images into abstract (mathematical) language; even Tyndall was very impressed.

In an open correspondence between Faraday and Tyndall, published in the *Philosophical Magazine* (1856), Tyndall says that he cannot imagine how empty space can have all these strange properties that Faraday ascribes to it, its pulsations with tensions and strains. In answer, Faraday declares him unimaginative, in need of a better developed intuition.

Is this not misplaced concreteness? Perhaps accommodating for it is, rather than its acceptance as is. We cannot tell. In the history of science this has diverse manifestations. We may accommodate for Faraday's muscle-like fields by filling space with matter ("ether") to accommodate its strains and stresses. We may suggest that the world is simple because we prefer simplicity, or economy of thought. We may suggest that science should be mathematical since reality is (Galileo: "The Book of Nature is written in geometrical characters"). We may suggest as a speculation that the world is composed of fragmentary units of "atomic facts" because we state our information about the world in such fragmentary propositions. The picture theory of language is perhaps one of the most significant manifestations of anthropomorphism insofar as it imputes to reality the limitations of our mode of representing it. In the twentieth century it was crystallized in the early work of Ludwig Wittgenstein (*Tractatus Logico-Philosophicus*, 1922), and, for a while Bertrand Russell advocated it too.

Is anthropomorphism still alive? We cannot know. One aspect of it is parochialism, and, typically, its holders do not consider themselves parochial. That is to say, we never know how parochial we are. We only know of some parochialism of our predecessors that we are free of. Similarly, it is quite possible that we still hold various versions of anthropomorphism that our successors may reject in their efforts to free themselves of our errors and parochial limitations.

In spite of this caution, it is possible to explain a few facts about the historical growth of science as it moves away from anthropomorphism. We saw examples of the interaction between ideas in the social sciences and those in biology and physics. Hence, we condemn not anthropomorphism but its parochialism. Now it is hard to draw a very clear line between parochial and non-parochial anthropomorphisms, as the main feature of anthropomorphism is its use of analogy from human to nonhuman phenomena and the idea of analogy is often very vague. Consider again the theory of space pulsating with stresses and strains, common to Faraday's metaphysics and to Einstein's science — his theory of relativity. It is easy to suggest that however abstract the idea of pulsating space is in comparison with the theory of the pulsating ether in space, there still is an analogy between Einstein's space and any piece of elastic material such as plain rubber. In other words, however abstract our scientific ideas are, we can draw analogies between them and more concrete ideas, and so we can claim that our ideas are always lamentably concrete and possibly parochial, that we are still rooted in our space-time environment, in local contingent conditions, whether physiological, biological, or social.

Although from time to time we may find analogies that are stimulating, exciting, and interesting, the substance of scientific progress cannot rest on analogies to the given, but rather on novel ideas, on ever increasing abstractions. This explains the situation alluded to early in this discussion: the more we go into the distant past, the more we see anthropomorphism in more stark-naked versions. The progress of science is a progress from the more immediate, from the more parochial, to the more abstract, to the more general. And this very increase of generality and abstraction moves us away from the anthropomorphic.

This characteristic explains why even our views of human nature, whether psychological, anthropological, sociological, economical, or any other, are increasingly less anthropomorphic, increasingly more abstract. There are very well-known, clamorous protests about making the science of humanity so abstract as to dehumanize it; for example, it is said that economists have defiled economics by the invention of that monster, the economic man. There may be some truth in such claims, but there is also a Luddite attitude lurking in them, the anthropomorphic, parochial, wish to destroy what looks threatening. Once we realize that anthropomorphism often leads to considering true the familiar and the comfortably acceptable, we see that anthropomorphism is uncritical and so it is objectionable even in the social sciences. Still, it is hard to speak against anthropomorphism in human sciences; we do better to speak against parochialism.

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8. Newtonianism Before and After the Einsteinian Revolution

Preliminary Remarks on the Concept of Approximation

Approximations are tolerable and even welcome inaccuracies. The right degree of accuracy is what we agree is a sufficiently good approximation. It has both practical and theoretical limits. The degree of accuracy of a given measurement is decided by theory; but how is the accuracy or precision of the theory itself to be judged? Often the theory contributes much less to the inaccuracy or imprecision of a measurement than do its practical aspects. Sometimes there is a better theory to inform us as to the conditions under which a result guided by a lesser theory is a sufficiently good approximation. But what if there is not better theory? Which given theory is best? Is the best available theory good enough? Is the best theory available not also a mere approximation to a still better, as yet unknown, theory?

This is a difficult matter. Given the truth, we can assess the degree of approximation to it attained by a theory or by measurement. The assumption that a new theory is true offers means for the assessment of the degree of approximation that the older one offers under some specific conditions. But if the truth is only asymptotically approached, then there is no way to decide this. What determines the degree of precision of a theory is not the idea of a better instrument or a better calculation, but the better theory that asserts under what conditions what result of the lesser theory is good enough an approximation. The claim that the end of an asymptote is true is that it is a virtually true description of the conditions under which extant theories are sufficiently good approximations. Without it, we are left uninformed as to what these conditions are.

This difficulty is well-known among philosophers of science these days; it will not be aired here. Suffice it to notice that this very difficulty depends on the understanding of certain concepts, the concepts of approximate measurement, of a theory being sufficiently approximate to a later theory and of the approximation of the (as yet unknown, possibly unknowable) true theory. In brief, there may be no answer to the questions, "what is a good approximation?" and "what are the conditions under which one quantity or function is a good approximation to another?" These questions are troublesome, because we understand their implications. It is painfully clear why Einstein left open the question "is an ultimate theory possible?" but insisted on the imperative to hope for one. This also explains the great difficulty of developing his philosophy and the great incentive to stick to the older philosophy that suggests that Newtonian mechanics is the last word.

A. Newtonian Mechanics Before and After Einstein

Newtonian mechanics as it is understood today is not radically different from Newtonian mechanics as it was understood in the last days of its reign — before the Einsteinian revolution, especially in the formulation as presented by Poincaré.[1] Its ideas of the laws of motion and of force are the same today as they were earlier, and so are its demands that forces should be conservative, stay in Euclidean space and act at a distance in absolute time. He maintained these ideas despite obvious reservations, whether regarding the claim that forces act at a distance or that space is Euclidean. This attitude invites examination, both as to the reasons behind it, and as to its meaning. Here we encounter a disagreement: thinkers differ, but most of the examination of this attitude takes place outside physics, whether it is physics as it is taught or as it is practiced in research. Within physics there is remarkable unanimity. In certain specified cases, the old theory is a good approximation to the new theory and in these cases the old theory is often the easier to use. Consider, for example, the common practice of presenting gravity as if it were constant rather than variable. For a wide variety of studies of events that take place near the surface of the earth, it matters not whether we take it to be constant, as did Galileo, or a variable, as did Gilbert and Newton. The same holds for the invariability of time intervals and of inertial mass that Newton has taken as constant and Einstein did not.

The concern of the present discussion is with the differences between Newtonian mechanics today and Newtonian mechanics in its last days. The most important one is, possibly, the current accent on the invariance of Newtonian mechanics to Galilean transformations, an idea that is post Einsteinian. It is hard for a physicist today, schooled in the characterization of a theory by the groups of transformations to which it is invariant, to realize that this approach is hardly hinted at before the advent of general relativity, or at least before Lorentz transformations replaced Galilean transformations. Indeed, the clearest presentation of Newtonian mechanics as invariant to Galilean transformations is in Einstein's classic *The Meaning of Relativity*. But even that idea is not quite absent from the classical presentation of Newtonian mechanics; it is adumbrated in the principle of inertia, as we call Newton's first law. Now that principle is worded in two different ways, as if they were one. Put as the principle of uniform motion under no force, it is a special case of the dependence of acceleration on force; seen this way Newton's first law is a special case of Newton's second law. Put as the law concerning the indifference of a system's behavior to any uniform motion, it is fully Galilean invariance.

The absence of recognized measures of degrees of difference or of criteria of similarity or dissimilarity makes it hard to judge how much the current view of Newtonian mechanics differs from the view of it held a century ago. It does seem clear, however, that some differences are rather marginal. For example, though we are more used now to derive a description from a Lagrangian or a Hamiltonian function than was customary a century ago, this is of little significance. It is generally true, it seems, that there is little significance to the differences in customary wording of the theory today from that a century ago.

This is not an obvious fact; not always does a theory remain unchanged through the ages. On the contrary, as Einstein has observed, it takes a long time before a theory is crystallized into a canonic version.[2] Thus, reading Newton's own texts is a struggles, and even with later ones it is, and the difficulty in reading the old texts is not merely a matter of different notation. There are differences in the intended meaning and in some significant perceptions of important characteristics of the theory. Consider Newton's theory of gravity. Whereas we view it as a theory of a conservative field par excellence, he himself thought otherwise. And since he considered his theory to be absolutely true, he assumed that now and then God Himself interferes in the solar system in order to supply the missing energy and ensure its stability. But even when there is no discrepancy regarding observed facts, there is the change of attitude to reckon with. The most conspicuous example is the change of attitudes to Newton's first law, his law of inertia. We take it to be a corollary to his to his law of force or a special case of it. He did not. Why? The first law has no special symbols and is worded in reasonably modem English, yet those not steeped in Cartesian physics will doubtless find it either redundant or hard to comprehend. No such difficulties impede readers of late-nineteenth century texts on Newtonian mechanics; after Helmholtz, Hamilton, and Jacobi, the picture was quite clear. Not that there was unanimity as to the different aspects of the theory, but the disagreement concerned the status of the theory, not its contents.

The situation was problematic and open to dispute, yet the disputed material itself was understood clearly enough, as the seventeenth and eighteenth centuries debates over what Newton's theory of gravity said. In the nineteenth century, there was no such dispute about the theory. The main dispute over the status of Newtonian mechanics in the end of the Newtonian era was led by Kelvin and Tait whom Mach followed.[3] It concerned the concept of force: can it be eliminated from the theoretical description?

That the situation remained controversial to the last, but that the controversy was clear enough, can be compared to the situation of the present dispute about quantum paradoxes and wave-particle duality. The question there is, are its two versions equivalent? The proof that they are (Schrödinger) is not general enough.[4] Einstein and Bohr disagreed on the interpretation of the whole system. This is much more disturbing.[5]

Assuming that Newtonian mechanics itself has undergone no alterations since Hamilton and Jacobi, one cannot help notice that it looks these days remarkably different from the way it looked before Einstein stepped in: it has not changed, but its historical settings have. And the historical setting of a scientific theory has two aspects. One is informative content and thus concerns both metaphysics (intellectual framework) and related scientific theories. The other concerns the status and methods of science in general. Let me now discuss the informative content of the theory leading to a discussion of its methodological aspect.

B. Newtonian Mechanics within and without Newtonianism

The *locus classicus* of the definition of the Newtonian system was the opening passage of the classic essay of young Hermann von Helmholtz, "On the Conservation of Force," of 1847.[6] Ernst Cassirer, unaware of field theories (and, anyway, controversies within science were beyond his concern), quoted this passage as the expression of the spirit of the age. In that famous passage Helmholtz said, no physical theory is satisfactory unless it assumes the existence of nothing other than particles interacting at a distance according to Newton's three laws. If a theory does not comply with this requirement, he adjudicated, it may be accepted, but as an empty, possibly useful mathematical formula, not as a meaningful informative theory. This judgment of Helmholtz is not simply a statement about Newtonian mechanics. It is the claim that no theory should be taken as satisfactory unless it fits the Newtonian system, even though it may be used in a tentative way or even taken seriously.

The distinctness of the Newtonian system from Newtonian mechanics is discussed in fascinating detail in I. Bernard Cohen's classic *Franklin and Newton*. He depicts there in fascinating detail the usefulness of the Newtonian system or intellectual framework for Franklin's researches.[7] The system, of course, neither follows from nor entails any specific theory. To be within the Newtonian system, or within its framework, a scientific theory has to include Newton's laws of motion and, in addition, a law specifying the dependence of some force on the distance between some sets of some particles. But the obedience of Newtonian gravitational forces to Newton's three laws does not oblige all forces do the same. (The forces in electromagnetism are not central; they are conservative only globally, not locally.) Nor does it follow from the demand that gravitation should act as a conservative Newtonian force that it should vary as the inverse square of the distance between interacting bodies. It was known before Einstein, and noted by Ernst Mach in his popular *On Mechanics*,[8] a work that helped lead Einstein to conclude that the Newtonian formula for gravity can be slightly modified by postulating that it propagates with the speed of light or by the addition to it of a small factor varying as the inverse cube of the distance, and that this modification has the advantage that it can explain the secular variation of Mercury's observed orbit from its calculated orbit. The added factor must be so small as to have an effect too little to observe except for the planets nearest the Sun.

The demand of Helmholtz that all theories of physics be squeezed into the Newtonian mold shows both its liberal and its illiberal sides. It was liberal of him to recognize such theories, to admit them to the club; it was illiberal of him to grant them only the status of visitors, until they came to behave in accord with the accepted rules.

The insistence of young Helmholtz on an illiberal rule, however, was not idiosyncratic. Being in no position to legislate, he was merely reaffirming in the opening of his essay the accepted code that he presumed his readers shared. He reaffirmed it perhaps because he was going to violate it, and he was anxious to look conservative so as to get away with his heresy. He was anxious to allow field theories on a pretext: since in the Newtonian system forces come in equal and opposite pairs so that their sum is always zero, orthodox Newtonians can admit the field theory of conservation of force. Though this solves one problem concerning the conduct of the young Helmholtz, it raises another, new and more difficult one, concerning most of the Newtonians in the nineteenth century: why were they rigid in their reluctance to consider non-Newtonian theories?

The intolerance that Helmholtz exhibited was much less stringent than that prevalent in his society. This explains the rejection of "On the Conservation of Force," that he then published privately. The official excuse, as recorded in the historical literature, is that it was suspected that he was a *Naturphilosoph*, a disciple of the philosophy of Schelling and Hegel, that (rightly or wrongly) whose highly anti-scientific was reputed.[9] This has to raise an eyebrow; what in that essay could incur the suspicion of conformity to the metaphysics of these philosophers? Is this suspicion valid?

Most historians of science are unwisely apologetic for science and for the great scientists of the past; so they do not wish to belabor past scientific mistakes. In addition, they are often guilty of praising what is obviously praiseworthy, through lack of courage, competence, or originality. The literature does not discuss the question raised here: why was Helmholtz suspected as an advocate of *Naturphilosophie*?

The accusation that one advocates *Naturphilosophie* is but the accusation that one is a deviant. Generally, all deviants from a given orthodoxy share certain ideas in their deviation from the same orthodoxy, and the establishment likes to them all place in the same basket and to dismiss them all for the same fault. This is particularly true of *Naturphilosophie*, since its exponents were notoriously ignorant, especially of mathematics. This can hardly be said of Helmholtz, but to claim that he was a deviant was more easily sustainable. His deviation was that he attempted compromise between the established Newtonian system of action at a distance and the newly evolved field system. He did so by finding something they shared. The law of action and reaction guarantees that the sum total of all active forces is zero. Hence within Newtonianism, forces conserve. The idea that forces conserve, the theory of the conservation of force, advocated by Helmholtz as an orthodox Newtonian idea, was, however, something introduced by the field theorists. This, of course, is an excuse, and it is too superficial; the reason field theorists claimed that forces conserve is that the magnitude of a force is unchangeable, that the magnitude of each force is constant, that forces are capable of transformation of quality (for example, an electric force can become magnetic), but not of quantity. In Newtonianism, by contrast, forces cannot transform (since each force is the quality of a specific matter), but their magnitudes change with the distance between interacting bodies!

One wonders then why so much maneuvering was necessary. Why not present the Newtonian and the field systems side by side and let readers decide for themselves? To understand this, we must move from the informative content of science to its status. Briefly, the dogmatic refusal to notice scientific mistakes, not to mention important scientific mistakes, rests on the faith in the claim for certitude of empirical scientific theories. The faith in certitude imposed the faith that all scientific theories are absolutely true.

C. Rationality as Scientific Proof

We come now to the most important difference between the accepted picture of Newtonian mechanics then and now: in its time it was deemed absolutely true, and it is now it is deemed merely approximately true. This distinction invites some explanation before any elaboration on the matter of nineteenth-century Newtonianism can be undertaken.

At the outset, an observation of great importance from the field of the sociology of science is called for. Today the status of a scientific theory is usually given by physical scientists to any theory that is considered verified. This happens either when a theory is properly verified or if it is an approximation — presumably to another theory that is considered properly verified. More accurately, a theory is often deemed verified when predictions based on it are sufficiently approximate to the information accrued in its empirical test. Now any valid verification of a theory is valid once and for all; a verified theory is true for all times. Hence there is a significant difference between verification and an approximation; many extant theories, such as Newtonian mechanics, are surprisingly good approximations; but they are not properly verified, not once and for all. No empirical theory is properly verified once and for all. Yet it is futile to tell scientists not to call an approximately true theory verified. They know the distinction between a mere approximation and a verification (or "confirmation" or "empirical support"). Yet, they do not shrink from imprecise, inadequate language. This is similar to, but rather more serious than, their continued use of the word "atom" for particles that are by now known not to be atomic in the original Greek sense: they are not indivisible. They do not care for words.

We need not censure physicists for their showing only passing interest in this question of when a theory can be said to be empirical or verified. The question is difficult, and it is a constant source of unwanted controversy; but it is central to the philosophy of science. It is advisable then to take it slowly, and to go to the history of science before tackling it head on. In order to understand the history of physics, perhaps one need not know the answer to our question. To understand the history of science, one can make do with the observation that among physicists the accepted answer to the question has undergone a serious change. This is important, since it is impossible to comprehend the views physicists had a century ago on the status of Newtonian mechanics without seeing that they deemed it properly verified, not a mere approximation. This is an observation from the social history of science, and it would be a bit difficult to examine it empirically. Yet the evidence for it is very strong and it was never contested.

Historians of physics are not in the habit of drawing attention to the fact that physicists have radically altered their view on the nature of science. They are not prone to stress that in the nineteenth century most physicists declared a theory scientific only if it had been verified, yet today they are prepared to consider an approximation to a verified theory scientific as well. On the contrary, most historians of science have allowed their readers and audiences to conclude that throughout the ages their own views of the nature of science, and their own answer to our question, are always shared by most scientists, or at least by most able scientists since the onset of the scientific revolution. The important exception is Pierre Duhem, at once a physicist and a great philosopher and historian of science. Duhem emphatically declared that his view of science contradicts the received view. According to the received view, he observed, scientific theories are empirically verifiable; in his own view, empirical verification is always impossible. He went so far as to say that, quite possibly, we should one day deviate from Newtonian mechanics. This was heresy, and to his fellow physicists Duhem was suspect. He was eager to go to Paris, and finally he was invited to go there—but only as a historian of science. He took offense and rejected the invitation.[10]

Our concern here is with history, not with physics. We might then have hoped to be enlightened by Duhem concerning the history of views about the status of science. Alas! He was a partisan for his view that scientific theories are mere instruments for prediction, because, I suspect (though contrary to his explicit disclaimer), he wished to reinstate the Roman Catholic view of science as free of metaphysics; he wanted to reinstate Aristotle, in accord with Catholic tradition. Furthermore, since he wrote before the Einsteinian revolution, and since he was opposed to Einstein to his dying day in 1918, he is of no help to us in the question of the impact of Einstein on the way we view Newtonianism.

Most studies of the history of Newtonian mechanics leave in their readers the very confused impression that there is no disagreement between the followers of Newton and those of Einstein. It is no accident that many historians of physics, including reputable physicists such as Sir Edmund Whittaker, declare Newtonian mechanics to be absolutely true in their work on the history of classical physics, but only approximately true in their work on the history of twentieth-century physics. This carelessness is itself of little importance, but it hinders the explanation of the rigidity researchers showed in the pre-Einstein years. This, however, the majority of historians of science totally ignore, since their works present science and its history as perfect. But the history of humanity shows that nothing human is perfect. It is interesting to note the rigidity of our predecessors, and for that we have to realize that they almost all accepted the theory that rationality requires proof and that the rationality of empirical science is that of empirical proof. The view that scientists generally received then, was that rationality equals proof and that science is the paradigm of rationality. And philosophers and the educated public hardly dared disagree. Even anti-scientific philosophers did not venture to contest this popular claim, and they gave vent to their anti-scientific or anti-rationalistic views by concocting odd and new forms of discourse that they were pleased to call proof.

To avoid misconception, let me add this as an aside. Nineteenth-century and twentieth-century irrationalist philosophies are different. Nineteenth-century irrationalists, to repeat, claimed scientific status for their pipe dreams; their later heirs jettisoned reason. Prominent among the philosophers hostile to reason in general and to science in particular then was Martin Heidegger. He said that scientific rationality is confined to science and that scientific truths concern technology exclusively so that science has no real intellectual value. It is poetic truths, he added, that really signify.[11]

To conclude, physicists before the Einsteinian revolution of 1905, deemed science the body of properly verified theories; not so in the twentieth century. The change makes it much easier to appreciate the difference in research methods before and after that revolution, its depth, and the difficulty of its execution. The place to begin is with the difficulty that the revolution had to surmount, both as to theory and as to methodology. Thus we turn to the traditional attitudes towards approximations, given the view that scientific theories are verifiable.

D. Approximations Then and Now

Once we declare a theory verified and hence true, there is no difficulty considering approximations to it. So the idea of approximation to the truth is highly intuitive and as old as the hills. It fascinated Galileo, who asked if there can be a general measure of proximity to the truth.[12] Newton developed powerful methods of approximation that are still used in number theory (not in analysis or physics, where the Taylor expansion opened much more powerful techniques). In the present context, it is scarcely possible to ignore Newton's perturbation method, of which he was rightly proud. His theory explained (with the aid of his perturbation method, it should be added) the minute deviations of the outer planets from their Keplerian orbits. He considered this a powerful empirical argument in favor of his theory. As is well known, the many-body problem of Newtonian gravity remains unsolved; there is no escape from the need to employ approximate solutions to it.

Nevertheless, approximately true scientific theories were considered inferior at best. For example, Galileo's discussion of Archimedes' law indicates clearly that he deemed it absolutely true, whereas we know it to be only approximately true. The Aristotelian theory of gravity assigned to bodies both gravity and levity, in measures that depend on the heavy and the light elements that they contain. The simple refutation of this seems to be the floating of a stone or metal container, placed on the water in such a way that it does not fill with water. Aristotle paid notice to this fact in the very end of *On the Heavens* and declared that it is the shape of the container that is responsible for the floating, very much like the needle that floats on the water if placed carefully enough so as not to break the surface of the water. To put it in modern terms, the floating body may be subject to surface tension in addition to the forces of gravity and of levity.

Aristotle's theory is a terribly bad one, even though some popular contemporary historians and philosophers of science, notably Thomas S. Kuhn and Paul K. Feyerabend, have tried to rehabilitate it in the context of Aristotle's own time. But this is not easy in view of his blunders. If a body is subject to gravity and levity alone, it cannot be subject to gravity and levity and surface tension too. He repeatedly amended his theories by small corrections and modifications, forgetting that logically the correction, however small, renders the uncorrected theory false. This is an important point that should constantly stay in focus during the present discussion. Furthermore, ascribing surface tension to water under a metal boat but not under a wooden boat is somewhat suspect. Aristotle simply did not work his theory out in detail. Nevertheless, surface tension does exist, and the floating body will be subject not only to the force given by Archimedes' law, but to surface tension as well. Hence Archimedes' theory does not hold, and is refuted in cases where surface tension is important, as in the case of a floating needle. Finally, we cannot accept Archimedes' wording of his law, nor Galileo's: a floating body is no more weightless than a book resting on a table! But all this does not reduce Archimedes' law to the level of the theory of Aristotle. This, quite contrary to the view of Kuhn and Feyerabend, is why we teach Archimedes but not Aristotle in elementary physics classes.

Galileo was the first to recognize the following simple fact: that the admission of a correction to a theory, even if it is very small and even if it appears only under very rare circumstances, renders the uncorrected version of the theory false, though it may be a very good approximation to the corrected version. This is a great discovery, and it must increase our admiration for Galileo's rigor and tenacity and his intellectual courage.

Galileo placed much weight on the absolute correctness of Archimedes' law, and chiefly because Archimedes theory is presented axiomatically.[13] We may remember that Galileo became a Copernican because he was an Archimedean and recognized that although Aristotle's theory of gravity conflicts with Copernicanism, Archimedes' theory does not. He therefore attempted to prove that Archimedes' theory is absolutely true.

His proof is very simple: since Archimedes' theory is axiomatic, is must be true. Hence, when a needle with a specific gravity greater than that of water floats, it must expel a quantity of water greater than its volume.

What Galileo assumed, in effect, is that a repulsive capillary force increases the volume of water expelled by the needle so as to enable it to float in accord with Archimedes' law. I hasten to add that neither surface tension nor capillarity were understood at the time in the same way they are today. In any case, Galileo declared his view not to be empirically refuted. Now clearly Archimedes' law is not absolutely true, as it does not, for example, fully describe the case of the floating needle. Historians of physics attempt to vindicate Galileo, thereby unwittingly granting recognition to some of his accusers. We should not consider him in need of defense.

When considering Galileo's theory of falling bodies, one need not be exceptionally clever to see that taking his theory of gravitational acceleration as constant implies a flat earth. Indeed, when he takes the limit of the inclined plane as a horizontal straight line he more than implies it — he makes it patently clear. There is, of course, no difficulty in viewing a small part of the surface of the earth as flat. The mathematical considerations are largely trivial, but interesting. The Galilean parabolic path of a projectile is an ellipse with one focus in infinity; taking the center of the earth as infinitely far away does make the earth flat. This kind of reasoning is now very familiar — recall that Einstein's radiation theory of 1917 was an exercise of deriving Bohr's equations from simple considerations with temperatures taken to the extreme. But it was not easy for Galileo's contemporaries to see this, and even Newton had to explain it in some detail in his First *Dialogue*.

What is surprising is that Galileo insisted that his theory is absolutely true. It was easy for Newton to see it as an approximation to his own, as he taught that his own theory was absolutely true. He did not. His discussion of Kepler and of Galileo (*Principia*, Book III; compare the different edition on this) is therefore convolute. And it is much harder to view the last word as only an approximation to some unknown, perhaps unknowable truth.

E. Problems with Approximations

While physicists need not address philosophical problems, they cannot avoid philosophy altogether, even in cases in which their philosophical views are inadequate and superficial. One can hardly expect them to be indifferent to the theory of rationality and to go on doing their job, leaving all considerations of rationality to philosophers. The idea of the Age of Reason — that they should officiate as their own personal philosophers — is still very popular among physicists; and to some extent this is simply unavoidable: one has to have some idea of the meaning and worth of one's activities.

In the middle of the twentieth century the opposite idea evolved; the idea that philosophers should cease studying the aim and meaning of science and, taking the worth of science for granted, should leave the details to scientists. This view deprives researchers of all feel for the idea that science has value beyond its predictive value that suffices for power worshippers who admire science from afar. Researchers value predictive also because they want to know that science is rational; it is not that prediction is valued more than rationality. As long as rationality is identified with verification, verification is what they crave. And the theory of rationality as verification either vague, or it is empirical proof in the sense of the nineteenth century that is known to be erroneous.

This is not to demand from scientists a new theory of rationality. It is unreasonable to expect them to develop such a theory, but it is to say that one is needed, and that a good theory of rationality may help them.

There is no easy way to alter the theory of rationality of science. Our choice of options is very limited and none is comfortable. As a result of the Einsteinian revolution it is known that the classical theory of rationality as proof is quite inadequate. This requires that we develop a new theory to answer the following question: what theory or theories do physicists consider empirically verified, and why, and with what justice? The classical alternative to verification is the view that scientific theory is a mere instrument for prediction. Supposing this to be true, how do we construct a system of the world? We have the option of having no system of the world at all, of sticking to whatever commonsense view we happen to possess, of taking up one upon the faith of our forefathers, or of taking one capriciously or at random. None of these options is to be seriously entertained, especially given our understanding of how powerful a means of fostering research such a system can be. When physicists praise a theory for its predictive value, they rarely suggest that there is no cognitive value to the theory beyond its instrumental value — the exceptions being the religious or the otherwise metaphysically committed physicists, whose commitments bar them from taking physical theories literally.

What other options are there? Two are to be found in the literature: the so-called inductivist option, the approximationist option, and possibly their conjunction or disjunction.[14] The inductivist view is popular among philosophers. When they attempt to apply it to science proper, however, it becomes painfully clear that they smuggle in the approximationist approach. It is clear, then, that the inductivist option alone is not seriously entertained. As to the combination of the inductivist and approximationist approaches, it raises two problems. First, is it necessary to have both? Second, is it possible to have both? The need to have both is felt because of the feeling that the latest theory justifies its predecessors that are approximations to it, but the latest theory needs inductive justification. As to the question of compatibility, it is linked to the question of the status of an approximate theory. On this, Galileo's observation still stands, and waiving it is to fall back on Aristotelian confusions. An approximation, he observed, however good it may be, is still false, since it contradicts the theory it approximates.

The theory of science as successive levels of explanation each inductively derived from their predecessors is now in jeopardy. It is bad enough, as Karl Popper observes, that one admits the need to have rules not validated by logic. To have rules contrary to logic is too much. But things go further. Leave induction as a bad job and agree with a number of classical thinkers such as Galileo, Descartes, Kant, and Whewell: initially a scientific idea is but a figment of the imagination. What makes it more than that, what elevates it from the status of sheer fantasy to the status of science, is its explanatory power; science is a series of theories each of which explains its predecessors. This idea is very appealing and rarely questioned, but the popular theory of explanation of one law as mere deduction of it from another is invalidated by the theory of approximations. For, if Kepler's theory contradicts Newton's, and Newton's contradicts Einstein's, then the theory of explanation forwarded by Whewell, Hempel, and Popper is false. (Popper has modified his theory to account for approximations, but has not withdrawn his original theory.) Where does that leave us? We can say that a theory is scientific when it is validated; when, in particular, it has passed a severe test.

This is the theory that Whewell advocated, and perhaps Popper did so too. Also, Whewell considered validation a complete verification, which Popper rejected, of course. He attempted to present his view as a version of approximationism. A new theory, he said, has to undergo a crucial test against the old, and prove better both as an approximation and as an account of the facts. Yet these two conditions are not identical; the claim that one theory is a better approximation to the truth, whatever it means, also means that any future crucial tests between the two will go the same way. That is to say, once we establish that the planetary orbit deviates from its Keplerian ellipse in accord with Newton, then there will never be an observation of an orbit that will show a clear preference for Kepler over Newton. It is, of course, quite possible that in a contest between two theories not all crucial experiments will go the same way. In this case we shall, of course, look for third theory that will do better either. But if experience has gone only in one direction, are we justified in expecting that it will continue to go that way?

Helmholtz explained matters.[15] If experience agrees with a given theory systematically, he said, then it may be accidental, or due to the theory being true. The more evidence we have, the more likely it is that it systematically verifies the theory because the theory is true. This lovely argument has been superseded by Einstein, who took a third option: it may be an accident and it may be due to the theory being true, as Helmholtz observed, but it may also be due to the theory being approximately true under special circumstances. Once we view Newton's theory as successful because it is an approximation to Einstein's, we cannot finally avoid acknowledging that Einstein's is successful because it approximates yet another, still unknown theory. Does this series of theories converge? There is no reason to think so!

The difficulty concerns the complexity of the very concept of approximation. Two of its elements are intertwined and commonsensical; one of them is very sophisticated. The two are of the more-or-less characteristic and of the under-most-common-circumstances characteristic. That is to say, an approximate result resembles the true result (a) when its quantitative measure is close enough and (b) when the conditions are of the common variety. That the two are intertwined is easy to see: under some conditions the inaccuracies can be magnified to any desired degree. All this is straightforward and known from the very early days of the scientific revolution.

The third aspect of the concept of approximation is irksome. Whereas the first two aspects relate the approximate to the true, the third correlates degrees of approximation and these allow series of approximations — perhaps even without ever achieving the truth. This is both sophisticated and problematic. The problems it relates to are now being aired in the philosophical literature, and the situation does not seem to be sufficiently under control.

As long as we operate with the concept of approximation to the truth, the difference between the true and the approximate is merely technical. As long as the true is in hand, or even around the corner, the approximate is a shortcut for some technical problems or a shortcut to the achievement of the truth. This is how Galileo looked at things and this is how things remained until the Einsteinian revolution. Things look very different when we have two false alternative theories and we do not purport to have the true alternative to them. How do we judge which of the two is nearer to the truth? Is there a reason to assume that there is a complete ordering of these theories? The answer seems to be in the negative. What this amounts to in concrete cases is that we either claim that relativity is true or that we do not know if our contention that it is nearer some unknown truth is at all significant. To indicate the enormity of the problem, it remains to show that the complete ordering of theories is impossible.

This is an abstract consideration. A more abstract one is that if we have no guarantee that the language we are using is capable of expressing the whole of God's truth about the universe, then it is meaningless to talk about the ultimate truth, the ultimate reality. But this invites total arbitrariness within science. To make the argument less abstract, let us consider some examples. In the nineteenth century, as we know, the theories of electricity that postulated action at a distance were much more popular among physicists than field theories — and for the reason that they seemed to be more in conformity with Newtonianism. The forces were not conservative, and that worried researchers, but fields of force worried them even more. There was a series of theories of electrodynamics: Coulomb's theory, Weber's, Ritz's theory, and more.[16] Except for Coulomb's theory, they have all ceased to be commonly recognized series of approximations to current theories. Why is this? If we should take Galileo's theory of gravity to be an approximation to Kepler's, we cannot take Kepler's as a possible approximation to Galileo's . In the cases of Newton and Einstein we could have viewed matters either way and needed experience to guide us. In his original 1905 paper introducing special relativity, Einstein showed that Newtonian kinetic energy is an approximation to the relativistic formula.[17] But insofar as the truth of the theory is concerned, it could equally have been the other way around.

In other words, only when we take verifications to be indicators as to the possible increased proximity to the truth can we take the idea of approximate truth seriously. But this is when we have no justification but mere indications. And indications can easily mislead: they belong to heuristics, not to epistemology. They may help us direct our thoughts, but have no knowledge claims attached to them. This surely requires a revolutionary new view of rationality. The very best we have is still not so refined —debugged — that we may deem the problem solved. We can thus easily imagine how hard it was in the nineteenth century to relinquish the classical theory of rationality, at a time when it looked as if Newtonian mechanics would never be refuted by empirical evidence.

F. Approximationism and Research

The pre-history of approximationism is rich and the discussion of it cannot be pursued here. One example should suffice: at times Newton declared the views of his predecessors, Kepler and Galileo, approximations to his own theory, and at other times he declared them absolutely true.[18] This is of course very embarrassing. Newton also declared that the Copernican system (the sun in the center of the universe) is absolutely true, even though it is not clear that it is consistent with his own system. This case is less embarrassing since it was declared to be true only in absolute space, and it is not clear what role absolute space has in his system (other than to account for rotations).

Without discussing these embarrassments, we may notice that what disturbed Newton was his realization that a theory that is an approximation to the truth, no matter how good, is false, and that no falsehood is provable, so that he was pressed by the theory of rationality as proof to declare Galileo's or Kepler's theory absolutely true, or not rational! Of course the way out is to devise a new theory of rationality, but to expect this even from Newton would be anachronistic.

It is a strange circumstance that the great defect of scholasticism is in its pretense that approximate theories are identical with the theories they approximate, that the great contribution of Galileo to the scientific ethos was his discovery of this contradiction, and that the great Newton confused the issue. Newton was not, of course, the only one.

Leibniz attempted to deduce Newton's theory of gravity from Kepler's laws, as did Newton's friends Colin McLaurin and Henry Pemberton, following Newton's lead here in accord with the way they understood the Principia. When the antiscientific *Naturphilosoph* Hegel used this to prove that (the Englishman) Newton had plagiarized from (the German) Kepler, national pride and the rescue of the honor of science forced thinkers to have the issue clarified. It was by no means an easy job, however. It was achieved by the great mid-nineteenth-century philosopher of science William Whewell, who attacked Hegel by proving that the two theories, Kepler's and Newton's, were logically equivalent only for a two-body system.[19] The proof is in the deduction of a contradiction from their equivalency in the three-body system. This is in keeping, we recall, with Galileo's idea that the modified version of a theory is inconsistent with the unmodified version of the same theory. This fact is very hard to accept, because of not only the Aristotelian tradition that still has a deep hold on us, but also the identification of the rational with the correct. Many philosophers of science are still ignorant of this point, despite its elaboration with admirable clarity in the writings of Karl Popper.

The difficulty concerned the very idea that a theory may be scientific even if it is false and in contradiction to a scientific theory that is true. Whewell himself struggled with this idea, and his philosophy got entangled and was soon rejected as heresy. Clearly the alternative open to thinkers in such a situation is to opt for approximations as scientific regardless of the contradiction between the latest theory and its approximation; yet this is fraught with difficulties too.

Physicists of the nineteenth century did not articulate such difficulties; they simply did not concern themselves with approximations. Thus when Wilhelm Weber developed a modification of Ampère's theory of currents acting at a distance, he declared Ampère's theory plainly a prejudice![20] James Clerk Maxwell was more generous. When he rejected the advanced potentials of Lorentz, he proposed to offer Lorentz's theory, as a consolation prize, the status of an approximation. Yet Maxwell clearly thought too poorly of that prize. For when he attempted to calculate the field energy of Newtonian gravity he found out quickly that the energy is divergent. He was very disturbed and decided not to continue the study of gravity; it did not occur to him to study the possibility that the Newtonian gravitational force acts not quite at a distance! It is not that he could not imagine that possibility, for it was precisely the possibility proposed by Michael Faraday, whom he deeply admired. The possibility simply seemed to him too revolutionary to be entertained seriously.

Excluding the possibility that Newtonian mechanics is but an approximation to the truth, it might seem that there is no choice but to stay within the Newtonian system. This, however, need not be so; one may choose any system that one thinks is consistent with it. This explains Helmholtz, and Kelvin and Tait, and Maxwell as well. These were all followers of the field system who were hoping to show that both the field and Newtonian systems were coherent with the Cartesian system. This gave them some freedom, but not much, since it forced the advocates of field theories to reconcile this idea with the Newtonian system — that, as Kelvin and Maxwell explained in great detail, can only be done in the context of theories about the aether. This is why the problem of the aether drift was deemed so central in the physics of the end of the Newtonian era.[21]

All this is not to say that Einstein developed approximationism in 1905. On the contrary, in "On the Electrodynamics of Moving Bodies" he came closest to holding the view that physics is better off without a system —

a view that he repeatedly called, somewhat humorously no doubt, the sin of his youth. The fact that his 1905 paper contains a formula for approximation does not of course make it revolutionary. The revolutionary aspect of it is that it dethrones Newtonianism and places the field system firmly and independently on the map. Of course Einstein did not view the field system in 1905 in the same way he did toward the end of his life. Rather, in his paper on the photoelectric effect of the same year, he saw Maxwell's electromagnetic field theory as an approximation to some future photon theory of light. In each of the three celebrated papers of 1905 he spoke of approximation in the sense of approximation to the truth. Yet in each he had no theory that the current and established theories were supposed to approximate. This showed great daring.[22]

It transpired only decades later, mainly through the insights of my former teacher Karl Popper, whom Einstein greatly encouraged and with whom he often agreed, that Einstein's work was pregnant with revolutionary ideas in methodology and in metaphysics no less that in physics.[23] And, as with the physics, they have raised in these fields too, more problems than solutions.

Notes and References

1. Henri Poincaré held a conventionalist philosophy of physics and presented dynamics in its field potential form, to render it as similar to electromagnetic theory as possible. He discussed this similarity is in his philosophical writings (*Science and Hypothesis, Science and Method, The Value of Science* [collected as *The Foundations of Science*, 1982]), and illustrated in his astronomical and mechanical monographs. The similarity was, however, very partial and superficial. His opposition to Lorentz's contraction stems from this effort to stress this similarity. See A. I. Miller, "Of Some Other Approaches to Electrodynamics in 1905," in *Some Strangeness in the Proportion: a centenary symposium to celebrate the achievements of Albert Einstein*, Harry Woolf, ed., 1980, 66-91, where Poincaré's conventionalism is presented briefly, on pages 68 and 87, in a one line text and a one line note (n. 6). Consequently, the paper tends to condemn rather than explain. Readers interested in Poincaré should not miss Jaki's *Uneasy Genius*, 1984.
2. See Albert Einstein, *The Meaning of Relativity*, 1921, 5th edition, 1970. Gerald Holton notes that Einstein did not call his theory "the theory of relativity"; he preferred the title of "*invariantentheorie*" ("theory of invariants"). See Gerald Holton, "Einstein's Scientific Program: the Formative Years," in Harry Woolf, *Some Strangeness in the Proportion*, 49-63, 57. It is interesting to compare this essay with that of Abraham Pais, "Einstein on Particles, Fields, and the Quantum Theory," *ibid.*, 197-265, especially Section 8. One should also compare 226 and 472 on Einstein's anticipation of de Broglie's material wave equation.
3. See Max Jammer, *The Concept of Force*: *a Study in the Foundations of Dynamics*, 1957. See also my *Faraday as Natural Philosopher*, 1971, 75, 114, 154, 194, 205, and 312.
4. On the alleged equivalence of Schrodinger's equation and matrix mechanics, see Mario Bunge, *Foundations of Physics*, 1967, 249 and 253 and his *Philosophy of Physics*, 1973, 113. On the formal aspects of the theory, see Michael Redhead, *Incompleteness, Nonlocality, and Realism*, 1987, that presents the quantum formalism

in its Dirac and its von Neumann formulation; the two earlier versions have proved too partial to serve as frameworks, and there is no proof of equivalence without some framework, of course. Schrodinger proved that the *eigenvalues* of his equation are the same as the *eigenvalues* of matrix mechanics in the (non-relativistic) cases that were central in the time, but already the next step, Max Born's application of Schrodinger's equation to scattering phenomena, makes the equivalence very limited, as scattering is continuous and matrices discrete.

5. What is now called entanglement was deemed impossible by both sides. The famous paradox of Einstein, Podolsky and Rosen is proof that quantum mechanics demands it and so they deemed it incomplete. Bohr said it was impossible to display it experimentally. Since it is, the debate is radically altered.

6. Hermann von Helmholtz, "On the Conservation of Force", *Scientific Memoires Selected from the Transactions of Foreign Academies of Science; Natural Philosophy*, Tyndall and Francis, eds., 1853. See also Ernst Cassirer, *The Problem of Knowledge: Philosophy, Science and History Since Hegel*, 1950, 87-88.

7. I. Bernard Cohen, *Franklin and Newton: An Inquiry Into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof*, 1956.

8. See my *Faraday as a Natural Philosopher*, 209 and 330, for the suggestion that no force acts at a distance, and Ernst Mach, *The Science of Mechanics: a critical and historical exposition of its principles* [1883], 1902, 334-35, for an endorsement of this view and a report of Paul Gerber's explanation (1898) of the secular motion of the perihelion of Mercury assuming that gravity propagates with the speed of light.

   It is well-known that this result is incorporated in the general theory of relativity. Some philosophers of science have concluded from the fact that Einstein reported being confirmed by this result that when he worked on his theory he was not aware of the perihelion advance. See my *The Gentle Art of Philosophical Polemics*, 1988, 346. Philosophers of science are bound to be increasingly entangled in scholasticism until they come to see that confirmation in technology is regulated by law; see my *Technology: Philosophical and Social Aspects*, 1985. In science, confirmation is either heuristic or an increase of explanatory power, or, as in a crucial experiment, a refutation in disguise. See my *Science in Flux*, 1975.

   Mach's readiness to criticize Newton's mechanics (see, for example, 245, 492, 507, 535, 562-3, and 570ff) despite overwhelming confirmation and his readiness to see it modified is presumably what made Einstein suggest that he could discover the special theory of relativity. Einstein thought that the greatest obstacle to the development of that theory was the unwillingness to deviate from Newtonianism. This is, indeed, the point of the present essay. Yet Mach's attitude was not as fallibilist as Einstein's, despite some clearly fallibilist statements of his. See my *The Gentle Art of Philosophical Polemics*, 21-32 on Mach.

9. Sir Edmund Whittaker, *A History of Theories of the Aether and Electricity,* Vol. 1*, The Classical Theories*, revised and enlarged edition 1951, 183.

10. See Stanley L. Jaki, *Uneasy Genius: The Life and Work of Pierre Duhem*, 1984, 374.

11. Any discussion of Martin Heidegger is bound to be controversial. The question here is not whether Heidegger advocated a theory of poetic truth, and not that he promulgated the view that poetic truth is higher than the prosaic, the scientific-technological; the question is, can the poetic legitimately constrain the prosaic? When one stays on the surface, one may evade this question, as does George Steiner, in his relatively lucid and uncomplimentary *Martin Heidegger*, 1979, 146. Laslo

Versenyi cannot afford this luxury, as his book is dedicated to *Heidegger, Being, and Truth*, 1965, 1984, where being is the poetic mode and where the truth is either in the inferior prosaic mode or in the lofty poetic one. He quotes the master, 41, to say that "we must presuppose truth" and declares that this cannot be taken literally [since it is a travesty on the truth]. Later, 92ff, he clearly sides with the master in the claim that art is superior to science and that artistic judgment of truth overrides scientific ones. Reiner Schurmann in his *Heidegger on Being and Action; from Principle to Anarchy*, 1987, in the notes to his section 24 on *alethea* (truth), cites a leading follower of Heidegger who reads him as I do and presents a more sane reading. There is always a way to read an obscure text as sane, of course; all one need do is ignore or reinterpret the obscure, especially when the unpleasant is put obscurely. Embarrassingly for his disciples, Heidegger at times expressed his unpleasant ideas crisply.

12. See the superb Isaac Todhunter, *A History of the Mathematical Theory of Probability*, 1865, quoted by Ilkka Niiniluoto, *Truthlikeness*, 1987, 163.
13. See my *Towards an Historiography of Science*; see also my *Science and Society*, 1981, 336.
14. The literature on the theory of rationality is swelling. See my "Theories of Rationality" in *Rationality: The Critical View*, J. Agassi and I. C. Jarvie, eds., 1987, 249-63.
15. See Hermann von Helmholtz, "Faraday Lecture", *The Faraday Lectures*, 1928, 133: "It is against the rules of probability that the train of thought which has led to . . . series of surprising and unexpected discoveries … should be without firm … foundations of truth …" This is known in the literature as Fisher's likelihood measure: a hypothesis is likely if it renders improbable observation reports probable.
16. See E. T. Whittaker, *A History of Theories of the Aether*. Also, my *Faraday*, 110-16.
17. It has scarcely been noticed that the special theory of relativity is incomplete in the sense that there are different methods and different targets of approximation between it and other theories; I know of no writer on this except for Mario Bunge. See his *Philosophy of Physics*, 184-85. On the whole, his discussion of inter-theoretic correspondence (chapter 9) is illuminating and substantially above the platitudes and popular errors perpetuated both by physicists and by philosophers as the philosophy of physics.
18. See William Whewell, *On the Philosophy of Discovery*, 1860, Appendix H, "On Hegel's Criticism of Newton's Principia." See also my *Towards an Historiography of Science*. For more details, see William Shea, "The Young Hegel's Quest for a Philosophy of Science or Pitting Kepler Against Newton," in *Scientific Philosophy Today: Essays in Honor of Mario Bunge*, J. Agassi and R. S. Cohen, eds., 1982, 381-97.
19. See my *Towards and Historiography of Science*. See also I. B. Cohen, "Newton's attribution of the first two laws of motion to Galileo," in *Atti del Symposium Internazionale di Storia, Metodologia, Logica e Filosofia della Scienza "Galileo nella Storia e nella Filosofia della Scienza," Collection des Travaux de l'Academie Intemationale d'Histoire des Sciences, no. 16, Vinci (Florence): Gruppo Italiano di Storia dell Scienza*, 1967, xxv-xxliv: "Galileo seems to have put forward the view, rather explicitly expressed in his book on the sunspots, his *Dialogue* … that such inertial motion could be and was uniform circular motion. Only near or at the surface of the earth did he conceive of a true linear motion, and even that was apt to be a small arc of a very large circle." All this cautious circumlocution could not save Cohen from inaccuracy: "a true linear motion" is never "apt to be [on] a small arc" no matter how "very large" the circle is.
20. See my *Towards an Historiography of Science*, note 79.

21. The opening paragraphs of Kelvin and Tait, *Elements of Natural Philosophy* (1894), seem to me to say this rather shamefacedly. See my *Faraday as a Natural Philosopher,* p. 194. This point was lost when Cartesianism lost its appeal after the Einsteinian revolution, especially as Mach condemned Kelvin and Tait for their unorthodox Newtonianism; see his *Mechanics,* p. 245. Maxwell's uncharacteristic reluctance to revise Newtonian gravity is reported in Whittaker's *History*. This, of course, is what made it so urgent to find the aether drift. Historians of science and textbook writers stress the significance of the aether drift and thereby also of the experiment that Michelson and Morley designed in vain effort to find it; they do not attempt to explain its importance or ask why it was considered important at the time. The same holds for the idea of a field in empty space was so strange sounding that the majority of 19th-century physicists that they deemed it utterly out of the question. I discuss this point at length in *Faraday as a Natural Philosopher*.
22. Gerald Holton's valuable essay on Einstein's program, citing in note 2 above, has no explicit reference to Einstein's approximationism or even to his having transcended Newtonianism. It does make the point, however, about Einstein's daring and ambition, and illustrates it in Einstein's earliest writings that were till then neglected.
23. See Karl Popper, "Three views concerning human knowledge," reprinted in his *Conjectures and Refutations*, 1963. See also Einstein's letter to him published in Popper's *Logic of Scientific Discovery*, 1959.

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