The Very Idea of Modern Science

BOSTON STUDIES IN THE PHILOSOPHY AND HISTORY OF SCIENCE

VOLUME 298

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The Very Idea of Modern Science

Francis Bacon and Robert Boyle

Springer

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ISSN 0068-0346
ISBN 978-94-007-5350-1 ISBN 978-94-007-5351-8 (eBook)
DOI 10.1007/978-94-007-5351-8
Springer Dordrecht Heidelberg New York London

Library of Congress Control Number: 2012954273

Printed on acid-free paper

Springer is part of Springer Science+Business Media (www.springer.com)

In memory of
Alexandre Koyré and
I. Bernard Cohen
two gentle souls and
the two great lights of the history of science.

Nor, however, can it be allowed for the intellect to leap and fly from particulars to remote and … most general axioms … and by means of their (supposed) immovable truth to prove and make intermediate axioms …. We must add not wings but weights and lead to the intellect, so as to hinder all leaping and flying.

Bacon

… a fateful "fear of metaphysics" arose which has come to be a malady of contemporary empiricist philosophizing.

Einstein

It has been asserted that metaphysical speculation is a thing of the past, and that physical science has extirpated it. The discussion …, however, does not appear to be in danger of coming to an end in our time, and the exercise of speculation continues as fascinating to every fresh mind as it was in the days of Thales.

Maxwell

It is not to be supposed for a moment that speculations are useless …. They are wonderful aids in the hands of the experimenters and the mathematicians…. Let the imagination go, guarding it by judgment and principle, but holding it and directing it by experiment.

Faraday

Abstract

This book is a study of the scientific revolution as a movement of scientific researchers, mostly amateurs, organized in scientific societies that functioned as clubs. It presents the philosophy of the Enlightenment Movement as their ideology, and describes their philosophy, their magnificent institutions and the admirable way in which they fashioned modern science. This book also presents what was missing in the organization of science as the cause of its having given way in stages to the professional science that has replaced it (first in industry and then in the academy). In particular, the book studies the contributions of Sir Francis Bacon and of the Hon. Robert Boyle to the process of the rise of modern science.

The philosophy of induction is notoriously problematic, yet its great asset is that it expressed the view of the Enlightenment Movement about science. This explains the ambivalence that we still exhibit towards Sir Francis Bacon whose radicalism and his vision of pure and applied science still play a major role in popular philosophy and comprise a major factor in the fabric of contemporary society.

Finally, the book discusses Boyle's philosophy, his agreement with and dissent from Bacon and the way he single-handedly turned a few barely educated aristocrats into an army of able amateur researchers.

Preface

This book is a rewrite of my doctoral dissertation, *The Function of Interpretation in Physics*, written under the guidance of Professor Karl Popper of the London School of Economics and Political Science and submitted to the University of London in June 1956. It was a part of a longer draft; the other parts served as drafts for works published in 1963 and in 1971. This is the last part to go to the printer.

This work deviates from the original 1956 dissertation marginally, and in a few ways. First, it is shortened. Second, its wording is altered, hopefully for the better. Third, it includes some minor corrections of the original. Fourth, passages are added to refer to and comment on some of the up-to-date literature. (These added extras are easy to detect, since they refer to works that appeared after 1956.) Finally, some paragraphs are added that contain some background information (plus references). Generally, the recent literature on Bacon and on Boyle and around them seems to me valuable, especially as additional background information; it does not add much to the narrative presented here, about the ideology and methodology of the time of rise of early modern science. My main secondary sources are classical: Leslie Stephen (1876, 1904), Martha Ornstein Bronfenbrenner (1913), Dorothy Stimson (1939, 1948), Paul Hazard (1935, 1948), Richard Foster Jones (1920, 1936, 1951) and Harcourt Brown (1934). I should add reference to a remarkable paper by Phillip George (1952) on the scientific movement as amateur, as seen in publications of the period. The vast later literature on this and related subjects contains little that comes close to the works just mentioned in scholarly quality or in empathic imagination. More to my concern, it has nothing that comes even close to a serious discussion of the early modern scientific movement as amateur. This is odd since the works just mentioned are clear enough about the situation. My discussion of it is less sociological and more ideological and methodological.

The institutions of the commonwealth of learning want some serious overhaul, and to this end a study of the ideology and methodology of early modern science and its founders may be of some use. The rise of modern science was a stupendous development whose ideology and methodology merit study anyway. Very few monographs deal with it. This neglect is largely due to methodological naturalism, to the naïve idea that the institutions of science are natural reflections of research practices that are the only natural option. Not so: research practices comprise institutions; these grew partly due to designs that were naturally not always the most felicitous. Their designers applied their views on proper research practices and on what is essential to their proper institution. Admirable as these are, they want overhaul.

Herzliyah
Summer, 2012

Acknowledgement

Ian Jarvie of York University, Toronto, and Daniel Cohen of Maccabee Seed Company, Davis CA, have my sincere gratitude for their careful study of the penultimate drafts of this work and for their innumerable valuable corrections and suggestions. I cannot thank them enough.

My gratitude to various people, all of them sadly long deceased, for their help to me that enabled me to write the initial version of this study that was my 1956 doctoral dissertation.

My greatest debt here is to Professor Karl Popper, my thesis supervisor at the London School of Economics and Political Science. To a large extent this study is an effort at an application of ideas present in his writings, especially in his *Logic of Scientific Discovery* (1935, 1959) and more so his "The Nature of Philosophical Problems and Their Roots in Science" (1952). It was while under its strong impression that I decided to become his student. Since then and for years he paid much attention to my work and helped me on every occasion possible and in many ways. He taught me how to write. Many ideas presented here I learned in his lecture courses. Many more came from him in private discussions of his work and of mine. The extent of the cooperation in which we worked has exceeded the usual framework of supervision. I cannot say how much I am indebted to him.

My next debt is to Professor J. W. N. Watkins, Popper's junior colleague at the School. He invested innumerable hours in patient help: he read my mammoth manuscript, corrected my faulty English, and made many extremely valuable suggestions for cuts and for improving my presentation. This provoked many critical discussions, so that he contributed also to the substance of this study. Many friends and co-students also helped in many ways and won my gratitude as well.

Vital help came from many London libraries, their managements, librarians and other staff. They all have my profound gratitude for their great help and for their abundant kindness and courtesy. Among these were the British Library of Political and Economic Science of the London School of Economics and Political Science, the Senate House Library of the University of London, the University College London Library; the Royal Institution Library of Science, the Library and Archives of the Institute of Electrical Engineering and the Royal Society's Library and Archives.

My very special gratitude for extreme generosity goes to the librarians and other staff of the Round Reading Room (now no longer in use) of the British Museum, where I spent most of my happy research time.

I could not possibly do it without financial help, for which I thank many generous people and institutions. Among them are the London School of Economics and Political Science and its Director, Sir Alexander Carr-Saunders, the Anglo-Israeli Association, especially Harry Sacher, the British Friends of the Hebrew University, especially Professor Norman Bentwich, Dr. Walter Zander, and Sir Keith Joseph (later Secretary of State for Education and Science), as well as benefactress Audrey Sacher and financier Siegmund George Warburg.

For permission to cite lengthy passages form the writings of Robert Boyle I am grateful to the Royal Society of London, to Michael Hunter and Edward B. Davis, editors of the 1999–2000 edition of his *Works*, and to their publisher, Pickering and Chatto, London.

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Introductory Note

A few monographs on the works of Sir Francis Bacon appeared in recent decades, partly due to the discovery of some hitherto unknown manuscripts of his and partly in efforts to present him as an original metaphysician.[1] They usually suffer from two defects. First, they should make it clear, as they do too seldom, that his metaphysical assertions are not coherent and that he fiercely opposed the presentation of metaphysical systems. Second, they usually disregard the traditional critique of Bacon's works, especially Justus von Liebig's magisterial if exaggerated derision for them and Ellis' admirably balanced critical study of them. We should remember them in agreement or not but in great respect.

This study endorses the two opposing traditions about Bacon—of the eighteenth-century admiration for him and of the nineteenth-century debunking of him. Here are my main points about his texts.

(a) They are hard to interpret, as they are often inconsistent with no record of any changes of opinion.
(b) They are most interesting when they deal with induction; the parts of his works that treat induction deserve the best defense before they come under attack.
(c) When treated with indulgence they help explain Bacon's tremendous influence as the father of radicalism[2] and thus of the whole of the Enlightenment Movement.

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[1]The best study of Bacon's output as systematic is Paolo Rossi, *Francis Bacon: From Magic to Science*, 1978. It is an interesting exercise on Renaissance thought.

[2]Only Charles Whitney touches upon Bacon's radicalism when he ascribes to him a revolutionary and prophetic character. See for example his *Francis Bacon and Modernity*, 1986. The review of this book by Brian Vickers, "Francis Bacon and the Progress of Knowledge" *Journal of History of Ideas*, 53, 1992, 495–518, 508, is an interesting survey of the Bacon literature. It is unreliable, however; for example, it takes as authoritative Peter Urbach's fickle "Francis Bacon as a Precursor to Popper", *British Journal of Philosophy of Science*, 33, 1982, 113–132.

Chapter 1
The Riddle of Bacon

I have taken all knowledge to be my province.

Bacon (*Works,* 8, 109)

There was a man born blind, who had several Apprentices in his own condition: Their Employment was to mix Colours for Painters which their master taught them to distinguish by feeling and smelling. It was indeed a misfortune to find them at that Time not very perfect in their lessons; and the Professor himself happened to be generally mistaken: This Artist is much encouraged and esteemed by the whole Fraternity.

Jonathan Swift (1841, 41)

From 1661 to 1831 the majority of the European thinkers and practically all those who were interested in natural science considered Bacon the father of the experimental method. It was common knowledge that experimentation is as old as humanity. What then was his contribution to it? They considered him the profoundest thinker of all ages except for Newton (Rees 2002, 379). Why? Bacon's high reputation declined: ever more critics considered him a mystic and an obscurantist (he believed in magic). Today most historians of thought hardly appreciate him and none view him as nearly as important as he was once reputed to be. Some of them seek a balanced view by ascribing to him some familiar ideas, usually ones that he expressed contempt for (as will be described later on). How did it happen that one and the same writer was once at the height of philosophical esteem and then for a short while a target of rather harsh ridicule and then entirely forgotten?

Parenthetically, let me confess, this problem has engaged me because of my peculiar appreciation of Bacon. He was as sloppy a writer as one can find, yet as brilliant and engaging nonetheless. I therefore judge reasonable both extreme opinions of him. Yet this observation is parenthetic: it seems to me that the riddle of Bacon is engaging no matter how we view him: why are opinions about him so diverse? Hardly any commentator on him has noted this great diversity of opinions about him. Why? This problem is derivative, however, and so it is less intriguing: commentators signify little in comparison with the whole commonwealth of learning.

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*,
Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_1, © Springer Science+Business Media Dordrecht 2013

1.1 The Problem of Methodology

It is no news that the blind read by feeling and that some chemists identify some substances by smelling. Science even helps us distinguish colors that we humans can never see and sounds that we can never hear. Swift, who mocked at the apprentices of the blind professor in the Academy of his fictitious Lagado, was poking fun at their metaphorical blindness to the fact that they were looking for the obvious in devious ways; that they preferred the blind professor's dubious method to the simple ordinary one. He felt that though the truth may hide at times, we should not pretend that it is always beyond reach, or that the obvious is in need of being discovered. As he put it in his terrific "Tritical Essay",

> … although truth may be difficult to find, because, as the Philosophers observe, She dwells in the Bottom of a Well; yet we need not, like blind Men, grope in the open Day-light.

The blind professor works in the experimental department of the Academy in Lagado which Lemuel Gulliver visits (during his voyage to Laputa) where the whole Fraternity gropes in the dark as if they were blind. They are what Bacon called Empirics.[1] For, Bacon, like Swift, scorned the pure experimentalists who work without ever intending to achieve theoretical knowledge. As Swift showed, they tried out any new experiment that they could imagine. For, there is one kind of natural history that is made for its own sake, and quite another kind that is the gathering of information in order for it to construct a new philosophy.

The theoretical department of Lagado s Academy works upon a similar principle. Its members, led by another Professor, consider with equal seriousness any theory whatsoever, especially theories written down at random by a roulette of words. (The words, we are also told, belong to a universal language.) The Professor is what Bacon called Reasoner or Rationalist. He takes seriously any idea whatsoever, no matter how obviously false it may be; he is imbued with ideas but blind to facts.

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[1] It looks as if no one fits Bacon's portrait of the empirics, that Bacon presents their portrait as a mere abstract option, that possibly in his extremism he wished to show himself less extremist than thinkers who did not exist. This is an error. On the contrary, most inductivists are empirics. To see this, we should notice three details. First, the facts under discussion are general facts — for all inductivists but the Bayes, who advocate the idea that identifies the (inductive) probability of a hypothesis with the probability of a chance event. This idea is an innovation of the early twentieth-century and even were it not a dud (see below) we should ignore it in the present context. Second, at least in astronomy it was impossible to speak of facts with no reference to mathematics. Ignorant inductivists ignored mathematics nonetheless, as Bacon did, and the better informed ones took it for granted, perhaps as *a priori* true knowledge, as Descartes did, or even as synthetic *a priori* knowledge, as Kant did. All this admittedly made less obvious the answer to the question, how do we generalize? Nevertheless, observations admitted in science are generalizations of observations (see below). Third, Petrus Ramus (Pierre de la Ramée) of the generation preceding Bacon's advocated this view, as did Newton and as did Liebig in his essay on Bacon. And the argument for this is from the consensus: it is the best theory that justifies the scientific consensus. Except that it is false, of course, as in science controversy prevails and generalizations are admitted by convention (see below).

The Reasoner is one who fell to obvious errors because he did not consult experiment, as he should have done (*Novum Organum*, 1, Aph. 63). Both the Empiric and the Reasoner are on the wrong track. As we all know, science is the offspring of what Bacon called the marriage between the intellect and the world, namely, the offspring of the world of experience that comprises a mix of Reason and Perception.

The question that Bacon, as every other methodologist, has tried to answer, is not whether there exists such a marriage, or whether it is at all fruitful. Even philosophers who deemed all knowledge *a priori* valid never dreamt of denying importance to the contribution of observation and experiment to the growth of science and the great value of observations. The question then is, how do theory and experience cooperate? How do reason and perception cooperate? This is the fundamental problem of scientific method. Opinions concerning it diverge, but one thing is clear: researchers have only to hear that a certain theory of scientific method is that of an Empiric or that of a Reasoner, and they will reject it.

Swift did not say how reasoning and experience do or should cooperate, as he did not pretend to be a methodologist. His report on the methodology popular at his time is exaggerated as befitting a satire. This satire brought an interesting comment from Ernst Mach, a leading methodologist of two centuries later (Mach 1896, 174; Cajori 1929, 55):

> I do not know whether Swift's academy … in which great discoveries and inventions were made by a sort of verbal game of dice, was intended as a satire on Francis Bacon's method of making discoveries by means of huge synoptic tables constructed by scribes. It certainly would not have been ill placed.

This is an interesting illustration of the confusion that surrounded the question, what is the proper cooperation between reasoning and experiment? For, as it happens, Mach's theory of discovery is the same as Bacon's. the theory they both condemned is that new theories inspire new experiments that lead to discovery; the theory they both advocated is that discovery comes to open-eyed and unprejudiced observers and it should lead to new theories. They both said, properly experience leads to theory whereas the other way around is improper.

It seems Swift was right when he said humbly that being a bystander he saw more of the game than those who played it ("Tritical Essay"). He would not have quarreled with Mach's contention that starting with theory without experiment is ridiculous, as his story of the game of words, of the *Ars Nova*, as well as of the idea of a universal language, clearly refer to the Continental speculative school. On the other hand, Swift also mocked at the other methodology, the one which starts from pure experiment. He had in mind Bacon, but it would very well fit Mach himself: researchers in Lagado perform experiments with an eye on their utility—on the benefit that may accrue from them. In the passage preceding that of the blind man, Gulliver reports about an architect, the Inventor of a new method of building houses, namely by starting from the top—a method that he justified by the like practice of those two prudent insects the Bee and the Spider .

This may allude to Bacon's demand of researchers to be always prudent and to his most often quoted aphorism (*Novum Organum*, I. Aph. 95):

> The Empirics[2] are like ants; they only collect and use; the Reasoners resemble spiders, which make cobwebs out of their own substance. But the bee takes the middle course; it gathers material from flowers but it transforms and digests it by a power of its own.

Now we are back at our starting point. All researchers know that they should employ both experiment and reason; their fundamental problem, the one that methodology and epistemology attempt to answer is, how is this done? What are the relations between theory and experiment?

That Swift alluded to Bacon when talking about the blind man who was appreciated by the whole Fraternity in spite of his always mistaken experiments may be difficult to prove, although no one fits this characteristic better than Bacon.[3] But it is possible to show that Swift's satire is more penetrating and better-aimed, even though no one might feel inclined to admit that at the first sight. For, although no researcher was an Empiric or a Reasoner, the theories of scientific method always tended to present science in the manner that Swift caricatured so well. Thus, Bacon claimed that he had found the middle way between the Empiric and the Reasoner, yet Swift was hardly mistaken in representing Bacon and the late seventeenth-century English school as empirics. (Nowadays most historians share his view). What is surprising is the disparity of the different ways that Bacon was understood at different times. The same, even more oddly, is true of Swift. Not even the similarity between Swift's mock Academy of Utopian Lagado and Bacon's serious Solomon's House in his Utopian *New Atlantis* seems to have been noticed.

More than a hundred years later, Robert Leslie Ellis, an empirical researcher and profound classical scholar of wide range, made an extensive study of Bacon's work that is still authoritative. Ellis began with great admiration of Bacon, and the more he studied him the more surprised he was at the gulf between Bacon's promise and performance. In his general preface to the philosophy of Bacon he said (*Works*, I, 66),

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[2] The translation is of Ellis but I preferred the term 'Empirics' of Kitchin to 'men of experiment' of Ellis. It has meanwhile gained some recognition.

[3] Nicolson and Mohler (1937, 323) identify the blind man with Robert Boyle for no other reason save that he discussed a very similar report. Boyle's discussion probably did provide Swift with the idea, but Boyle was the least mistaken leader of the Royal Society. Swift could have known only about his mistake concerning alchemy (the transmutation of lead to gold) that he shared with Newton almost to detail. Bacon's mistakes, on the other hand, were notorious around Swift's times, though only the opponents of the new philosophy, Swift among them, mentioned this fact. There is, however, an argument in favor of the view I here reject: The blind man is mentioned in the "Tritical Essay" that, like the famous "Meditation on a Broomstick" is a wonderful parody on Boyle. But even this argument may be reverted. It may be that Swift imitates Boyle's method of obscure allusions to Bacon when criticizing some inductivist ideas.

> Whoever considers his writings without reference to their place in the history of philosophy[4] will I think be convinced that he aimed at giving a wholly new method,—a method universally applicable, and in all cases infallible.

Yet Ellis understood Bacon to have been an empiric in Bacon's sense of the word, Bacon's own protest to the contrary notwithstanding. Ellis suggested that this prevented Bacon from adding to scientific method or to the progress of science in any possible way.

Ellis was no methodologist. His task was not to discuss scientific progress but to find out whether Bacon's description of this process is at all reasonable. And he showed that Bacon's mistake was obvious: he declared that science starts with the method of the empiric, with collection of facts. True, unlike the empiric, Bacon considered this factual knowledge to be the bricks for the erection of the house of science, the basis of future scientific theories. This is why he did not consider himself an empiric. Ellis tried to emphasize what Swift had meant to say, namely, that collecting information à la Bacon—by the empiric's method—is extremely foolish. One cannot find new facts without thinking, namely, without theorizing. Yet precisely this is what Bacon repeatedly advocated.

This point is the culmination of Ellis' criticism of Bacon that he deemed quite devastating. Bacon s work was designed to answer the major epistemological and methodological problem of all ages, the problem of how Theory and Experience interact. But, Ellis rightly observed, Bacon had failed to give an answer that should safely lead researchers to the truth. His description of scientific method breaks down completely in its first step.

We may conclude, then, that Ellis was very just to Bacon when, after presenting his argument, he completely dismissed Bacon's methodology: "That his Method is inapplicable cannot, I think, he denied" (*Works*, 1, 38). He was lost for words to describe his surprise and disappointment at his own verdict, as he began his vast undertaking out of a profound admiration for Bacon. His successor in the editorial enterprise, James Spedding, could not stomach this negative verdict (see below).

The very validity of the forceful criticism that Ellis launched against Bacon (and already Ellis himself expressed appreciation of this force) raises a great problem, and one that he and all other nineteenth-century critics of Bacon had to face: why, if at all, did Bacon deserve the great fame and the applause that he won? What was the source of his tremendous repute? The more you view him as unimportant, the harder you find it to explain his reputation among leading thinkers from Locke to Kant. In my view, despite his ignorance, poor scholarship and total misconception of anything to do with science, he is one of the greatest thinkers of all times, since his erroneous doctrine of prejudice is the root of all radicalism, scientific as well as socio-political, that is the leading characteristic of the ever so glorious Enlightenment Movement. I will explain all this in detail.

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[4] The expression "without reference to the history of philosophy" seems to me to mean "without much interpretation" or "in a straightforward manner".

1.2 The Criticism of Bacon's Writings

The story of the criticism of Bacon's ideas and works is unusual (Fowler 1878). It was highly criticized in the seventeenth century, idolized in the eighteenth, and savagely criticized in the nineteenth.

In the year 1818, *The Philosophical Transactions of the Royal Society of London* published a paper by M. Napier, entitled "Remarks illustrative of the Scope and Influence of the Philosophical Writings of Lord Bacon" (Napier 1818). That authoritative paper shows clearly how outstanding was Bacon's reputation until then; how much he was applauded and how surprisingly little open criticism commentators had leveled against his work.

James Spedding (whom Lisa Jardine describes at the end of her introduction to Bacon 2000 as "Bacon's devoted nineteenth-century editor and defender of his reputation") continued Ellis' task of editing Bacon's *Works* after Ellis premature death. He noted that even as late as the middle of the nineteenth century, if not later, Bacon was practically unanimously recognized as the father of modern science (*Works*, 1, 509). Even David Hume shared this view, although he dared say that Bacon shared this honor with others. He wrote (Hume 1868, (1754–62); Appendix to Reign of James I; Fowler 1878, 135–6) of Bacon,

> … he is justly the object of great admiration. But if we consider him merely as an author and philosopher, the light in which we view him at present, though very estimable, he was yet inferior to his contemporary Galileo, perhaps even to Kepler. Bacon pointed out at a distance the road to true philosophy: Galileo both pointed it out to others and made a considerable advance on it.[5]

Even this mild criticism was not popular. David Brewster repeated it in 1831, and in the same year as John Herschel expressed veneration for Bacon. They were both then scientific authorities. Herschel then published a book wholly devoted to methodology, a rare event following the rise of the Royal Society in the second half of the seventeenth century and prior to the crisis in physics of the turn of the twentieth. Herschel did not explain the reasons for having written the book. Curiously enough, he did not describe any problem, nor even mention any. Yet, as the book was intended to serve as an introduction to a series of scientific books, its title, *A Preliminary Discourse on the Study of Natural Philosophy*, appeared as if its publication was self-explanatory. Yet in it Herschel was facing a scientific crisis; he tried to solve the highly topical problem that the disturbing overthrow of Newton's optics had raised. Herschel was one of the individuals who precipitated the crisis, as he was the first scientific authority in England who legitimized the wave theory of light and thus overruled Newton, no less. This most significant event shook the authority of Bacon, and, indeed, started a wave of criticism directed against him. Similarly, as late as

[5] Bacon said he only pointed out at the road (Introduction to *Novum Organum*). Hume's expressions here indicate that he was familiar with the Baconian silent disapproval of Kepler's speculative bent. Incredibly, Hume said here Galileo and Bacon agreed on scientific method. This is a most unpleasant exaggeration.

1962, when Thomas S. Kuhn presented his celebrated suggestion as to how to cope with scientific revolutions, he, too, had to argue against Bacon's idea that science is inductive. Since (perhaps under Kuhn's immense influence) today the idea that repeated revolutions punctuate the growth of science is quite popular, it looks strange to us that the change in the received optical theory comprised a crisis. This crisis seeks an explanation. This explanation rests on the early nineteenth-century picture of science, one that is very different from today's picture. The idea that scientific doctrines are solid and beyond dispute clashes violently with the refutation of Newton's theory of light, thus causing great embarrassment. This embarrassment rested on Bacon's doctrine of prejudice that dismisses as prejudice every belief in a false theory. Herschel, whose book praises Bacon the methodologist, and propagates the doctrine of prejudice, exempted Newton from the harsh sentence and blamed only his followers. But Brewster, who published in the same year a biography of Newton (Brewster 1831), had to consider the problem more closely. The result was a criticism not only of the doctrine of prejudice, but also a devastating comment on Bacon's output in general. Indeed, Brewster was the first critic of inductivist methodology.

Brewster put his criticism as an argument against the vulgar view that Newton owed everything to Bacon since he employed Bacon's method that had promised certain success.[6] He said that, on the contrary, Bacon, who in other respects was a great man, made no contribution whatever to methodology. Brewster's argument ran like this: if we interpret Bacon as saying that science starts from observations, then others had said this clearly and emphatically: Leonardo, Tycho Brahe and many others had insisted that the first step in science consists of collecting information. But, if Bacon meant to say, as we must assume, that it is possible to generate science by a mechanical method (an algorithm), then he was utterly mistaken.

I shall return to this criticism of Brewster's later. Let me first refer briefly[7] to the criticism of some of his successors: of Macaulay, the essayist and historian; of Ellis, the editor of Bacon's *Works*; of Whewell, the scientist, methodologist, and historian of science; of Liebig, the celebrated founder of organic chemistry; and of De Morgan, the mathematician and logician. All of them wrote about Bacon around the middle of the nineteenth century.

What they elicited was quite embarrassing. Apart from Macaulay, who considered Bacon's induction a matter of commonsense, all of Bacon's critics declared his method inapplicable. Ellis showed in detail his ignorance of the science of his age. Liebig considered his own conclusive evidence that Bacon never heard of the principle of the lever sufficiently overwhelming. Ellis and Liebig went into detail to show that Bacon had two sources for his factual information. One was second-hand information—books, letters, a lost manuscript (of William Gilbert) that he

[6] This vulgar view is still popular: currently, the prestigious *Stanford Encyclopedia of Philosophy* on the internet states it with some amusing caution: http://plato.stanford.edu/entries/enlightenment/

[7] For a fuller account see Thomas Fowler's Preface to his 1878 edition of Bacon's *Novum Organum*.

concealed, and similar channels. The other source was his imagination. He guessed results of experiments and he described them as if he had observed them ("It was indeed a misfortune that he happened to be generally mistaken"). In all his informative works, his *Sylva Sylvarum* and other writings, there is not a single line that is both original and significant[8]. That he did not acknowledge the fact that he used the various sources of information is not extraordinary. He was rather unusual in a funny way: he transcribed from books that he condemned and he tried to belittle those to whom he owed his information. Thus he was particularly unfair to Aristotle to whom he owed practically all the elements of his theory of induction. (I should add that he was also most unfair to Bernardino Telesio this way.) Ellis showed all this mercilessly, but very fairly. Already Hume knew of Bacon's ignorance, especially in the field of mathematics. Ellis and De Morgan went further and showed that there is no room for mathematics in Bacon's methodology. Whewell, Liebig and De Morgan attacked him, claiming, contrary to his teaching, that science starts not from experiment but from theory, from thinking.

In response to Macaulay's debunking, Spedding wrote a long book in which he went far in defense of Bacon's views and conduct. On the strength of this book, Ellis bequeathed to him his unfinished work to continue as he found fit. Spedding then could not defend Bacon's idea that Ellis had exposed, namely, that Bacon hoped to erect within a few years a new and complete science all by himself, with the aid of a few laboratory assistants (*Works*, 1, 380).[9]

After a century of debunking of Bacon, one might wonder why all this tremendous effort was necessary. It is really less important that he was a plagiarist, a dilettante, an ignoramus; the more important deficiencies of his output are much more obvious: there is hardly one subject which he mentions more than once whether the *Book of Job*, Aristotle's *Problemata*, or alchemy, scientific method, religion, or metaphysics, on which he did not flatly contradict himself. If evidence for the tremendous importance of Bacon is required, then one of the best pieces extant is the great effort that his critics made to study and examine his works. For, by their verdict, his works do not deserve their studies. Even Ellis, who spent the greatest efforts in this direction, fully agreed with Brewster (and with Liebig) that Bacon was an impostor (*Works*, 2, 326, 328; 3, 45) a mystic and an obscurantist (*Works*, 1. 51–3, 2, 326; see also 3, 228).

But why was he so important? This is the riddle of Bacon.

[8] The two known possible exceptions are poor. The first is Bacon's prescription for measuring the velocity of sound by using a canon. (His first great disciple, Father Marin Mersenne, tried it.) Of course, there were no instruments to measure this. The second is Bacon's crucial experiment between Galileo's theory of gravity (according to which it is independent of height) and Gilbert's (according to which it diminishes with the distance). Herschel showed that it was unrealizable then. But, of course, in my view he was very original; on which more later.

[9] In *Novum Organum*, 1, Aph. 78, Bacon mentions that one of the causes of past failures to erect the ideal science is the brevity of the three periods of learning, the Greek, the Roman and the Western European, each of which, he said, consisted of about two centuries. Spedding removed the contradiction between this and Bacon's assessment of his ability to erect science within a few years by noting that in Bacon's view, his own new method raised the speed of discovery considerably.

1.3 The Past Suggested Solutions

Although none of Bacon's critics stated their aims explicitly, each of them, including Hume, tried to explain Bacon's fame. It was suggested that there was a popular myth behind this fame, and that in its turn this myth rested on ignorance. Augustus De Morgan (2007, 75) also claimed—quite explicitly—that Bacon was not widely read. This is easy to refute. His works were published quite often, and his jargon became part-and-parcel of the idiom of science right up-to 1905 and beyond. De Morgan's contention is true that Bacon was a figure of a myth and that his fans "pronounced blind" all of his potential critics. But it is far from being explanatory; it is the problematic situation. The problem remains: how and why did Bacon become a myth figure?

Some familiar answers are readily available. Bacon was very witty and sharp-tongued; he made propaganda for the new experimental science; he mocked effectively at the old science; he inspired people who were more able than he was. In short, he was a herald, an excellent propagandist. This is a true but insufficient explanation: propagandists for science do not become influential methodologists only because of the success of their propaganda.

Many authors have mentioned that Bacon had an important share in the formation of the Royal Society. It is well known that he wrote a fragment of a book, a utopia: his *New Atlantis* (first published posthumously in 1627). It includes a description of a college with a "spacious wonderful garden"[10] equipped with laboratories—the Solomon House—where people are engaged in discovery. That this idea inspired many people is well known. Just how utopian this vision was may become more obvious in consideration of the fact that the first English academic laboratory appeared in 1874 in Cambridge (the Cavendish), instituted by James Clerk Maxwell. He said at its inauguration that its poverty was a blessing in disguise, as it forced students to forge their own instruments and thereby learn something. Bacon's influence began in earnest over two centuries earlier, in 1659, when John Evelyn wrote a letter to Robert Boyle, suggesting to him a detailed and elaborated plan for a residential college with a quasi-military (or perhaps quasi Boy Scout) discipline, in which he referred to Bacon's ideal Solomon's House. The meeting (of the members of the Invisible Philosophical College and others) that Boyle and John Wilkins called to discuss Evelyn's letter is recognized[11] as the

[10] The "spacious wonderful garden" that is a scientific lab appears in Bacon's speeches written in 1592, *A Conference of Pleasure*, in 1594, *Gesta Grayorum,* and in 1595, *Device on the Queen's Day* (Abbott 1885, 41), before it appears in his *New Atlantis*. Obviously, this was an image of Bacon that left a strong impression on him and on others, possibly together with his doctrine of the idols (Gaukroger 2001, 45; 2005, 176). See also Bacon , "On Gardens", *Essays*.

[11] See *Dictionary of National Biography*, Art. Evelyn, John. Boyle coined the imaginative term "invisible college" in a letter to his tutor when he was very young and jus joined them. Significantly, he complained that they were too few (see Webster 1974), which is surprisingly imaginative and quite promising.

beginning of the formation of the society that soon received a Royal Charter. Thus, Bacon was recognized as the originator of the very idea of a scientific society.[12]

As an explanation for the rise of the myth of Bacon this story is the most satisfactory but still insufficient. Gratitude does not necessarily lead to acceptance of an authority and definitely not to one that prevails for so long. Evelyn's and Boyle's low opinion of Bacon's capacity as a methodologist and a researcher (see below) is the best evidence for this.

Attempting to solve the riddle, Macaulay declared Bacon a father of utilitarianism. This is partly true, but only partly (as Ellis rightly pointed out, *Works,* 1, 58). Liebig praised Bacon's essays, suggesting that his fame rested solely on his literary success. For this, he had even Bacon's own backing: in a private letter he claimed that of all his writings, only his essays would go down to posterity. Spedding found in him a man who had a deep feeling for the need for the new, for something better. Ellis considered him mainly a metaphysician. Without going into detail (these will come later) it is clear that although these attributions may help save Bacon's future fame, his past fame was as a methodologist, and this is not satisfactorily explicable by reference to other aspects of his active life.

In the twentieth century only C. D. Broad returned to the riddle of Bacon (Broad 1926). He follows Ellis and presents Bacon only as a metaphysician who never had any influence on research. Nevertheless, he felt some need to explain Bacon's past fame. The only explanation he could provide is that it was all a small mistake.[13] The *encyclopédistes* praised him as the first modern atheist, he contended,[14] because they were misled by his sharp tongue. It only remained for Broad to explain how Bacon's fame reached so far as to gain the notice of the *encyclopédistes*. The chief editor of the *Encyclopédie*, refutes this solution (D'Alembert 1995, xxxiii).

Broad too took an interest in Bacon's religion only because Bacon is so famous. Already in the seventeenth century this problem had arisen. Some opponents of the Royal Society smeared him as an atheist, while its Fellows endorsed Boyle's opinion[15] to the contrary, and Boyle was the Fellow whom even the strongest opposition to the Society respected. (He was friendly with Henry Stubbe or Stubbes, the Society's chief opponent.)

[12] This is not true. Learned academies appeared independently of Bacon's vision. See Hale (1915).

[13] William Kneale (1949, Ch. 12) and Georg Henrik von Wright (1951, 152–3) solve the riddle by ascribing to Bacon of induction by elimination. This comes close to making sense of his tables of inclusion and exclusion. Incidentally, Von Wright came nearest to stating the riddle of Bacon (*loc. cit.*).

[14] In his essay on superstition, Bacon contended that socially or politically superstitious religions are more dangerous to public peace than atheism. In his essay on atheism he said, "I had rather believe all the fables in the Legend, and the Talmud, and the Alcoran, than that this universal frame is without a mind. … a little philosophy inclineth man's mind to atheism; but depth in philosophy bringeth men's minds about to religion." This was not original in the least, but it was quite influential.

[15] Boyle's role was under-estimated until recently, when Shapin came up with the overrated suggestion that he was the anchor of the scientific revolution because he was a model gentleman, a scientist and a pious Christian. See my 1997 review of his book.

There is no escape from the problem, and if one wants to find out the reasons for Bacon's methodological influence one must find them partly at least in Bacon's methodological writings. For, it is easy to see that his methodology was influential from the mid-seventeenth century to the end of the nineteenth century if not to date.

Chapter 2
Bacon's Philosophy of Discovery

The wittes therefore of the Utopians inured and exercized in learnynge, be marueilous quycke in the inuention of feates helping annye thinge to the aduantage and wealthe of lyffe.

Thomas More (1516, 120)

My solution to the riddle of Bacon in no way contradicts the ones already mentioned. It is true that there was (and still is, especially in some secret societies) an imposing Bacon myth. It is true that his propaganda and utopianism helped him achieve his influential position. This is no explanation, however, of the respect that such giants as Boyle, Faraday and Herschel[1] had for him. They quoted his *Novum Oragnum* and recommended it to young researchers: they sincerely viewed themselves as followers of Bacon in some sense or another, and they could not possibly overlook his methodology. His stress on method was new: his methodology is the centre of his view of science, and his influence is much due to this. Unlike other philosophies, his is the view of science as a process, that of an assured continuous discovery ("in streams" and "in buckets and vessels"); it is ever progressive. This is a utopian view of science. Also, Bacon's philosophy was utopian in its suggestion that the progress of science will bring progress in general. This utopianism played a significant role in the rise of modern science, as it was a great contribution to the rise of the ethos and structure of the scientific fraternity.

Commentators systematically overlooked this obvious fact, and for two reasons. First, as Ellis stressed so much, Bacon never tried to fulfil his promise to tell his readers what they should do to acquire success in their research: he said repeatedly

[1] Herschel said (Youmans 1867, 376),

> Let, them have the glory—for glory it will really be—to have given a new impulse to public instruction by placing the *Novum Organum*, for the first time, in the hands of young men educating for active life, as a text-book, and as a regular part of their College course. It is strong meat, I admit, but its manly nutriment.

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_2,

that he had a new view of induction but he never said what it is. Second, these commentators shared some of his utopian ideas with him and deemed them trivial and they rejected other ideas of his that they deemed sheer fantasy. And, indeed, it was all fantasy; but this is no ground for dismissing it as trite or as unimportant. After all, most of what most religions offer is just this: fantasy. We may remember that towards the middle of the nineteenth century Auguste Comte introduced positivism explicitly as the religion of science, albeit in a somewhat new sense of the word. Nonetheless, that religion is Bacon's doctrine of prejudice: science is the source of all progress and metaphysics is but an obstacle to it. *Grosso modo*, already in the eighteenth century it was the received opinion among researchers with almost no exception. Dissent grew within the ranks of science, but the turnover came with Einstein in 1905 or in 1917.

To spell it out, the received opinion was this. First, researchers cannot help going on discovering new facts and new theories as long as they proceed with their proper scientific research. Second, the results of their discoveries are unassailable truths. In short, the received opinion was that we possess a science-producing machine of sorts. More abstractly, the received opinion was that humanity is in possession of a science-producing machine.

This idea is less popular today. The change in popular opinion was the recognition that the obstacles to the use of the science-producing algorithm are more formidable than tradition supposed. But faith in the algorithm is by no means extinct. Artificial intelligence researchers once took it for granted, and some of them still do. But let us return to history.

Bacon's philosophy is essentially the philosophy of steady, successful scientific progress. Now famous researchers like Joseph Black or Alessandro Volta stopped producing. This allegedly shows that they left research. At the time this caused indignation; public opinion considered even ill health barely a sufficient excuse for a desertion of the field of research. Research must go on; and if it does, then it must be fruitful—eventually. Of course, the facts of frustrated research are all too familiar. Manifestly, top-rank researchers, such as Ørsted, Planck, or Einstein, conducted research for years without success. These facts do not refute Bacon's view, however, as it guarantees success only to proper research, and obviously these researchers did not follow the rules with all the care that is required. For, why else did they fail? Followers of Bacon found it inconceivable that these researchers behaved properly yet were frustrated. What was the violation of the rules? What were the violated rules? It is the rule that researchers must be utterly free of prejudice, of course.

2.1 Bacon's Utopianism

James Spedding's life of Bacon (Bacon 1861–1874) is monumental. He was less analytically disposed than Ellis, but much more informed about their joint subject and much more cordial to him. At times this forced him to make comments that are quite odd. He found surprising that Bacon hoped to complete the whole enterprise of scientific research within a few years, yet he found not too pretentious that Bacon hoped to succeed Aristotle: he deemed Bacon learned enough to try to replace Aristotle. He admitted reluctantly that Bacon never thought that he himself might be deficient or limited like other people. As Spedding put it, Bacon "miscalculated the amount of his own force" (*Works*, 3,509–10). In other words, Spedding admitted that Bacon exaggerated his assessment of his own ability to conduct empirical research; yet Spedding possessed ample evidence showing that Bacon had not the slightest ability to conduct empirical research. Even this fact alone suffices to show how little seriously Bacon should be taken as an independent thinker. For, he happened to write most about this same subject, about people's ability to conduct research. Bacon produced one truly imaginative idea and he presented it in a very naïve manner, but very effectively. Because his idea was so utopian a vision, so naively utopian, the people in whom he planted it were moved to do what they understood him to have instructed them to do. Had his utopianism been confined only to his *New Atlantis*, he would have had hardly any influence, since secular colleges came into being about two centuries after his demise and academic empirical research came still later. Bacon's idea that disappointed Spedding is precisely what made Bacon influential: his hope for erecting the whole of science within a short time. He planted hope and even genuine anticipation of the great miracle of the rise of the structure of a new science to happen here and now. His person and his personal faith in his own abilities, that so interested Spedding (who wrote about him extensively and enthusiastically) made very little impression on the founders of the Royal Society; his argument that science can and must progress marvellously here and now did kindle in them an unequalled enthusiasm. He generated a messianic or utopian atmosphere, an enthusiastic tense anticipation of the great thing that may come any moment. This tension found its partial relief in the foundation of the Royal Society, in the success of Newtonian mechanics, and in the steady and hardly interrupted progress in science. All these things happened partly due to his influence. But this was mere atmosphere: there was no substance to it.

Bacon did not say what induction is and this disappointed Ellis so much that he considered his lifework of editing Bacon's *Works* possibly a waste of time. His reading of Bacon is obviously right: with one exception Bacon's works say nothing about induction. The exception is this: the human mind is capable of developing science unaided (see, e. g., the end of Book I of the *Novum Organum*). Yet he promised to offer aids to the intellect and he did not. The reason is very simple: he had no idea; he offered a dream, not a reasoned methodological treatise. The main thesis of Book I of the *Novum Organum* is that induction assuredly brings about scientific knowledge and that is the main hope of humanity. And whatever it is, it is good to hope for something that will bring about such a mighty thing as science. It is in the climax of this book that Bacon says (Aph. 92),

> I am now therefore to speak touching Hope; especially I am not a dealer in promises and wish neither to force nor to ensnare men's judgment, but rather to lead them by their hand with their good will.

Let as turn to his utopia, his *New Atlantis*, to see what is the ideal that he hoped for, and on what ground.

Bacon accepted the prevailing view that science is certain and finite. But he gave to this idea an interesting twist. The certainty of science and its finitude only made him hope that it would be very easy to discover. He suggested that proper research should always lead to the discovery of new facts of nature. He was not too consistent when he taught that science is both knowledge that will soon reach completion and constant discovery. This, the view that looks to us so very shallow was nonetheless a great contribution to philosophy.

Bacon emphasized (*Novum Organum* Aph. 84 & 85) that his very demand for progress is a slight on Antiquity. On this he was surely right. The Aristotelians, who were then the vast majority among the intellectuals, were consistent when they found disturbing each new discovery that showed that the ancients, including Aristotle, were deficient in their knowledge. The idea that Aristotle knew everything was the basic element of the scholastic authoritarianism; Galileo aimed well when he directed his arrows of sharp irony at this point saying in his first Dialogue Galileo (1632) 1953:

> — Now if Aristotle had seen these things, what think you he would have said, and done, Simplicius?
>
> — I know not what Aristotle would have done or said, that was the great Master of all the Sciences, but yet I know in part, what his Sectators do and say, and ought to do and say, unlesse they would deprive themselves of their guide, leader, and Prince in Philosophy. …

Bacon could not understand Galileo's physics, as Spedding reluctantly admitted. But, ignorant as he was, he said the same things against Aristotle. He viewed as a mere prelude to something bigger the wonderful innovations, the accurate stellar motions and new lands, compasses and telescopes, handguns and cannons. Independently of Galileo he understood that Aristotle blocked the way to this rosy future and he assumed that his contemporaries needed confidence to overthrow the authority of their "Prince in Philosophy" and "Master of all Sciences". He therefore altered the picture: he stated rather dogmatically his idea that proper scientific research must constantly lead to new discoveries. The effect of this dogma was quite anti-dogmatic.[2] Even Newton's authority, and the greatest authority on science and on scientific method it was, counted less than the authority of any new discovery, and so discovery could override Newton's authority. Indeed, Newton's theory was valued so much just because it led to constant discovery until the end of its reign. This idea of Bacon's merits great appreciation; and it was new. Philosophically speaking, it was an optimistic utopia; methodologically speaking, it gave a new mark of science (as Bacon called it), a new criterion of demarcation (as Popper called it) between science, theology and metaphysics: science is ever fruitful, ever

[2] That the rise of modern science is due to the Renaissance anti-dogmatism is no news. Nevertheless, few mention the anti-dogmatism of Copernicus Burtt (1924) 2003. Galileo's influence in this respect is incomparably superior to Bacon's, since Bacon's theory and his influence were psychological. Galileo's approach was rationalist: he expected his readers to trust their own reason, at least more than they trusted other people's writings. He was not the first Renaissance rationalist or the greatest Renaissance methodologist. The view, now unanimously endorsed, that he is the father of modern science, is nevertheless not under consideration and I do not challenge it.

active and progressive; metaphysics is doubtful and dangerous for science as it may cause a rift in the Fraternity and break it up into schools[3]; only theology is barren ("like a virgin dedicated to God"). Technically speaking, Bacon's idea did unite all researchers though they had diverse religious and metaphysical views: they agreed to lay aside every other discrepancy when discussing science. Socially speaking, Bacon s idea raised the status of the members of the Fraternity by his claim that science must have technical applications. He viewed the utility of research, its new technical applications, as an essential characteristic of science but not as the aim of science. His prophesy of a technocratic society was self-fulfilling.

The New Atlantis, whose inhabitants are Christians (even its Jews[4] believe in Christ), is special only in its possession of a research college and its high social status. The story-line is thin and a copy of Thomas More's *Utopia*, to which Bacon adds two items. First is a picture of the procession that takes place as the Father of the College comes to town: he enters town like a king in a splendid, well-organized procession. (Ellis says, this is Bacon's report of the procession of the coronation of James I.) All the citizens observe the ceremony and receive blessings of the Father of the College. Second and most significant, Bacon reports the interview of the Father with the narrator, with which the book stops abruptly.[5]

Members of the College, we learn, are esteemed members of the community. The College is an independent state within a state. It has the greatest research facilities, storehouses, special laboratories, instruments, etc. The members assemble for consultations in which they decide which of the new discoveries to publish, which to keep as state secrets, and which to keep even from the state! As for "ordinances and rites", they have golden and silver statues of great inventors and iron and marble ones of others. Besides, they pray to God to help them to make more discoveries.

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[3] The anti-Baconian view that scientific schools are good for science is still unwelcome, let me report: my inability to publish in a learned periodical my paper "Scientific Schools and their Success" (Agassi 1981) taught me about the extant reluctance to admit this fact. In quantum theory the Copenhagen school dominated the field and it presented itself openly as a school. This, however, physicists often view as an aberration due to an unwelcome metaphysical disputes between Bohr and Einstein, a dispute whose substance is more philosophical than scientific.

[4] There can be no Jews on the island of New Atlantis. But one cannot argue with a fact, to cite a Jewish saying. Of all of the New Atlanteans, it was a Jew who had befriended our reporter and who had arranged a meeting for him with the Father of Solomon's House. In that meeting, we remember, the Father conveyed the only information we have about Solomon's House; the rest is history. See Chapter 4.3 note 5.

[5] Spedding tried to explain why Bacon never finished writing that book. This again reflects his excessively high opinion of Bacon. For, Bacon hardly finished any original work of his, he had many problems in the story that he could not solve, and he stopped when he made the main point of the work, which is very much to say for a loose writer like Bacon. The popular view that *The New Atlantis* has some literary merits seems to me remarkable. As it is a take-off on Thomas More's *Utopia,* one might expect it to have some literary merit. It is rather disappointing, mainly because, as usual, Bacon tried to hold the tension by promises to the reader what he could not fulfill. The story slows down quickly and the descriptions become increasingly unimaginative. The peak of the story is its conclusion, which comprises the narrator's conversation with the Father of the College. It is a dry monologue. Yet this monologue made history because of the exciting idea that it conveys—of a successful secular research college.

All this is utopian even in our own advanced days of technocracy.[6] In Bacon's time the great researchers were amateurs: Gilbert and Harvey were court-physicians, Kepler was a court-astrologer, and Galileo was an academic turned a court mathematician; St. Roberto Cardinal Bellarmino warned him that his fate would soon follow that of Giordano Bruno if he did not submit to Church authority. Bacon's dream of technocracy was not very influential since in his days researchers could not dream of seeing science as a position of power. The Fellows of the Royal Society in the seventeenth century declined honour and power. Their acknowledged leader, Robert Boyle, repeatedly refused peerage and bishopric, and Newton, his follower in this position, is reported (presumably in jest) to have uttered nothing in parliament when he was a member there save a request to a porter to close a window to stop a draught that he had noticed disturbed a speaker. Bacon's idea was the first and impressive picture of a society in which research is an independent occupation, honoured (*Novum Organum*, 1, Aph. 91), and not for knowledge but for steady advancement of learning, for constant discovery (*Novum Organum*, 1, Aph. 90), and for many-sidedness (*Works*, 3, 323–8, 502).

This identification of science with learning is a new philosophical element that Bacon patched on to the scholastic philosophy that he otherwise fully endorsed (more carelessly than deliberately) in addition to his expression of contempt for their allegedly pointless disputations.

2.2 Bacon's Metaphysics

Ellis suggested that Bacon was a metaphysician rather than a methodologist. His view is nowadays generally endorsed. In its time it caused no small surprise. For, no one has ever denied that Bacon was one of the fiercest opponents of metaphysics.[7] He declared all metaphysical systems (other than the future, science-based, true one) "idols of the theatre". Metaphysicians are people who promote their own prejudices—the plays that they imagine—instead of the truth, as their purpose is founding schools of thought. "For, the introducing of a new doctrine … is an affection of tyranny over the understanding and beliefs of men" (end of *Sylva Sylvarum*). This does not rule out the possibility that Bacon did intend to found a school in spite of his firm denials, since, it is a secret no longer, he particularly envied Aristotle, whom he considered the greatest tyrant "over the understanding and beliefs of men", and since he tried to inherit Aristotle's throne as even Spedding admitted.

[6] Jacob Bronowski complained in his paper read to the educational committee of the British Association in 1955 that of the many statues in the Albert Hall in London not a single statue was of a scientist. More seriously, just yesterday the United States victimized its national hero J. Robert Oppenheimer for his refusal to cooperate in military research (Stephanson 1989, 239).

[7] I find it rather distasteful that some modern commentators on Bacon's metaphysics ignore his hostility to metaphysics. It sounds quite deceptive. The same goes for many comments on Newton although his opposition was not to metaphysics but to its inclusion in philosophy, namely, in science.

Still, it is only fair to note that Bacon intended to supplant Aristotle by erecting a demonstrable science, not by spreading a dogma; that Bacon's metaphysical remarks are very scanty and casual, and they all refer to science. Bacon did hold metaphysical ideas of sorts, and he was not hostile to metaphysical ideas as such. He wanted researchers to avoid error, however, and he contended that we should wait for a scientific metaphysic to emerge out of science. Hence, we need not reject Ellis' idea (that Bacon was a metaphysician rather than a methodologist), as we may instead shift the emphasis back from metaphysics to the philosophy of science, in full accord with Bacon's intentions.

In this context, Ellis discussed Bacon's Platonism and the view of Leibniz on Bacon. He contended that Bacon influenced Leibniz. This contention lacks substance and significance. It rests on two arguments. First, Ellis found in Leibniz' works one positive comment on Bacon and one echo of his ideas. The comment is this:

> We do well to think highly of Verulam [Bacon], for his hard sayings have a deep meaning in them.

This is too obscure to signify. If anything, it shows that Leibniz found it necessary to praise Bacon but difficult to digest his wholesale accusations. As to the obscure passage in the metaphysical work of Leibniz that echoes another in Bacon's works, it concerns atomism. It does not signify. Anyway, Ellis' view of Bacon's atomism is by now refuted.[8]

Ellis' observations on Bacon's Platonism are different: he has enriched our understanding of Bacon. Plato's influence on Bacon cannot be denied. It is mainly limited, not surprisingly, to the philosophy of science. Bacon's philosophy of science was the received opinion until 1905, while his methodology, though still very influential, was never practiced and never fully endorsed by any scientist or philosopher, save, perhaps,[9] Ludwig Wittgenstein. A philosophical attitude prescribes what one may expect science to achieve, leading to the methodological problem, how to attain it.

The major idea of Ellis' interpretation of Bacon's philosophy of science, we remember, is that scientific method is infallible and hence that scientific theories are absolutely certain. The method (by which one achieves this certainty), said Ellis, is the one that Bacon accepted from Aristotle, and to which he added only minor technical developments of his own as well as the demand to be prudent and observe more facts before making induction.

By Aristotle's philosophy, the ideal science starts from definitions and proceeds by syllogisms from these definitions to explain all known facts of nature. The question, say, whether swans are necessarily white or whether their whiteness is only contingent

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8 Cp. Ellis' Introduction to Bacon's *Thoughts of the Nature of Things* and Lemmi (1933, 50, 57, and esp. 60, n. 58) that presents that work of Bacon's as an expansion of the chapter on Cupid in Comes (1581).

9 Wittgenstein's cryptic remarks on methodology are too oracular and deliberately mystifying to allow for a definite decision as to how much his views were Baconian or near-Baconian.

so that we may find some day a black swan (as indeed is the case), can be settled by our definition of the word "swan". If the word is defined as to mean, "a white long necked bird, etc." then the black bird that is otherwise similar to the white swans we usually see is by definition not a swan. Nevertheless, following Aristotle (as well as Bacon) we may conclude that having found these black birds we now know that we should not have defined "swan" with reference to its colour. Whiteness then is not an essential property of swans, as there can be a swan that is not white. A proper definition is one that refers only to the essential properties of the thing defined, the properties that characterise the thing defined, the properties that belong to it inasmuch as it is what it is. If we know the essential property that makes a thing what it is, we can define the word that signifies it properly. Plato's idea of the ideal science is similar to the one that Aristotle articulated in more detail, though Plato's paradigm of science was geometry and Aristotle's was biological: his term, "definition" (or rather "*orismos"*, the Greek word for boundary), is a substitute for Plato's term "*axiom*" (the Greek word for valuable). For, Plato meant by it a self-understood or a self-evident statement, just as "definition" meant for Aristotle. Plato's axioms refer to forms, to shapes; Aristotle's definitions refer to essences that he named *oussia* (the Greek word for being). This is the major difference between Plato and Aristotle that a substantial part of traditional philosophy is devoted to. Plato taught that Forms belong to another realm, to Plato's world of ideas, to the Platonic Heaven so-called; Aristotle taught that the Form of a thing is in the thing itself, or, rather in its innermost centre (speaking metaphorically, of course), and it reveals itself in the thing only partly and during a certain phase of the duration of that particular thing. It reveals itself during the highest stage of its maturity.[10]

The process of learning, of arriving at axioms is dialectics in Plato's teaching; in Aristotle's teaching it is the process of induction (*epagoge,* the Greek word for leading) leading to definitions (*orismos*). As to the process of acquisition of true knowledge, in Plato's teaching it is through access into the world of ideas; in Aristotle's teaching it is in the merger of the intellect with the object of knowledge.[11]

Aristotle's philosophy of science deals with essences and is called Essentialism. The idea that the method of science is that of finding the essence of a thing by the proper definition of that thing is Methodological Essentialism. This theory is more common than it may seem to the scientific reader. When we say that Man is free or that Man is naturally good, etc., we do not speak of a particular human being but of the Idea of Man or of Man's Nature, or of Human Nature or of the Essence of Man. If a property under study is essential to Man, then, all humans have it inasmuch as they are human. Essences or Forms are Universals. The classical problem of logic, the problem of Universals, is the problem of existence and location of the Essences

[10] This is only one facet of Aristotle's animism; the other is his identification of the word properly defined with the thing it properly designates, which is a refined magical attitude towards language. This is the essence of savage thinking, says Claude Lévi-Strauss.

[11] For more detail, see the celebrated passage of Popper on Aristotle's theory of definition (Popper 1945, Chapter 11, section ii) as well as Grene (1974, Introduction and Ch. 1) and Loy (1988).

that the Universals supposedly designate. The distinction between metaphysical and methodological essentialism is not sufficiently popular,[12] although only methodological essentialism is magical in that it welds names with the things they designate (Lévi-Strauss 1966, 172, 190, 198, 204).

The fundamental importance of the problem of Universals lies not in logic but in metaphysics, and likewise in the philosophy of science. The significance of the problem springs from the fact that ever since Antiquity, the view has been that a theory concerning an essence is an Axiom (Plato) or in a Definition (Aristotle) so that it has to be certain, and so it must have scientific status. These days, irrationalist philosophers have managed to throw a wedge between philosophical (metaphysical) and scientific certainty. In the context of the present study this distinction cuts no ice. The terms "certainty" and "scientific certainty" are synonyms; the words "scientific" and "certain" however are synonyms only in classical contexts.

Bacon presented his methodology as pertaining to Essences, but he was not decided, and understandably he was not interested in finding out, where they are housed, whether in Plato's Heaven or in the Aristotelian hearts of things. As Ellis first pointed out, he constantly and systematically, and apparently quite consciously mixed the Platonic and the Aristotelian terminology: he regularly spoke of axioms and definitions (rather than axioms or definitions). As a result, Ellis observed, there is no need to reconcile the two modes in which Bacon spoke of form, namely, of the essence that dwells in *ipsissima res*, the Aristotelian essence that dwells in the hearts of things, and of the law, the Platonic form that dwells in the Platonic Heavens. On one point Bacon explicitly committed himself to Plato's theory of forms—as stated in the dialogue *Parmenides* (in his own odd interpretation) that describes the world as finite, explicable by few laws that in their turn should be explained by still fewer laws, leading to the one law. This concept in the ladder of axioms in the process of abstraction, carries research from the world of observation (natural history) to the world of science (Axioms, Middle Principles, the *Axiomata Media* that are the most important and powerful ones), from the world of science to the world of metaphysics (from the many axioms to the few) and from metaphysics to the intellectual ceiling, to theology (from the few to the one). This is the peak (*Of the Advancement of Learning*, Bk. 2, Metaphysics):

> And therefore the speculation was excellent in Parmenides and Plato, although but a speculation in them, that all things by scale did ascend to unity.

Bacon mixed systematically the Platonic and Aristotelian ideas, or rather their terminologies, because he intended to be above the classical dispute. (That he failed here, as elsewhere, is a different matter.) Whether science pertains to the innermost centres of things or to their outermost shapes, he would say, is quite unimportant; the main thing is that there it is, very simple; we should discover it and thus attain

[12] Thus, as the essentialism of Ludwig Wittgenstein was metaphysical and not methodological, debate rages as to whether he was an essentialist or not, with both parties having evidence to support them, as is usual with confusions.

the desired certitude. Bacon's huge if shallow optimism (that so shocked Spedding when he discovered it) is easily explicable by Ellis' picture of Bacon's metaphysics, but only in the following slight modification: there is no reason to view Bacon as a metaphysician proper, his own metaphysics notwithstanding, as it is not serious enough to examine it for consistency. Bacon's metaphysical view was designed as a philosophy of science, as means for supporting his tremendous, shallow optimism concerning the great abilities to produce a real science here and now.

This brings us straight to his views on scientific method.

2.3 Bacon's Induction

As Ellis has stated, Bacon had two theories of scientific method, only one of which he discussed at length, the one that leads to certainty. The other he mentioned very briefly, and in cryptic remarks ("in parenthesis" as Ellis said), since it does not lead to certainty. As Ellis has observed, the two versions were well mixed and it was only the careful and patient analysis of Ellis that brought to light this mixing.

This mixing of the two methods is significant, since it gave the illusion that Bacon's method is never far from arriving at the desired certainty although there is no bridging of the gulf between certainty and very high plausibility. This mixing then was the basis of Bacon's peculiar optimism. And so it may count as a sleight of hand. It is possible, for the main part, to ignore this misleading optimism and observe somewhat in the abstract the method that supposedly brings about certainty, the ideal method of induction. Whatever the process of (Baconian) induction may be, its results are certain theories, or definitions. In other words, presumably, proper learning is identical with proper defining. Ellis has put it succinctly when he pointed out (*Works*, 1, 37),

> … it is remarkable that induction … is mentioned for the first time in the *Novum Organum* in a passage which relates not to axioms but to conceptions. Bacon's induction … is also a method of definition; but the manner in which systematic induction is to be employed in the formation of conceptions we learn nothing from any part of his writings.

Not so. This problem is easily soluble by showing[13] that Bacon used as synonyms form and essence, as well as axiom and definition (*Novum Organum*, 2, Aphs. 20 and 26). True, as Ellis has observed, Bacon's idea that we arrive at definitions of words by induction leads to an infinite regress: there would be no words to start

[13] According to Aristotle, Socrates was the inventor of the method of induction that leads to definitions. Ellis ignores here this item, as well as its role as the origin of the link between induction and definition that plays a great role in Aristotle's theory of science. Popper discussed this at length (Popper 1945, Ch. 11). Bacon introduces his theory by contrasting idols with the ideas of the divine mind (*Novum Organum*, 1, Aph. 23). For, he differed from Aristotle about hypotheses: the one allowed for them (Dickie 1922, 478–9) and the other viewed them as the source of all impediments to scientific progress.

with. (Modern logic allows that the construction of any system starts with some undefined terms.[14]) But this difficulty happens to be inherent in Aristotle's theory that, as Ellis ruefully noticed, Bacon had endorsed lock, stock, and barrel. The next thing to observe is that in many places Bacon also used the words induction, and dialectics as synonyms, stating that Plato had known how to perform induction. He observes (*Novum Organum*, 1, Aph, 105) that induction is

> … a thing which hitherto has not been done, nor indeed attempted, save only by Plato, who for the formation of his definitions and ideas has certainly used this form of induction to a certain point.[15]

The only place where Bacon says explicitly and clearly what he means by "induction"[16] is just before he offers examples of scientific research proper (*Novum Organum,* 2, Aph. 19; see also 1, Aph. 105). He says there, in parentheses, that the task is

> rendering man's intellect equal with things and nature.[17]

That is the Platonic-Aristotelian, mystic-intuitionistic formula for process of enlightenment as worded in the medieval (Muslim and Christian) tradition.[18] It is the view of knowledge as the unity or equality (or merging) of the intellect and its object in knowledge (i.e., in Plato's Heaven).

This exposition of Bacon's theory of learning is not important for the study of his influence, since, insofar as methodological essentialism was endorsed, this was so on ancient authority. What Bacon's followers learned from him is that experiments and observations are vital for the "rendering man's intellect equal with things and

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[14] David Hilbert's idea that the undefined terms are subject to implicit definitions shows that all the traditional theories of definitions are wanting.

[15] I use Kitchin's translation here, since Ellis translates "discussing" instead of "formation" in his translation of "executiendas definitiones et ideas", I do not know why. Bacon's claim that Plato used induction only up to a point alludes to his famous complaint that Plato mixed science with metaphysics as well as with natural theology. Ellis notes here (in a note to the Latin text) that this is clear evidence that Bacon never claimed to be the first inductivist. (Another is the end of his *Filum Labyrinthi*.) He did not see here a repeated claim for his idea that free of prejudice induction is a very new game. Properly speaking, dialectic is not induction but *propaedeutic*, the preparatory to it that in Bacon's system is the cleaning of the mind and the collection of information.

[16] Possibly, Bacon used the word dialectic like Petrus Ramus (Pierre de la Ramée), whose terminology, Ellis has observed, influenced that of Bacon (*Works*, 3, 530n.). Ramus used "Dialectics" to mean both "logick" and "Method". He then sub-divided Logick into Invention and Judgment. His Method was the demand to start from definitions, and these he identified with natural laws, i. e., descriptions of the essences of the things defined. Ellis emphasized that Ramus had a great influence on Bacon (*Works*, 1. 47, 91 and esp. 205). Bacon's seemingly criticized Remus's theory of definition (*Works*, 3, 407 and 4, 453); this shows that he had nothing against it. Ellis rightly said, Ramus was Aristotelian despite himself.

[17] Again, for "ut faciamus intellectum humanum rebus et naturae parem" I use Kitchin's translation, since I do not understand Ellis' "that of rendering the human understanding a match for things and nature".

[18] (Grene 1974, Ch. 1): by the ancient and mediaeval formula, enlightenment is the realization of some sort of unity of the mind and the whole universe; knowledge is the unity of the knower and the known. This formula is ubiquitous (Loy 1988, *passim*).

nature" whatever they may have understood that intuitionistic formula to be. This belongs to another part of this story. Bacon's faith in the intuitionistic formula for learning explains his tremendous optimism and supports Ellis' view that (a) there is nothing new in Bacon's theory of learning, and that (b) the inconsistency of the *Novum Organum* is at least partly due to the fact that the difficulties only turned up while Bacon was writing. Probably, his hope of solving them soon shows that he never realized the immensity of the problem.

2.4 Bacon's Inductive Machine

To put Bacon s central idea very bluntly, his main point was that the induction previously known should be used gradually, and systematically to make more and more discoveries and inventions. This is his idea of the inductive machine; he uses the expression "machine" very rarely but very prominently, in the Epistle Dedicatory to his *The Great Instauration, Prœmium*, no less. He says there,

> I have provided the machine, but the stuff must be gathered from the facts of nature.

Everyone then should observe as much as one can. How can one discover systematically when the whole process of discovery can be a few years or a few generations or at most a few centuries? I do not know. But this hardly matters. Bacon would surely answer, let us discover now systematically as long as there is something to discover. What will happen later we may find out then, when we shall be wiser. We may then even improve upon his own inductive method, he generously allowed (*Novum Organum,* end of Book 1).

How does the induction machine work? The main answer that Bacon gave is, it works as long as it absorbs information. How many observations are necessary for the machine to produce results? What information does the machine need? These are important questions, Bacon admitted, but not the most important ones. In the last Aphorism of Book 1 of his *Novum Organum*, he promised that science will necessarily shine forth if only we (1) get rid of prejudices, (2) observe diligently, and (3) exercise mental restraint for a while so as to avoid making the most general axioms. The method Bacon was going to expound in Book 2 is quite important, he added, but not essential, since induction is the natural process of the human mind. The main thing is to remain unprejudiced and to continue observing. Then the mind will necessarily digest the available information, even without any advice from Bacon. He may have written this remark casually, but this is unlikely, both since he often repeated it in his works and since he placed it at the very end of Book 1, just before the commencement of the exposition of the method that he declared there inessential. Possibly, he intended to outdo Aristotle who declared his own induction to be the natural process of the mind, but this also is not very likely. The historical significance of this passage is obviously very great. Ellis and de Morgan (rightly) argued that the significance of Bacon's induction machine is that everybody can work it. The end of Book 1 of *Novum Organum* shows that Bacon insisted that every careful and open-minded observer could contribute to science. This was the most important idea that Bacon contributed to the rise and *modus operandi* of the Royal Society of London and the whole of the Enlightenment Movement that followed it.

The view is very exciting that science is a thing for everybody to try here and now, not limited to the experts or for the especially gifted or for some special occasion. It matters little whether this idea has originated with Bacon or with some of his interpreters. What matters very much is that people tried to put this idea into practice, this leading to the rise of modern science and of science-oriented society in a movement that deemed its task prosaic and implemented it with religious fervour (*Deuteronomy*, 30:11–13).

> For this commandment which I command you this day is not too hard for you, neither is it far off. It is not in heaven, that you should say, Who will go up for us to heaven, and bring it to us, that we may hear it and do it? Neither is it beyond the sea that you should say, Who will go over the sea for us, and bring it for us, that we may hear it and do it? But the thing is very near you; it is in your mouth and in your heart, so that you can do it.

Of course, the commandment referred to in these lines is not to create science. But the point is the very same as that of Bacon (*Works*, 3, 273):

> And that learning should take up too much time or leisure; I answer, the most active or busy man … [has] many vacant times of leisure … and the question is but how those spaces of times of leisure shall be filled and spent, whether in pleasure or in studies … No man need doubt that learning will expulse business; but rather it will keep and defend the possession of the mind against idleness and pleasure, which otherwise, at unawareness may enter and prejudice both.

This is the subject matter of the First Book of *The Advancement of Learning*, in which the just-quoted passage appears: there is no excuse for not starting to advance learning; everybody can start here and now, even the busiest. Ellis was thus in error as he considered this not a part of Bacon's teaching but a mere piece of propaganda. He preferred to consider as Bacon's conviction the idea that the natural process of the mind is to make anticipations, to guess. And, of course, in Bacon's system the two views that are at issue here clash violently: that our natural bent to is learn and that our natural bent is to be lazy and to guess instead, Bacon viewed these as opposite extremes and as both true. (He thus was the first to describe an inbuilt affective conflict, centuries before it surfaced again in the romantic literature.) He repeatedly admitted that the requirement to avoid guessing is an artificial restriction. This is the structure of his *Novum Organum*: Book 1 offers the criticism of the method of anticipation, of guessing, and praise for unbiased observations, and Book 2 discusses Bacon's positive method. He then mentions that he still remembers that he is out to teach as the method of making induction "(viz., rendering man's intellect equal with things and nature)". And then, instead of coming to the point at last, he suddenly changes gears and gives "permission to the intellect" to conjecture, to make a guess—a very small anticipation. Unexpectedly, Bacon even offers an argument in favour of guessing (*Novum Organum*, 2, Aph. 20):

> … since truth will sooner come out from error than from confusion …

Since we err regularly, and since in science error is more easily and thus more often open to exposure than elsewhere, in the scientific literature of the Age of

Reason this claim of Bacon's was most popular: many Baconian researchers were glad to quoted it with great approval when the worst came to the worst and a scientific theory had to be explicitly declared an error (Watts 1724, 49; Huxley 1887, 4th para; Broad 1926, 53; Bloom 1987, 1304; Musgrave 1993, 51; Barondes 2003,3).[19] Ellis was not impressed. He rightly contended that this permission to make small guesses has no place in Bacon's methodology that lays so much emphasis on artificially restricting the mind from using any guess whatsoever. The very expression "*permissio intellectûs*" (permission to the intellect), says Ellis, sufficiently indicates that in this process the mind is suffered to follow the narrow course imposed on it, and reverts to its usual state (*Works*, 1, 36). Here is a clear contradiction and also, by the way, the point of disagreement between Spedding and Ellis (*loc. cit.*, note). It is easy to resolve (although at the cost: Bacon's doctrine—that started as extremely simple—becomes increasingly complicated and involved).

The intellect is restricted, as Ellis observed, in that it is forced to reject prejudices and to proceed slowly, avoiding making any hypothesis. For, any guess whatsoever, being uncertain, is unlike induction and contrary to it. And, Bacon repeatedly stressed (*Novum Organum*, 2, Aph. 28), anticipation is very appealing, since it is both much easier and much more engaging to the imagination. It is therefore "now in fashion". Hence, Ellis is quite right on this: according to Bacon, anticipation is a natural bent, and a dangerous one at that. Practice has rendered the bent a habit and we must first abandon it. And then we may let the mind operate in its other natural manner. Anticipation is lazy.[20] We must first get rid of our natural tendency to be lazy and then to work naturally, said Bacon (*Works*, 3, 244), and produce science (*Of the Interpretation of Nature*, opening of Chapter 15):

> Of the great error of inquiring knowledge in Anticipation. That I call Anticipations the voluntary collections that the mind maketh of knowledge; which is everyman's reason. That though this be a solemn thing, and serves the turn to negotiate between man and man (because of the conformity and participation of men's minds in the like errors), yet towards the inquiry of the truth … it is of no value.

Thus, we see, the mind is restrained, as Ellis said, from its normal habit of anticipating. But, otherwise, provided one observes with no Anticipations in mind, then,

[19] As this practice may upset rationalist readers, let me mention that Claude Lévi-Strauss wisely viewed it as common in the myth world and as reasonable: to switch between extreme options inconsistent with each other is better than to stick to one of them dogmatically. A reform of the system is still better but not easy. Indeed, the polarity that that Lévi-Strauss describes results from reforms within systems in which deletion is impossible. No matter how mythical radicalism is, here it is the pole opposite to the myth-world. The new system allows for error without deleting it. This is the glorious achievement of Popper's rationalism.

[20] Patricia Kitcher says (Kitcher, 1990, 38), Kant "had no sympathy for nativist lazy hypotheses" (*ignava ratio*, Critique, A689 B717): a priori knowledge is not inborn but acquired through mental activities triggered by sensory data. Kant's theory of learning and his methodology are empiricist, even Baconian; his epistemology is decidedly not. Bacon's epistemology is not clear. Commentators from Locke to Ellis took it for granted that it is empiricist, and that became a trend.

but only then, is the mind in that original natural state in which it once was—in its pure natural state in which it has the bent to generate induction and science (*Novum Organum*, 1, Aph. 130):

> For interpretation [i. e., induction] is the true and natural work of the mind when freed from impediments.

Thus, although the phrase "*permissio intellectûs*" shows, as Ellis has said, that induction is in a sense artificial, it does not mean that it is a complicated technique. On the contrary, Bacon said, a small limitation sets free the immense powers of the mind—of anybody's mind. The limitation does not make science a matter for the specialist who learns, but, on the contrary, it is a small dose of self-discipline that at least in principle enables everyone to contribute to science (*Novum Organum*, 1, Aph. 61):

> Our method, however, of discovering the sciences is one which leaves not much to acumen and strength of wit, but nearly levels all wits and intellects. For just as with the view to the production of a straight line or a perfect circle, much depends on steadiness and practice of the hand, if it be described by the hand alone, whereas if a ruler be made use of, or a pair of compasses, little or nothing depends on them, so exactly is the case of our method.

This solves a contradiction in Bacon's writings and settles a discord between Ellis and Spedding. It does not suffice to make Bacon's system coherent, especially since whenever we encounter a difficulty in the system, Bacon and more often his followers, would explain this with ease. They would say that the cleaning of the mind, that is, no doubt, just a small limitation, is perhaps not such an easy task to perform, because prejudices, like their modern analogues, class prejudices (Marx) and neuroses (Freud), have their own techniques of embedding themselves in their victims' minds. Inductivists can always accommodate difficulties that ensnare science, explaining in many ways how and why it is that the removal of prejudices, though methodologically a small or almost no limitation, is a very difficult psychological task to perform. This would not help, for induction supposedly equalizes intellects. Can it do this job? If prejudices are hard to remove, then perhaps the method does equalize intellects; were prejudices easy to remove, then many unprejudiced people have failed to generate infallible knowledge.

This is no small matter, since the idea that the method of induction equalizes intellects is at the heart of the teaching of Bacon that has excited his followers. It is the idea behind many stories that historians of science fabricate. They explain success as natural and failure as due to prejudice. Thus, they explain the discovery of Galvani by the conjecture that he used frogs' legs in his experiments since they served as a cure for an alleged illness of his wife; that James Watt was lucky to see the top of a pot with boiling water pushed up by the steam; that Galileo was lucky to look at the movements of a chandelier in the cathedral during mass, and so on. And they explain failures of great minds such as Galvani, Volta, Priestley, and Lavoisier, as evidence that because these great lights became prejudiced they stopped shining.

This is no whim: it links with Newton's funny story that he hit upon his great theory of gravity while seeing an apple fall. All these stories are false simply because they are conjectures[21] meant to illustrate the false thesis that induction equalizes all intellects. This doctrine is part-and-parcel of the scientific tradition. The founders of the Royal Society of London and of the scientific revolution and the heralds of the Age of Reason, they took Bacon for their patron saint. Such top researchers as Priestley and Lavoisier, or as Davy and Faraday, struggled with the orthodox interpretation of Bacon's writings. Their careers are not understandable without reference to Bacon's ideas, especially his proscription of conjectures that comes with his permission to make some small conjectures after one has worked hard collecting and organizing as much information as one can.

So much for the phrase "*permissio intellectûs*". As to the permission itself, Bacon has offered no argument for it; it is simply permission to perform just a small anticipation. Book 1 of Bacon's *Novum Organum*—Ellis has shown—is devoted to arguments in favour of his doctrine of induction. Then, when he had convinced himself of the truth of his view that empty minds do naturally make induction all by themselves, as well as of the truth of his assertion concerning the vital importance of the total emptiness of his own mind, he at last pulled up his sleeves and started to implement his doctrine of induction. Then, Ellis continued, a tiny doubt entered the empty but hopeful mind. This doubt did not deter him from his great activity; he simply provided at one stroke a surrogate, a new method, in case the old one (of Book 1) would not work after all. Following the preparations for making induction and having completed the task, Bacon, not noticing the irony of the situation, permits himself a small anticipation in its stead, should this be necessary. The *permissio intellectûs*, the permission to the intellect, is better known by the other names Bacon gave it: The First Vintage or The First Attempt: it may be induction, a Vintage, or it may be just an attempt. He thus casually introduced that dreadful thing, uncertainty. This is why we need a special permit for it. To this Ellis responds indignantly (*Works*, 1, 36): "It is a parenthesis in the general method."

The trouble arises from efforts to take Bacon literally, from considering his texts seriously, and from analysing them too carefully. It is doubtful that Bacon could ever dream that he would be so idolized; he would be astonished at Ellis' serious criticism of works so carelessly written. To understand Bacon's admittedly interesting and most important ideas, we must try and take his views in a more easy-going, informal way; this is the essence of Bacon's teaching, after all: Forget all past theories, Aristotle and all that, and start observing anything you feel like, and do not bother about science: it will come out all by itself if you do your honest bit and observe diligently. This idea comes out repeatedly in Bacon's works. His maxim is, ignore past learning, observe, and hope for the best.

This plain message, crudely presented, was extremely influential just because of its shallowness, because of its appeal to ordinary readers, because of its tremendous optimism. It also has much truth in it. It suggests a very forceful maxim: engage in science and do not waste time discussing it. The trouble is that to regard this as

[21] The story of Newton's apple is outstanding as it is reported in his name. See Westfall (1983, 143, 154–5, 427–8). See also White (1997, 86).

Bacon's message means to regard Bacon as an anti-methodologist rather than as a methodologist proper. Strictly, this is incorrect. He did provide a theory of method, however poor; that theory suggests the idea that it is preferable to do science than to discuss its methods. This blocks discussion of it. Bacon's works are thus similar to the ladder that Ludwig Wittgenstein compared his own teaching to (Wittgenstein, Ludwig 1921–22, §6.54), and for the same reason: you use his texts as a ladder to climb to a high rooftop and then you throw the ladder away as no longer needed. And then you are trapped there. Already Bacon used this idea (end of the Author's Preface to his *Novum Organum*):

> … if anyone would form an opinion or judgment [that contradicts Bacon's teaching] … let him examine the thing thoroughly; let him make some little trial for himself of the way which I describe and lay out; let him familiarize his thoughts with that subtlety of nature to which experience bears witness; let him correct by seasonable patience and due delay the depraved and deep-rooted habits of his mind; and when all this is done and he has begun to be his own master, let him (if he will) use his own judgment.

This then is a partial explanation of the overwhelming influence of Bacon's ideas. No matter why they originally found favour, they remained in favour, and then they acquired the reflected prestige of the founders of the Royal Society of London and of the incomparable Newton. Once endorsed, one would have expected them to attract criticism. This barely happened. Bacon's followers took seriously his recipe for scientific research, and then they agreed that one should proceed hopefully instead of stopping to argue about methods. They tried to suppress any methodological discussion because they wished everybody to get on with the job of observing as much as possible, in anticipation of wonderful results. This is the major point of Bacon's method: Observe! Do not bother about past theories or about methodology. Observe!

There is more to Bacon's methodology. Observing with a mind washed clean of all opinions, we will benefit if we follow some of Bacon's technical advice, advice that again reveals his extremely naïve optimism. He taught that as essences (a) appear with the natures with which they are associated, (b) disappear with them and (c) increase with them, these facts should be arranged in tables of presence, of absence, and of comparisons. To take Macaulay's jocular but reasonable example, we make tables of those meals after which we had stomach ache and of the meals after which we did not get stomach ache, and so on. We then deduce, with the aid of common sense, what food disagrees with us. Matters are not quite as simple as Macaulay's way of presenting them suggests, since the cause of stomach ache need not be food. Yet he is right in presenting Bacon's induction in this naive way.

The main criticism of Bacon arising from Macaulay's presentation would be that common-sense procedures offer no guarantee for certainty: since common-sense is often in error, especially when it comes to scientific matters. Many attempts were made to interpret Bacon's method of demonstration. For, he repeatedly stressed that his idea of demonstration differs from those of all his predecessors. The rationalistic interpretations suggested that (viewing the universe as finite) he contemplated first a simple idea. Begin by writing down all the possible explanations of any given "nature"; and then by tests (*Novum Oragnum,* 1, Aph. 186), refutations

(*Novum Oragnum*, 2, Aph. 12), and crucial experiments (*Novum Oragnum*, 2, Aph. 36), exclude all but one of these explanations, and that one is thereby indirectly proven. This is the process of induction by elimination. It is deductively valid. This then would render induction no more the use of logic in procedures of thorough testing when such procedures are possible.

Induction by elimination is common-sense; detective novels writers regularly display it. Common-sense operates in context, however; context is alterable and detective novels writers use this fact regularly to tease readers. To render induction by elimination context-free is the dream of inductivist methodologist. It is impossible, Pierre Duhem noted, as there are infinitely many alternatives. Mill and Keynes wanted to limit it but did not say how.

Those who do not mind proposing hypotheses present induction by elimination as conditioned on some assumption. The paradigm (= chief example) for a conditional induction by elimination is analytic chemistry. It rests on hypotheses and so its experimental results depend on these hypotheses, so that they are not proven but hypothetical. The major hypothesis of analytic chemistry is that the given piece of "pure" matter comprises identical stable atoms and molecules. The counterexamples to these hypotheses are very interesting; analytic chemists ignore them and find them seldom troublesome: they choose cases for which for all they know the hypotheses are true. As to absolute induction by elimination, it is impossible, since absolutely speaking, there are infinitely many possible hypotheses.

Bacon did not advocate induction by elimination. In particular, advocates of any such interpretation cannot possibly explain Bacon's fierce and repeated attack on the use of syllogisms in scientific research, so that the permission that he granted to the intellect to reap the "first vintage" cannot rest on logic alone as induction by elimination surely does. And there are many other parts of Bacon's method that conflict with this interpretation. Here is but one example: he says (*Novum Oragnum*, Aph. 95) "and then, after a sufficient number of negatives, to conclude upon affirmatives": he had no criterion for sufficiency. Ellis stressed this point: there is no reason, he observed, why the rejection of alternatives should yield the true one, let alone with certitude. Were the induction here by elimination, this would not be so: the absolute induction by elimination does yield certitude (it is repeatedly employed in logic and in mathematics). Ellis contended that Bacon was a mystic whose best account of his methodology was his narration of the myth of the birth of Cupid, a myth that describes how, from the depth of the darkness, the light of dawn slowly spreads.

All that was said thus far on Bacon's idea of induction put together is too little. Perhaps this is too simple and naïve to be true, but Ellis found too few observations on induction in Bacon's works, perhaps because he hardly explained what Bacon had meant by "induction" or by "demonstration". Some hints and a few casual cryptic remarks is all there is on this matter.

Bacon's view of scientific theory is so shallow that he expected tables of presence and absence to be helpful in the search for laws of nature. He even wrote (*Novum Oragnum*, 1, Aph. 7) of tangible essences that are denser than thin essences. This is so unbelievable that we have to remind ourselves that centuries later John

Stuart Mill echoed his ideas of tables in a way that is more unbelievably naïve. Of course, commentators repeatedly noted that Bacon's (and Mill's) tables had never been employed in the service of scientific research proper. This, however, matters very little in view of Bacon's declaration that his tables are inessential. The significance of his tables is that they comprise confused mixtures of relevant and irrelevant factual observations. Out of these compilations of information gathered from all directions are going to give rise to excellent and most useful theories. In this vague, general sense, his tables were imitated for centuries in every case in which a researcher turned out to be scatter-brained. This sounds as agreement that the tables were useless. Not so: researchers can always improve. This will turn out repeatedly in the discussion of Boyle's function as the teacher of the army of amateur researchers.

Ellis' interpretation of Bacon's method as an easy method of discovery raises two problems. The first is, why was Bacon's simple, easy and obvious method never tried before? The answer that his critics gave is that it was never applied because it is inapplicable; it is one that his followers are unwilling to entertain. They endorsed Bacon's own explanation: there is a small prerequisite that is absolutely essential for rendering the whole venture simple, easy, obvious, and successful. And this small prerequisite, Bacon claimed, was entirely his own discovery. This already answers the second question that Ellis' reading of Bacon raises. That question is, what is new in Bacon's teaching, unknown to his predecessors? I shall now discuss this question of Ellis and its solution.

Chapter 3
Ellis' Major Difficulty

The difficulty that Ellis had is one that all of his commentators should share. All the defects that Bacon exhibited contribute to the riddle: why did generations of serious thinkers of all sorts praise him so liberally? Ellis was convinced that there must be some just reason to this praise. He could not find it.

Justus von Liebig had not one single word of appreciation for Bacon the methodologist, nor did he pretend to exhibit any good will towards him. He viewed Bacon as a mere juggler and an impostor, a deplorable specimen. Ellis' attitude was very different. He found it worthwhile to invest a tremendous amount of work in spite of his ill health and his preoccupation with mathematics and the natural sciences. He showed tolerance towards the negative characteristics of Bacon, many of which he was the first to expose mercilessly, including plagiarism, failure to understand the fundamentals of science (for instance, Archimedes' law), inability to perform an experiment or devise any test, and, above all, an incredible conceit and insatiable love of boasting.[1]

Ellis tried hard to credit Bacon for the origination of a few important ideas, such as the technological applicability of science. (He surely did not notice that in this he was unfair to some of Bacon's illustrious predecessors whom he mentions in his impressively scholarly disclosure of the sources of Bacon's scholarly texts.) He tried hard to show that even though Bacon always exaggerated, he had some ground for his excessive self-admiration. Still, Ellis was not satisfied. He ascribed to Bacon some new ideas but he admitted that Bacon's view on himself was too high to take these as his great innovations. All things considered, Ellis admitted (*Works*, 1, 24), the major problem of his own interpretation of Bacon lies in the fact that according to that interpretation

[1] Bacon's personality and his contributions to fields other than methodology are not under discussion here. For these matters the best still is Macaulay's already mentioned *Bacon*. See also Koch (1958). The significant aspects of his person here are his disposition to exaggerate in every which direction (even when confessing a misdeed), and his stunning brilliance. It was reported that as he spoke in Parliament beside an open door, passersby stopped to listen.

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_3,

> it becomes impossible to justify or to understand Bacon's assertion that his method was essentially new … He … speaks of himself as being "in hâc re plane protopirus, et vestigia nullius sequutus."[2] Surely his language would be out of place, if the difference between him and those who had gone before him related merely to matters of detail … And it need not be remarked that induction itself is no novelty at all. The nature of the act of induction is clearly stated by Aristotle as by many later writers.

Ellis is obviously right: we should be able to ascribe to Bacon some radically new idea to explain his amazing historical standing. Ellis was unable to find it, nor was Spedding, who also searched for it (*Works*, 1, 369),

> … even now, though Mr. Ellis' analysis of the Baconian induction has given me much new light and considerably modified my opinion in that matter, I am still inclined to think that Bacon himself regarded it not only as a novelty but as *the* novelty from which the most important results were to be expected; and however experience may have proved that his expectations were in great part vain and his scheme inapplicable, I cannot help suspecting that more of it is practicable than has yet been attempted, and that the greatest results of science are still to be looked for from a further proceeding in this direction.[3]

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[2] "In this course altogether a pioneer, following no man's track" (*Novum Organum*, 1, Aph. 113). That Bacon did follow the track of the ancients he stated in the Author's Introduction to his *Novum Organum*, and in Book 1, Aph. 125. This contradiction is resolvable (see below). Also, we may note that Bacon's bragging hardly says what it sounds in isolation, as it is next to Bacon's statement that he had no colleague to consult, so that he may have bragged that he had no teacher in the ordinary sense of the word. (That this may be inaccurate matters very little: the preceding statement describes him as a sick man, contrary to the testimony of his first biographer, Dr. William Rawley, *Works*, 1, 16–17.) Further, Bacon's statement bears a striking resemblance to that of Dr. George Hakewill (1627):

I have walked (I confess) in an untrodden path, neither can I trace the prints of any footsteppes that have gone before me, but only as it led them to some other way, thwarting, and upon the by, not directly: some parts belonging to this discourse, some have slightly handled, none thoroughly considered as a whole.

The similarity between Bacon and this author expresses (not his possible acquaintance with Bacon's work but) the general mood of the Renaissance. Counter to the common faith in the Fall and Decay, these authors preached the doctrine of rejuvenation that is not just the repetition of the past but also the return to past abilities to create. The return to antiquity and modernism are often mingled this way. For Hakewill's philosophy see Jones (1961, Chapter 2). Bacon's comment on the myth of the Fall of Man is discussed below.

[3] Spedding was not ready to change his view radically despite Ellis' influence on him. His preface to *Interpretatione Naturae Proemium* and the one to *Parasceve* are mainly expressions of appreciation disguised as rational analyses. He speculated on the conceivable cooperation between Bacon and Newton had they been contemporaries. He did not answer Ellis' vindication of Harvey's dismissal of Bacon. Spedding explained Bacon's oversight of Galileo's work as his reluctance to speak of what he did not know. Ellis refuted this. Denying the suggestion (of Joseph de Maistre) that Roger Bacon influenced Francis Bacon with his discussion of the hindrances to the attainment of true knowledge (in the opening of his *Opus Majus* that was published only in the eighteenth century with — Bacon, 1733), Ellis said (*Works*, 1, 90) that it is unlikely that Bacon would "take the trouble" to read it in manuscript were it available to him. In response to this Spedding said he could hardly consider Bacon so careless. There is no need to argue against Spedding here. Since Liebig has shown how much of Bacon's works rested on mere hearsay, however, the view of Ellis that he did not know of Roger Bacon is doubtful too. Possibly Roger Bacon influenced Francis Bacon; his opposition to Aristotle and his "*Vivificatio sapientiae, quae mortua jam a multis temporibus jacuit, me valde instigabat*", *Op. tert.*, Chapter 4; quoted in Heinemann (1944, 243) suffices to suggest that Francis knew of Roger, although not that he read him. Incidentally, Heinemann's essay discusses the riddle of Roger Bacon.

Spedding too was unable to show any new element in Bacon's induction, not even what (rightly or wrongly) Bacon deemed the new element in it. He suggested (*Works*, 1, 389) that the novelty lies in Bacon's demand to start from pure observations and to theorize only afterwards, after a large volume of observational material will have been collected. This suggestion of Spedding flatly contradicts Aphorism, 125 of Book 1 of the *Novum Organum*, where Bacon assures his readers that Thales had arrived at his theory (that everything is water) by using some observation-records that he never made public and that are now lost. Spedding has suggested seriously that Bacon's method can and should be tried out. In the Royal Navy, he noted, sailors were ordered to make meteorological observations without any knowledge of meteorological theory. To this William Whewell wisely answered that scientifically these observations are inherently useless.

Spedding suggested that we are too quick to condemn Bacon's methodology, that before doing so we should try it: we should take up Bacon's new idea of observing without any knowledge of theory. This way he dismissed Ellis' major criticism of Bacon. It is Ellis' contention that following the method of observing without previously theorizing condemns us to remain empirics forever. Now Bacon had claimed that the idea of observing without theorizing was not his but that of the empirics; against them he said that observations bring about the theories that rest on them. At times he said, under proper conditions, theories come all by themselves. At other times he spoke differently: what is required for making them come by themselves is waiting for them to come by themselves. It is necessary to be discontent with facts alone. As exceptions, the empirics were contented to live with no theory. The expectation that theory will come, Bacon added, is natural too: the empirics too were people who initially did expect theories to come; they gave up their expectation in frustration. Why then were they frustrated? Because their minds were polluted; because they were prejudiced.

The difference between Bacon and the empirics is that while they had ignored all theories, past and future, Bacon ignored nothing. He declared all theories important, but in different ways: all past theories are poison and all future theories are strong nutrition. Only those whose minds are totally empty of any preconceived idea will soon find themselves empirics no longer, but discoverers of new and unassailable knowledge. This is the quite new and extremely important element in Bacon's view, his utter radicalism. My solution to Ellis' difficulty then is my ascription of radicalism to Bacon plus the claim that it is important: Bacon was rightly appreciated and he was rightly severely criticized. Radicalism is both important and false. And as a scholar he was most unusual, simultaneously both so admirable and so reprehensible.

Two objections to my ascription of radicalism to Bacon are obvious. First, he did not advocate it. Second, it was no novelty when he advocated it. To the first objection my answer is this. Bacon's immediate disciples ascribed to him the radicalism that they endorsed; and their adversaries agreed about the ascription of radicalism to him and opposed it. The French *Philosophes* too acknowledged to him their radicalism. Their extension of radicalism from research to politics by developing new and allegedly unprejudiced political theory led to the French Revolution. Edmund Burke condemned the Revolution as radical and radicalism as a prejudice that leaves its followers with "naked reason"—with no laws [Burke, 1790, 130]. To the second objection my answer is this. True, in a way already the ancient were radicals. Yet the application of radicalism to research is new and contrary to ancient dialectics, as Bacon repeatedly stressed.

For the riddle of Bacon today see Appendix A.

Chapter 4
The Function of the Doctrine of Prejudice

The function of the doctrine of prejudice is to explain any failure of devoted scientific research by blaming its operatives, or rather the prejudices that pollute their minds, since observers who endorse some theories before they observe see things wrongly. This is also the explanation of why all the labors of centuries of past research bore less fruit than what Bacon intended to achieve within a few years, or at least within a few generations (*Novum Organum*, 1, Aph. 178). Indeed, any optimist must have an answer to the obvious question, why was the past so bad when the prospects for the future are so bright? What is the cause of the expected radical change?

Some optimists like Leibniz may answer that the past was not so gloomy after all. But no one had a lower opinion of the past than Bacon, who viewed all or almost all past thinkers as sophists of one kind or another. (He even smeared Socrates as pretentious; *Works*, 4, 412.) Intellectually (not politically) he was a relentless radical who repeatedly demanded in the strongest terms that his followers should completely forget all past intellectual achievements (and remember only unadorned facts). His doctrine of prejudice is the full justification for his radical demand to start afresh.

4.1 Radicalism

Almost everywhere all politics is conservative. The exception is the politics of ancient Greece before Alexander and the modern West. There political thought is split between radicals who want sweeping changes and reactionaries who demand going back to the past. The moderate reforms that most parliaments implemented were always expedient: no arguments in their favor were offered before the twentieth century. Those who wanted reforms usually wanted sweeping reforms. How sweeping? The only answer given to this fundamental question was vague: delete all past institutions, customs, laws; forget the past.

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_4,

The radicalism that philosophers of science advocate is not political but it is the application of political radicalism to thinking: forget past opinions. This is Bacon's radicalism. His argument for it was his doctrine of prejudice: holding on to old ideas is prejudicial and it blocks the growth of science.

Perhaps the most important aspect of the doctrine of prejudice is that by explaining any failure to execute induction it renders the theory of induction (inductivism) irrefutable.[1] Whatever the process is, we can ask its advocates to employ their marvelous machine and produce a theory. (Bacon said, he was too busy to do that; *Works*, 1, 380.) If they manage to produce an interesting theory, and if the theory that they produce is scientific, then we may try to refute it. If we succeed, then the moment the theory is refuted, the hope is shattered that the inductive machine produces only demonstrable theories. The doctrine of prejudice, however, restores the picture. We may always blame the user of the machine for inability to use it properly. Thus, even the great discoverer Priestley was prejudiced, since his phlogiston theory is false and thus a prejudice and an impediment. All the followers of Volta claimed that Galvani was prejudiced, and they, in their turn, returned the compliment. The followers of Lavoisier hardly began to enjoy their superiority over the advocates of the doctrine of the phlogiston, when Davy's disciples blamed them for prejudice too.

What is known nowadays of this doctrine of Bacon and of its function? To begin with, he spoke of prejudices as idols, thus drawing attention, but not to the heart of the matter. The historical literature mentions his "doctrine of the idols" now and then, but as mere embellishment—without attention to what it embellishes. The idols are classes of prejudices.[2] Ellis found it "one of the most remarkable parts of the *Novum Organum*" (*Works*, 1, 125), but all he said on it is that it drew attention at the time and that "The word *idolon* is used by Bacon in antithesis to idea. He does not mean by it an idol or false object of worship" (*Works*, 1, 126, 157–8). Incidentally, the *Novum Organum* has four Idols: of the Tribes, stemming from the innate disposition to guess and become prejudiced; of the Cave, stemming from the disposition to have a prejudice peculiar to one's interests (as magnetism was to William Gilbert); of the Market-place, stemming from commonly accepted views; and of the Theatre, stemming from metaphysical systems. All this hardly signifies: not being theories, names are changeable with relative ease.

Bacon's condemnation of past prejudices like scholasticism, astrology and alchemy was well known and rightly valued, of course, regardless of whether the word "idols" was familiar. Condemnation of the past is surely not peculiar to Bacon, although he was the first radical extremist. Still, this in itself may be only a part of his propaganda for the new method that is "preferable to anything hitherto known." Yet his early readers did not take seriously his admonition against superstition, especially as it was agreed not to advertise his own embarrassing superstitions, and his allegiance to magic, alchemy, and astrology. Some educationalists still mention

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[1] "Irrefutability is not a virtue of a theory (as people often think) but a vice" (Popper 2002, 48).

[2] Bacon offered two or three different classifications of the human prejudices; *Works*, 5, 602.

his demand to be aware of prejudices that like robbers stand watchfully in a dark corner of the road to wisdom to deprive us of our reason. But demands are not doctrines—though doctrines may reinforce or weaken them. This is a bit frustrating. Macaulay has put it nicely (Macaulay 1837, 91):

> It is very well to tell men to be on their guard against prejudice, not to believe facts on slight evidence, not to be content with a scanty collection of facts, to put out of their minds the idols which Bacon has so finely described. But these rules are too general to be of much practical use. The question is, what is a prejudice?

The poet Samuel Taylor Coleridge tried to answer this question. In the Introduction to *Encyclopedia Metropolitana* (1845), where he refreshingly takes Bacon's induction to be Platonic rather than Aristotelian (24–5), he says (128),

> His peculiar use of the word idols is again a proof of faulty method in his style; for it gives a sort of pedantic air to his reasoning; but in truth he means no more by it than what Plato Means by opinion (*doxa*), which the latter calls a medium between knowledge and ignorance.

This is just right and well put. It overlooks Bacon's idea that an idol is an object for blind worship. This matters little, as any opinion (*doxa*), any conjecture, soon becomes an idol, since its advocates boastfully consider it knowledge. Admittedly, Bacon was a pedant and an ignoramus. He always divided items to classes and subclasses, talked in parables from books, scolded and gave marks like a schoolmaster. This too matters little. Here is what his admirer Spedding wrote on his inability to notice that he was not the peak of human perfection (*Works*, 3, 512):

> A sufficient explanation of this … may be found … partly in the excessive activity of his discursive faculty, which … seduced his attention from the exact point at issue and flattered him that the time was come for a *permissio intellectûs*; partly in the great pains which he took to lay his subject out in titles, articles, sections, divisions, and subdivisions all named and numbered, the effect of which would be to give his investigations an appearance … of exact and delicate discrimination; and partly in the magnanimous hopefulness of his nature ….

In brief, Spedding said, Bacon had failed because he was over-engaged in propaganda, in classifications that look scholarly, and in vain hope. Bacon deserves better defense. It is regrettable that Macaulay, Coleridge, Spedding and even the great Ellis, all paid attention to secondary aspects of Bacon's doctrine of prejudices rather than to the role of the doctrine in the philosophy of Bacon and of his disciples.

Prejudices are not just false hypotheses; they are the prejudices of those who believe that they are true; and this is a direct answer to Macaulay's question, what is a prejudice? Not that this has escaped his eye, but that he did miss something about it all the same. His famous essay on Bacon is a sort of a review of a new edition of Bacon's *Works*. And had he taken the repeated and passionate declarations of Bacon on the danger of Anticipations seriously, he would not have posed his question. This is not to blame Macaulay—or Ellis or any of Bacon's later critics or commentators for that matter—for having ignored Bacon's texts. If one tries to consider all that Bacon wrote with the same caution, one will never be able to produce a comprehensible interpretation of Bacon's output as a whole. My aim is to add a few passages to those upon which Ellis erected his interpretation—while leaving still many other passages as unintelligible, inconsistent, or in contradiction with those that Ellis considered significant. To repeat, had Bacon not been so influential, the whole work of interpretation of his works would be of no use. As the major part of the messy job of making sense of Bacon has been so well performed, it is a pity to omit those passages that exhibit those ideas of his that were so influential—especially since they fit well within Ellis' reading. (This is how it should be; for otherwise my interpretation would be questionable.) The exercise is not too hard: it takes a little mental agility to reconcile any given set of statements. My suggestion is not so much a reconciliation of some sets of statements but also and chiefly a solution to the problem of Ellis and Spedding, the problem of the novelty of Bacon's method: the view endorsed more-or-less officially and more-or-less wholesale through the eighteenth century and the first half of the nineteenth century, one that Dr. Isaac Watts (whose *The Improvement of the Mind* was a very popular standard text) advocated, and was taken very serious by such leading researchers as Black, Priestley, Cavendish, Laplace, Davy, Wollaston, Herschel and Faraday.

4.2 Radicalism Invented

And so, to the problem of Ellis and Spedding. To see how acute it is, let us notice (as Ellis did) that Bacon admitted in the Introduction to his *Novum Organum* that he was following the ancients, and to notice also (as Spedding did) that he did so while claiming significant novelty for his methodology.

> For I should profess that I, going the same road as the ancients, have something better to produce … as though in this there would be nothing unlawful or new (for if there be anything misapprehended by them, or falsely laid down, why may not I, using the liberty common to all, take exception to it?) yet the contest … would be an unequal one … As it is however—my object being to open a new way for the understanding …—the, case is altered … and I appear merely as a guide to point out the road—and depending more upon the kind of luck than upon any ability or excellency.

Clearly, in this passage Bacon tried humbly to avoid comparison between him and the ancients (i. e., Plato) as an originator of the inductive method; he was only following them, as they were the founders of the true method. (Hence, the common complaint that he claimed priority here is false.) Still, he was also making a proud claim: they were in error about the preliminary conditions for the proper employment of their method; they "falsely laid down" these conditions and he offered the correction: he discovered the right conditions for the use of induction. He is thus the guide to the correct way to the correct application of the ancient method. And, finally, he proudly and humbly admitted that he was able and excellent as well as just lucky.

It is clear then that unless the novelty of his method is explained, this passage (as well as many similar ones) will surely be hardly comprehensible.

In the same Introduction to his *Novum Organum* Bacon asks, how is it that the ancients, who knew about dialectics, did not achieve certain knowledge? His answer is clear and reasonable (if it is permissible to talk about reasonable error concerning a mystical method of learning). This question Bacon answered:

> This remedy, however, came altogether too late; for things were desperate, after that the mind thanks to the daily habits of life, was both filled by polluted discourse and teaching, and possessed the vainest idols. And so that art of dialectics taking heed to this too late, and nohow restoring the matter, was potent rather for the confirmation of errors than for the disclosing of truth.[3]

Thus, before Bacon appeared on the stage the induction machine did not work because of the pollution of minds with metaphysical theories. And, Bacon repeated emphatically, (*Novum Organum*, 1, Aph. 125), the major difference between his method and that of the Ancients is that he demanded that one should be slow and cautious and not leap to the most general axioms.[4] He repeatedly declared that leaping to too general an axiom leads to holding to an uncertain axiom that is possibly false and is therefore a prejudice. Moreover—and here Bacon used the doctrine of prejudice to reveal the secret of past failures—if the method is applied by a mind occupied with anticipations, then during research the situation worsens, for the inductive method only strengthens beliefs in prejudices instead of leading to demonstrable truths. This is inevitable, since prejudiced people are disposed to look for confirmations of their views instead of looking for truths of Nature. And this inevitable disposition spoils everything as only the pure of mind can succeed in this sacred task. And so, Bacon taught the need to watch for the temptation to make hypotheses, so that researchers will remain worthy of the task of uncovering the truths of Nature.

4.3 Radical Methodology

This, the doctrine of prejudice, is entirely new in the context of scientific research. Not otherwise: Bacon surely was not the first person to fight against prejudices. But whereas Epicurus had taught that learning expels prejudice; while Gilbert tried to refute some prejudices by the use of some experiments and by the appeal to his readers to try to repeat these experiments, Bacon was the first to demand from scientific researchers that they should first clean their minds and become humbler and more patient before they start to make any further preparations for the advancement of learning. Such theories were not uncommon concerning studies of religious mysteries, and notably, the Kabbala (with which Bacon had only some faint familiarity).

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[3] I use here Kitchin's translation. Ellis regularly put "logic" where Kitchin put "dialectics". This is a mistake that goes beyond terminological niceties: Bacon used "dialectics" rightly to denote criticism; he was disappointed in the scholastic tradition that allowed criticism to fade into logic-chopping. All Renaissance mystics opposed scholastic logic-chopping. Bacon deemed it unavoidable and suggested instead taboo on promoting an unproven theory. Kant deemed advocates of hypotheses impostors and recommended preventing the display of their wares (first *Critique*, Preface Axv).

[4] Bacon could not distinguish between wild and tame generalizations (see *Novum Organum*, 2, 17).

Already Ellis indicated the Kabbalist influences on Bacon. The works of Kabbalist Robert Fludd, he noted, were quite popular in Bacon's days, and Spedding noted that Bacon alluded to his work.[5]

The Kabbalist doctrine of true learning is mystic-intuitionistic. It is also "operational", to use Bacon's term. The term used in the Hebrew Kabbala is "applied Kabbala" or "practical Kabbala". It designates the knowledge that empowers its possessor to change reality for the better. The conditions for the ability to acquire this power are not too clear. This is of no importance here. What is important here is that the list of conditions includes as a matter of course the possession of qualities such as humility, sincerity, good will, purity, pity for humanity and understanding of the greatness and difficulty of the task, not to mention the total absence of any self-concern. These appear in Bacon's works repeatedly.[6] These personal qualities are *sine qua non* for the Kabbalist practitioner. So are the tense efforts on the road to purification. These efforts usually fail and this is the reason why generations of Kabbalists failed to perform the miracles or to produce the salvation that their knowledge should enable them to perform. Noticing the high value of purity in Bacon's teachings (Lemmi 1933, 102) renders obvious the Kabbalist aspects of the innovation that Bacon spoke of.

Insofar as the doctrine applies to learning in the ordinary rational sense of the word, it is a possible solution to a very important problem, the problem of observation; not that Bacon had the problem in mind, but that his followers did. The function of Bacon's doctrine of prejudice, to repeat, is to explain past failures[7]: unless researchers are utterly humble and give up wholeheartedly all past theories, there is no hope for the new science. This is a new philosophy of change: radicalism.

Even radicalism is not as new as it seems. The thesis of the MA dissertation of Petrus Ramus suffices to show this. It was more-or-less, "All the things that Aristotle has said are inconsistent because they are poorly systematized and can be called to mind only by the use of arbitrary[8] mnemonic devices. Everything that Aristotle taught is false" (Ong and Johns 2004, 36 ff.). Yet Ramus was unclear, since the

[5] Bacon alluded to Fludd in his *Cogitate et Visa*. The Kabbalist aspect of *The New Atlantis* drew attention only recently (McKnight 2006, 23 ff.)

[6] Gaukroger refers to this aspect of Bacon's project for reform—his demand for the personal reform of the researcher—and to the historical background to it (Gaukroger 2001, 7, 10, 13, 102, 104–5 and more).

[7] The Copernican Revolution undermined the authority of the ancients, as Copernicus noted early in his book. Buonamico then discovered that Aristotle and Archimedes were in disagreement and his student Galileo then tried to unite these two discoveries, thus steering a revolution in physics.

[8] Notice that Ramus took memory seriously (Ong and Johns 2004, xxiii, 194) and that he took for granted the expectation that the true classification will be remembered with no mnemotechnical device. This is a huge item in Renaissance mysticism that is inherently confused. It also links the backward-looking aspects of radicalism to ancient preliterate practices. This is an example for the thesis of Gershom Scholem (Scholem 1965, 7): "All mysticism has two contradictory or complementary aspects: the one conservative, the other revolutionary". This holds for Bacon very significantly.

principle was denouncing was arcane,[9] whereas Bacon's sweeping condemnation of all past ideas, as well as the principle that he announced, though new, were very easy to comprehend.

The worst of it is that these days hardly anyone thinks that that starting afresh is possible, that the full implementation of radicalism is possible. This indicates yet another possible solution to Ellis' major difficulty. Radicals take it for granted that starting afresh is possible. Bacon rightly observed that words are vehicles of tradition, and he therefore demanded of his followers that they lay aside all conceptions, and begin with a vocabulary that comprises names only. This is impossible[10] and yet Bacon insisted that it is a *sine qua non* for his system (*Novum Organum*, 1, Aph. 31):

> It is idle to expect any great achievement in science from the super-inducing and engrafting of new things upon old. We must begin anew from the very foundations, unless we would revolve for ever in a circle with mean and contemptible progress.

This passage is powerful and its radical character is obvious. Like the Kabbalists, Bacon demanded humility and attention to small things and expected "great achievements" in return. Writers referred in this way to two optional attitudes to tradition, grafting new material on old or removing the old first. This is a trap. The only way out of it is to observe that influence appears either by the endorsement of the old or by criticism if it. We then have three options, endorsement, removal, and critical attention. What brings this about is the view of serious criticism as an expression of respect.

Here is one more passage from Bacon to illustrate his view of his radicalism as of the essence (*Novum Organum*, 1, Aph. 97):

> No one has yet been found, of so great constancy and sternness of mind, as to have determined and set it as a task to himself, utterly to abolish common theories and conceptions, and apply afresh to particulars and intellect cleared and leveled … But if anyone in his ripe age, with his senses whole and mind purified, should apply himself anew to experience and particulars, we should have better hope for him. And on this point we promise ourselves the fortune of Alexander the Great … [whom] Livy considered … truly … [contending] 'that he did nothing beyond justly daring to despise what was vain'.

(The autobiographical element in this passage is obvious. Those who glorified Bacon regularly overlooked this.)

The reason for starting afresh is that (at least since the days of Parmenides and Democritus) people have allowed speculations or anticipations that made all past learning "revolve in a circle with a mean and contemptible progress". Aristotle had taught people to make hasty inductions that do not lead to certainty and that only increase the stock of anticipations. And, Bacon affirmed, unless we can completely

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[9] It is always possible to ask whether a radical move is real. Bacon's nineteenth-century critics said there was nothing new in his teaching. Thomas Spencer asserted that the claim of Petrus Ramus for having differed from Aristotle is erroneous (Spencer 1628, 213). So was his, of course.

[10] Ellis did not notice that as a radical Bacon was naïve. His naïveté hides behind the complexity of his ambitious spirit: he compared himself with Plato, Aristotle and Alexander the Great. We should consider this most naïve rather than censure it (Liebig) or explain it away (Spedding).

rid ourselves of this "childish" and "vicious"[11] sort of anticipation there is no hope of advancing knowledge. As he put it, probably long before the *Novum Organum*, in a sketch of a book (*Works*, 3, 245–6),

> Of the errors of such as have descended and applied themselves to experience, and attempted to induce knowledge upon particulars. That they have not had the resolution and strength of mind to free themselves wholly from Anticipations, but have made a confusion and intermixture of anticipations and observations, and so vanished. That if any have had the strength of mind generally to purge away and discharge all anticipations, they have not had that greater and double strength and patience of mind, as well to repel new anticipations after the view and the search of particulars, as to reject old which were in their mind before … That if any have had or shall have the power and resolution to fortify and inclose his mind against all anticipation, yet if he have not been or shall not be cautioned by the full understanding of the nature of mind and spirit of man, and therein of the seats pores and passages both of knowledge and error, he hath not been nor shall not be possibly able to guide or keep on his course aright.

In short, prejudice paralyses research: to succeed, the mind of the researcher must be empty of all knowledge. That Bacon demanded knowledge of the need to have an empty mind before and during the whole process of research and must stay on guard against pollution of the empty mind, to know no less than the whole of the psychology of research, is perhaps a trivial contradiction. But contradictions are easy to remove. The attempt of Bacon to put psychology before research still has a tremendous effect on philosophy. Its rationale is the idea that to avoid the temptation of guessing while observing is terribly difficult, yet it is absolutely necessary to resist this temptation for those who wish to erect a true science.

Thus, the new element that Bacon introduced explained not only all past failures; it also explained future ones. Those who will use his inductive machine effectively will assuredly reach demonstrable results. If a theory achieved this way is refuted, however, then, its very refutation is proof that its originators were prejudiced when they presented it to the public: they were hasty because they were greedy for fame and not humble enough.

The doctrine of prejudice was applied in this manner repeatedly and systematically and with no objection until the mid-nineteenth century; it is still applied here and there to this very day. Even some of the greatest researchers were declared prejudiced upon the replacement of their theories. When nearly two centuries after Bacon's demise Joseph Priestley was branded prejudiced, he objected, but not because he rejected Bacon's doctrine of prejudice, but because he viewed his opponents as prejudiced. His evidence was the fact that giving up the doctrine of phlogiston won more popularity than clinging to it. And when his criticism of the views of Lavoisier were justified, Lavoisier was branded prejudiced too. This, however, did not rehabilitate Priestley. Finally, Lavoisier won rehabilitation—by historians of science, and a century later. They did not refer to Davy's testimony that he was not prejudiced and they did not try to repeal Bacon's doctrine of prejudice. Clinging to it,

[11] Ellis was the first to notice—with surprise—that Bacon deemed uncertainty vicious. The reason for that lies in his doctrine of prejudice that Ellis took lightly.

they concealed the fact that Lavoisier's theory met with valid criticism, using a technique that seems somewhat questionable (Agassi 2008, 163–8). Lavoisier did not live to witness the variations of his reputation. Luigi Galvani at his time and Davy later, both won great fame that was later shattered. They both felt obliged to leave their home countries when people jeered at them.

What stopped this practice was the replacement of Newton's optics by the wave theory of light—due to admiration, but of his theory of gravity, not of his person: if he was prejudiced about light, then he could be prejudiced about gravity too, which was unthinkable. At the time, the aim of methodological discussions was to show that the fate of Newton's optics could not possibly herald a similar fate for his theory of gravity.

The repeated application of the doctrine of prejudice made research a dangerous activity: publication of results of research was of no personal use except for fame, and the risk of meeting with censure instead was very real. Researchers hesitated and postponed publication. Of course, this could deprive them of priority. The famous case was the discovery of the decomposition of water. Henry Cavendish was the first to observe it; he hesitated to go to print about it and so priority for it goes to James Watt. Such losses of rewards led to the submission of sealed notes about some discoveries. This practice, however, violates the rules of priority and so it is outside the present discussion, except to say that with the decline of the popularity of Bacon's doctrine of prejudice the custom of submitting sealed notes disappeared. It is hard to fix a date for the decline of the doctrine, especially since the doctrine is still popular. Yet, clearly, the replacement of Newton's theory of gravity with Einstein's was a major factor here. The same factor gains a simpler expression in Bertrand Russell's assertion that to claim that anyone is free of all prejudice is humbug (Russell 1956, 77).

Bacon's utopianism and his doctrine of prejudice had a tremendous effect because the educated public endorsed them and because leading thinkers took him very seriously. It led them to condemn all mistaken views as anticipations and therefore as prejudices. Although the word "prejudice" then had a specific philosophical meaning, admirers of Lavoisier were rightly reluctant to admit that he was prejudiced. They had the choice between discarding Bacon's theory of prejudice and declaring Lavoisier's theory absolutely true and rejecting the criticism leveled at it—thereby becoming prejudiced, of course. I shall later try to look for the reasons that prevented them from rejecting Bacon's doctrine of prejudice. Before that let me discuss the original version of the theory, the way Bacon presented it.

Chapter 5
Bacon on the Origin of Error and Prejudice

Bacon's *Wisdom of the Ancients* (1609) is his most popular work next to his *Essays* (1597, 1612, 1625). It deals exclusively with ancient myths. This is the best indication of the great change in popular taste between our age and the late Renaissance. Today we find no use to reading meanings into ancient myths; the appeal of this literature to the readers of the late Renaissance makes no sense to us. My discussion here takes the concerns of Bacon and his readers as given while relating it to his view of science and in particular his view of scientific research and what will make for its success, even though the value of his studies of myths is obviously more literary and historical than methodological. It is the irony of history that the artistic merit of the book is what raised suspicions as to its original authorship, suspicion that research on it fully justifies (Lemmi 1933). Yet our concern with his view of research is a concern with what was decidedly peculiar to him.

Spedding reports (*Works*, 6, 609) that this work won great popularity at the time. The major points of it occur both in the *Advancement of Learning* (1605) and in the *Novum Organum* (1620a, b, c) that were very popular too, not to mention *Beginnings and Origins of Things* (1624) that supplied material for Ellis' research into Bacon's metaphysics.

This is not to say that all of Bacon's early followers were as mystically oriented as Bacon was. Presumably, only some of them took his interpretations of the ancient myths to mean that they are literally true. I follow here studies of R. F. Jones (1951) of the serious dispute about the myth of the fall and decay of the early seventeenth century. On any case, clearly, Bacon's early followers endorsed his major myth, the myth that the operation of the induction-making machine is restricted to those who obey the authority of their senses. Lemmi's most interesting comparative research into Bacon's mythology made it quite clear that Bacon was much more serious about the myths than his later followers allowed: he genuinely believed in myths—in the fashion in which myths are objects of belief, of course—this in agreement with both Ellis and Spedding. We should be more understanding about such things. My own interest here is in the metaphysical idea behind these myths. It survived them and became a part of the Baconian idol of the market place. According to

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_5,

Bacon, God created the universe and the mind on one schedule, making learning most easy and natural, so that wisdom prevailed.[1] Then the Fall of Man came to pass. People wrongly made anticipations, speculated, interpreted facts so as to agree with their pet views, grew impatient, weak-minded, and eager for fame; they became impostors. Since then the art of learning degenerated: wisdom withdrew.

This is but one part of the myth. The other is the idea of rejuvenation. It is due not to the whims of our imagination but to the goodness of Nature ("Deucalion or Restoration"). Bacon's philosopher's stone and elixir of rejuvenation purifies our minds and gets it out of the habit of speculating acquired since the Fall. We may thus return to the Golden Age of Knowledge. (As Lemmi noted, Bacon argued with the alchemists taking them more seriously than most commentators allow.) Bacon's opposition to alchemists, then, is that they were after the wrong philosopher's stone, namely, the ideas of their predecessors that comprise anticipations. The new philosopher's stone is not speculations but observations of Mother Nature. Bacon referred in this context to the myth of Deucalion—the myth that symbolizes restitution, renovation or restoration.[2] By his interpretation, the restoration of wisdom is the correction of popular errors accrued by "improper procedures". Rationalists like Laplace and Faraday endorsed this part of his philosophy. They took it for granted that if research is not successful, then it is the researcher who is to blame, that researchers are bound to be successful if they humbly agree to "woo Nature … with due observance and attention" (*Works*, 6, 36). Following the prescriptions of the doctrine of prejudice is all that is needed.

Here is Spedding's view of Bacon's *The Wisdom of the Ancients*, or rather on Bacon's interpretation of myths (*Works*, 6, 607; 3, 124):

> The object of the work was probably to obtain more favorable hearing for certain philosophical doctrines of Bacon's own; for it seems certain that the fables themselves could never have suggested the ideas, however a man to whom the ideas had suggested themselves might find or fancy he found them in the fables. But the theory on which his misinterpretation rests, namely, that a period of high intellectual cultivation had existed upon the earth, had passed out of memory long before the days of Homer, was, I suppose, seriously entertained by him; nor was it a thing so difficult to believe then as it seems now.

Thus, the credulity that Spedding ascribed to Bacon did not upset him as much as his judgment that Bacon's interpretations are quite a-historical. Oddly, what seemed to him so unpleasant is quite permissible to present-day taste. Spedding noted with needless irritation that the myths in *The Wisdom of the Ancients* are just pegs on which Bacon hung his ideas; we find this easy to allow for. It even makes forgivable the book's being almost all plagiarized. Artless[3] as Bacon's

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[1] Kant said, by this idea God "gives the lazy superiority of the indefatigable researcher" (Kant 1819, 137). See also note 5 below.

[2] Kotarbinski praised Bacon's anti-metaphysics and suggested that his overarching aim had been the transmutation of the elements (Kotarbinski 1935). This makes Bacon more of a single-minded thinker than he was. It also makes inexplicable his myths as well as the early publication of his *Globus Intellectualis*.

[3] Bacon's literary efforts refute with ease the idea that he wrote plays, let alone Shakespeare's.

works are, technically they do not differ from the loftiest works of art (verbal or musical) that are but rewrites of older ones, at times in all details yet with heightened artistic power. Except that Bacon's power was not artistic. But power it was, and also not small.

This fact Lemmi recognized. His study of Bacon's myths is therefore much more to the taste of today's readers than Spedding's, but its value is limited since he has ignored Bacon's methodology. (Oddly or not, he deemed it right but commonplace.) He showed, first, to whom Bacon was indebted, and what a small part of the work appears to be original. Further, he was interested in Bacon's metaphysics. He tried to show that in this domain Bacon's debt is chiefly to Aristotle and to the alchemists, all his expressions of contempt for them notwithstanding. Finally, he was concerned with Bacon's mentality. His view of Bacon is much more modern that those of Ellis or Spedding or even Liebig. Yet, unfortunately, intellectually his view is too unfavorable to Bacon and artistically too favorable—perhaps in the wish to compensate and perhaps as an explanation of the popularity of Bacon's fables. Nevertheless, his characterization is apt (149, 211):

> The middle ages were perhaps nearer to Bacon's England than to Galileo's Italy, and Bacon was by no means entirely divorced from his times. His profession of universality smacks of the mediaeval thesaurus, his mind mediaevally assimilative rather than modernly constructive; his was the schoolman's love of classification and his preoccupation with religious issues, he has the mediaeval tendency to slip into metaphysics. In his vigorous but more or less unreasoning hostility to the mystic aspects of astrology and alchemy and to the almost mystic deference paid to Aristotle, he can better be compared to Petrarch than to Galileo. … His intuitions are the more remarkable. He denounces astrology … He is not the supreme genius that towers above his age, but rather a robust sagacious common sense that pushes its way through contemporary thinking and with rare good judgment seizes upon the best.
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> Bacon was not singular in his times for his belief in mythological symbolism; but he certainly was in so far as the astonishingly copious use which he made of it in his prose … Bacon's symbolism is not original. But … it is not difficult to recognize the hand of the artist in the transformation.

Lemmi's results resemble those of Ellis. (He does not mention Ellis and alludes to him only once.) But he is much more outspoken when he shows how very mediaeval Bacon's taste was. And he completely destroyed Ellis' view that Bacon had a deep insight into the atomic doctrine. This changes the merit of Bacon; he now ceases to be a metaphysician and becomes a quasi-mediaeval artist.[4]

Lemmi praised Bacon for a "discovery of the true nature of heat". Surprisingly, he did not see (a) that Bacon's theory of heat is also borrowed (from Telesio) and (b) that this theory is only another symbolic myth, an animistic one at that (*Works*, 2, 382 and 438 *et seq.*). As Lemmi also suggested that Bacon had made some valuable contributions to astronomy, we may view this as his (polite and irrelevant) effort at softening his debunking of Bacon. What is valuable in Lemmi's study of Bacon is his enlightening explanations of many obscure passages in Bacon (mainly concerning his theory of matter which is alchemical), on which his comments are novel and

[4] The contention that Bacon wrote Edmund Spenser's *Fairie Queen* is thus slightly less preposterous than that he wrote Shakespeare.

refreshing. Still, though he threw much light on Bacon's myths, it is regrettable that he overlooked their expression of Bacon's methodology, particularly of the doctrine of prejudice.

The most important myth in Bacon's *Wisdom of the Ancients* is that of Pan—the Universe or Nature. Bacon calls the myth of Pan "a noble fable … and big almost to bursting with secrets and mysteries of Nature". His view of Pan's horns as symbols of the hierarchy of Forms that ascends to unity is not original, nor is his view of Pan's music as the law of nature. His interpretation of the myth of the discovery of Ceres (or Demeter, the goddess of the earth and of fertility) is this:

> As for the tale that the discovery of Ceres was reserved for this god [Pan], and while he was hunting, and denied to the rest of the gods though diligently and especially engaged in seeking her, it contains a very true and very wise admonition, which is not to look for the invention of things useful for life and civilization from abstract philosophies which are, as it were, the greater gods, even though they devote all their strength to the purpose; but only from Pan, that is from sagacious experience and the universal knowledge of nature; which oftentimes by a kind of chance, and while engaged as it were in hunting stumbling over such discoveries.

Discovery is the outcome of hunting, i. e., experimenting. Experimenters must have a universal knowledge of nature, no less, and yet discovery is accidental, not theory-based. The universal knowledge of nature is oddly the theory of Forms and the theory of Induction. The first part of Book 2 of the *Novum Organum* offers his metaphysical view of the universe and the second part of it offers the method of induction. Bacon would not call his views metaphysical. In the hierarchy of forms (the myth of Pan's horns), the highest forms belong to metaphysics and the lowest to natural philosophy, namely, to science (*Works*, 3, 357). The last statement is not metaphysical (but meta-metaphysical). Bacon contended here that knowledge of this, the universal knowledge of Nature, necessarily precedes proper research! In his *De Augmentis Scientiarum*, (*Works*, 5, 28) he assured his readers that his method follows the pattern of Nature: following Bacon's method we move in parallel with nature.[5] This hypothesis assures us that true progress is within reach.

> And it is excellently provided that of all discourses or voices Echo alone should be chosen for the world's wife. For that is in fact the True Philosophy which echoes most faithfully the voice of the world itself, and is written, as it were from the world's own dictation; being indeed nothing else than the image or reflexion of it, which it only repeats and echoes, but adds nothing of its own.

The myth of the birth of Pan occupies a special position in *The Wisdom of the Ancients*.[6] It comes in three versions, we learn. Some say, he is the son of Mercury: he is born through the word of God (since Mercury is the divine messenger who bears messages verbally). Others say, Pan is born from the seeds of things. The interpretation of this myth is one of two ancient cosmologies, of the *Homaeomerae*

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[5] Kant called this lazy or perverted reason; first *Critique* A690-1/B718-19. See also note 1 above.

[6] Bacon narrated and discussed the myth of Pan already in his early *The Advancement of Learning*; in his Latin translation of it, *De Augmentis scientiarum*, he enlarged upon his interpretation.

and of the atom. Bacon prefers here the *Homaeomerae* that belongs to Plato and Aristotle, "who have presented matter as entirely despoiled, shapeless and indifferent to form … For they have made matter as a common harlot and forms as suitors".[7] This version also deems Pan born to Penelope from her suitors. I cannot comment on this. Bacon mentioned a third myth, the traces of which Lemmi could not find; it may very well be quite original with Bacon: Pan is the son of God and Sin, of Jupiter and Hubris. The fall of Man was due to Hubris and hubris is the motive behind research into religion. (See also Bacon's essay "On Goodness and Goodness of Nature".) This Pan is another Pan, Bacon discloses the mystery, born after the fall of Adam (*Novum Organum*, conclusion):

> For man by the fall fell at the same time from his state of innocence and from his dominion over creation. Both of these losses however can even in this life be in some part repaired; the former by religion and faith, the latter by arts and sciences. For creation was not by the curse made altogether and forever a rebel, but in virtue of the character "in the sweat of thy brow shalt though eat bread" it is now, by various labors … at length, and in some measure subdued into the applying of man with bread; that is to the use of human life.

Again, this last passage appears not in the mythological *The Wisdom of the Ancients*, but in the very end of the ultra-positivist *Novum Organum.* The labor that is due to the fall is evidently the self-purging, the need for which might also be the need for exercising exclusions before resting on the affirmative instead of going straight from the senses to the affirmative.[8]

In the myth of Erictonius the impostor Bacon found the same deep moral. Vulcan (fire) tried to rape Minerva (the wisdom of nature). In the struggle, his seed was scattered on the earth and gave birth to Erictonius, whose upper half was human. He invented chariots to hide his lower limbs. Bacon repeatedly stressed that baseless speculations, anticipations, are imposture: rape, vexation, and torture, enforcing and imprisoning Nature, etc. We must woo Nature with due observation and attention. Power over Nature is the result of submission to her. Observation, then, is a ritual.

The myth of Atlanta, the swift runner, who left her course to pick up golden apples, thus losing the race, likewise has the same meaning. Atlanta is the art of interpreting Nature; the distraction of picking up golden apples is alchemy that is all anticipation that causes the loss of true knowledge.

The myth of the proud and vain Icarus whose flying too high led to failure has an obvious reading: he was taken with metaphysical investigation.

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[7] Not so: Aristotle spoke of the desire of matter for form (Physics, II, 1, 192b35). The free use of sexual metaphors, incidentally, is the hallmark of the mystic literature. Carolyn Merchant discussed Bacon's sexual metaphors as if they are revealing (Merchant 2001, 42); they are hackneyed.

[8] The exclusions that are central to induction by elimination have no room in Bacon's system or even in Plato's. The process of dialectic in the mature Plato plays the role of purging the mind in preparation for the mystic enlightenment. Bacon argued against dialectic, but perhaps he allowed it after sufficient facts will be collected. More likely, the hypotheses that are excluded are formed only half-way, since in their full expression they pollute the mind of the researcher; they are shot down before they make a full appearance.

The same goes for Scylla and Charybdis, the mythical straits. Steering a ship through them requires wisdom: science should be neither too general nor too particular. (No one has ever asked, how are we to notice and measure the excesses of generality and particularity? The wise researcher knows.) The picture on the cover of the first edition of the *Novum Organum* is of a ship passing between the two pillars (of wisdom). In the Introduction to the *Great Instauration*, Bacon boasts that he has given a compass to the ship—the same ship that, according to Kant's report (Introduction to his *Prolegonona*) Hume has left on a reef where it was stuck until Kant provided her with a new compass. The ship is *Plus Ultra* ["To boldly go where no man has gone before"]. The book of Joseph Glanvill's famous defense of the Royal Society is *Plus Ultra* (1668). The title of John Locke's *The Conduct of the Understanding* is another allusion to the same ship, since to conduct is etymologically to navigate.

Bacon viewed Proserpine as the spirit of matter (in a slightly wider sense than other interpreters had it), expressing Bacon's animism, says Lemmi. It may also allude, let me add, to the affinity of the intellect with the universe that guarantees success to research properly conducted. (See notes 1 and 5 above.)

Bacon's interpretation of the myth of Narcissus is a sharp observation: the narcissist is not a show off; sufficiently satisfied with himself, he lives in seclusion and does nothing. This then has no methodological import.

> In this fable are represented the dispositions, and the fortunes too, of those persons who from consciousness either of beauty or of some other gift with which nature unaided by any industry of their own has graced them, fall in love as it were with themselves. For, with this state of mind there is commonly joined an indisposition to appear much in public or engage in business; because business would expose them to many neglects and scorns, by which their minds would be dejected and troubled. Therefore they commonly live in solitary, private and shadowed life; with a small circle of chosen companions all devoted and admirers, who assent like an echo to everything they say, and entertain them homage; till being by such habits gradually deprived and puffed up, and besotted at last with self-admiration, they fall into such a sloth and listlessness that they grow utterly stupid, and lose all vigor and alacrity. … Men of this disposition turn out utterly useless and good for nothing whatever, like the way of a ship in the sea [they] pass and leave no trace. …

Lemmi found the realism of this passage striking; he conjectured it was a genuine portrait. It agrees wonderfully with Liebig's portrait of James I (as he depicted it in his study of Bacon). As it makes Bacon hardly a narcissist, it fits only some kinds of narcissism, not all.

Here a myth system somehow enlivens Bacon's terminology and mitigates its vagueness. It explains how he could view discovery and induction as so natural and simple yet so difficult and obstructed by habitual intellectual faults (hubris and contumely). It suggests that he took seriously and literally his magical idea of speculations as the torture[9] of Nature. His preoccupation with myths explains

[9] Bacon advocated experiments as the torture of witnesses, much to the annoyance of Macaulay, who observed that England had then already abandoned torture. He took Bacon too seriously and yet he did not notice that the witness is not the feminine Mother Nature but a masculine Thing or even its (somehow) masculine Nature. Do not ask why.

the failure of attempts—especially those of valiant Ellis—to find out clearly what he meant by induction. For, such efforts belonged to the world of science not of myth.

We should take this with a grain of salt. Making sense of Bacon's mediaeval muddles is possible by interpreting them to mean that even the greatest geniuses must devote themselves to Nature if they want to be genuine researchers. The converse of this is very dangerous: ill success is a sign of lack of sufficient devotion to Nature and even ill-treatment of Her. Not so: the very division of the intellectual sphere to two distinct domains, the mythical and the scientific, is itself a part of Bacon's myth of the pure mind. Overriding it, we may observe that efforts of scholars to interpret the *Novum Organum* rested—and still rest—on the myth that they share with Bacon, the myth that Nature will certainly disclose Her secrets to those who woo Her kindly. This is no less than the very myth of the induction-making machine that most philosophers of science still half-believe in today, when new papers and new books about inductive logic so-called appear steadily as if they roll off a conveyor belt.

This is not surprising. Bacon's idea has a great appeal. As Popper has observed, it is a version of science-worship, and this has an authoritarian element in it. It seems rational, as it is anti-authoritarian: it is the idea that we must reject all authority because we must submit to the authority of science and of it alone. Not in vain do most critics of Popper's methodology direct their barbs against his view that research rests on the agreement. He suggests that the agreement is to partake in research and to submit voluntarily to its rules. They follow Bacon and deem research as natural as our bodily functions. Of course, they have to explain how it is then that research is limited to western culture alone. This question came up forcefully in the writings of Montesquieu and others, and crystallized in the writings of Lewis Henry Morgan and Edward Burnett Tylor and others down to Joseph Needham. Yet the fact remains: inductivists cannot but find fault with people who possess such a wonderful apparatus as the induction-making machine and they do not employ it properly. Putting the blame on them is not enough: we have to see how this happens. The theory as to how this happens is Bacon's doctrine of prejudice.

Bacon's myths that present sin as hindering progress may be fictional or allegorical. Their appearance in the *Novum Organum* may be mere embellishment or substantial. In that book Bacon blamed William Gilbert once for having gone too much into magnetic details and once for his having generalized too much—"until he himself became a magnet" as he "erected a whole world on the needle". These expressions, obviously meant as comic relief, are no substitute for a serious discussion of criteria: what is the exact measure of detail and how far can one generalize? At the very least one should take Bacon's censure of Gilbert with a grain of salt. Yet leading historians of science evidently took it too seriously and condemned Gilbert for his errors (Agassi 2008, 127).

Of the many accidental factors on which the posthumous fame of Bacon depended, the rise of Puritanism is first, and the success of Newtonian astronomy is second. R. F. Jones (1936) and Robert K. Merton (1938) claimed that Puritanism and Bacon's philosophy shared values. The basic feature that they share, let me spell it out, is the requirements apply strict discipline, to spend every free moment usefully, and to keep the mind in as total a purity as is humanly possible; mental impurity is both error and sin; reason and virtue thus unite (not in the way of rational self-interest but) in the way of stern morality. Not only pious puritans held this doctrine; even the great atheist Laplace did (Laplace 1809, conclusion). Similarly, young Faraday wondered how it is at all possible that the arrogant and dishonest find truths of nature. He did not deem it surprising that the humble and the devoted are capable of finding truths: Nature whispers Her secret to the worthy, patient investigator. This, according to all inductivists, is quite as it should be. Humility and devotion to truth are the condition for insight. What surprised Faraday was that Nature disclosed Her truths to the unworthy. That the worthy do not find truths everyday he meekly accepted: patience and devotion are necessary for the successful search for truth (Agassi 1971, 24). This myth is (very regrettably) false. Of course, we do prefer our researchers to be humane rather than beastly. But to say that inhuman individuals cannot contribute to science would only justify the brutality and prejudices of those whose prejudices, cowardice and brutality did not prevent them from partaking in the process of discovery.

Chapter 6
Prejudices of the Senses

The problem that troubled most of philosophers of science today, after the rise of the new logic, is that of observation. Since science discusses not observations but observation reports, this raises the question, how do we verbalize what we observe? This question is very troublesome. It invites scientific theories of observation and of language[1] yet as it is at the basis of science it should precede science (Popper 1935, §25).

What do people see? More precisely, what do people see when they have no ideas? Why is the second version preferable? It looks suspicious, as it raises the question, can people have no ideas? Since other animals cannot articulate, we may ask, perhaps, how do they see facts? How do they see the world? We do not know. This question becomes too abstract and too remote from traditional philosophy of science, so that here we may ignore it. We may then take an item that is standard in the physical sciences since 1917: the photographic plate. The simplest way to deceive the scientific community is to mislabel a photograph of some experiment. We may dismiss this kind of example too, and again as too abstract. Our disposition towards naïve realism is very strong, and it tells us to ignore this problem altogether. To bring the problem home, so as to prevent its dismissal as too abstract, suffice it to observe that the *Encyclopedia Britannica*, first edition (1768–1771), endorses the phlogiston theory, viewing it as a successful set of pure observations-reports—ones free of theory. This portrays the theory of phlogiston much too favorably. The current edition of the same encyclopedia ignores all this; it portrays the same theory much too unfavorably.

To learn from this story, we may wish to be cautious and not take for granted as quite unproblematic any observation that looks to us as plain as our noses.

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[1] The claim that observation reports are unproblematic combines the view that some observation reports are theory-free, that they are verifiable, and that the ideal language shares a structure with the world—the picture theory of language so-called. All this is too naïve for words.

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_6,

6.1 The Problem of Observation

We hardly doubt the commonsense view that the world of experiences (as Kant has called it) comprises theory and factual information combined. How they combine is not obvious. The problems of methodology concern the combination of thought and observation, namely, the accord between theory and experiment. Since theories are questionable, they undergo tests by observations. Galileo said, a test is no good if it is guaranteed to defend the theory come what may: "So no one can never win against you, but must always lose; then it would be better not to play" (Galileo 1953, 439). Now the theoretical part of any observation may bias it sufficiently to prevent it from refuting the theory under test, so that the result of the test is then assured. This is an important idea that Bacon has discovered: all observations are theory laden. He declared then all tests of theories sham.

Naïve realists cannot dismiss this any more than "those who travel along a street by night being followed by the moon, with steps equal to theirs, when they see it go gliding along the eaves of the roofs. There it looks to them just as would a cat really running along the tiles and putting them behind it" (Galileo 1953, 256). Bacon noticed the same fact and noted the difficulty involved in this. The question that he addressed has a general (and ancient) variant: does the process of (scientific) learning start by observing or by reasoning and arguing? Although starting from scratch is impossible, it is nevertheless possible to modify the question to fit any stage of research (within science). It is possible to compare, say, science in 1900 with science in 2000, or in 1800, and find that (as science was progressing between these stages) the later stages witnessed both new theories and new information. How does this progress happen? It is perfectly possible to imagine that at some point a researcher found a new fact quite independently of the theories previously known, and that this new discovery stands behind a new theory—possibly the new information has prompted the development of the new theory. This prompting may have been the rise of the theory out of the extant information (as inductivists say) and possibly researchers tried to invent new theories to explain that information (as deductivists say). Swift illustrated and Ellis affirmed the observation that the inductive process is most unlikely (and, strictly speaking, quite impossible; but never mind this). Presumably, then, our newly discovered information (normally) is in some way related to theory. The problem that this situation raises is this: what are the logical relations between the known theory and the newly discovered information?

Bacon's answer is that the new information always confirms some old theory, simply because people always look for confirmations. Thus, they always rescue any theory and reconcile it with any item of information that it seems to endanger. The scholastics did so regularly, to Bacon's repeated complaint and expressions of derision.

Making hypotheses, then, researchers will always confirm them empirically, making the belief in false ideas hard to jettison. This involves another danger, and a bigger one, the danger that we shall see facts wrongly, that we shall observe them in a distorted way turning them into confirmations of our false hypotheses. This makes it not only harder but also impossible to refute theories. This is a great discovery that probably Galileo and Bacon made independently. These days philosophers ascribe it to Duhem and to Otto Neurath, and with some justice, since their version of the discovery is newer: it integrates well with their philosophy of science according to which scientific truths are true by convention and not by nature (conventionalism).[2] Here is what a modern conventionalist, the highly and rightly praised (Cohen 1994, 112) Herbert Butterfield, said about it that is impressive in its detailed explanation (Butterfield 1949, 4–5):

> … it is not relevant for us to argue that if the Aristotelians had merely watched the more carefully they would have changed their theory of inertia for the modern one. … It was supremely difficult to escape from the Aristotelian doctrine by merely observing things more closely… [since] the modern law of Inertia is not the thing you would discover by mere photographic methods of observation—it requires a different kind of thinking-cap, a transposition of the mind of the scientist himself. For we do not actually see ordinary objects continuing their rectilinear motion in that kind of empty space which Aristotle said could not exist and sailing away to that infinity which also he said could not possibly exist. And we do not in real life have perfectly spherical balls moving on perfectly smooth horizontal planes; the trick lay in the fact that it occurred to Galileo to imagine these. Furthermore, even when men were coming extraordinarily near to what we should call the truth about local motion, they did not clinch the matter—the thing did not come out clear and clean—until they had realized and had made completely conscious to themselves the fact that they were in reality transposing the question into a different realm—they were discussing not real bodies as we actually observe them in the real world, but geometrical bodies moving in a world without resistance and without gravity—moving in the boundless emptiness of Euclidean space which Aristotle had regarded unthinkable.

Butterfield echoed Duhem's attack on Bacon's claim (*Works*, 3, 229),

> … if some of the ancient philosophers had been perfect in the observation of astronomy … they would never have gone to invent the theory of the spheres.

[2] It is no news that for decades now much of the energy invested in the philosophy of science goes to the erroneous suggestion that the possibility of tinkering is novel and thanks to two great thinkers, Pierre Duhem and Willard Van Quine. The same goes for the erroneous suggestion that the claim that scientific theories are refutable is novel and thanks to Popper and answerable by conventionalism, that Lakatos has shown Popper in error since science regularly evades criticism and rescues any theory that is under attack until and unless a new theory comes to replace it. This rider is a hackneyed idea; Lenin branded it the demand for constructive criticism. Allegedly, it is novel and thanks to two thinkers, Thomas Kuhn and Paul Feyerabend. To see how astute Bacon was, we may notice that he took all this as too obvious yet still in need of airing since it is more harmful than people realize. And he denounced current science as its theories are refutable; Popper was far from having originated this idea. One of the first people who have saddled him with this alleged innovation is the famous physicist-sociologist of science John Ziman who, in his 1959 review of Popper's *The Logic of Scientific Discovery* said, he could not understand the fuss Popper was making about refutability, since "it sticks out like a sore thumb". It does. To answer Ziman, what is new in Popper is the idea that refutability is a virtue, and that it suffices as a characterization of science: refutable explanations are scientific. Moreover, this solves the problem of induction: it explains how we gain theoretical knowledge from information. It never ceases to surprise me that learned inductivist commentators dismiss this idea as wild (see next chapter). Yet it surely is more surprising that inductivist commentators (such as Carl G. Hempel) rest their view of induction on the availability of theory-free data and endorse the demand for constructive criticism.

Butterfield did not defend Duhem, or any other followers of Aristotle. He suggested that improved observations would not help one escape from the system of Aristotle's astronomy, because inbuilt into that system is a ready-made treatment for any possible refutation of it: add another epicycle to take care of the refuting information. Bacon had already discovered that the followers of Aristotle who pretended to save the phenomena (the appearances) this way were saving their theories. Orthodox conventionalists see nothing wrong with this method. Why then do they prefer any theory to any of its competitors when all competitors are in possible agreement with the available information? Their answer is that it is convenient to hold these views rather than the others but that the facts provide us with no final reason for any preference: choice is always open.

Butterfield's conventionalism does not go so far, and it is therefore more open than Duhem's conventionalism to the following criticism (that I shall soon answer). Like Duhem, he viewed mediaeval astronomy as irrefutable, and unlike Duhem, he considered it a prejudice, a theory that impedes research. He considered Galileo's mechanics equally irrefutable, or else friction and air resistance would refute it. Why then did the Moderns (as Bacon called them) reject mediaeval astronomy? With Copernicus, they all emphasized their dissatisfaction with that theory and its method of explaining away one irregularity after another by introducing ever newer epicycles arbitrarily or, to use the technical term for it, *ad hoc*. In the face of historical facts Butterfield alleged that the mediaeval thinkers and Galileo had shared a method. He was in error. The deviations of the facts from Aristotle's astronomy were left unexplained whereas similar deviations from Galileo's theory were calculated with the aid of Newton's theory of viscosity (and with later and better theories). This would hardly convince conventionalists, who refuse to see a methodological difference between adding an epicycle and adding a friction factor.

Why did Butterfield oppose Aristotle? This is not clear to me. As a conventionalist, the most he said against Aristotle's theory is that it is somehow less convenient than Galileo's. The following is a possible argument for Butterfield's views. Conventionalists recommend rescuing from refutation only convenient theories—or rather, the most convenient theory available. Most conventionalists would agree: the conventional method of introducing *ad hoc* increasing numbers of auxiliary assumptions (or new parameters) will eventually make the theory too complex and thus very inconvenient, as was the case with mediaeval astronomy. (Poincaré's contention that his adoption of Euclidean geometry is only a matter of convenience was the break from the dogmatism of the nineteenth century in spite of his having held at the time that this geometry will always remain the most convenient: after Einstein he change his mind.) The question then is, when is the patching-job so excessive that a new framework is timely? Michael Polanyi's and Thomas S. Kuhn answered this question, saying, this is up to the scientific leadership to decide upon (Fuller 2000, 71, 231). In this vein, Butterfield could join them and say, around the year 1600 the need arose for a new system, Copernicus and Kepler offered the heliocentric system to that end, and it soon became the received scientific choice.

The motive behind the scientific revolution was not scientific. Copernicus and Kepler were Platonist sun-worshippers (Burtt 1924, Ch. 2). Kepler and Galileo advocated the use of mathematics for similar reasons. The Catholic leadership granted that the Copernican hypothesis was convenient, but not that it is true. Galileo fought against this and demanded from the Church the freedom for everyone to trust their own judgment and declare or not declare it true as they find fit.

In the face of all this Butterfield could still insist that Aristotelian prejudices were hindering research in astronomy: the scholastics had saved the old system in spite of its inconvenience. The scholastic adherence to Aristotle matters no less and no more than the Modern adherence to Plato: the scientific criterion of convenience is above personal inclinations and free of it. Galileo stated his (and Bacon's) great discovery: theories are not personal matters; they prescribe ways of seeing things. Let me repeat: without a theory, Galileo observed, strolling down the street on a moonlit night one sees the moon jump from rooftop to rooftop like a cat.

Bacon's view on all this is not as vivid as that of Galileo: he was no observer and had little feel for observation, scientific or other. His argument was abstract. He realized that the dependence of observations on theory, whatever it is, undermines their authority and opens the door to skepticism or dogmatism. This, he said, is how the prejudices of the senses turn researchers inept or dogmatic and hinder scientific growth. Once people understand this fact, he had hoped, they will exhibit sufficient good will to relinquish all of their ideas and all of their preconceived notions. They will then be able to see things as they are. And then science will advance "in buckets and vessels immediately where it springeth" (*Advancement*). The possibility of scientific growth Bacon-style thus admittedly depends on the possibility of utterly theory-free observation reports—on the possibility of absolutely raw data, to use current jargon. In the twentieth-century, inductivist philosophers of science invested a large portion of their efforts into the search for these absolutely raw data. In 1935, Popper proved this impossible (Popper 1935, §25), but to no avail (Creath 2012, *passim*). Consequently, since that time all search for raw data bespeaks a prejudice.

Popper's argument is from the fact that all observation reports are within some language, and that language is an imperfect artifact. He went further and claimed that all nouns are dispositional: when we use a noun, we recognize that the object it designates has certain dispositions: they have some properties that they exhibit when properly coaxed. For example, when we report having observed a glass of water we use the word "glass" thus referring to an object that is easily breakable. If we find that this is not the case we may suspect that it is not made of glass. Even "broken" and "not broken" are dispositional, said Popper, since we do check claims that some objects are or are not broken.

6.2 Prejudices of the Senses

> The inquisition of this subject in our way (which is by induction) is wonderful hard; for the things that are reported are full of fables; and new experiments can hardly be made with extreme caution for the reason which we will thereafter declare.
>
> *(Bacon, Works, 2, 564)*

The history of science is full of cases of prejudices of the senses. Each confirms the doctrine of prejudice, boosts conventionalism, and proves vain Bacon's hope that careful observations would eliminate them (as Butterfield has declared). Since Bacon's advice is impossible, it could not bring about modern science. His observation of an obstacle is right and his suggestion as to how to remove that obstacle is not. We are stuck: how was the revolution in science at all possible? Since wild guesses impede scientific progress and since getting rid of them proves impossible, is it possible to make them work for science instead of against it? If so, how?

William Whewell answered these questions (mainly, he reported, under the influence if Immanuel Kant). His starting point was his criticism of Bacon's inductivism (he considered himself an inductivist, but not a Baconian): Kant's chief thesis was that we see not with our eyes alone but with our eyes and brains combined. Today, when it is very well known that certain sorts of brain damage may incur certain kinds of blindness, this is no longer contested. (Inductivist philosophers of science in search of raw data still ignore these facts; they deny them tacitly and unawares.) This was not new to Bacon. He had hoped to overcome this difficulty; Whewell declared this hope vain. Bacon said, to overcome this difficulty one must avoid proposing any theory whatsoever. This solution is inadequate, but the problem that it comes to solve is genuine: we do indeed interpret all our observations in the light of our theories. And, if these theories are false, they may mislead us at every moment that we try to learn from observations. Theories stand, as it were, between us and the world. True, they act as useful interpreters, but they are quite unreliable all the same.[3]

Interpretations of observations appear as the tendency to see things as anticipated. Observers focus on significant facts, as theories decide what signifies. Observers avoid seeing things supposedly unrelated to the theories that observers entertain. Entertaining different views on the world leads to seeing the world differently. (Just think of the testimonies that led to executions on the charge of observed practices of witchcraft. Bacon testified to having seen effective magic acts.) Hence, no observation can impose a change of opinion. Researchers may then belong to different schools, each school advocating different views and observing the world differently. Their pictures of the world would then be merely imagined dramas, Idols of the Theatre, as Bacon named them. This feature of the situation is the one he understandably disliked most, requiring science to have no schools.

Now if the problem of observation is insoluble, as Whewell and the conventionalists have conceded, then it is nothing short of a miracle that researchers so often agree among themselves.[4] The prevalence of agreement about observation that

[3] Of course, a true theory does not mislead. That a mere conjecture will turn out to be true is most unlikely, said Bacon in response to this. Still, true observations are theory-laden too. They thus reflect the true theory. This is why it is possible to squeeze the true theories out of (sufficiently many and sufficiently varied) true observation reports—like wine out of ripe grapes (*Novum Organum*, 123): just avoid making mistakes and the truth will reveal itself to you.

[4] Thomas S. Kuhn found unanimity explicable only by the hypothesis that scientific communities oust dissenters (Kuhn 2000, 209). Instances for and against this hypothesis abound. Yet the cure he refers to is ineffective. Banishment is ineffective even as a silencer of religious disputes; and in politics even the death penalty does not quench dissent.

scientists exhibit convinces many philosophers that the problem must be soluble. It is a fundamental problem, and one that methodologists admit that they must solve. The inability of both inductivism and conventionalism to solve it reveals the weaknesses of both of these schools. Yet thins go on as usual. Inductivists cling to their refuted theory that all scientific observations are true. When they bump into a false scientific observation, they declare it unscientific. Asked for explanation they say, someone was prejudiced. Told that the error was prevalent they express indignation.

The conventionalist methodology (Duhem, Neurath) is friendlier: its adherents admit and even emphasize that observation involves the imagination even in the simplest cases. Still, they fail to explain the scientific agreement about observations—even among researchers from different schools of thought. True, as Bacon had observed, agreement about observations is no guarantee, as frequent refinements of observations prove. The question still is, how does science reach agreement on observations? Bacon said, one metaphysician charms all his colleagues and they all grant him their consent and then science gets stuck in the mud. This explanation is highly unsatisfactory since science attains agreement across scientific schools.

Take experimental research in its highest stages of development as it operates in fields where researchers are not yet intuitively at home. A photographic plate of cosmic rays, for example, will not appear to one who knows nothing of modern physics as traces of elementary particles or as their cascade showers or collisions. For, the plate displays but a mixture of grey lines and dots on a dark background. To learn to see elementary particles, one has to learn theories and techniques of calculations and be informed about the conditions under which the events that photograph displays occurred: students of such photos take upon faith the reports about these conditions.[5] Thus, far from being recondite, the methodological problem of observation is concrete and difficult. This offers a hint at Bacon's enormous influence on his followers. For, although he reached his solution of the problem in a different manner, considerations such as the example of the photographic plate suggest that they have persuaded generations of his followers.

Here is a quotation from Charles Babbage (the inventor of the first calculating machine). The passage comes after an attack on the view (due to John Locke) that better observations are results of a higher sensitivity of the sense organs. True to the doctrine of prejudice that he ardently endorsed, Babbage could not state that observations depend on theories. His presentation of the problem of observation is by narrating the story of his having learned from Herschel to observe spectral lines (Babbage 1830, Conclusion): Herschel said,

> "I will prepare the apparatus, and put you in such a position that they [the spectral lines] shall be visible, and yet you shall look for them and not find them: after which, while you remain in the same position, I will instruct you how to see them and you shall see them, and not merely wonder you did not see them before, but you shall find it impossible to look at the spectrum without seeing them" … the prediction of Mr. Herschel was completely fulfilled.

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[5] The observation that it is so easy to cheat seems to suggest that trust is not itself under scrutiny. This is an error: fraud is usually found out fairly quickly. This argument is puzzling as it looks circular. It is not: it is bootstrap operation (Agassi 1975, 155).

I do not understand this. I have never heard of an example of students facing the same problem as Babbage, perhaps because spectroscopic images are these days so much clearer and photos of them appear in standard physics textbooks. What this passage of Babbage clearly indicates to me is that the problem of observation troubled him so much that he was not even clear how his narrative related to the received opinion (that was Bacon's, of course). Hence, it would be too unfriendly to criticize the Baconian tradition to which Babbage belonged. Bacon's theory became a dogma only after Einstein, and a rather innocent one at that: the search for raw data still proceeds because too many researchers take the assumption behind it for granted.

Thus, the tradition was once rational and is no longer so. This is hard to digest. It is easy to see that the production of an expensive commodity ceases to be rational when demand for it stops. The case of adherence to a view being rational and then ceasing to be so is quite different. It goes against the traditional theory of rationality as proof: unlike the demand for a commodity, the proof of a proposition is forever. But, proof in science is impossible. The commonsense idea of reasonableness should take its place, as it serves the present discussion well enough. Bacon rightly criticized the academic a tradition as scholastic, as encouraging apologists who cared more about appearing right than about being right, who cared more about reputations than about truth. He realized then that they defended their views by appeal to observations, but that their observations were biased. He wanted to replace their method with one that still argues from facts but avoids all bias, with one that avoids all prejudices of the sense. All this is most impressive. It turned out however that even if we deem Bacon's problem still unsolved and even though we do not know if science can avoid all prejudice of the senses, science still progresses, at least by the elimination of some past errors.

The problem that Bacon discussed is not quite new, but his discussion of it is. Democritus composed a little dialogue between the intellect and the senses, where each side shows the other fallible. As Ellis has found, Bacon followed Bernardino Telesio in asserting that people can see facts as they are. Bacon then asked, we remember, how then did the ancients commit observational errors? He explained all past failures to observe correctly by his doctrine of prejudice. This qualifies (Telesio's and) his theory of observation *ad hoc*: people can see things as they are, but only on condition that they are well tuned. This is the history of Bacon's doctrine of prejudice of the senses. This doctrine is the result of rescuing an axiom (proper observations reveal facts as they are) by "a frivolous distinction; whereas the true course would be to correct the axiom itself" (*Novum Organum*, 1, Aph. 25). Bacon took it for granted (as did Gilbert and Galileo) that nothing would convince rigid Aristotelians. He said, ancient prejudices would not have arisen had the ancients avoided speculation. As Aristotle was prejudiced, he could not observe properly. Hence, his observations are less than useless (*Novum Organum*, 1, Aph. 63):

> Nor may any weight be given to the fact, that in his books … there is frequent dealing with experiment. For he came to conclusion before; he did not consult experiment as he should have done … but having first determined the question according to his will, he resorts to experience, and bending her into conformity with his placets, leads her like a captive in procession. So that even on this count he is more guilty than his modern followers, the schoolmen, who have abandoned experience altogether.

To quote Butterfield (32),

> It has been pointed out concerning some writers of the sixteenth century that though they talked of the importance of seeing things with one's own eyes, they still could not observe a tree or a scene in nature without noticing just those things which the classical writers had taught them to look for.

This is no longer so. Why? Why do we not go on seeing trees as the ancients did? This question raises a more fundamental one: how come people see new facts? Researchers report new facts and thereby improve the way of looking at things. This is no guarantee as yet that they are right, that the facts they report are indeed new. It is easy to say that having observed a new planet we hardly doubt its novelty after an examination of an up-to-date catalogue. But even this case is not so simple. The observation of the sky is not considered new because of the theory that after sunset the sky revolves beneath the earth to reappear the next morning. With no theory, there is no argument against Heraclitus's contention that every day we see a new sun. (This point Hume rediscovered millennia later, this time in reliance on Locke's theory of perception.) Hence, the identification of today's sky with yesterday's sky rests on no observation and so it is a prejudice of the senses. Perhaps observations allow the conclusion that the sun of today is similar to the sun of yesterday. Is this still true after the discovery of the sunspots?

To take a more troublesome problem, let me return to the cosmic-ray photographic plates. One must have knowledge of physics, even advanced knowledge, to be able to see whether or not it is a record of tracks of some new particles. The question whether some particles are new or not is open.[6] Observing and identifying any faint star demands knowledge of a different kind. These examples reflect the methodological need to characterize novelty, to give a criterion for discovery.

6.3 Bacon's Theory of Discovery

Current methodological literature[7] almost[8] totally avoids characterizing the novelty of observations that Bacon discussed in great detail. His theory is vague; as usual it reflects his acute sense and keen interest in discovery, for which he deserves high credit as a leading philosopher of science. Here is what Bacon said of the novelty of an observation and of discovery. He began with examples (*Novum Organum*, 1, Aph. 109): before the discovery of explosives, one could not imagine "a new invention

[6] The solution of the tau-theta paradox by declaring the two particles identical is a point in case.

[7] The psychological literature on novelty is also oddly scarce. Daniel Berlyne is possibly the leading experimental psychologist who explored this area seriously (Berlyne 1960).

[8] The exception is my discussions of novelty (Agassi 1975, 51). Perhaps some essays by Imre Lakatos and his groupies deserve mention here (Motterlini 1999, 109); perhaps they are better ignored, as they ignorantly echo Bacon's texts in their discussions of some odd confirmation theory.

by means of which the strongest towers and walls could be shaken and thrown down to a great distance": anyone told about it would try in vain to imagine how older means might do this. Similarly, told about silk one would try to imagine it by reference to familiar means; "if anyone had said anything about a tiny worm, he would no doubt have been laughed at". Finally, information about the compass before its discovery "would have been judged altogether incredible."

Bacon's examples are very impressive indeed but the idea they represent is possibly quite limited. (This is no criticism, as Bacon did not claim that his criterion was exhaustive.) In efforts to examine it, we added some modern examples. The discoveries of subatomic particles are obvious ones. Now Bacon's idea holds well for the discoveries of the electron and the muon: peers did find reports of them incredible. The same may hold for anti-matter, but hardly for the rest of the elementary particles: they hardly met with incredulity. Likewise, it may hold for galaxies, but not for black holes: much as they were obviously new, their discovery was theoretical, and so observation reports about them met with delight. (Earlier, astronomers regarded them as mere curiosities.)

Bacon's lovely examples do not suffice for decision on these new cases. The end of his discussion may be more helpful:

> Yet these things … lay so many ages … concealed from men, nor was it by philosophy or the rational arts that they were found out at last, but accident and occasion; being indeed … altogether different from anything that was known before, so that no preconceived notion could possibly have led to the discovery of them.

This criterion is decidedly partial: Bacon had the "preconceived notion" that the philosopher's stone exists and awaits discovery, yet he would deem it a novelty. Likewise, assuming that unicorns belong to fables, we will deem a striking novelty observing one tomorrow in the local zoo. To diffuse possible puzzlement let me clarify. Bacon offered here two versions of his theory of discovery: one is that novelty is surprise coupled with the inability to imagine; the other is that novelty is any surprise. Perhaps he meant novelty is surprise and the inability to imagine is a sure criterion for surprise. This is important, since he taught that surprise comes accidentally and this is a very important idea. It is definitely broad: for all we know people may hunt for unicorns and they may bump into one; Bacon proscribed hunting for unicorns and prescribed instead hunting for whatever may come our way. This is how Bacon's theory of discovery fits his doctrine of prejudice.

Let us consider Bacon's theory of discovery at its face value. Is his identification of surprise with accident right? Doubts about it are easy to raise. Thus, it allows for no distinction between discovery and invention, as the aphorism following the above quoted one indicates. That aphorism concerns the invention of printing which is indeed a great surprise, so that both discovery and invention are surprising, yet we do suppose that discovery is more novel than invention. Yet Bacon clearly never intended to distinguish between the two.[9] Nor could he allow for discoveries that

[9] Discoveries that are also inventions, like Faraday's dynamo, are more surprising than ingenious inventions like Edison's reduction of the hysteresis in the dynamo.

rested on "preconceived notions" like Gilbert's discovery of the magnetic poles of the earth without having visited them. Not that Bacon was a utilitarian; he shared the Renaissance enthusiasm about the new horizons that new actions and new science were opening. There hardly was then any understanding of the situation. Bacon's contention that knowledge is almost identical with power (*Novum Organum*, 2, Aph. 4) is an expression of a dream, not of a theory.[10] Bacon is admirable for this imaginative dream, even though it was a dream of a science divorced from all dream and resting on hard observed facts, on facts as they really are. Bacon's discussion of the effects of a discovery is theoretical, not empirical.[11] The empty mind, if it ever exists, will observe no discovery of a new fact since it will have to consider every fact new.[12] Bacon's discussion of allegedly raw observations in his histories of nature are always involved in theories—even in the trivial sense—and in ones that were overthrown in his own time or slightly later. Bacon's own examples of discoveries (of glowing wood and deformed bodies) indicate that his idea of discovery as surprise is mediaeval. When Galileo argued against the search for oddities, saying simple repeatable facts of Nature are marvelous enough, Bacon still found oddities hints that Nature offers Her suitors. Most of his commentators ignore in embarrassment this part of his *Novum Organum* that takes up most of its Second Book. Others dismiss it all as superstitious. We need here a more historical approach.

Looking at the situation more broadly, what is so painfully missing in this discussion is the social sciences, including the social impact of the technology that Bacon so looked forward to. He was a conservative and he could not imagine that his philosophy would create the radical social sciences of the Age of Reason, that it would render first radicalism and then liberalism the hallmark of western civilization. And he did not imagine the impact of his theory of perception on abstract social science and on practical politics. But we must move on.

Perhaps I am exaggerating. Perhaps Bacon considered it obvious, as we all do, that novelty strikes us by its being odd or by its being interesting. After all, his criterion of novelty does not help research. He was the first propagandist for science; now they are many and they often and vociferously declare many facts to be great novelties, soon to forget them silently in recognition of their being slight variations of other well-known facts. This happened already in the first half of the nineteenth century. One of the most attractive research interests then was the search for new sources of electricity. Advocates of science heralded the discovery of electrification

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[10] Contemporary utilitarian philosophers are apt to judge all action by their usefulness; in the Renaissance the ability to act and expand the horizons was valued in itself. Going for novelty distinguishes it from the Middle Ages. The symbol of the era was thus the useless cupola of the Florentine Basilica di Santa Maria del Fiore that Brunelleschi constructed using ancient procedures. Is this an anti-utilitarian attitude?

[11] Gerolamo Cardano singled out as important novelties gunpowder, the needle and printing; Bacon followed him.

[12] The idea that empiricism makes every experience new is the criticism that Jorge Luis Borges has launched against it in the form of his famous story "Funes the Memorious".

by steam as very important. It was soon ignored when Faraday showed that it was only ordinary friction electricity. The opposite kind of assessment occurs too. When Heinrich Hertz discovered that ultra-violet light facilitates the formation of electric sparks he assumed it was of no importance, since it was a special case of resonance.[13] He briefly reported on it (1887), and, it seems, forgot all about it. Even consequent research on it did not decide against Hertz's judgment, until in 1905 Einstein showed how new it (the photoelectric effect) really was. Now this evaluation rests on Einstein's new theory. Should its refutation impose on us a reevaluation of the effect? Is this not too divorced from the historical approach? If so, what is the way to assess historically commentators with no sense of history?

It transpires—both intuitively and methodologically—that Bacon was right in his intention of characterizing the novelty of discoveries as something not to be judged by doubtful hypotheses but he was in error in his effort to characterize novelty by an appeal to facts alone. Wishing to avoid dependence of assessment on doubtful hypotheses, finding all hypotheses doubtful, and finding hypothesis essential for assessment, we are stuck. Here, it seems, we have arrived at a complete deadlock, and only because rather than use our healthy intuitions we seek a criterion to judge novelty. Researchers repeatedly discover new and even most surprisingly new facts. The criterion for the novelty of these facts is connected to the attempt to explain how they make these new discoveries. Now starting by making new experiments independently of past theories (as Bacon has recommended) is impossible. Experiments that received theories imply, however, rest on prejudice. Hence, there seems to be no way out. Two marginal escapes from this deadlock present themselves. The one is Bacon's view; the other is due to Whewell and Duhem. The first escape is the idea that discovery is accidental. The second is that new theories may yield deductively new factual assertions and that therefore as long as researchers go on inventing new theories and testing them, they may go on discovering new facts.

It seems quite clear that even without endorsing Bacon's theory of the cleansed mind, it is still possible to endorse his[14] conclusion from it, his theory of accidental discovery that is, oddly, the most popular view of discovery among researchers and methodologists, not to mention historians of science. Let us then look for its strength.

Bacon also explained how his predecessors were able to make discoveries of new facts: they did it by mere luck, by accident. (He also claimed that artisans had made most past discoveries, as they are less spoiled than scholars, being less prejudiced, having less access to theories. He offered this as an observation, but it is a mere

[13] Schrödinger viewed the Hertz effect as resonance too, but in a much wider sense.

[14] There are predecessors to Bacon's theory of accidental discovery, such as the ancient story that Pythagoras discovered the physical basis of acoustic harmony by accident while passing by a blacksmith workshop (that resembles the story that a physician prescribed frog legs to Galvani's wife) and of Archimedes Eureka. They all make inspiration humdrum. One may view this as debunking or as promising: discovery awaits the prepared mind, said many an important scientific leader, including Lagrange and Pasteur.

conclusion from his theory, and one that renders it refutable.[15]) His predecessors were groping in the dark and he lit a candle (*Novum Organum*, 1, Aph. 82). He even condemned them for their having waited for luck in order to make discoveries instead of looking for the (his) method that assures that discoveries stream in.

Thus, the word "accident" has two distinct though deeply linked meanings: one is happenstance and the other is independence of any theory. Any mind may happen to make a discovery here and there, Bacon admitted, but an empty mind will be inundated with discoveries—"not in treacles but in streams and buckets".

Bacon assumed that all genuine discoveries are accidental—unless they are aided by theories arrived at by induction, of course. For, once an observation is due to proper prediction, it is not surprising. In one sense, all genuine discoveries are accidental. In another sense, only past discoveries were accidental, being the products of polluted minds. This explains Bacon's terrible allegation that all past theories were fruitless, since he admitted that no attempt at proper induction ever took place yet. Future discoveries, however, are going to be systematic products of researchers who will be humble and pure of minds (*On the Interpretation of Nature*, Ch. 17; *Works*, 3, 247).

> … chance discovereth new inventions by one and one, but science by knots and clusters.

Moreover, these discoveries will lead to knowledge of the true axioms, and (*The Great Instauration*, "Plan of the Work")

> Axioms once rightly discovered will carry whole troops of works along with them, and produce them, not here and there one, but in clusters.

This is unadulterated epistemological optimism. It is catchy. I know of no science fiction author who imagined the exhaustion of the sources of discovery. People now tend to believe that discovery is a matter of scientific routine: that it is systematic (though still accidental). This is a serious error: there is no guarantee for discovery and there can be none. A theory may assist discovery by removing some obstacles on its way, but clearing the way is no guarantee that using it will lead to discovery. Moreover, there is no guarantee for the truth of that theory.

It is better not to take discovery for granted: it might choke the process altogether. For, past discoveries depended not only on luck but also on attitudes that are now lost. In particular, Bacon's hostility to academics, justified as it might have been,[16] is now pernicious anti-intellectualism. How do we find good problems to study? The attitude towards this question is usually ambivalent, especially among the scientific anti-intellectuals. They take pride in the success of science, including

[15] Today, when so much background knowledge is needed to identify discoveries, Bacon's observation that technicians will outdo researchers becomes barely thinkable; yet his complaint that discoveries are limited to the hypotheses that experts employ to generate them becomes all too obvious.

[16] As Bertrand Russell read scholastic literature in preparation for his *A History of Western Philosophy*, he was surprised that some of its texts impressed him favorably. This shows the power of Bacon's impact.

those new theories that may lead to predictions of new facts. They resent the idea that scientific progress is doubtful and so they cling to inductivism. They ignore the inductivist claim that factual discoveries are accidental and they suggest that somehow theories facilitate factual discovery and *vice versa*. This is a lovely idea; it deserves exploration. Its advocates seem aware of its fragility since, let me observe, they suppress discussion of it (Smith and Bender 2008, 245).

One of the most ardent inductivists of all times, Judge William Robert Grove, issued in 1842 a strange program according to which researchers should only record facts. Three years later he took it back—in a letter to a colleague (Agassi 1971, 75–7). He suggested there that facts had lost all value and he was waiting for a new theory to rectify matters:

> I think chemistry is being frittered away by the hair-splitting of the organic chemists: we have new compounds discovered which scarcely differ from the known ones and when discovered are valueless, very illustrations perhaps of their refinement in analysis, but very little aiding the progress of true science.

Contrary to the inductive program, Grove strongly felt that at the time some facts were very insignificant for the progress of true science, and some were hardly relevant, as the situation invited improvement by some new theories.

This claim is right: "a just history of nature" is useless for a science that is after the discovery of new facts: these must have some characteristics that make novelty context-dependent—dependent on some theory. Bacon's doctrine of discovery is unsatisfactory; he had in mind something different when he urged people to discover new facts. What are these?

6.4 Whewell's Theory of Discovery

Surprisingly, the Bacon literature ignores Bacon's proposal for a criterion for the novelty of a discovery despite its astuteness. Even those profound thinkers who rightly attacked Bacon's view that we can ever make a purely accidental discovery, especially Ellis, Liebig, and Duhem, did not try to offer an alternative criterion for the novelty of a discovery. Possibly the task was too difficult for them, and possibly they overlooked it. This may be true for Ellis and for Liebig, not for Duhem since in the meanwhile Whewell had developed an alternative to Bacon's view and so we may judge Duhem's oversight evasive. Perhaps he noticed that his theory of science could hardly allow for such a criterion.

The need for a (reasonably satisfactory) criterion for the novelty of an observation, or for the novelty of a discovery, is as obvious as the need for such a criterion for the novelty of an invention. And all modern societies, as well as the international community, have (adequate or inadequate) criteria for invention (that diverse patent offices and law courts employ). The criterion for novelty in science (discovery) and in technology (invention) must differ: having different aims, technology and science musts differ no matter how much they overlap. Hence, (technological) invention is not the same as (scientific) discovery—even though many a discovery is also an invention. (The paradigm case, we remember, is Thomas Edison's discovery of the thermionic effect that he registered as a patent application.) We may further specify the question of novelty and try to ascribe degrees of importance to discoveries. (It need not be numerical: it may be comparative and partial.) That some discoveries are more important than others is hardly controversial. Otherwise, histories of science would never be what they are: the longer ones report on more discoveries than the shorter ones, and this offers a ready-made partial order of their import. Grading inventions by their significance, incidentally, is impossible, since it depends on the significance of the goals that they serve and of the value of such services. The paradigm case is barbed wire, the minor invention that made history (Liu 2009).

Some pages ago we were stuck in a trilemma. Observations that rest on known theories are in an obvious sense not new; those that rest on conjectures are as unreliable as these conjectures (these are the prejudices of the senses); there remains the option that Bacon advocated: new observations independent of any theory. Those who discarded this option, it seems, could not possibly offer a new one within the same narrow framework. Thus, some historians of science, Andrew Norman Meldrum and Thomas S. Kuhn in particular, suggested that the search for a logical criterion of novelty should give way to a search for a psychological characterization of novelty, and the psychologist Daniel Berlyne offered some empirical studies in this direction (Berlyne 1960). This proposal is quite unsatisfactory (although Berlyne's study is valuable), especially since psychology is conjectural too, and usually less well articulated than physics.

Enter William Whewell. He approached this situation (in the middle of nineteenth century) from a very different angle. The event that was most important in his intellectual life was the overthrow of Newton's optics. This event shattered Laplace, for example: it seemed to him a threat to the very future of science. Whewell came to the rescue. Particle optics was never tested, let alone confirmed, the way mechanics was, so that there was no danger that mechanics might suffer the fate of particle optics.

What the overthrow of particle optics also showed was that it amounts to the overthrow of Bacon's doctrine of prejudice, and at the same time. Particle optics had all the qualifications for being a prejudice Bacon-style, and even abundantly so, yet it was abandoned. This is Whewell's astute observation. Herschel, who was involved in the process more than Whewell, censured bitterly those who resisted the new wave optics. It would have been better had he criticize the admirable Bacon instead (Agassi 1981, 391), but he was too Baconian to be simultaneously critical and appreciative: he could not accomplish this feat. This is sad. The opposition to the shift from particle to wave optics was surprisingly smooth, at least in comparison to the shift from the phlogistonism to anti-phlogistonism of but a few decades earlier (Agassi 2008, 166). So we may suspect that Herschel's ire was somewhat disingenuous.

Without Bacon's doctrine, we may ascribe novelty to any information that does not follow from received theory but follows from a new hypothesis that implies it. Of course, most hypotheses that occur to us are likely to be false, as Bacon wisely observed, and so the new predictions that rest on them will meet with refutations and exit science unceremoniously. Whewell was the first[17] methodologist to offer a criterion for proper confirmation. This is scarcely credible, since Bacon already condemned so loudly spurious confirmation that he said is very common. But then perhaps his view that all past confirmation are spurious may have lowered interest in the matter.

Whewell said, the test for a new hypothesis has to proceed cautiously. First, find factual statement that follows from the hypothesis but not form earlier theory; second, perform the observation that this statement describes and record it. If the two statements, the test statement and the observation report agree, then the outcome is genuine success and the hypothesis is scientific. Whewell was a great historian of science and he took as his paradigm the researches of Kepler, who had reported his series of errors candidly. He then deemed his own history scientific.

This is Whewell's theory of research and of discovery. He was very proud of it. The major criticism of Kant is that his transcendental philosophy allowed for no discovery: it declares us all born with the disposition to generate Newtonian mechanics compleat. Whewell saw his philosophy as a version of Kant's but making discovery possible. He declared himself an inductivist of sorts.

Whewell's contemporaries rejected his view, as they wanted their philosophy of science to present research as some sort of science-making machine, whereas he said scientific progress depends on the constant but unguaranteed flow of new hypotheses, most of them false and none of them having any guarantee for survival.

Although Whewell offered no guarantee for the appearance of hypotheses, he offered some aids for coaxing them out; he used for it the recently invented word "heuristic": as researchers seek hypotheses they welcome any possible stimulation to the imagination. New hypotheses have to explain older hypotheses and new facts (as the task of science is to unify). Where do such items as new factual discoveries come from? What items of science should the next abstraction include and how do researchers identify them?

A new fact cannot appear unless anticipated; but anticipation robs it of its novelty. Hence, it is new because the theory that leads to its anticipation is new. This new theory is then confirmed. But this new theory should explain old theories and new facts. Where do these come from? He could not say. This turned out to be the Achilles heel of his theory.

Whewell did not appreciate the German philosopher Hegel. In a private letter he said he could not appreciate anyone who took Hegel seriously. On one point, however, he took Hegel seriously enough to refute him. Hegel had said that Newton had plagiarized his theory from Kepler. He said it for political reasons: Kepler was a German and Newton was an Englishmen. This did not mattered to Whewell; what mattered to him was the idea that quite possibly Newton's theory follows from Kepler's. This looks to us odd, yet at the time it looked different. First, there were efforts then (and later) to eliminate forces from Newton's mechanics—in *Principles*

[17] Anyone who will find a predecessor to Whewell's criterion for confirmation should have it published.

of mechanics and Dynamics by William Thomson Kelvin and Peter Guthrie Tait and in *The Science of Mechanics* by Ernst Mach. Second, logic was very vague and the idea of a deduction of a theory from a more abstract one was unclear; the leading logician then was Bishop Richard Whatley, whose opinion on his matter was anything but clear. Whewell pulled up his sleeves then and refuted Hegel. He showed that though for a two-body system Kepler's first two laws follow from Newton's theory of universal gravity, for more bodies they are in contradiction. Historically this was hardly news, since already in his *Principia* Newton noted that his theory explains the deviations of some outer planets from their Keplerian ellipses. Methodologically, this was an earthquake: if Newton's theory contradicts Kepler's, than one of them must be false, yet they are both scientific and scientific theories are by definition certainly true. Whewell noted the difficulty but dismissed it with a vague excuse. This detail was forgotten, ignored or suppressed. When Einstein's general relativity replaced Newton's theory of gravity the story was over. The methodologist who addressed this was Karl Popper.

6.5 Popper's Theory of Discovery

My bias in favor of Popper sends me to an opposite assessment of his life work. The one that perhaps qualifies most is that of David Papineau. I chose a review from his pen of two posthumous books by Popper, published in the excellent popularizing *Times Literary Supplement* of June 23 1995.

Reputation is transient, observes Papineau in the opening of this review; Popper had it to excess in his lifetime and now the weaknesses of his doctrines lead to a different attitude, one which Papineau states at the very end of his paper: "Perhaps it would be best now if we remember what Popper preached, and lay the rest of his doctrines quietly to rest." What old doctrines deserve sending to oblivion? Bacon had a clear answer: all doubtful and refuted theories should go. This cannot be: Plato and Aristotle disagreed and we fervently wish to remember both. There is a better answer to this, but it holds only within science; it belongs to Einstein. In science only those theories that constitute approximations to their successors deserve to say on record. All this does not trouble Papineau. He offers his comprehensive criticism of Popper as if it were generally received among the experts and as if it is obviously right. That it is possibly not obviously right he hints in his reference to Popper aficionados (myself included). What suffices for the dismissal of Popper's doctrine, he suggests, is the fact that Popper has stressed the negative in science whereas research is always the search for the positive. It is, Papineau adds in indulgent mood, Popper's overreaction to Einstein's overthrow of Newton's theory.

As it happens, the contrast between the view of novelty in science of Bacon, Whewell, and Popper that is the subject matter of the present discourse. I take it Papineau would not like the public to forget Bacon and Whewell. My presentation of the superiority of Popper's idea of novelty over theirs may then serve a response to his dismissal of Popper.

Like the criteria of novelty of Bacon and of Whewell, that of Popper is not exhaustive. They all have to be, since, in a way, every observation is new: one cannot enter the same river twice (as Heraclitus of old has observed). When inductivists, for example Mill, endorse this assertion, they confess that it bewilders them. For, if every fact is new and thus unique, then there is no repeated observation from which to generalize; and then, *a priori*, no generalization can be true and no science need explain them. Popper's position is entirely different: researchers postulate both generalizations of observation reports and theories to explain them. Both kinds of postulate are hypothetical. Whereas inductivists find the fact that ever so often generalizations are puzzling, Popper takes this puzzlement as the starting point of new research: why do we observe the facts this way?

Research then is attempted explanations: just like any generalization, a theory is a presentation of all the events that satisfy certain conditions in specific ways. There are two known facts that Popper's theory explains and that most inductivists find inexplicable. One is that researchers seek independent tests for any given hypothesis. For, if research is the search for confirmations, as inductivists say, then these are easier to find with no tests. (This argument is known as Hempel's paradox. He had no satisfactory resolution of it.) Popper's view is that research is the quest for the truth that expresses itself as efforts to eliminate error, to refute given hypotheses. Tests, then, are efforts to refute. The other fact that puzzles inductivists and that Popper's theory explains is that researchers may miss discoveries that they make, and they may even report them as unimportant and well-known phenomena. (This is a very rare occurrence, of course, since discovery is rare anyway. The puzzlement is that it occurs at all.)

Enter Popper. Confirmations are failed refutations, he said. If a fact confirms both the new theory and a received theory, he added, then the test leading to it was not independent (of the old theory) and so it is neither a discovery nor a confirmation of the new theory. It has to follow from the new theory and contradict the received ones, he said. Hence, novelty is relative: it is relative to the theory that it refutes.

That an observation is new relative to one theory and not new relative to another theory is very commonsense. Only this way can we explain what we mean when we speak about independent tests or when we praise the first appearance of a theory and condemn the claim that it is true after it was refuted. It is perhaps intuitively understandable that, say, the famous experiment of Albert Abraham Michelson that failed to find the ether drift is new relative to the best theories of its time but not relative to Einstein's theory of relativity that explained it. Without a theory, any two different statements of fact are independent. Only when a theory enters the picture does dependence becomes possible. Refutations of a theory are new then as long as they are independent of previous refutations of the older theory. Later the new system unify the phenomena that earlier researchers never suspected might have anything in common. This change brought surprising mistakes in inductivist writing of the history of science: inductivist historians of science try to describe novelties absolutely, with no reference to theory. As this is impossible, they rely on the up-to-date science-text-books instead. This may be proper but it is not inductive.

Viewing new observations as refutations of received theories presents the ones as novelties relative to the others. This is not always obvious. Hence, observers may miss the novelty of the facts they observe. This was Heinrich Hertz's mistake: he assumed that the photo-electricity he had observed (1887) fits the theory at hand, viewing it as classical resonance rather than as a quantum phenomenon. This error Einstein corrected (1905).

To repeat, Popper's criterion of novelty is not exhaustive; (nor is it meant to be;) it is nonetheless interesting since it covers at least all those observations that are generally recognized as scientific discoveries (though not of patentable inventions, for example).[18] This links Popper's theory of discovery as refutation with the element of truth in Bacons theory of discovery as surprise. The empty mind Bacon-style has to employ intuitions in order to be surprised when observing novelty. Can an empty mind have intuitions? We do not know. It is hard even to imagine an empty mind, as there are no pure observations for it to contain. There is no problem with a refutation of a received opinion, however: it clearly is a surprise. Had it been known before the theory was invented, said Bacon, then that theory would not have been endorsed.[19] Viewing surprise intuitively one cannot explain (a) discoveries that pass unnoticed at the time, and (b) discoveries that are the result of new theories and that do not in the least surprise the originators of these theories, but only the scientific world that adheres to the old theory. The example par excellence for case (a) is Hertz's discovery of the photoelectric effect. He regarded his discovery that ultraviolet light facilitates the formation of electric sparks as a special case of resonance, a phenomenon that was well known at the time. He published it anyway because at that time Maxwell's electromagnetic theory was still very controversial and Hertz thought that considered as an electric resonance phenomenon his discovery provided confirmation of Maxwell's theory and was thus a refutation of the older alternatives to Maxwell's theory. Those who pursued Hertz's research continued to consider it a resonance phenomenon and they tried to explain similar effects on this assumption. In 1905 Einstein showed that the photoelectric effect contradicts Maxwell's theory and follows Planck's radiation theory. Planck had consciously deviated from Maxwell's theory of radiation, yet he was surprised to learn from Einstein how wide was the gap between his own theory and that of Maxwell—a gap that constantly widened in spite of his own continuous efforts to bridge it.

An even more impressive instance is the discovery of hydrogen and oxygen. These gases were isolated at least as early as the seventeenth century. Boyle collected hydrogen in a vessel and marveled at the beauty of the blue fire that it sustained. Hooke and Mayow collected oxygen in vessels. Nevertheless, those two gases were not discovered then: there was no theory available to refute by their observations. Whether their observers were surprised or not in the intuitive sense of

[18] As technology caters for social ends, it is goal-directed and so it applies the rationality principle. Its study thus may qualify as a social science proper.

[19] This idea sounds reasonable. It is not. Niels Bohr's theory of the atom was in agreement only with the first column of the periodic table at most, yet it was rightly hailed as a great achievement.

surprise, they missed the novelty in the scientific sense. The gases were discoveries by minds polluted by the theory of the phlogiston—allegedly accidentally. For, according to the Baconian myth, a mind-polluting theory like the phlogiston theory cannot possibly help science to progress. It brought Priestley and Scheele independently to the same discovery because they were both testing the same theory as Lavoisier was. By contrast, both Hooke and Mayow missed it independently because neither could use his observations as tests. Independent simultaneous discovery comprises confirmation of Popper's theory of discovery. Inductivist methodology cannot explain simultaneous discoveries since the simultaneous occurrence of a rare event is very improbable.[20] The ardent phlogistonist Priestley and the anti-phlogistonist Lavoisier made the same discovery almost simultaneously. This is a glaring refutation of the theory that acceptance is commitment and that commitment inhibits. It accords with Popper's theory of acceptance in research as the wish to test (Popper 1935, §25). The only commitment that counts, he said, is the commitment to the rules of the game of science, and these rules prescribe tests as attempted refutations.

Popper's theory of discovery thus explains Bacon's theory of discovery as a first approximation. Discovery is surprise, though not in the abstract intuitive sense, but relative to given theories. Like every theory that is only a first approximation, Bacon's theory contradicts the theory that supersedes it. Bacon's theory and Popper's are in conflict on two points. One is over attitudes towards theories; the other is over the interpretation of surprise.

6.6 Bacon's "Mark" of Science

If surprise is an intuitive measure, then Bacon's theory is false and even trivially so. His own example that he mentions twice should do. The first mention is in his *The Advancement of Learning* and the second in *De Augmentis Scietiarum* that is almost a literal translation of the first into Latin. Here is the first (*Works*, 3, 385):

> you will rather believe that Prometheus struck the flints, and marveled at the spark, than that when he first struck the flints he expected the spark: and therefore we see the West Indian Prometheus had no intelligence with the Europeans, because of the rareness with them of flints that gave the first occasion.

Bacon thus suggested explicitly a theory of accidental discovery and declared that the more probable an event is, the more probable is its discovery: what is less common is less known. In the West Indies flint is less known and its spark is therefore unknown. Turning to *De Augmentis Scietiarum* (*Works*, 409) we find the theory still there, but with the West Indian Prometheus omitted. Omissions are very rare in this augmented translation. The reason is obvious: he could not rub two

[20] Bacon's theory of discovery explains the simultaneous discovery of anything that suddenly becomes generally available but not what is there from time immemorial, for example the common gases.

sticks together for a few minutes utterly empty-mindedly. He could not have done it by chance. Ellis, in a learned footnote to the above quotation, observes that in old Europe—including ancient Greece—all fire used for ritual ends was produced by rubbing sticks together. This way, it seems, Ellis suggested a hypothesis: the Greek Prometheus also rubbed sticks together. Be this hypothesis true or false, it renders Bacon's comment on the flint of Prometheus a questionable speculation, not a historical discovery. The way the archetypal Prometheus, European or West Indian, discovered fire is perhaps by keeping some forest fire alive. The production of fire anew was an unknown late event. But all this is an aside: whatever the right methodology is, at most it should hold for scientifically minded society, as Popper was the first to notice, not necessarily for magically oriented society.

Consider the novelty of a discovery then to be relative to a theory rather than to the psychology of the discoverer. This invites degrees of novelty and even the reintroduction of psychology—but only as a secondary factor, since it depends on methodology. Thus, researchers will find more surprising and enchanting a beautiful discovery in areas of interest to them than a greater discovery elsewhere. Much more generally, having ascribed degrees of significance to theories, by Popper's methodology the more significant a theory is, the greater a discovery is its refutation and the more surprising. Thus, Michelson's discovery is significant because it refutes a very significant theory: the merger of Newtonian mechanics with Maxwell's electrodynamics. Faraday's discovery of the dielectric effect is less significant, though still important, because it came to serve as a possible[21] refutation of Coulomb's magnificent electrostatic theory. The refutation of Richardson's theory of magnetic spin by the experiment of Einstein and De Haas is important, though less so than the refutation of Niels Bohr's theory by the experiment of Davisson and Germer, simply because the latter was one of the most important theories of the age. True, this discovery was guided by another theory, by now refuted too, that of De Broglie. Often a refutation is a crucial experiment, a discovery from the point of view of the old theory, not from the point of view of the new. Whether researchers devise a test of a theory before they devise an alternative to it or they devise an alternative theory first, the main task is to test whatever theories exist. The choice between the two options is up to the active researchers. They usually try both options and the easier one often comes first.

Just as a discovery may suffer neglect, so its significance may suffer erroneous assessment. This shows again how skeptical we should be towards our own intuitions. Popper's theory explains this fact also. A refuting experiment that causes a slight modification of a theory (for instance, the experiments of Hippolyte Fizeau concerning the ether drift) is less significant than an experiment that leads to a major modification (for instance, Michelson's experiment concerning the ether drift). The assessment of the value of a refutation thus depends on its consequences and thus (according to Popper's theory of discovery) it is not immediately visible; and,

[21] As Faraday showed, it is possible to reconcile Coulomb's theory of action at a distance with di-electricity by postulating dielectric dipoles (Agassi 1971, 272).

indeed, the assessment of the import of a discovery is usually determined only after the assessment of the success or failure of attempts to reconcile the theory and the new fact. The announcement of a refutation, of an apparent discovery, raises immediate attention. The discoveries of animal electricity and of steam electricity were heralded as very significant because they seemed to refute the theory of the unity of electricity. After much labor, Faraday reestablished the unity of electricity, thus depriving them of their expected value. The alleged discovery was then lost and not mentioned in texts or histories of electricity.[22] It was not a discovery, although by inductivist standards it was. When Kelvin declared (Kelvin 1904, Preface) that Michelson's discovery is reconcilable with the classical theories, he reduced its stock effect. When Einstein showed the opposite 1 year later, the experiment entered the limelight once more. Inductivists always herald great discoveries with great enthusiasm, partly in order to hide the confusion that discoveries always cause them: they find it hard to admit that valuable received theories are not the last words on their subject matters. Few of them never faced the inductive generalization against induction: all past great discoveries are refutations; therefore, all discoveries are refutations. The inductivist emphasis is on confirmation, on support. Refuted theories are to the inductivists Bad Things, and so they cannot but view refutations of good theories as calamities. (The refutation of Newton's theory is still known as the crisis in physics.)

The surprise criterion of novelty served Bacon as an argument for his view that discovery is accidental and that view came to support his doctrine of prejudice: get rid of all old error and Mother Nature will reward you by showing her secrets. We should therefore switch now from the criterion of discovery to the road to discovery. Undeniably, some discoveries drop into researchers' laps accidentally. They do not wait for them and they do not seek them.[23] This is why it is usually researchers familiar with the problem situation, who observe a new fact and declare it a discovery. Thus, what discoverers do, quite contrary to Bacon's advice (*Novum Organum*, Aph. 69) is first to learn the received theories or invent better ones, or invent competing theories—in any case, they have some theories, some notion of how nature behaves under certain conditions. And then, and only then, they may try to see the relations between fact and theory. In the search for discovery, then, researchers must have theories in mind, but whether they assent to them or not does not matter as long as they are critically-minded. This, we remember, is the retreat from Bacon's theory of discovery to that of Whewell.

This, together with the generally received doctrine that researchers seek confirmations, renders discovery impossible. Confirmations, even for the most incredible theories, are easily available. The preference for discoveries over

[22] We forget that Mary Shelly's Dr. Frankenstein brought his monster to life with animal electricity (1818).

[23] This is the source of the word "serendipity" for accidental discovery: it has to do with a certain tale of a search for one thing that led to the finding of another, much like the story of Saul who, looking for his asses and bumped into Samuel who anointed him the first king of Israel.

confirmations is obvious. Even the wish (psychologically speaking) for theories to endure does not stop researchers from recording facts that are in disagreement with their favorite theories; otherwise, their competitors will have priority in the discovery of these facts.

Thus, despite Bacon's argument, the possibility of discovery renders too narrow his theory of interpreting observations. Not only is his demand to observe with no interpretation impossible; interpreted observations need not agree with received theories. Bacon noted that the scholastics interpreted their observations in accord with their theories and this allowed them to stick to them. He ignored the option that we can compare competing interpretations and see which is better; this is what Copernicus did before Bacon was born. In this case, we may legislate that only the best interpretation is a candidate for the status of confirmation.

More generally, and this is the crux of the matter, interpreting observations means viewing them as relevant to some theories. The (exclusive and exhaustive) set of logical relations between couples of statements incudes deducibility, contradiction, and independence. Relevance is either deducibility or contradiction, yet people often ignore contradiction. Bacon said, new observations are independent of received theory. Whewell said, with no theory no observation takes place or its occurrence is of necessity unnoticed. Rather, he said, new observations are deducible from new theories. This shifts the problem of novelty form observation to theory. Some historians of science, Andrew Norman Meldrum and Thomas Kuhn, said, novelty is not a relevance function but a psychological affair. Yet we have not finished the discussion of relevance: there remains one option: that of contradiction.

We may interpret observations then as either in accord or in discord with a given theory. Bacon said, our psychology makes us refuse to admit discord. Whewell said, the refutation of Newton's optics disproves this. Others[24] say, this is possible only when there is an alternative so that Bacon's observation is true in the absence of an alternative and the presence of one allows us to overrule it—usually when under the pressure of a crucial experiment.

In conclusion, received methodology rests on the assumption that researchers always look for confirmations of the theories that they identify with. Almost every discovery refutes this theory. Science is possible only because researchers are ready to discover new facts even at the cost of abandoning the most established theories. As long as people were too concerned to retain their theories as sacred, they committed discoverers to the stake. When discovery became too important to ignore, the picture changed: theories were sacrificed, not their critics. As Popper said, we let our theories die in our stead. I do not know if I agree: good refuted theories survive—as refuted, and they find their applications, if at all, only within the limits that newer theories prescribe for them. The idea that discoveries of new facts will never clash with old theories is a Baconian dream. There is no need to protect theories against further enquiry into nature.

[24] The allusion is to Kuhn, Feyerabend and Lakatos (Agassi 1999).

Bacon said, when we will be in possession of true theories, deductions from them should lead to many discoveries. As he has argued, these would not be new. He taught that, properly open-minded, we will discover all the secrets of nature, and find the essences of all things. His optimism and his doctrine of prejudice say that we necessarily see only what our theories permit us to see, and that therefore we should make sure that all the theories that we endorse are true. For, otherwise we should be blind to the truth. This leads us to the doctrine of prejudices of opinion. Let us examine Bacon's distinction between scientific theory and prejudice and then discuss his theory of prejudice of opinions. Its relevance today is obvious from such cases as the comments of Papineau on Popper. He finds it important to bury Popper's philosophy as the defense of induction and confirmation still involves the eradication of prejudice and in the eye of a consistent inductivist no prejudice is worse than anti-inductivism such as that of Popper.

Chapter 7
Prejudices of Opinions

7.1 Suspension of Judgment

Francis Bacon is the originator of the demand to suspend judgment about a given theory first and then to commit oneself to it only to the degree to which extant evidence supports it, to the degree of belief in it that is rational given available empirical information. This demand is very widespread and deserves special attention. Before showing that it goes back to Bacon and before explaining why he and his followers were and still are its ardent advocates despite all the criticism that diverse critics have leveled against it, let me discuss the view itself no matter who may have been its originator. The best argument in favor of this theory that I have found is in Russell's charming *Skeptical Essays* of 1928. His presentation of his view is a part of his introduction:

> … The doctrine in question is this: it is undesirable to believe a proposition when there is no ground whatever to suppose it true.
>
> … The scepticism that I advocate amounts only to this: (1) that when the experts are agreed the opposite opinion cannot be held to be certain; (2) that when they are not agreed, no opinion can be regarded as certain by a non-expert; and (3) that when they all hold that no sufficient grounds for a positive opinion exist, the ordinary man would do well to suspend judgment.

Russell's skepticism is an argument in support of his demand for tolerance. He suggests that the more doubtful are the less unkind to those who do not share their views. I chose the following beautiful passage from his book (Ch. 14) as representative:

> Freedom of opinion … is the most important freedom, and the only one which requires no limitation whatever.
>
> … The fundamental argument for freedom of opinion is the doubtfulness of our beliefs. If we certainly knew the truth, there would be something to be said for teaching it. But in that case it could be taught without invoking authority, by means of its inherent reasonableness … From our human point of view, it is an ideal, towards which we can approximate, but which we cannot hope to reach.

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*,
Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_7,

This passage represents the fundamental element of classical rationalism. Russell suggests that the basis for it is the idea of rational degree of belief (or rather of disbelief) based on extant evidence. His version of it is unusual. To begin with, he recognizes that we are all ignorant of the extant evidence and of its reliability. Also, he trusts experts on this: he assumes that their accord rests on evidence so that that both their agreements and their disagreements are reasonable. This of course is very problematic: it clearly does not hold in non-scientific societies, namely, for the majority of humanity even today.

For the sake of freedom of opinion and of speech, let us now agree, it is good to know that our views are doubtful. This does not mean that doubt is necessary for tolerance. Indeed, Russell demands freedom of opinion even concerning certainty. Moreover, in addition to his demands for unlimited freedom of opinion, Russell declared that the superiority of certain views over doubtful ones is no license to teach them authoritatively but, on the contrary, we may teach them just because it is possible to do so without invoking authority. This can work as a criterion for certainty: teaching a certain view requires no authority—by appeal to its reasonableness. This would allow critics to criticize views that they do not consider sufficiently reasonable. Thus, the demand for freedom of speech includes the demand for freedom of criticism; throughout this book, Russell's argument from skepticism is that critics may be right. This argument is valid but redundant, since of course, the right to criticize does not depend on it. Independently, also, by Russell's view skepticism includes the readiness to accept criticism, the readiness to admit having been mistaken that Heinrich Heine declared an inalienable right (Heine 1835, end to forward to second German edition). Hence, the demand that Russell makes for the sake of freedom of speech, the demand to be skeptical in the sense of recognition of our fallibility and the demand to hold any [dis]belief to the rational degree that becomes it in the light of evidence are all different and independent. If these come together in any reasonable way, what may connect them is the psychological theory that feeling uncertainty, like the feeling we have when we walk on thin ice, is conducive to the readiness to defend the freedom to criticize and to accept criticism.

This psychological hypothesis is traditional. It is the major and most influential contribution of Bacon to methodology. The theory gains its significance from his doctrine of prejudice: erroneous ideas disqualify their advocates from performing proper scientific research. Russell, a sworn fallibilist, rejected this idea off hand, let me hasten to report. He said: claims for total freedom from prejudice are humbug (Russell 1956, 77), but he also spoke against "the modern fear of humbug" that makes life "more drab and less dramatic" (Russell 2003, 202).

Anyway, today's version of the doctrine of prejudices of opinions is the idea that readiness to accept criticism depends on feelings of different degrees of assurance and or the willingness to suspend judgment. Its empirical refutations abound. It is equivalent to the identification of the feeling of certainty (to excess) with dogmatism. This identification is both refuted and politically dangerous, as it reassures skeptical dogmatists of their dogma. Skeptical dogmatism is common and viewed as the creeping doubt that is the devil's temptation of the faithful.

Readiness to accept criticism and to change one's views accordingly is possible even about one's most fundamental convictions. The deepest conviction will evaporate when observing an event to the contrary. This happens repeatedly to many people. This happens even without refutations, when people once deeply convinced of some irrefutable sectarian system of ideas—be it a traditional religion, Marxism, Freudianism or libertarianism—later change their views. Their acts deserve respect as they are the fruits of intellectual honesty. This attitude is most explicit in science. Indeed, hosts of scientists who were deeply convinced of the utter certainty of Newton's mechanics nevertheless allowed criticism to persuade them to give it up—more or less overnight. Thus, certitude need not be dogmatic. Nor is incertitude a guarantee against dogmatism. Many people who feel very uncertain about their faith in their religious or socio-political creeds never listen to criticism nor take notice of empirical refutations, or they explain them away in full accord with Bacon's description. This sort of doubt, which is very barren in itself because it leads neither to a critical approach nor to tolerance, is regrettably too common. As Bacon has noticed, it may result from intellectual tiredness: people who have made great efforts to achieve their present views may become most pessimistic when they discover that they are uncertain but too tired to make another upheaval and change from one doubtful view to another. The result may be that they will stick all the more desperately to their dogma the more doubtful it seems to them. They may become impatient with criticism, seeing in it nothing fruitful, rightly perhaps as far as they are concerned. And their hostility to criticism may easily make them intolerant. Finally, doubt may lead to dogmatism through cynicism. This is the (wrong) conclusion that as all views are doubtful, which one you adopt does not matter, that ideas do not matter anyhow.

The distinction between critical or intellectual skepticism and mental or emotional skepticism is the same as that between the impersonal and the personal, between the certainty that tradition labeled mathematical and the feelings of certainty. Mathematical certainty is the result of demonstration; it is independent of feelings. Russell discussed certainty in terms of demonstration in contradistinction to the certainty that rests on authority (or that violence inculcates, as George Orwell has described it in his *1984*). This depicts his concept of skepticism as impersonal. Therefore, his claim that for the sake of skepticism one must suspend judgment and hold the proper rational degree of [dis]belief is redundant.

More generally as this discussion pertains to empirical psychology, it is hardly useful for the methodology that should guide research into empirical psychology (Popper 1959, §7). Regrettably, Bacon's methodology rests on his doctrine of prejudice that is plainly psychological.

7.2 What Is a Prejudice?

The fundamental assumption of Bacon's doctrine (of which traces remain even in Russell's work) has an antecedent in the doctrine of prejudice of Moses who commented (*Deuteronomy*, 16:19),

> You shall not take a bribe, for the bribe blinds the eyes of the wise and perverts the word of the righteous.

Bribery not only might blind a judge; it definitely will. This is asserted as a universal (psychological) law. Now Bacon deemed the philosopher a judge, we remember. He repeatedly mentioned the delight and vanity of judges as they see their anticipations come true. It is easy to see then what a trap Nature sets: the bribe and the blindness of a judge come together: observing facts as confirmations (bribes) amounts as seeing them distorted (blindness).

Bacon exaggerated. He deemed delight and vanity extremely strong motives, and so he made the greatest efforts to teach researchers whom he viewed as judges of theories to shun temptation by not prejudging, by not making any anticipation, by avoiding making any prediction whatsoever until after the pronouncement of the verdict, the sober, valid induction.

To go into further detail, Bacon had two doctrines of prejudice of opinion, each coupled with the corresponding theory of induction. The first is simple; the second, the First Vintage, is not. The first doctrine of induction is this: induction proceeds from facts, slowly but directly moving toward certain, demonstrable theories. The first doctrine of prejudice that corresponds to it is this: never entertain any view whatsoever unless you could demonstrate it (inductively). The second doctrine of induction is this: induction proceeds from facts, slowly but directly moving toward the first vintage that is not quite certain. The second doctrine of prejudice that corresponds to it is this: never entertain any view whatsoever unless you have collected lots of facts and sorted them by similarities and differences. As the First Vintage is not necessarily the last one, it does not yet possess full demonstrability, and so it is still open to refutation. Accordingly, Bacon introduced "a frivolous distinction" (to use his needlessly derisive expression) into the doctrine of prejudices of opinions. From now on the intellect is kindly permitted to hold doubtful views but only diffidently. Researchers should make sure, then, that their conclusion does not become anticipation, heaven forbid, and that it will not lure them into temptation—that it will not blind them. It is like Russell's fear, except that he feared intolerance in human relations and Bacon feared frustration in research. Finally, the second version is a parenthesis in the system (to quote Ellis); so is its companion, the second version of the doctrine of prejudices of opinions. Bacon hardly articulated it.

Bacon's view is false: not all anticipations are closed to criticism and not all people reject all significant criticism. Russell's view is false too: it is rare that people who agree about beliefs also agree about tolerant conduct or display the same degree of toleration in normal action. Russell's practical suggestions are reasonable nonetheless, but they are independent of the idea of rational belief. Indeed, it relates to matters that only experts can discuss rationally and we, ordinary mortals, have little choice but to follow them; we should do so warily.

How much and which way are people able to command their beliefs? Do we know exactly the degree of our commitment to them and the degree to which this commitment is rational? And do we know the way to raise or lower these degrees? We know much less about our beliefs than about their objects. Bacon ignored this (*Of the Interpretation of Nature*, Ch. 17):

> That if any have had or shall have the power and resolution to fortify and inclose his mind against all anticipations, yet if he have not been or shall not be cautioned by the full

> understanding of the nature of mind and spirit of man, and therein of the seats, pores and passages both of knowledge and error, he hath not been nor shall not be possibly able to guide or keep on his course aright.

This is great prose but it is not serious: to this very day no one knows that much about the psychology of learning and of beliefs.[1] Yet it is essential for the doctrine of prejudice. One cannot learn[2] without having previous knowledge of the psychology of knowledge. And the psychology of knowledge that Bacon advocated is, of course, a gross exaggeration. And relying on that doctrine he prescribed no anticipation whatsoever, and then he softened his prescription to the demand to hold the exact rational degree of belief in any theory in the light of extant empirical evidence. Bacon declared: one is prejudiced and thus disqualified from proper research who does not "suspend judgment" but proclaims one's convictions "when there is no ground whatever to suppose it true" (to use Russell's expression).

This theory is false. Of the superstitious people who believed in theories "with no ground whatever" and refused or were unable to "suspend judgment", some were famous researchers who were ready to change their views when new factual evidence or any other argument is brought to their notice but stuck to some irrefutable ideas that guided their researches. Indeed, in some cases both competing schools of thought that were guided by opposing views and both had members who were terrific researchers.

Also, prejudices obviously abound but not all mistaken judgments are prejudices. Those are prejudiced who are determined to stick to their views in the face of criticism. In particular, those are prejudiced who admit the strength of criticism of their opinions and still hold to them on the supposition that there exists somewhere an answer to that criticism, an answer that would transform the criticism from refutations of their views to their confirmations. Such people cling to their views and they may wait for the authority to deliver them from their discomfort and design ingenious theories to extricate them of their need to change their views. In short, prejudice is opposed not to the suspension of judgment but to the critical attitude. Russell's text suggests that he deemed the two attitudes interdependent. This is not always the case.

This is important: all humans make mistakes and harbor some prejudices, but not all humans are prejudiced. The most dangerous prejudice, Popper observed (in his lectures), is the view that one is free of all prejudices. These people have lots and lots of prejudices. They just do not know it.

The demand for the suspension of judgment rests on error, but this error is not necessarily a prejudice, because its advocates offer some arguments in its favor. This does not hold for contemporary inductive philosophers, the students of inductive

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[1] The classic in this field (Rokeach 1979) is a sad echo of Bacon in a quasi-empirical guise. Amusingly, its immense bibliography mentions neither Bacon nor Russell.

[2] This is an exaggeration. In his more mature (if this is the word) works he admits: anticipations have great advantages yet as they impede progress, we should gladly give them up.

logic: most of them are prejudiced, since these days the force of Popper's criticism of inductivism is generally recognized but they hope to find some arguments that allow them to stick to their inductivism.

A famous version of the inductivist theory of rational degree of belief is that of Rudolf Carnap (see next section). He said, the degree of rational belief in a theory in the light of given experience is the same as its probability in the light of that experience. He did not demand belief in it; rather, he proposed that daily business decisions should follow it. He promised that this practice would bring about success in the long run. On the assumption that business people already do that, for example in insurance, his proposal is redundant. To the extent that he went beyond current practices, he did so "with no ground whatever". And of course no one has followed his advice.

7.3 Bacon and the Logical Empiricists

> As for Modern Methods of philosophizing …
>
> Substantial Forms, Occult Qualities, Intentional Species, Idiosyncrasies, Sympathies and Antipathies of Things, are exploded; not because they are terms used by Ancient Philosophers, but because they are only empty sounds, Words whereof no man can form a certain and determinate Idea. Forming of Sects and Parties in Philosophy … is, in a manner, wholly laid aside … Matter of Fact is the only thing appealed to … the New Philosophers, as they are commonly called, avoid making general Conclusions, till they have collected a great Number of Experiments or Observations upon the thing in hand; and, as new light comes in, the old Hypotheses fall without any Noise or Stir. So that Inferences that are now a-days made from any Enquiries … though perhaps they be set down in general Terms, yet are (as it were by Contest) received with this tacit Reserve, as far as the Experiments or Observations already made, will warrant.
>
> (Wotton 1694, Ch. 27)

Wotton's description of the practices of the Royal Society of London in its early days (with its language slightly modernized) might easily pass as an excellent report of another new philosophy. According to Bacon, proofs distinguish science from the arts and fruitfulness distinguishes science from theology ("barren like a virgin dedicated to God"; *De Augm*. iii. 5). This application of his idea of fruitfulness and discovery that expels theology from science was in an effort to prevent researchers from dividing into schools on religious grounds. It is a very important contribution of his towards the important termination of the gratuitous quarrel between science and religion. Bacon had still other categories of thought: metaphysics, prejudices, superstitions, hypotheses and all that. These are good for one purpose—for the purpose of founding schools and conquering kingdoms of the mind (*Sylva Sylvarum,* last sentence). They are dangerous and abominable to anyone "clean of imposture".

Let me compare Bacon's doctrine to that of Ludwig Wittgenstein as expressed in the early twentieth century in his famous first vintage, his *Tractatus Logico-Philosophicus*. This comparison, if it is as close as I will venture to show, should show that Bacon's views have a great persuasion power and that they are still broadly received.

Wittgenstein distinguished between science and metaphysics by the same old criterion of certainty, in oversight of the crisis in physics. Still worse, he presented the old idea in a new form, allegedly a linguistic form, contending that in the ideal language only demonstrated statements can be affirmed (so that also only refuted ones can be negated). Under this pretext, he declared that metaphysical and other speculative claims are non-statements (nonsense) rather than prejudices, meaningless rather than erroneous. The linguistic side of his theory is but an embellishment. For, linguistically, to find out if a statement belongs to his language one has to analyze its structure, whereas epistemologically, to find out if a statement is verifiable one has to consider its content, for which one has to know its meaning. In other words, Wittgenstein's coupling of linguistic and epistemological considerations makes it necessary to understand a statement before it becomes possible to find out if it is understandable (Popper 1945, Ch. 11, n. 46).

Wittgenstein did not exclude statements like "the sun will rise tomorrow" from his language, although he readily admitted that it is a hypothesis (Wittgenstein 1922, §6.36311)—because it is verifiable in principle, whatever that principle may be: we only have to wait until tomorrow and see whether the sun will rise then or not. But he would not allow such statements as "God is good" into his language as there is no hope of verifying them or their negations in any time. That the theory of observation behind this metaphysics is merely another variant of Telesio's theory is rather obvious: according to Wittgenstein, observing the fact to which a statement corresponds imparts knowledge as to whether it is true or false. For this we have his solemn assurance: the fact, he said, shows itself (Wittgenstein 1922, §§4.022, 4.121, 4.461, 5.5421, 5.631, 6.12). Maybe.

Let us turn then from Wittgenstein's theory of observation to his theory of science. The totality of true propositions is the totality of natural science, he said (Wittgenstein 1922, §4.11). Had Ellis heard this he would surely repeat what he said once (*Works*, 1, 386),

> No such collection could be formed; and were it formed, general laws and principles would be as much hidden in a mass of details as they are in the world of phenomena. … Teaching the philosopher what observations he is to make … seems necessary …

Like Bacon, Wittgenstein objected to ranking of information (§§6.4, 6.41):

> All propositions have equal value. …If there is a value … it must lie outside the whole sphere of what happens …. For all that happens … is accidental.

The principle that Ellis was looking for, then, is outside our world; in our world all is accidental. Nonetheless, Wittgenstein did not object to the introduction of theories, but all he let theory do is insert order: Newton's theory, for example, brings the description of the universe into a unified form. There is no need to question it, as it is not a statement (§6.431). Wittgenstein tried to present scientific theory as meaningful and metaphysical theory as meaningless, ended presenting scientific theory as meaningless, and saw nothing wrong in this. Still, it is a complete reply to Ellis' possible objection after a fashion: in science à la Wittgenstein all observed facts are recorded and organized in a unifying form. Wittgenstein thus achieves an ideal picture of science. This raises a few minor problems drawn from the real, imperfect science.

The most obvious question here is, how did this ideal form appear, what additional information it imports, and what is the logical status of this information? Wittgenstein had precisely these questions in mind when he says (§6.343), "Mechanics is an attempt to construct according to a single plan all *true* propositions which we need for the description of the world".

Now we know: theories are not only forms but also attempts. This means that we do not know whether the additional information—the predictions that the theory hauls out of the extant information—is true or false. The additional information is perhaps the subject of the following statement: "We must not forget that the description of the world by mechanics is quite general" (§6.3432) Now we may be sure that the "attempt to construct" a theory is uncertain forever and is therefore unscientific, that is, metaphysical in Bacon's sense and in Wittgenstein's sense alike; a prejudice in Bacon's terminology and non-statements or nonsense in Wittgenstein's. This is Ellis' criticism of Bacon's First Vintage or first attempt to generate a scientific theory: it is uncertain and therefore unscientific, and we know this even before it appears. Such minor difficulties do not deter people who are determined to throw metaphysics overboard. By reason, however, if together with the overthrow of metaphysics the universal laws of science have to go too, then the program has gone astray.

Enter Moritz Schlick, the physicist whom Einstein appreciated, who later became Wittgenstein's ardent follower. He considered universal laws employed in science not as statements but as rules of inference. Now the very idea of non-statements had to separate the scientific from the metaphysical. And now the need appears for a new distinction between Newton's scientific non-statements and Plato's metaphysical ones. Schlick introduced one promptly. He declared that although hypotheses are unverifiable and so they are non-statements, they are still significant: natural science needs them. And now another distinction appears: between rules of inference proper and improper, ones that assure and ones that do not assure the transmission of the truth of their premises to their conclusions. For, the trouble with these universal laws of science is that they are unverifiable, and making them rules does not change this defect: they yield "no conclusion but conjecture" (Bacon, *Works*, 3, 387). This further step, the newer distinction, due to Hans Reichenbach, is between "certain implications" and "probability implications". This is a hybrid between the view of probability as the measure of rational degree of belief and the mathematical theory of it as the study of the frequency of events. (This adds to the probability of events and the probability of hypotheses a new factor: probability implication.) The situation is so bad, that even when inductivists allow for hypotheses they cannot cope with them and so they want them grounded in factual information, and when this fails they return to Schlick's idea with a variant (Agassi 1995). And so Rudolf Carnap has one variant of it (Carnap 1950, 572) and Stephen Toulmin another in (Toulmin 1953, 92).

Hence, Ellis' standard by which he criticized Bacon was too high: he took Bacon too seriously. His ground was, he said, that Bacon was so famous. Not all famous philosophers deserve serious examinations of their views. To deserve this at the very least they should inform their readers when they change their minds. My attempt to explain Bacon's reputation by showing his significance should not be read as the suggestion that the fame of all famous philosophers is due to some important contributions, much less profound ones.

John Maynard Keynes dismissed Bacon's system, adding (in allusion to Ellis) "that it is full of errors, and even absurdities, is, of course, a commonplace criticism". The "commonplace criticism" is valid yet no reason for dismissing him. That his errors reappear three centuries later speaks of the force of his claim that even Keynes took for granted: it "all depends on keeping the eye steady fixed upon the facts of nature and so receiving them simply as they are" and the rest is less significant if not insignificant altogether (Bacon, *Works*, 4, 32). This is, of course, the method of the notorious Empiric, the ant that only collects and stores. This metaphor, Russell commented (Russell 1946, 449), is unfair to the ant: it overlooks the process of selection. Bacon, like his followers, hoped that he would be able to teach people what factual information to collect and how to use the collected material. They were all busy with trying to find how this could be done, not seeing that perhaps it cannot be done, that perhaps researchers never collect and store information except for definite purposes, and these purposes serve as rules of selection. For, as Ellis has observed, the bulk of information is just as confusing as the world of facts, and, as Whewell has observed, people take notice of facts to the extent that they are relevant to their theories. This criticism makes it unnecessary to criticize the doctrine of prejudices of opinions. But the theory refuses to go away. A serious problem imposes it on some of the greatest researchers.

7.4 Bacon's Double Game

Bacon introduced his new idea of the First Vintage and the theory of degree of belief in order to allow science to progress. Although both the First Vintage and the degrees of belief comprise a mere "parenthesis in the system" of Bacon's, as Ellis rightly noted, the parenthesis comes at the most dramatic point in Bacon's *magnum opus*, when the Master pulls up his sleeves and discovers a law of nature, no less. It was therefore most influential. Behind the problem of belief thus stands a genuine problem—the problem of demarcation between science and metaphysics—but in a misrepresented form. To begin with, prejudices of the senses are merely one kind of prejudices of opinion. As Faraday put it (Faraday 1854, 88),

> Errors result occasionally from our senses: it ought to be considered, rather, as an error of judgment, than of the sense, for the latter has performed its duty; the indication is always correct … Where, then, is the mistake? Almost entirely in our judgment.

Only reluctantly did Bacon give his famous Permission to the Intellect to reap the First Vintage. What he was always after was certainty; he wanted theories that are empirically demonstrated. As anticipations can never be proved and any research may confirm them, one must avoid the stage of uncertainty from which there is no transition to certainty and proceed slowly, and without surprise from certain facts to certain theories (*Novum Organum,* 1, Aph. 22):

> Both ways set out from the senses and particulars, and rest in the highest generalities, but the difference between them is infinite. For one just glances at experiment and particulars

> in passing, the other dwells duly and orderly among them. The one, again, begins at once by establishing certain abstract and useless generalities, the other rises by gradual steps to that which is prior and better known in the order of nature.

Thus low and high speeds are matters not of time but of method: not just slowly but surely, but slowly to ensure—by covering the whole ground with certainty. The slowness is in the making of concepts, in the very forming of them as true discoveries (*Novum Organum,* 1, Aph. 28):

> For the winning of the assent, indeed anticipations are far more powerful than interpretations; because being collected from a few instances, and those for the most part of familiar occurrence, they straight way touch and fill the imagination; whereas interpretations on the other hand being gathered here and there from very various and widely dispersed facts, cannot suddenly strike the understanding; and therefore they must needs, in respect of the opinion of the time, seem harsh and out of tune; much as the mysteries of faith do.

The doctrine of prejudice renders very important Bacon's demand to form our concepts slowly. By the usual reading of his demand for slowness it is no more than the demand to wait for the right moment when the accumulation of data suffices for induction and to develop the theories in the medium levels of generality—the *axiomata media* in the ladder of axioms. The demand to take one's time is no longer popular; the demand to stay with the *axiomata media* still is (Merton 1968, 57). Anyway, as an interpretation of Bacon all this is faulty: even when the stock of factual information suffices, Bacon's demand for slowness still holds for the process of moving to the next level. The doctrine of prejudice includes the demand that theories should undergo tests not only before believing in them, but also before having them fully in mind, before thinking them explicitly. For, having an explicit theory in mind, a researcher will sooner or later use it for predicting the results of the test for it. This prediction will vitiate the test; the very prediction will render the theory a prejudice. A proper test of a theory then must occur before having it in mind explicitly and that is possible only if it crystalizes or takes shape in a slow process, in prolonged stages of passing from vague intuition to clear explicit statements that comprise knowledge.[3] Indeed, a new theory is also a new definition, a new concept, which for us continually and gradually ("per gradus continuos", *Novum Organum*, 1, Aph. 104) unlike a hypothesis that can easily and superficially form by combining ordinary generalization and old and inadequate concepts into sentences in infinitely many ways (*Works*, 3, 243). This explains why Bacon spoke of testing and discovering theories (*Novum Organum*, 1, Aph. 105), rather than of discovering and testing. All this supports Ellis' view that the First Vintage or the First Attempt belongs not to the first and leading version to Bacon's methodology, but to the second, short version that is the First Vintage. In the first version no element of doubt enters into the system, provided it is run properly: with all previous doubtful views having been purged and with the intellect constantly on guard against

[3] Although Einstein never agreed with the idea that humans possess knowledge, he did agree with the (mystic) view that "our thinking goes on for the most part without use of signs (words) and beyond that to a considerable degree unconsciously" (Einstein 1949, 9).

introducing any new Anticipation—on guard against haste. The process then is not from doubt to certainty but from vague knowledge to clear, explicit knowledge. The question that rises next is one that bothered Bacon very much, as he discussed it several times. It is the question whether the First Vintage is anticipation, if not also Ellis' affirmative reply to it.

The demand to develop theories in small steps is more marked than usual in his discussions of astronomy, since it troubled him that he could not imagine something less general than the usual astronomy that he deemed too general so he had to identify all extant astronomical theories with schools, speculations, and futility (*Novum Organum*, 1, Aph. 104). In contrast with known astronomers, he wanted much less general theories, and these are thus certain and fruitful. He was humble and they were founders of schools (*Novum Organum*, 1, Aph. 116):

> For this is not what I am about; nor do I think that matters much to the fortunes of men what abstract notion some may entertain concerning nature and the principles of things; and … many old theories of this kind may be revived and many new ones introduced, just as many theories of the heavens may be supposed, which agree well enough with the phenomena yet differ with each other.
>
> But for my part I do not trouble myself with any such speculations and withal unprofitable matters. My purpose, on the contrary, is to try whether I cannot in very fact lay more firmly the foundations, and extend more widely the limits, of the power and greatness of man. And although on some special subjects and in an incomplete form I am in possession of results which I take to be far more true and more certain and withal more fruitful than those now received, (and this I have collected in the Fifth part of my *Instauration*) yet I have no universal theory to propound.

Here Bacon reports his having performed induction and written it up. This is too obviously a day-dream; it has not escaped notice yet it was not dismissed off hand as it should be. Spedding mentioned it is casually; Ellis considered the fifth and sixth parts of the *Instauration* as mere drafts. Whewell found more disturbing Bacon's dismissal of Copernicus (here and more so in his *Globus Intellectualis*); he tried to defend him (rather feebly; see his *Hist. Ind. Sci.*, Additions to the third edition). The following part of Bacon's Preface to his *Great Instauration* is about himself, especially about his humility. First, the humility of the competition is false, especially since it is hardly more than the submission to the opinions of others:

> But these mediocrities and middle ways so much praised, in deferring to opinion and customs, turn to the great detriment of the sciences. For it is hardly possible to admire an author and to go beyond him …

This claim (Preface to *The Great instauration*) is evidently refuted by every case of respectful dissent. But this matters less than what Bacon says of himself as evidence for his ability to perform proper research:

> For my own part at least, in obedience to the everlasting love of truth, I have committed myself to uncertainties and difficulties and solitudes of the way, and relying on the divine assistance have upheld my mind both against the shocks and embattled ranks of opinion, and against my own private and inward hesitations and scruples … in hope of providing at last for the present and future generations guidance more faithful and secure. Wherein if I have made any progress, the way has been opened to me by no other means than the true

> and legitimate humiliation of the human spirit. For all those … before me … have but cast a glance or two upon facts … and straight away proceeded, as if invention were nothing more than an exercise of thought to invoke their own spirit to give them oracles. I, on the contrary, dwelling purely and constantly among the facts of nature, withdraw my intellect from them no further than may suffice to let the images and rays of natural objects meet in a point, as they do in the sense of vision; whence it follows that the strength and excellency of the wit has but little to do in the matter … And for myself, if in anything I have been too credulous or too little awake and attentive, or if I have fallen off by the way and left the inquiry incomplete, nevertheless I so present these things naked and open that my errors can be set aside before the mass of knowledge be further infected by them; and it will be easy also for others to continue and carry on my labors. ….
>
> … … …
>
> The requests I have to make are these. For myself I say nothing; but on behalf of the business which is in hand I entreat men to believe that it is not an opinion to be held but a work to be done …

In addition to its being yet again a display of the poverty of Bacon's thought, it also displays his brilliance in suggesting that a text must be clear, so as to be open to criticism. It is also is very impressive in its rhetoric. Immanuel Kant, for one, took from it the motto for his *Critique of Pure Reason*: "For myself I say nothing"; "it is not an opinion to be held but a work to be done". Kant was right in viewing Bacon as the author of "ingenious proposals" (first *Critique*, Bxii). This spirit was surely taken up and Bacon declared that this is important, especially for people who will dismiss the rest of his work:

> … they are entitled to judge and decide upon these doctrines of mine; inasmuch as all that premature human reasoning which anticipates inquiry, and is abstracted from the facts rashly and sooner than is fit, is by me rejected … as a thing uncertain, confused and ill built up …

In the preface to his collected works Bacon contended first that the majority of past thinkers had merely created new systems, others had begun their research without first rejecting all past theories, and still others who managed to be free of old prejudices soon introduced new ones. This will not do: one must start research without having any theory in mind—received or invented. The time to create one will come later! We must be slow and patient! With divine assistance, Bacon succeeded in emptying his brains; and if he succeeded, it is only because he was so humble and so pure of heart and so sincere and industrious. He therefore dwelled among the facts of nature, neither too far from the facts nor too close to them. He did not invent theories; he merely sensed them as the rays of wisdom emanated from matter and fell straight upon his mental retina.[4]

What Bacon requested of posterity is that his readers should believe in absolutely nothing but his methodology. Three centuries later, Wittgenstein re-discovered this

[4] In Bacon's view facts seen by the innocent eye are also theory-laden! That theory, however, is true and accessible by induction as a natural process. Spedding was thus very far from jesting when he suggested that all Bacon wanted was no more than a pair of intellectual spectacles; *Works*, 3, 513. Odd as this sound to us now, it is in tune with the philosophy of nature of the time, as was the (Kabbalistic) demand for humility. More seriously, the current devastating critique of the search for pure data impregnated with theory does not touch Bacon's inductivism. This again shows how brilliant were his *aperçus* despite the drabness of most of his texts that Ellis so sadly complained about.

idea. Proclaiming that philosophy is not a theory but a useful activity that he invited people to join him in the act (Wittgenstein 1922, §4.112: "Philosophy is not a body of doctrine but an activity. … Philosophy should [!] make clear and delimit sharply the thoughts which otherwise are, so to speak, opaque and blurred"). Both asked nothing for themselves, but they did ask: Bacon asked posterity not to lay aside his theories without reasons, in spite of the fact that other theories must go. He admitted the possibly that his theories are anticipations—although he still had high opinion of them. He only wanted that his followers should be critical towards him, as he taught them to reject his ideas if indeed they are anticipations. What Wittgenstein asked his followers is too cloudy to state, let alone discuss.

What decides whether a theory is anticipation or proper induction? Bacon gave no reply to this question. (David Brewster called his method oracular; Ellis said he had none.) The following quotation renders an answer to this question unnecessary. At the outset of the Preface, we remember, everything was satisfactory. Towards the end doubts started to creep in. A short "Plan of the Work" that succeeds the Preface makes this clear:

> I include … such things as I myself discovered … not however according to the true method of interpretation … but by the ordinary use of the understanding in inquiring and discovering.

This must be a tremendous disappointment. But the worst is yet to come:

> For besides that I hope my speculations may in virtue of my continual conversancy with nature have a value beyond the pretension of my wit, they will serve in the meantime as wayside inns, in which the mind can rest and refresh itself on its journey to more certain conclusions. Nevertheless I wish it to be understood in the meantime that they are conclusion by which (as not being discovered and proved by the true form of interpretation) I do not at all mean to bind myself. Nor need any one be alarmed at such suspension of judgment, in one who maintains not simply that nothing can be known, but only that nothing can be known except in a certain course and way, and yet established provisionally certain degrees of assurance, for use and relief until the mind shall arrive at knowledge …

The idea that today's best theories comprise but "a wayside inn" is very nice. Einstein said it of his own best theories. Yet it is contrary to almost everything else Bacon wrote with the exception of the one that Ellis considered admission of bankruptcy. Nevertheless, there is a great novelty to Bacon's assertion that he did not bind himself to his own ideas, that he suspended judgment about them. He even introduced a new term: "certain degrees of assurance"—although only provisionally.

Now Aristotle had already introduced the idea of induction as provisional (since in his view definitions are best). Bacon's idea here is different. First, we must "lay aside received opinions" and "suspend judgment". The flagrant inconsistency with which Bacon took the liberty to speculate on the flimsy ground that he did so "in virtue of my continual conversancy with nature" did not upset his followers. They took it as admonition: first work hard at the laboratory and then perhaps you may win the right to offer a small speculation. This was a rule for scientific publications: the end of an empirically rich paper may include a sentence or two of speculation. Speculation must come after observation; otherwise, it is a prejudice. As Bacon put it in the Preface and the Plan that ends in the following declaration.

> And all depends on keeping the eye steadily fixed upon the facts of nature and so receiving their images simply as they are.
>
> For God forbid that we should give out a dream of our own imagination for a pattern of the world; rather may He graciously grant to us to write an apocalypse or true vision of the footsteps of the Creator imprinted on his creatures.

Another surprise awaits readers in the part of the Preface that the Plan describes as strictly confined to "facts as they are". In the section called "The rule of the present history", Bacon said (*Works*, 5, 136):

> Speculations, and what may be called rudiments of interpretation concerning causes, are introduced sparingly, and rather as suggesting what the cause may be than defining what it is.

Definitions, we remember, are verified theories. The expression "rudiments of interpretation" is new (in all of Bacon's texts "interpretation" is a synonym for "induction"). As this passage, according to Ellis, was written after the *Novum Organum* (within the last 5 or 6 years of Bacon's life), it may have remained unexplained, like many other of his terms, because of his sudden death. This however is clear: Bacon started by claiming that there is no bridge over the gulf between doubt and certainty. This text introduces the bridge. It enters the stage with the aid of two new terms: "degrees of assurance" and "rudiments of interpretation". Is the First Vintage then anticipation or is it a rudiment of interpretation? It does not matter much; what matters is his demand to introduce hypotheses very sparingly. This had a tremendous influence and commentators took it as the reconciliation between Bacon's two versions of induction—without the First Vintage and with it. For this, all that they have to do is to ignore Ellis' arguments that thus remain unnoticed. Now, why do people try to reconcile seeming contradictions in a text? Evidently, because they wish to understand it, to find a solution to the problems which it presents. Indeed, the problems of methodology are difficult and important. Bacon claimed to have solved them. Enthusiasts for science were determined to find his solutions and to apply them. But first they had to interpret his texts as consistent. Bacon has offered two different solutions. He did not contrast them, although the contrast is obvious. Its removal would not change the logic of the situation: readers still have to choose between the two.

Bacon knew this situation and he addressed it, but while carefully avoiding the contrast. This amounts to a double game. It was this. If the magic formula exists, I taught the way to apply it, even though I did not know how to apply it. If the magic formula does not exist, I taught people to be slow and careful, to be unprejudiced, to observe and to discover.

This is not a fair game, but it shows real concern for teaching people something about discovery—its method and its significance. And this is to Bacon's credit and it releases him of the charge of imposture that some commentators repeatedly charged him with. He was after the method of discovery, he tried, and he erred; he tried again and he erred again. He lost his bearings and recognized, however faintly, that he was confused and ignorant, and that his confusion was dangerous to science. This is manifest in his most quoted saying (*Novum Organum*, 2, Aph. 20),

> truth emerges from error more easily than from confusion

that patently clashes with all that he said against error. It is, indeed, the best refutation of Bacon's doctrine of prejudice. Of course, there is a grain of truth in that doctrine—when applied to the prejudiced and the narrow-minded, etc. Hence, truth emerges from error only when the critically minded apply themselves to criticizing the error. Otherwise it may remain unscathed for millennia.

The saying is probably true. And the greatest confusion that Bacon engendered for generations results from the impression that the First Vintage is a parenthesis in the main method although the main method leaves no room in science for trial and error. By adding the First Vintage as a parenthesis, Bacon gave the impression that he did allow some trial and error, but not as science—only as a preamble to it. For three centuries he induced methodologists to think that error is pernicious because it precludes unerring science. Why did people accept this idea? Why did they accept his limitations on the imagination and on the freedom to err?

7.5 The Origin of Scientific Theories

Ellis contrasted Bacon's first and second versions of his methodology and viewed his transition from the first to the second as an admission of bankruptcy, no less. He saw the permission to the intellect that heralds the second version a violation of Bacon's exhortation to avoid making any hypothesis and his hypothesis, his First Vintage, evidence that he had no idea about induction. Ellis' interpretation is open to a very strong objection: it is pedantic and extremely exaggerated. Even though Bacon did contradict himself, methodologically it is easy enough to reconcile the two versions of his methodology, and perhaps commentators should reconcile them. One may argue that the two versions of the theory have much more in common than in contrast; we may thus judge them complementary rather than contradictory.[5]

The argument for the reconciliation of Bacon's two versions is inductivist, offered on the supposition that science is inductive. It may run this way. Bacon's ideal solution is to allow no imagination to enter into research. Suppose while discussing induction he developed a fear that by concentrating on the important task of blocking anticipation researchers will neglect the major task of theorizing. Suppose that this made him change his mind just a little bit and decide to allow some small risk-taking, to allow the first attempt in the hope that in spite of the risk all would go well, especially as he provided some tools and intended to provide still more tools

[5] This may be the theory of eliminative induction. To repeat, a few commentators attribute it to Bacon. By it, researchers refute and thus eliminate from the set of competing explanations, one option after another and thereby they raise the probability of the remaining options. This may fit the second version of Bacon's induction—uncomfortably, though. This matters little: the mix of Bacon's two versions had a great influence in practice; eliminative induction is but a commentator's nicety.

to minimize the risk. For, one may argue, arriving at a theory, a researcher cannot immediately be sure that it is the product of induction rather than of anticipation. Examining this should then remain for a later stage.

This way of reconciling Bacons two versions seems a reasonable way out. It is a possible interpretation of Bacon's change of mind and it is not clear whether it accords with Ellis' interpretation or opposes it. (Ellis suggested that Bacon had no idea as to what the process of induction is. On this reading Bacon followed Aristotle on induction, except that he suggested it is a controlled process.) Clearly, common to both versions of Bacon's methodology is the repudiation of freely imagined theories plus the endorsement of theories drawn from facts. Most of Bacon's followers tried repeatedly to derive theories from facts. Einstein said, (Einstein 1933, 17th paragraph),

> The natural philosophers of those days were … possessed with the idea that the fundamental concepts and postulates of physics were not in the logical sense free inventions of the human mind, but could be deduced from experience by "abstraction"—that is to say, by logical means. A clear recognition of the erroneousness of this notion really only came [in 1917] with the general theory of relativity …

Einstein considered inductivism before 1917 reasonable. And it is of course what the two versions of Bacon's theory of induction share: the idea that the right way to do research is abstracting theories from facts. Possibly Bacon could claim that the difference between the two methods—of induction and of anticipations—concerns abstract concepts (they declare concepts inferred from facts or free inventions respectively). Were Bacon able to argue this way, it would be possible to reject the claim of Ellis that granting permission to the intellect and to perform the First Vintage is an admission of bankruptcy. For, however dangerous the First Vintage is, Bacon could then declare that the researcher's familiarity with very many facts of nature is the source of the permission to conjecture. After all the two versions of Bacon's theory of induction are almost indistinguishable—at least by comparison with Einstein's view that "conceptual systems are logically entirely arbitrary" and are judged only late in the day by their accord with experience and by the paucity of their assumptions (Einstein 1949, 13).

The discussion here, to repeat, concerns the view that Bacon's two version of induction are sufficiently close to each other, especially in comparison with Einstein's view. This sounds reasonable. One rather obvious reason speaks against it. In the style common in the Middle Ages, Bacon stressed that without exception all theorizing is by induction; the difference being between the common and the new, the fast that is invalid and the slow that is valid (*Novum Organum*, 1, Aph. 19):

> There are and can be only two ways of searching into and discovering truth. The one soars high from the senses and particulars, to the most general axioms … and this is the way now in fashion. The other derives axioms from the senses and particulars, rising by a gradual and unbroken ascent, so that it arrives at the most general axioms last of all. This is the true way, but yet untried.

Bacon admitted that even those who speculate wildly must "throw a glance or two at facts", that even anticipations draw their inspiration from the outside because all our abstract concepts and theories are formed *a posteriori*—formed after some information is absorbed and processed. Even Bacon's use of the concepts "analysis" and "abstraction" shows this: the difference between the "abstraction" of the accepted method and the "analysis" of his new method is in analysis being slow and careful. (*Novum Organum*, 1, Aph. 60; cp. *op. cit*., 2, Aph. 40). The difference between Bacon's "yet untried" method and the one "now in fashion" is a matter of degree: anticipations are over-hasty abstractions. No abstract concept is possible to imagine before any experience. The following passage shows this adequately (*Works,* 2, 654):

> Imagination I understand to be the representation of an individual thought. Imagination is of three kinds: joined with the belief of that which is to come; and of things present: for I comprehend in this imagination feigned and at pleasure, as if one should imagine such a man to be in the vestment of a Pope, or to have wings.

The three kinds of imagination depend on the three tenses, concerning the past, the present, and the future. They all concern facts, not theories. This is why Ellis spoke to the point when he offered his criticism of Bacon by reference to the fact that the advancement of science increases the theoretical character of observed facts (*Works*, 1, 61). Bacon took for granted that observing things as they are is quite possible. A debate on this took place between the two World Wars among Wittgenstein's disciples in Vienna. As it happened, it gained great notoriety because Wittgenstein—and his disciples—declared that philosophical disputes are impossible. They are quite possible, but at times discussants close them. As to the debate on the matter of pure observation unadulterated by theory, it is indeed closed. The fashionable view today is that there are no pure factual observations. This consensus took centuries to reach. (Philosophers of science regularly overlook it when speaking of the probability of hypotheses given factual data, but let us overlook this.)

The trouble over theories was bigger: how are they ever applicable? This was a serious traditional difficulty, and it was very confusing until twentieth-century logic and methodology cleared it up. The received assumption, that all possible information is empirical to some extent, even the products of the wildest imagination, that once looked so obvious, made theories into objects from an altogether different universe of discourse. This is hard to envisage today: the situation is viewed now as too vague and as hardly relevant to the problem-situation of the philosophy of science. The context in which it took place is the discussion of true knowledge, and the claim that it is not empirical but due to intuition, acquired in a trance of sorts. (This was the received opinion in the wake of Plato and Aristotle.) This has greatly changed, of course, but let us ignore the modern changes. Suppose then that true knowledge is possible. How can it at all apply to the world of experience? It would be most unfair to blame Bacon for this difficulty: he could not possibly solve it. He was aware of it, however, and used it as an argument for his methodology and against the dialectical method (*Novum Organum*, 1, Aph. 24).

> It cannot be that axioms established by argumentation should avail for the discovery of new works, since the subtlety of nature is greater many times over that the subtlety of argument. But axioms duly and orderly formed from particulars easily discover the way to new particulars, and render science active.

Why should this argument hold against the dialectic method and not against the inductive method? If Bacon arrived at his doctrine of induction by induction, then he would have demonstrated his doctrine (by empirical means). This is not the case, since he admitted that induction had never been attempted. If it is no induction, then it is anticipation, and so it should be set aside. It seems that Bacon, who often applied ideas to themselves, had exactly this question in mind when he wrote (*Novum Organum*, 1, Aph. 35),

> I would have my doctrine enter quietly into minds that are fit and capable of receiving it; for confutations cannot be employed when the difference is upon first principles … and even upon the forms of demonstration.

This is a bit cheap, but to complain about it is unfair as the question is real and Bacon was not the first to offer question-begging answers to it.[6]

Let us admit for now that there are two methods, differing only in that the old one that is hasty and the new one that is gradual. Let us ignore the inconsistency of this admission being non-empirical and let us return to our two questions. First, is the First Vintage anticipation or induction? Second, how important (from Bacon's point of view) is the gap between Bacon's two versions of induction? How serious an offence (from his own point of view) did he commit against his own canons, when he left strict induction to allow for his First Vintage "the ordinary use of the understanding in inquiring and discovering"? Since the ordinary method, he explicitly says, is no proper induction, he gave up everything, even if he did it in virtue of his "conversancy with nature". Ellis' conclusion is thus hard to avoid.

Let us return then to the inductivist proposal to reconcile the two versions of Bacon's induction and lay emphasis on the demand that scientific theory is abstracted from facts rather than "feigned at pleasure". Different items appear here and their interrelations are not too clear: hypotheses, laws of nature, concepts, definitions, and forms. What is induction about? Hypotheses are putative laws of nature. How statements of laws of nature relate to concepts is not too clear. Concepts are supposed to be introduced by definitions, and these are certain and so unchanging. The question about the way hypotheses and concepts relate becomes, then, how do hypotheses relate to definitions? This is unclear, since the nature and status of definitions is not simple: traditionally they are both verbal and informative. Modern logic rejects this as impossible. But we are dealing with classical logic here. As definitions are true, can there be hypothetical definitions? I do not know the traditional answer to this question, if there is any. As a definition is both verbal and informative, it was somehow felt that it wants some sort of defense. Yet a certainly true statement wants proof, not defense. This is a mess.

[6] Cp. William Gilbert, *On Magnets*, Book First Chapter 1: … philosophers must be made to quit the sort of learning that comes only from books, and rests only on vein arguments from probability and upon conjecture … they waste oil and labor, because, not being practical in the research of objects of nature, being acquainted only with books, being led astray by erroneous physical systems, and having made no magnetic experiments they constructed certain ratiocinations on a basis of mere opinions, and old womanishly dreamt of things that were not.

Bacon endorsed as a matter of course Aristotle's view of the essences or the forms that definitions describe. This is the subject of many current studies that seem to me to take this aspect of his works much too seriously. Suffice it that he viewed them as laws of nature and so the word "theory" should suffice for discussions of Bacon's methodology. Thus, the whole problem-situation just presented becomes hardly relevant here. It shifts the discussion from the problem of truth to the problem of origin. This makes researchers look backwards instead of forwards. We look back and are mystified: how did researchers create their theories? What was right that led them to their successes and what was wrong that led them to their failures?

We may use a rubber-stamp reply to this question. We need not investigate the history of an idea; we know *a priori* that those who were successful derived their ideas from facts of nature and the others offered formulas that were too abstract. To this Voltaire had the right response (Voltaire 1764, 122):

> Strange! We know not how the earth produces a blade of grass; how a woman conceives a child; and yet we pretend to know how ideas are produced!

This criticism is still valid, except that in the meantime we have learned something about conception and about the way grass grows, so that our ignorance about the origin of ideas is now relatively greater. Yet inductivists maintain that our ideas come from the facts. The problem of the origin of ideas is very interesting from a psychological point of view but not from a methodological point of view. All we need to agree upon from the methodological point of view is that we shall go on trying to examine our theories critically and that we shall go on seeking new theories relevant to the problems we wish to solve. As Poincaré has argued (Poincaré 1914, 52), a good idea may emerge out of a good cup of coffee. As Russell has put it (Russell 1914, §1, second paragraph),

> It is such considerations that necessitate the harmonising mediation of reason, which tests our beliefs by their mutual compatibility, and examines, in doubtful cases, the possible sources of error on the one side and on the other. In this there is no opposition to instinct as a whole, but only to blind reliance upon some one interesting aspect of instinct to the exclusion of other more commonplace but not less trustworthy aspects. It is such one-sidedness, not instinct itself, that reason aims at correcting.

This way Bacon's doctrine of prejudice loses its force; and then so does his view of induction. Wherever researchers receive their ideas from matters not. Today even ardent inductivists agree about this. This renders Bacon's teachings obsolete. This obsolescence was inbuilt.

7.6 Science and Imagination

> We must not add wings but weights and lead, to the intellect, so as to hinder all leaping and flying.
>
> (Bacon, *Novum Organum*, 1, Aph. 104)

> The imagination impatient to arrive at causes, takes pleasure in creating hypotheses, and often it changes the facts in order to adapt them to its work; then the hypotheses are dangerous.
>
> (Laplace 1951, Ch. 17. 183)

> Sir, I am persuaded that had Sir Isaac Newton applied to poetry, he would have made a very fine epic poet.
>
> (Dr. Johnson, in Boswell 1785, 27)

Dr. Johnson's *Rasselas* has a story about an astronomer who in order to relax indulged in day-dreams about dominating the weather. One day, as he was day-dreaming, he commanded a cloud to rain. It did. Using his hypothesis, he calculated how long it would take the cloud to follow his command. The calculated lapse of time agreed well with the observed one. It might have been accidental, but he came across further confirmations. Worst of all, just when he was becoming convinced that he was right, he found out on the basis of his hypothesis that he could not change the total amount of rain. As all he could do was to divert rain and as this amounted to robbery, he concluded that never again might he exercise his control over the clouds. Thus, commented the wise hero of Dr. Johnson's book, Rasselas was in constant danger of subordinating his great power of judgment to his forceful imagination, which is insane. In a way, the hero maintained, we are all slightly insane, mixing in our judgments some imagination instead of acting on the exact measure of evidence of facts. Imagination, he observed, sounds harmless at first, as long as we keep it far away from our reasoning; but, he concluded, it is potentially dangerous: one day we shall find ourselves unable to detach it from our rational thinking. The hero's companions are convinced. A young woman in the entourage promises never more to dream that she is a queen; the princess promises no more to imagine that she is a shepherdess. This story exhibits Bacon's doctrine of prejudice the way early twentieth-century psychological novels often exhibit psychoanalysis.

This is but one example that illustrates the influence of theories. It also presents a problem, and one that troubled Dr. Johnson's scientific contemporaries: do researchers imagine, may they imagine, and if they may or have no choice but to imagine, whether and how their imagination has closer contact with reality than that of Dr. Johnson's astronomer.

It seems strange to wish to draw as sharp a line as possible between science and poetry, a line as sharp as possible between science and imagination. The problem behind this is real and even insoluble: is research with no hypotheses possible? Laplace's *System of the World* is an eminent effort to present Newton's astronomy without reference to any hypothesis. After Einstein, this seems obviously wrong. The success of Newton's mechanics, however, suggested that it is absolutely true. And it was—still is—very hard to understand how the imagination of the very same kind that led Dr. Johnson's astronomer astray could possibly have led Newton to his great discoveries. Even Hume, by consensus the greatest skeptic of the Newtonian era, expressed (Hume 1793, 122) his full conviction that "the severest scrutiny which Newton's theory has undergone, proceeded not from his own countrymen, but from foreigners, and if it can overcome the obstacles which it meets with at present in all parts of Europe, it will probably go down triumphant to the latest posterity."[7]

The alleged certainty of Newton's theory was not a result of imagining principles of a fictitious character (to use Einstein's idiom). But this assumption was awkward. It was well known that Newton did employ his power of imagination, as can be seen, for example from the praise that he won for his having thrown away the ladder (of the imagination or of hypotheses) after having climbed it, or for having taken away the scaffoldings (of the imagination or of hypotheses) after having erected his magnificent structure. In short, the standard presentation of induction was of it as the method a researcher should follow, but not necessarily the method that the researcher has followed. One can get away with violating it. In that case, what made a theory inductive was not its case history but the alleged fact that it was possible to present it as if it evolved inductively. Inductivists labeled this "rendering a theory inductively legitimate". A hypothesis "feigned at pleasure" (to use Bacon's derisive expression) is dangerous, misleading, prejudicial, illegitimate. A theory may admittedly be initially illegitimate: its natural parent is the imagination; it is possible to legitimize a theory by adoption: its foster-parent is induction.

As Newton suggested when he disclaimed scientific status to his speculations (*Principia, Scholium Generale*), if we do not mind whether we deem an idea scientific or not, we need no justification for it. Consider then the distinction between the context of discovery and the context of justification that Hans Reichenbach borrowed from William Whewell. Traditionally, the discussion of legitimation was very tense: publishing an error in the scientific press appeared then as an unforgivable sin (despite Bacon's adage about truth emerging quicker from error than from confusion and notwithstanding Newton's suggestion that one may publish speculations without claiming for them scientific status); Whewell bravely opposed the demand for justification for a publication; Reichenbach took this for granted and yet he endorsed the received opinion, thus performing the traditional function of inductivism (not as providing a new theory *à la* Bacon but) as justifying an existing theory *à la* Newton. Instead of viewing induction as a way of justifying theories we may view it as a way of demarcating science from fiction, metaphysics, poetry, and even (*pace* Einstein) from the arbitrary products of the imagination. Today we follow this and take the discussion of legitimation lightly, as (*pace* Bacon and Wittgenstein and the cohorts of their followers) we have no taboo on the imagination.[8]

The problem of the role of the imagination in science, the problem of how our free imagination and intuition lead to a better knowledge of reality, is not easy to solve. It was made more difficult by the unquestioned endorsement of the ancient theory that science is knowledge rather than opinion. There was a great opportunity

[7] Hume considered foreigners less prejudiced in favor of Newton. His skepticism was not a matter of disbelief but of logic. Nevertheless, notice that his declaration of a triumph is conditional—just to be on the safe side?

[8] A once famous adventurer, Thor Heyerdahl, viewed his research that way (Heyerdahl 1958, concluding chapter). He reported a discussion with his Aku-Aku (fairy godmother), in which he had confessed his sin of anticipation and obtained absolution, since his hypothesis is true.

for overthrowing this theory during the Renaissance, after the overthrow of Aristotle's theory, especially since its heir, Descartes' theory, survived for less than a century. This opportunity faded when Newton endorsed Bacon's ideas in his own free interpretation and gave them the prestige of his own natural philosophy. His expression "*Hypothesis non fingo*"—I invent no hypothesis—reverberated throughout the age of the absolute rule of his theory over the physical sciences. Was it then forbidden to produce a conjecture or merely to declare a conjecture scientific? As in any myth system, the answer is indeterminate.

Here are some comments by Augustus De Morgan, who partly endorsed the ban on conjectures, and by David Brewster who rejected it. De Morgan said (1915, 1, 79),

> To us, Bacon is eminently the philosopher of error prevented, not of progress facilitated. When we throw off the idea of being led right and betake ourselves to that of being kept from going wrong, we read his writings with a sense of their usefulness, his genius, and their probable effect upon purely experimental science, which we can be conscious of upon no other supposition. It amuses us to have to add that the part of Aristotle's Logic of which he saw the value was the book on refutation of fallacies.

De Morgan noticed the two versions of Bacon's teaching and he rejected only the early one. In this he followed all earlier inductivists; he deviated from tradition only in the interpretation of Bacon's texts, not in methodology. That researchers employ their imagination was always common knowledge, and researchers always strove for the ideal of eliminating it. De Morgan's view differs from the first version of Bacon's teaching not in direction but in being less extremist; he was as moderate as the Baconian tradition was, and as Bacon himself became in his second version, towards the end of his life. This is so in spite of De Morgan's recognition that Bacon had put the problem the wrong way as he always confused the scientific and the juridical sense of the word "judgment". The above quotation from De Morgan continues thus:

> In the case before the court, generally speaking, truth lurks somewhere about the facts, and the elimination of all error will show the truth in the residuum. The two senses of the word "law" come in so as to look almost like a play upon words. The judge can apply the law as soon as the facts are settled. The physical philosopher has to deduce the law from the facts. Wait, says the judge, until the facts are determined. … Wait, says Bacon, until all the facts, or all the obtainable facts, are brought in: apply my rule of separation to the facts, and the result shall come as easily as by rulers and compasses. … It seems to us that Bacon's argument is, there can be nothing of law, but what must be either perceptible, or mechanically deducible, when all the results of law, as exhibited in phenomena, are before us. Now the truth is, that the physical philosopher has frequently to conceive law which never was in his previous thought—to educe the unknown, not to choose among the known. …

This is De Morgan's assertion (and Macaulay's) that Bacon's induction is induction by elimination; this induction, he added (contra Macaulay), is not and cannot be a theory of discovery. This is insightful.

Much as De Morgan disagreed with Bacon's juridical view, his own view is similar. We must judge the theories this or the other way, or else we indulge in free and unfettered imagination and thus lose contact with reality: science cannot be as free as poetry. De Morgan had no better solution to the problem of demarcation of science from poetry. He did not know how facts limit the imagination. De Morgan's view is nearer to the second version of Bacon's doctrine than he admitted, except for this: the second version is only a parenthesis. No Bacon text justifies De Morgan's characterization of his philosophy as conceived by a person with the mentality of lawyer[9]; Ellis' characterization of him as a blunderer is better. Consider Bacon's short intellectual autobiography *Interpretatione Naturae Proenium*. It is a piece of fantasy. Spedding considered it one of his most candid works. He was candid, of course (like Dr. Johnson's astronomer). He declared,

> I found that I was fitted for nothing so well as for the study of truth; as having a mind nimble and versatile enough to catch the resemblance of things (which is the chief point), and at the same time steady enough to fix and distinguish their subtle differences, as being gifted by nature with desire to seek, patience to doubt, fondness to meditate, slowness to assert, readiness to consider, carefulness to dispose and set in order; and as being a man that neither affects what is new nor admits what is old, and that hates every kind of imposture. So I thought my nature has a kind of familiarity with Truth.

De Morgan is right: Bacons main self-selected task was the avoidance of error, keeping the eye steadily fixed upon the facts of nature and simply receiving them as they are. Scientific theory, Bacon taught, will come in due course to the diligent and patient who do not try to rush Nature: to facilitate progress is dangerous anticipation. And this is exactly why the whole impact of Bacon's philosophy is lost when reading him without noticing that his major point is his doctrine of prejudice. This is why almost all of Bacon's twentieth-century commentators misread him.

The introduction to *The Great Instauration* presents all attempts at learning as either inductive and scientific or as fanciful and prejudiced. Bacon noted that the Copernicans had "made passage for their own opinion … taking it all for open matter and giving their genius full play" and he considered this a prejudice. What did his followers say about this? Did they consider Copernicus prejudiced? The following passage from Brewster has discussed this in his powerful debunking of Bacon (Brewster 1831, Ch. 19):

> … Bacon was a man of powerful genius … The necessity of experimental research, and of advancing gradually from the study of facts to the determination of their cause, though the groundwork of Bacons method, was a method not only inculcated but also successfully followed by preceding philosophers. … and while it is admitted that even they had thrown off the yoke of the schools, and had succeeded in experimental research, their credulity and their pretensions have been adduced as a proof that to the "bulk of philosophers" the method of induction was unknown.

The received view that Brewster opposes is that the early "bulk of philosophers", Bacon's illustrious predecessors, had retained remnants of the scholastic modes of thought: not acquainted with Bacon's doctrine of prejudice, they were credulous. Brewster rejects this allegation:

> The fault of this argument consists in the conclusion being infinitely more general than the fact. The errors of these men were not founded on their ignorance but on their presumption.

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[9] John Aubrey records that Harvey admired Bacon's "wit and style, but would not allow him to be a great Philosopher. Said he to me, He writes Philosophy like a Lord Chancellor, speaking in derision; I have cured him" (Aubrey 1680, on William Harvey).

> … had they lived in the present age, their philosophical character would have received the same impress from the peculiarity of their temper and disposition.

Brewster's description of Bacon's doctrine of prejudice is valuable testimony. It shows that his (Brewster's) contemporaries who were Bacon's apologists ascribed it to Bacon. He (Brewster) rejected it, declaring Bacon's psychology of discovery irrelevant and his methodology an erroneous description of the process of discovery:

> Whatever this process may be in details, if it has any, there cannot be the slightest doubt that in its generalities, at least, it is the very reverse method of induction. The impatience of genius spurns the restraint of mechanical rules, and never will submit to the plodding drudgery of inductive discipline. The discovery of a new fact unfits even a patient mind for deliberate inquiry.

The impatience that Bacon's illustrious predecessors exhibited, says Brewster, is quite unavoidable.

> Conscious of having added to science what had escaped the sagacity of former ages, the ambitious spirit invests its new acquisition with an importance which does not belong to it.

The ambition and pretension of a researcher are integral parts of the process.

> He imagines a thousand consequences to flow from discovery: he forms innumerable theories to explain it and he exhausts his fancy in trying all its possible relations to recognized difficulties and unexplained facts.

This passage of Brewster's summarizes the celebrated study of Kepler's researches that William Whewell published but 6 or 7 years later (Whewell 1837, Bk. 5, Ch. 4). Whewell's view is much more intricate than Brewster's, but the break-through, the transcendence of Bacon's theory, is already in Brewster's life of Newton: discovery is the fruit of the imagination checked by empirical evidence:

> The reins … freely given to his imagination are speedily drawn up. His wildest conceptions are all subject to the rigid test of experiment, and he has thus been hurried by the excursions of his own fancy into new and fertile paths, far removed from ordinary observation. Here the peculiar character of his own genius displays itself by the invention of new methods of trying his own speculations, and he is thus often led to new discoveries far more important and general than that by which he began his enquiry. For a confirmation of these views, we may refer to the history of Kepler's discoveries; and if we do not recognize them to the same extent in the labors of Newton, it is because he kept back his discoveries till they were nearly perfected, and therefore withheld the successive steps of his inquiries.

Brewster hit the doctrine of prejudice in its weakest spot: no imagination, no science. His criticism is correct, yet to inductivists it is not very convincing. They knew that Newton had used his imagination, tested its fruits, and discovered. What they admired in Newton is the elimination of the imagination and the presentation of its fruits as general facts (i. e as generalizations of statements of observed facts). They did not know, perhaps, quite how a piece of imagination happens to be a true theory. But they felt that if we start with premature imaginings we shall never arrive at new facts; we will then be swept along by fancies instead. Brewster's attack considers inductivism as a method, while inductivists, when pushed to the wall, always admitted that inductivism is less of a method and more a justification, an epistemological criterion. Induction does not so much describe how Newton arrived at his theory, but why this theory was generally received. Brewster's explanation of the history of Newton's presentation of his theory is incomplete. It is necessary to add to it that Newton held back his theories until they were as perfect as he could make them, because he wanted to eliminate all imagined hypotheses from science to "render them legitimate induction". No statement of Newton is more famous than his "hypothesis non fingo", I do not feign a hypothesis. This exclamation appears in the end of his *Principia*, together with a most daring speculation. Hypotheses, he contended, have no room in experimental philosophy but they are otherwise not taboo. This Newton stated more extensively in the end of his *Opticks*:

> As in Mathematicks so in Natural Philosophy, the Investigation of difficult Things by the Method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy.

In this passage, Newton contrasts the method of analysis with the method of feigning hypotheses at pleasure. Brewster could not bring himself to criticize Newton and so he glossed over his disagreement with Newton that this passage clearly displays. This was very detrimental to his cause. By Analysis Newton (like Bacon) meant here slow and timely abstraction, seeking the more important statements of fact to generalize. Newton admitted that he did not succeed to eliminate arbitrariness fully. He claimed that this method is the best. In his view the rule of method is, be careful, discipline yourself, keep your imagination under control; confine your belief to the more probable view; in Brewster's view the rule is, be reckless and feign hypotheses at pleasure and then test them severely. By Brewster's testimony, Newton's methodology was the most popular. If additional evidence is called for, then here is Hume's popular comment on (Boyle's and) Newton's achievements (Hume 1775, 265):

> … there flourished during this period a Boyle and a Newton; men who trod with caution, and therefore with more secure steps, the only road which leads to true philosophy.

This "road to true philosophy" is the road of caution. And caution is distrust of one's imagination. Hume was so determined on this that he did not hesitate to censure the great Boyle for his adoption of the mechanical philosophy:

> Boyle was a great partizan of the mechanical philosophy; a theory which, by discovering some of the secrets of nature, and allowing us to imagine the rest, is so agreeable to the natural vanity and curiosity of men. …

Contrast this with the relevant part of his eulogy on Newton:

> In Newton this island may boast of having produced the greatest and rarest genius that ever arose for the ornament of instruction of the species. Cautious in admitting no principles but such as were founded on experiment; but resolute to adopt every such principle, however new and unusual: From modesty, ignorant of his superiority above the rest of mankind; and thence, less careful to accommodate his reasoning to common apprehensions: More

> anxious to merit than acquire fame: he was from these causes long unknown to the world; but his reputation at last broke out with lustre, which scarcely any writer, during his own life-time, had ever before attained. While Newton seems to draw off the veil from some of the mysteries of nature, he shewed at the same time the imperfections of the mechanical philosophy; and thereby restored her ultimate secrets to that obscurity, in which they ever did and ever will remain.

Hume emphasizes that caution is important: a little boldness is permissible, provided it does not lead us astray, provided it merges with inductive caution. A theory founded upon experiment should be the result of inductive generalization and stand up to tests—past as well as future: a truly inductive theory must be certain.

Back to Brewster. He suggested the idea that a freely imagined hypothesis that has stood up to tests will stand up to all further tests. This idea is not obvious in the least. We do not need Hume's analysis to see that past success does not promise future success: Bacon hammered this in all of his writings. So the next step that Brewster took was to justify this idea. Historians now ascribe Brewster's justification to Whewell—perhaps because he was the methodologist who won the greatest authority in this matter. (His methodology seems to have influenced many researchers and many of them quoted his *History of the Inductive Sciences*.) The idea is that observations of facts include the interpretation of the facts in the light of theories. Thus, the restriction on imagination turn into the hope that tests transform the most arbitrary imagination into the most compelling conclusion: that having stood up to tests renders theories verified.

A strong protest against this view came from Maxwell, who commented on his discussion of atomism thus (Maxwell 1890, 1, 419):

> The method which has for the most part employed in conducting such inquiries is that of forming an hypothesis, and calculating what would happen if the hypothesis were true. If these results agree with the actual phenomena, the hypothesis is said to be verified, so long, at least, as someone else does not invent another hypothesis which agrees still better with the phenomena.

This is ironical: the said-to-be-verified-until-a-competitor-appears is the merely-allegedly-verified, not the truly-verified. Maxwell continues thus:

> The reason why so many of our physical theories have been built up by the method of hypothesis is that the speculators have not been provided with methods and terms sufficiently general to express the result of their induction in its early stages. They were thus compelled either to leave their ideas vague and therefore useless, or to present them in a form the details of which could be supplied only by the illegitimate use of the imagination.

This is a repeat of the received opinion: prior to proper induction, the use of the imagination is illegitimate. Maxwell continues:

> In the meantime mathematicians, guided by the instinct which teaches them to store up for others the irrepressible secretions of their own minds have developed with the utmost generality the dynamical theory of a material system.

This is ironical: in his claim that mathematicians use their imagination freely, Maxwell employs derisive overtones that are not to take seriously.

> Of all hypotheses as to the constitution of bodies, that is surely the most warrantable which assumes no more than that they are material systems, and proposes to deduce from the

> observed phenomena just as much information about the conditions of the material system as these phenomena can legitimately furnish.

This is a remarkable allusion to the metaphysics of Descartes: it assumes nothing about bodies except that they are material and thus occupy finite portions of space. Both Kelvin and Maxwell were Cartesians throughout; they tried repeatedly to present Faraday's fields of force as Cartesian.[10] To end the quote from Maxwell,

> When examples of this method of physical speculation have been properly set forth and explained, we shall hear fewer complaints of the looseness of the reasoning of men of science, and the method of inductive philosophy will no longer be derided as mere guesswork.

Maxwell's solution of the problem of induction then is the old one, but with a new twist. Imagination is illegitimate in science. Researchers must use it as sparingly as possible. Assume almost nothing! Then the method of inductive philosophy will no longer meet with derision as mere guesswork. So far, the traditional view. The twist is that for physics Cartesian metaphysics is what the doctor orders.

Maxwell's hopes were shattered as his superb *Matter and Motion* (1877), his very last work, attests. In the introductory chapter there he criticizes Descartes, and adds a note that admits that Newton's theory of space and time is a hypothesis, that to develop a view of laws of nature science assumes the stability of the observed world, and that this "perhaps puts a limitation on any postulate of universal physical determinacy such as Laplace is credited for." In other words, since science is inductive, it is limited, so that, for example, scientific determinism is not scientific (especially in view of statistical mechanics). Later on in that slim volume (§148) he allows for the use of hypotheses with no qualms.

Maxwell was controversial, but after his death his electromagnetism gained appreciation and when Einstein developed his special theory of relativity (1905) it won full recognition. In 1919, we remember Einstein's assertion, freedom to use the imagination was no longer under dispute. In 1920, John Maynard Keynes published his authoritative *A Treatise on Probability* that altered the problem of induction unrecognizably. The problem of induction was no longer the quest for a machine—or an algorithm—that generates theories (given all our empirical information), but rather the quest for a machine—or an algorithm—that selects one theory from a set of competing theories (given all our empirical information). This is pathetic, since at the very least we should speak not of all possible theories but only of those that we know, and even then, there are always variants on extant theories that all researchers deem too leg pulling to consider.[11] Moreover, whenever good luck allows for the presence of competing theories that are equally reasonable, the method of choice between them is the time-honored method of crucial test. (The leg-pulling variants of extant theories are unimaginative, easy to manufacture, and not given to such tests.)

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[10] Maxwell even made an effort to reinterpret Newtonian gravity in his field framework on the way to reconcile Newton with Descartes. He failed, of course, and gave it up.

[11] Alas, Carl Hempel, Nelson Goodman and their fans declared that the theory of induction must present rules for the dismissal of leg-pulling hypotheses. This has set many a commentator on a wild goose chase.

Let me stress, the idea discussed here, that scientific research employs hypotheses, is not new in the least. Bacon declared it the received method and he did not even deny that the great researchers of the past used it repeatedly and as a matter of course. On the contrary, he stressed this and claimed originality and priority only for his prohibition of the free use of hypotheses, only for his doctrine of prejudice. The solid part of that doctrine, let me repeat, is the empirical discovery (priority for which goes to Bacon and to Galileo) that often observations mislead. This discovery led to the discussion as to whether it is at all possible to experience pure observation reports, ones free of all theory, incorrigible ones. Perhaps on the authority of Quine, today most philosophers of science take for granted that such observation reports are impossible. Yet they go on seeking a selection machine for the choice of hypotheses, without bothering to answer Bacon's argument that shows this impossible, that observations polluted by theory led to the choice of the polluting theory.

All this should make amply reasonable Galileo's famous remark (in his first *Dialogue*, Third Day, 1953, 328) that Popper has quoted with great approval (Popper 1962, 102),

> … the experiences which overtly contradict the annual movement are indeed so much greater in their apparent force that, I repeat, there is no limit to my astonishment when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance of the latter, the former became mistress of their belief.

Likewise, Newton's idea that the fixed stars are not fixed was at the time imaginative and incredible. Newton was more daring and imaginative than Dr. Johnson's astronomer, not less. This is obvious, more so because it is nearer to observed facts. Bacon's medieval theory of the imagination is thus false not only as pertaining to science, but also as pertaining to literature and drama. Shakespeare's plays were thus more imaginative than the plots he borrowed, just because they are nearer to reality. One does not draw one's ideas, plots, or theories from mere observations of facts. Plots and theories are both products of the imagination, and subsequent observations and thinking bring them nearer to reality, each in its own way. Bacon's theory of the imagination originated, as its author has confessed, as a theory of art criticism rather than of scientific method. It is by now commonplace that only unimaginative artists and art-critics claim that drawing sphinxes and hydras demands more imagination than drawing real people and real animals, that describing unseen lands is more fanciful than writing a realistic novel about people whom we meet every day. This unimaginative theory still prevails in theories of scientific method, although it has long departed from the territory of art-criticism.

Facts can be surprising, fiction commonplace. The reason for this is, as Galileo observed, that more imagination is necessary to marvel at ordinary facts than to marvel at far-fetched fancies. *Scientia sine arte nihil est.*

Chapter 8
Bacon's Influence

By demonstrative Philosophy
They playnly prove all things are bodyes,
And those that talke of Qualitie
They count them all to be meer Noddyes.
Nature in all her works they trace
And make her as playne as nose in face.

(Ballad of Gresham College, 6, Anon. 1663).

That Bacon's scientific works are of a very low quality is all too obvious. They are presumptuous, vulgar, full a contradictions, obscurantism and scholasticism; admittedly both his inductivism and his doctrine of prejudice are different aspects of the same myth of science manifest. Still, his influence was a major factor in the great awakening of critical intelligence which followed his age, and the strongest factor behind the scientific research carried out by his followers. Bacon did provoke a critical attitude within the field of science. True, his demand for infallibility and his hope of avoiding all error caused much prejudice and grief. But he could not have foretold that his theory would provide such a forceful argument in favor of his myth and that Puritanism would provide such a favorable habitat for it. His influence was different before and after Newtonian mechanics won its great reputation.

8.1 Influence on Immediate Posterity

Nothing testifies more to Bacon's influence than the famous quarrel between the Ancients and Moderns, between the traditionalists and radicals, the quarrel that engaged the western intellectual world of the late seventeenth century. R. F. Jones, the

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_8,

historian of literature and language, documented the tremendous influence of Bacon on the whole of the cultural life in Britain in the seventeenth century.[1]

A most interesting example is the following quotation from the important educationalist, Amos Comenius. He reported that Bacon (Verulam, as he wanted posterity to call him) had made him look at things in a simple manner and accept them as he observed them. Having expressed the hope that Bacon had implanted in him he commented (Comenius 1651, Preface; Jones 1982, 292–3),

> Yet it grieved me again, that I saw, most noble Verulam present us indeed with the true key of Nature, but not open the secrets of Nature, onely shewing us by a few examples, how they were to be opened; and leave the rest to depend on observations, and inductions continued for several ages. Yet I saw that my hopes were not quite left in suspense; … some great secrets of nature … were now plain … For now with a more sound way of Philosophie … I saw and rested in it.

Comenius was disappointed that it would take several generations to open up all the secrets of Nature. For Spedding the picture was the opposite, we may recall: the *naiveté* that Bacon had exhibited in expressing the hope that finding all the secrets of nature might take a few generations disconcerted him tremendously—he ignored the fact that Bacon was much more sophisticated than Comenius and others in this respect. Were Spedding able to tell Comenius how much longer and still more difficult the way to the secrets of nature was than Bacon imagined, who knows if he would not have lost the courage to look at things in all simplicity and "to rest in it" as he put it so charmingly.

Bacon contrasted his doctrine of prejudice with dialectics, we remember. He said, scholasticism shows that people evade criticism and so criticism is insufficient. This may suggest that the unprejudiced should admit criticism with gratitude. This led to a new attitude to criticism that works under Bacon's influence displayed, the earliest among them being Thomas Browne's *Pseudodoxa or Vulgar Errors* (1643) and Joseph Glanvill's *The Vanity of Dogmatizing* (1661) that began a literature on popular errors that survives to this day (McCartney 2010). Although Glanvill's book was a dogmatic recapitulation of Bacon's doctrine of prejudice, it did encourage criticism (Jones 1982, Ch. 4, note 65). The formula for it throughout the Age of Reason was, debate is permissible for one or two rounds; after that, it produces more heat than light. Of course, the doctrine of prejudice reinforced this mode of conduct by inviting parties to a dispute to call each other prejudiced. Nevertheless, now that we have cold light, it should be easier to enhance the cold light of reason and to allow rational debates to be as long as they are exciting. But this is another story. What is particularly relevant regarding Bacon's tremendous influence that has survived to this day is his tremendous authority even on people who have never heard his name: he has entrenches his contempt for hypotheses and more so for debates about them; he inspired a style of debate that he began and that evolved in the Royal Society of London in diverse ways.

[1] Jones 1982. For example, Chapter 3, The Bacon of the Seventeenth Century and Chapter 9, The "Bacon-Faced Generation". See also pp. 115, 256 and 291, note 1 there. See also Jones 1951, esp. p. 69, notes.

The most pertinent document on the period is Bishop Thomas Sprat's famous *History of the Royal Society* (1667) that was semi-official in spite of its obvious serious drawbacks. These are arrogance, chauvinism, fabrication of myths, and wholesale smearing of pretty well everybody save Bacon[2] and the King who had granted a royal charter to the Society. Some historians still cite this worthless document as authoritative.[3]

Example. Sprat's book alludes to Copernicus but merely as a dogmatic reviver of an ancient school. The first part of Sprat's book is a short history of human knowledge. It is a version of Bacon's *Advancement of Learning*. It displays an embarrassing hero-worship of Bacon. In the letter of dedication to the Society, Sprat compares Bacon to Moses. He also says (p. 35),

> … I shall mention onely one great man who had the true Imagination of the whole extent of his Enterprise, as it is now set on foot; and that is Lord Bacon. In whose books there are every where scattered be at arguments, that can be produc'd for the advancement of Experimental Philosophy; and the best direction to promote it.

Another example. Speaking of the attitude of the Society towards Bacon, Sprat says (p. 153),

> Whatever they have hitherto attempted, on these principles, and incouragements, it has been carried on with vigorous spirit and wonderful good spirit …

The book includes some papers given at the Society. These come to illustrate his suggestion, or his mere hint, that the Society had contributed to human knowledge in the 6 years of its existence more than all others put together did in 6,000 years (since Creation), an observation that provoked the hostility of the universities.

Sprat displays a sense of inferiority towards the other nations of Europe. (Their Renaissance came earlier.) He expresses contempt for all English literature, in which there is nothing worth reading twice save the works of Chaucer. (If he did not know of Shakespeare, which is rather unlikely, he knew of Ben Johnson and of John Milton.) Yet he couples all this with a strong desire to belittle as much as possible the achievements of all other nations and to praise as much as possible all domestic

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[2] (Marie Boas Hall 1991, 10):

> … many of the early Royal Society saw themselves as dedicated Baconians. Bacon figures centrally in the frontpiece to the semi-official *History of the Royal Society* (1667) by Thomas Sprat, and certainly Bacon's writings had been inspiration of many of the founders (perhaps especially Boyle).

[3] Sprat's book won unqualified praise in *The Cambridge History of Science: Early Modern Science* (Park and Daston 2006, 354). For another example, see Skouen (2011), whose Abstract says,

> It is the most frequently cited work when it comes to describing the relationship between science and rhetoric in seventeenth-century England. Whereas previous discussions have mostly centered on whether or not Sprat rejects the rhetorical tradition, the present study investigates his manner of approaching past authorities. As a writer, Sprat demonstrates the same kind of utilitarian attitude towards the handed-down material in his field of knowledge as he says is characteristic of the Royal Society's natural philosophers. Making good use of Ciceronian ideas, Sprat emerges, not as a condemner, but as a rescuer of rhetoric.

achievements. The book contains only one novel idea, a proposal to erect an Academy to preserve the English language and to purify it from all polluting foreign elements. Fortunately, it had no effect.

C. R. Weld, the nineteenth-century historian of the Society, complained (*History of the Royal Society*, 1848, l, 83) that Sprat had failed to report the important events of his time. This is rather unfair, since the book was a piece of propaganda, and as such, it met mixed feelings towards the Society.

To repeat, the main task of Bacon's teaching was to persuade people to shun disputes and to make simple experiments on his promise that they will thereby reveal the grand secrets of Nature within a few generations. Those who were thus persuaded developed a new tradition. In the early days of the Society, a new tradition evolved that allowed a limited place for criticism and dispute. Bacon was an exception: his followers soon showed a reluctance to criticize him. In the early days of his popularity, the leader of the Society, the great Robert Boyle, declared openly that Bacon's experimental results are unreliable. Critics of the Society attacked Bacon aggressively. His reputation survived and became unassailable (until the refutation of Newton's optics won recognition. Bacon's critics then prefaced their comments with assertions of tremendous admiration for him).

In the early days, the most vitriolic critic of the Royal Society of London (and of Bacon) was Henry Stubbe (physician, scholar and advocate of religious toleration). Posterity ignored his criticism and maligned him as a hired pen (Jones 1982, 337, note 16; Main 1960, 46). He said (Stubbe 1671; Jones, 1982, 237),

> No Law ever made him *our Dictator*, nor is there any *Reason* that concludes him infallible: Nay it is manifest that he was frequently *deceived*. And since the *Gardiners* have *protested* against him, and that justly: Since the Chymists, and the Mathematicians disclaim him: Why may not a Physician [Stubbe] refuse to be tried by him? … 'Tis by his great *Example* that the *Baconian Philosophers* are such *Plagiaries*, *and Relators of false and defective Experiments*: Contemners of the ancients …

These last words are most indicative: Stubbe recognized the new movement as radical and radicalism did not appeal to his temperament: he found conservatism more tolerant than radicalism. He began as an ardent devotee of the new philosophy; he was disheartened because he saw the dangers of the new enthusiasm, with its radical condemnation of all past learning. And he saw the dangers correctly. Jonathan Swift was of the same mind and he articulated these fears better. Swift's attitude too was dismissed as personal: De Morgan accused him of bias against Newton. Also, the public forgave him more easily, on the suggestion that he was more interested in the literary side of the dispute than in the scientific side.[4] This idea finally lost its appeal as R. F. Jones destroyed it with very strong arguments of different sorts.

[4] This is interesting: it places, perhaps rightly, the origins of the division of culture into two in the find aesthetic but not intellectual merit in the criticism of the scientific ethos. Snow accused artists for the gulf, viewing them as natural luddites. He ignored the public-relations spokespeople for science who are natural apologists for it who find excusable distortion for a good cause. Defense of science is a menace for science and all.

All of Stubbe's attacks on the Royal Society were emotional and extravagant. Yet they included many truths that the Society preferred to ignore. His most informative work in this matter is his *Legends, no Histories* (1670), which is a review of Sprat's *History* (1667). As a review, this book is no doubt of mixed merit, yet it does devastate Sprat's prestigious *History*.

Robert Boyle considered Stubbe's criticism sincere and helpful. Isaac Disraeli found ample evidence (see his *Calamities and Quarrels*, Art. "Royal Society") for his sincerity, truthfulness, civil courage and devotion for his cause:

> he was sacrificing his personal feelings to his public principles; for Stubbe was then in the most friendly correspondence with the illustrious Boyle, the father of the Royal Society, who admired the ardour of Stubbe, till he found its inconvenience.

Stubbe's flamboyance worked against him, suggesting he was inaccurate. He was not. His claim that gardeners and chemists reject Bacon's views probably refers to Boyle's writings. Ellis and Liebig have fully confirmed his claim that Bacon was a plagiarist and that he had reported fictitious experiments. Stubbe's claim that some experimental reports were defective is probably an allusion to Evelyn's work on making cider (1664) that damaged the cider industry. Newton confirmed this in his famous letter to Oldenburg on cider (Brewster 1831, beginning of Ch. 6). Nevertheless, his claim that the Baconians were plagiarists and "relators of false experiment" is a gross exaggeration. The worst is this (Introduction to *Legends no Histories*):

> 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.

This Stubbe said against Sprat, and by extension, against the Royal Society. Sprat had exaggerated, of course. Yet, as he was defending the Society, its other members indulged him. For, the movement applied the doctrine of prejudice that encouraged distance from anyone who harbors mistakes, at least until that fellow's conversion to the new faith. Bacon had stated, "it is hardly possible at once to admire an author and to go beyond him" (Preface to *The Great Instauration*) and they did admire Bacon.

8.2 Permission to Propose a Hypothesis and to Assert Metaphysics

When R.F. Jones inquired into the Influence of science on the English language, he found that rhetoric and figurative language were not merely deserted: the impact of Bacon's rhetorical condemnation made them taboo. Jones conjectured that the refusal to accept Sir Thomas Browne as a Fellow of the Royal Society, his scientific merits notwithstanding, was due to his style. He compared the case of Browne with that of Glanvill. The style of Glanvill's *The Vanity of Dogmatizing* raised objections and the Society advised him to rewrite it and only then he was admitted as a Fellow.[5]

The inductive method that became characteristic of the inductive school or sect (to use Bacon's phrase) is much more important and has more rules than Jones has observed and they were vague. He rightly considered important the anti-rhetoric taboo and the taboo on figurative speech that the Baconian fraternity imposed, and he saw this importance in the literary style of the *belles lettres*[6] that the Enlightenment movement foisted. My original interest was in the style of scientific communications and particularly in the stylistic revolution that Faraday introduced (Agassi 1971, Index, Art. Style). At this point, it is more important to comment on the characteristics of the style in question that express the anti-speculative rules of the Baconian fraternity, and on the demand that scientific reports should be essays,[7] brief and informative and as free of speculation as possible. The hatred of speculations is traditional but it was never fully imposed.

As early as 1670 Stubbe had claimed, when pointing out that the Royal Society's hostility to speculation is blind, that their mechanical philosophy is itself a speculation (as Robert Boyle too repeatedly emphasized):

> Are not the principles of Des Cartes, and the figures of the Atoms of Gassendus, as precarious as those of Aristotle, and less subjected to sense?

For a long time this obviously valid criticism was ignored. It is not an objection to the mechanical or the atomistic principle. It is a criticism of the objection to all speculation in general, especially when this hostility to speculations led to pronouncement that atomism is not a "Chymerical Conceit", to quote a famous paper of Sir William Petty, a charter member of the Royal Society (Hull 1899, 2, 603). This is surely too naïve. The view that atomism was accepted as inductively educed or evolved from phenomena, resulted not from naïveté but from dogmatism: naïveté is pre-critical; dogmatism is the refusal to listen to criticism. Not only the bitter Stubbe, but also the enlightened Davy and Faraday considered atomism a hypothesis and were unable to oust the myth that it is not speculative but scientifically established.[8]

Boyle combined the opinions of Descartes and Gassendi into what he called "corpuscularian philosophy". It became the "idol of the market-place" of the Royal Society, as well as for the whole of the scientific community. Boyle regularly

[5] Glanvill explained in his open apology that his earlier style was due to immaturity. This made Jones smile (Jones 1951, 90).

[6] I find it uncanny that the literary style of Bacon and Boyle altered the style of nineteenth-century European Hebrew *belles lettres*, even though by then their works were almost forgotten even in England.

[7] Jones observed that for the third version of Glanvill's *Vanity of Dogmatizing* he turned it into essays besides abbreviating it once more. Regrettably, he did not find this significant.

[8] Towards the end of the nineteenth century, Wilhelm Ostwald and his energeticist school, as well as Ernst Mach and his positivistic school, rejected atomism as speculative. The Cartesian critique of atomism, by contradistinction, was not about status.

declared his indebtedness to Descartes and Gassendi, as he regularly declared that his view was indeed a speculation—a hypothesis (these two words are here used as synonyms). Even Petty declared his indebtedness to these great thinkers, although he would not have liked to admit the speculative character of his atomism.

Petty did not respond to Stubbe's attack; most of his targets did not; only Glanvill made a short reply, not to the argument quoted above, though. The view of the Royal Society encouraged this.

Let me quote from an anonymous publication of Newton (*Phil. Trans*., 29, 1714, 173; Thomson 1812, 297) that may hint at the answer he might have given to the question, how much metaphysics did Bacon's followers allow? Newton drew the following distinction between himself and his arch opponent Leibniz:

> It must be allowed that these two gentlemen differ very much in philosophy. The one proceeds on his evidence arising from experiments and phenomena, and stops when such evidence is wanting; the other is taken up with hypotheses, and propounds them, not to be examined by experiments, but to be believed without examination.

These are the two extremes: no hypothesis (Newton) and unexamined one (Leibniz). Missing are hypotheses plus their examinations. This comes next.

> The one, for want of experiments to decide the question, does not affirm whether the cause of gravity be mechanical or not mechanical. The other, that it is a perpetual miracle, if it be not mechanical. The one, by the way of inquiry, attributes it to the power of the Creator, that the least particles of matter are hard; the other attributes the hardness of matter to conspiring motions, and calls it a perpetual miracle, if the cause of this hardness be other than mechanical.

This is not quite fair, as Newton and Leibniz redefined the concept of miracles and accused each other of appeal to miracles by which they meant, they deviated from Descartes' views (Koyré 1957, 178, 186, 193–6).

> The one does not affirm that animal motion in man is purely mechanical; the other teaches that it is purely mechanical, the soul or mind (according to the hypothesis of a *harmonia prae stabilita*) never acting on the body so as to alter or influence its motions. The one teaches that God (the God in whom we live, and move, and have our being,[9]) is omnipresent; but not a soul of the world; the other, that he is not the soul of the world, but *Inteligentia supra mundane*, an intelligence above the bounds of the world; whence it seems to follow that he cannot do anything within the bound of the world, unless by an incredible miracle.

This again is a mute affair: as long as Leibniz did not assert that his metaphysics was scientific there was no methodological dispute here.

> The one teaches, that philosophers are to argue from phenomena and experiments to the causes thereof, and thence the causes of those causes, and so on till we come to the first cause: the other, that all the actions of the first cause are miracles, and all the causes impressed on nature by the will of God are perpetual miracles and occult qualities, and, therefore, not to be considered in philosophy.

[9] This is a quotation from St. Paul's speech to the Athenians in *Acts* 17:28 that was very popular. For example, it appears in Spinoza's letter to Oldenburg of December 1675.

Here Newton opposes not Leibniz's reasoning but his view of it as scientific.

> But, must the constant and universal laws of nature, if derived from the power of God, or the action of causes not yet known to us, be called miracles and occult qualities, that is to say, wonders and absurdities? Must all the arguments for a God, taken from the phenomena of nature, be exploded … ? And must experimental philosophy be exploded as miraculous and absurd, because it asserts nothing more than can be proved by experiments, that all the phenomena in nature can be solved by more mechanical causes? Certainly these things deserve to be better considered.

This passage is remarkable. There is no mention here of any genuine difference between Newton and Leibniz about physics. All the physical speculations of Leibniz that Newton mentions here he accepts, but not as a part of experimental philosophy (and not as miracles, simply because their views on miracles differ, which fact here Newton suppresses). The passage quoted is an attack (not so much on views but) on commitment to views independently of experience. (Again, this is true but misleading, since Leibniz deemed all experience unnecessary in principle but not in practice.)

Newton endorsed the mechanical philosophy—much more than Leibniz—but he assumed it "by the way of inquiry" whatever this may mean exactly. (By the way, commentators repeatedly ignored his endorsement of the mechanical philosophy because he also showed that Descartes was mistaken.) Since the disagreement Newton presents here is more methodological than scientific or metaphysical, since it is vague or ambivalent about hypotheses, and since it was the model of scientific reasoning, obviously in practice classical inductivism was vague or ambivalent. Indeed, it could not have survived otherwise.

8.3 Permission *De Jure* and *de Facto*

Hence, R. F. Jones went wrong when he described the philosophical principle of the Fellows of the Royal Society as plain sailing. Let me disregard literary and other parts of Jones's research for a while and refer to his description of the battle between Ancients and Moderns on matters of principle. He rightly ascribed to Sprat and to Swift the true assertion that the philosophies of Bacon and of Descartes are not in harmony. He was in error, let me argue, when he assumes that this truth was generally received. And he was likewise in error when he suggested that Swift had taken this to be the case. This raises a difficulty for Jones's reading of Swift's work, and on a point that he (rightly) deemed crucial. He admitted that according to his reading Swift's librarian (presumably Robert Boyle; see *Battle,* first footnote and text to it) should have placed Bacon and not Descartes next to Aristotle's books. Why then did he not do so? Jones offered an *ad hoc* hypothesis to explain this as the wish to avoid offending by ridiculing Bacon (Jones 1951, 39). This is uncomfortable, as it would restrain Swift from showing his knowledge of the situation in many other respects. Moreover, he had presented the situation correctly as he mocked Bacon and describes him as "killing" Descartes in the next few pages (with an allusion to comets and vortices):

> Then Aristotle observing Bacon advance with a frivolous Mien, drew his Bow to the Head and let fly his Arrow, which mist the valiant Modern, and went whizzing over his head; but, Des Cartes it hit … and went into his Right Eye. The Torture of the Pain, whirled the valiant Bow-man round, till Death, like a Star of superior Influence, drew him into his own Vortex.

This refers to Newton's refutation of Descartes' physics, not to his—and others'—view of Descartes' mechanical philosophy.

Jones's contemporary E. A. Burtt got it right (Burtt 1924). He has produced strong evidence that Boyle and Newton were indeed Cartesians. Stubbe had noticed that having endorsed Bacon's philosophy, the Fellows of the Royal Society ought to have fully rejected Descartes' mechanical philosophy. This, he added, they had avoided. Instead, he further observed, they had condemned Descartes personally. Glanvill's *Vanity of Dogmatizing* (1661) offered further evidence. As Jones noted (*Jones* 1951, 89), he changed his view on this, for he revised a sentence there (240):

> And the sole Instances of those Illustrious Heroes, Cartes, Gassendus, Galileo, Tycho, Harvey, More, Digby; all strike dead the opinion of the Worlds decay, and conclude it, in its *Prime*.

In the revised edition of 1664 that he called *Scepsis scientifica*, it becomes the following (Glanvill, 1664, 209):

> that Constellation of Illustrious Worthies, which compose the Royal Society.

Names are dangerous, for one might be found to have praised the wrong people. In 1694, the Newtonian William Wotton declared Descartes fully in disgrace, and his verdict was not defeated until Newton's theory had been superseded. Wotton wrote (Wotton 1697, 370):

> Now as this Method of Philosophizing laid down above is right, so it is easier to prove that it has been carefully followed by Modern Philosophers. My Lord Bacon was the first great Man who took much pains to convince the World that they had hitherto been in the wrong Path, and that Nature her self, rather than her Secretaries, was to be addressed to by those who were desirous to know very much of her Mind. Monsieur Des Cartes, who came soon after, did not perfectly tread in his steps, since he was for doing most his work in his Closet, concluding too soon, before he had made experiments enough; … yet by marrying Geometry and Physics together, he put the World in Hopes of a Masculine off-spring in process of Time, though the first production should prove abortive.

This quotation is typical of the influence on immediate posterity that Bacon had (even in his pseudo-Kabbalistic sexual metaphors). The work of Wotton appeared after the triumph of the Royal Society over the universities, when the lessening of the tension between them was quick. The condemnations should have somewhat lessened too, but Wotton's work, of which the last quotation is representative, proves otherwise. The Baconian myth in it is manifest even in the figurative speech that the Society tolerated only because it is borrowed from the master, who warned against abortive productions due to impatience and imprudence. The entrenching of the Baconian myth is a by-product of the success of Newton's theory, as Laplace noted about a century later. The doctrine of prejudice matured. Its popularity was first due to rash and premature enthusiasm and later due to success and complacency. Had Newton's theory not passed the severest tests so amazingly, inductivism would have been seriously undermined. As it was, its success wanted an explanation and inductivism was ready to hand—with Newton's blessing. True, everyone knew that he made hypotheses and that he was a Cartesian. In spite of the non-committal character of his expressions of his Cartesianism, his strong repudiation of non-Cartesian interpretations of his theory was famous. But as Bacon said, it is always possible to explain away or ignore all difficulties. Thus, Bacon's doctrine of prejudice was the new prejudice of the Enlightenment Movement and the symbol of the Age of Reason.

In practice, both methods were used: Newton's use of hypotheses was explained away by a metaphor (in spite of the taboo on metaphors): he used hypotheses as ladders and as scaffoldings so that his induction is legitimate. This metaphor is Bacon's. He had claimed (*Novum Organum,* 1, Aph. 125) that ancient knowledge rested on accumulated factual information used as ladders and scaffoldings but remained unpublished for technical reasons. This myth was then turned around: the ladders and scaffoldings signify facts no more; from now on and for over two centuries, they signify hypotheses (Goethe 1949, 9, 653, §1222; Herschel 1831, 153). A myth was remolded to save the phenomena. Induction thus underwent a transformation. And thus Bacon receded to the background. The *de facto* license stays to feign at pleasure myths, speculations, and hypotheses. Baconians were very critical—too critical—but they wanted something to justify their critical attitude, and as this cannot exist, they adopted a myth to that end. The resistance of Newton's theory to the strongest criticisms and tests supported the myth that the knack of producing irrefutable theories is the reward for open-mindedness.

8.4 Legitimation Versus Criticism

The critical attitude did not allow researchers to stay complacent. Newton's predilection for the Cartesian system met with severe difficulties. Some members of the research community demanded that its members should ignore difficulties. In the present case, they meant to deny Newton's debt to Descartes: they over-emphasized Descartes's errors to debunk him. Such a procedure would be reasonable were the doctrine of prejudice true. Taking it for granted, they made Descartes the black sheep, the Judas Iscariot, the Trotsky of the research community. The ambivalence about Descartes that we see in the Newtonian era beginning with Wotton's work remained noticeable until the Einsteinian revolution and beyond (see below).

A more modern inductivist illustrates the censure of Descartes according to the formula: the nineteenth century historian of science, Johann Christian Poggendorf (Poggendorf 1879, 307):

> Instead of following the road of experience recommended by Lord Bacon and trod so gloriously by Galileo, he followed again in the footsteps of the ancients.

The clear allusion to Hume here is no evidence that Poggendorf got it from him. Anyway, his view is now corrected: this time Galileo is put in his proper place. From now on, he is a follower of Bacon. The word “gloriously” is indicative: any use of ornamental language by inductivists should arouse readers' suspicion.

The only exception to the wholesale condemnation of Descartes' physics is of the famous historian of materialism Frederick Albert Lange. He reported (1866) that the anti-Cartesian attitude was then the fashion, but that it rested on the certainty of Newton's philosophy (Lange 1950, 312):

> Descartes was by no means alone in deducing, as he did,[10] gravity from the collision of ethereal particles. It has in our time become a custom to condemn severely his daring hypothesis as compared with the demonstrations of a Huyghens or a Newton. We do not remember that these men, undoubtedly all most thoroughly agreed with Descartes, through whose school they had passed in … the mechanical conception of Phenomena.

This did not help Descartes' reputation, perhaps because Lange admitted that Newton's theory was proven.

Amusingly, the refusal to move from Newton to Einstein made the leading historian of physics E. T. Whittaker cling to his ambivalence towards Descartes (Agassi 2008, 204, 208, 222, 500). It appears forcefully in his *History of the Theories of the Theories of the Aether and Electricity*, 1910, second revised edition, 1951. In 1910, the condemnation of Descartes was still in vogue, and Whittaker only explained briefly that whereas Bacon had realized that scientific research lasts generations on end, Descartes hoped to complete all science by himself. This is an orthodox Baconian attack. It is sharper in the book's second edition even though by then the fashion of condemning Descartes was over. Whittaker nonetheless stated there that Descartes had believed in the mechanical philosophy because he believed in clarity and distinctness, and then he dismissed the theory of clarity and distinctness as a myth. The philosophy itself, the idea of the universe as a machine, he considered a “revolutionary suggestion”. But, although as a suggestion Whittaker was ready to see its merit, he condemned Descartes for whom it was more than a suggestion: he declared it *a priori* true. Of this *apriorism* Whittaker said (7),

> his general practice was to represent phenomena as the effects of preconceived dispositions and causes. In this respect he departed from the sound doctrines that had been preached a generation earlier by Francis Bacon and Galileo.

This appeared soon after Whittaker's publication of his *The Modern Approach to Descartes' Problem* (1948), in which he explicitly endorsed Descartes' view of clarity and distinctness. This is not to censure the readiness of this mathematical physicist and historian of science to change his mind. This is merely to draw attention to the place of an idea as a myth.

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[10] This is a careless locution: Descartes did no deduce but only tried to deduce a theory of gravity from a mechanical model of it.

This then is my view of the popularity of Bacon's doctrine: it was both a myth uncritically endorsed and a boost for the critical spirit. The critical spirit did not destroy the myth because the myth was an explanation of the success of Newton and the failure of Descartes. Thus, Laplace declared that the crucial observations to decide between the views of Descartes and of Newton in astronomy was also the crucial observations to decide between the views of Bacon and of Descartes regarding scientific method. This is terrific. To update it we may say, the 1917 crucial observation to decide between the theories of gravity of Newton and of Einstein should also decide between the received inductivist opinion of Newton and the new hypothetico-deductivist opinion of Einstein on scientific method.

8.5 Bacon's Influence

The myth of the induction-machines gained support from the stupendous progress of science. The myth was that while metaphysical disputes bring about no progress, scientific research enjoys unanimity and it progresses. Despite the dogmatic spirit behind assent to the doctrine of prejudice, it also sustained a version of the critical spirit. It did not allow researchers to disregard observations that refuted received theories; eventually, refutations won recognition, and led to the abandonment of theories, no matter how reluctantly, and even Newton's reputation did not save his theories. To begin with, Bacon's myth gave people the courage to become rationalists—as the case of Comenius illustrates. It is possibly a blessing that Bacon was such a poor dilettante. The Royal Society of London collected all sorts of information in an admirable disorder. From this collection somehow admirable science did emerge, though in a non-Baconian manner, for sure. But how this happened is secondary to the fact that it did happen. Before Bacon's era, science was isolated. Afterwards, science was—it still is—a movement with faith and unity, able to face criticism and hostility with relative equanimity, as its status does not depend on the success or failure of the criticism.

Chapter 9
Conclusion: The Rise of the Riddle of Bacon

It should be clear by now why the riddle of Bacon arose. Generations of researchers admired him for his theory, and this theory did not allow them to criticize this theory respectfully. So the individual Bacon was criticized to save his theory. Stubbe criticized his plagiarism and fake experiments. This criticism was ignored until Liebig repeated it with some bitterness.[1] Here is Liebig's conclusion to his comment on Bacon's natural history (Liebig 1863, 244):

> With Bacon, all is external: nowhere in his work do you find a trace of the inner joy or love which animated a Kepler, a Galileo, or a Newton, in their examinations or discoveries or the humility which the accomplishment of a great work called forth, or beholding how much more and how much greater things were still to be done. These men, whether persecuted, disregarded, or oppressed, never deprecated or detracted from what others have done; and not one by them ever thought of claiming the reward or the approbation of the crowd for work which in themselves afford so profound a satisfaction. Compared to those men, Bacon shows like a quack-doctor, who, standing before his booth, tries to make his rivals appear as ignorant as possible, who vaunts his wondrous cures and praises the remedies with which he promises to raise the dead and banish illness from the world; and, finally hints that such services to humanity are not unworthy of recompense. 'Our *Sylva Sylvarum*' says Bacon 'is, to speak properly, not natural history, but a high kind of natural magic'.

[1] Thomas Fowler criticizes Liebig's style (Fowler 1878, 133 note) rather than his views: "it almost seems, as if Bacon had been a personal enemy of" Liebig. Indeed, when he could criticize a contention of Liebig's he did so, and with no less hostility, although it was on a minor point: Liebig conjectured that Bacon was not in full command over the Latin language and so he surmised that the originals of Bacon's Latin texts were written in English; and Fowler refuted this conjecture. Even on style Fowler had a point: his expressions of hostility greatly differed in style from that of Liebig. The difference is between the reserve of the English style and the expression of frankness more appreciated on the Continent. Fowler claimed that Liebig's arguments exhibit preconceived opinions (157). Not so: its nastiness reveals the source of the notorious and tremendous hostility that German professors show towards any criticism whatsoever. This is a mix of psychological sensitivity (that was already manifest in Newton's conduct), the authoritarian status of most old-style German professors, and Bacon's doctrine of prejudice. For Liebig's excellent character see Holmyard (1928, 103).

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*,
Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_9,

Liebig's general impression of Bacon is hard to dismiss, yet he did miss the main point. He considered humility as a part of scientific research. This is undoubtedly false, as Brewster had argued. Bacon was not humble, but neither was Laplace. Bacon's dissatisfaction with his own work that Liebig sensed (and which explains his inability to finish most of his works), speaks in Bacon's favor. He did prefer discovery to plagiarism and it is hard to blame him for his ineptness. He tried the make-believe method without convincing either himself or his contemporaries. He convinced posterity by a kind of fluke. Their admiration for him blinded them (in agreement with his doctrine) and they were ready to accept any idea that increased his status as long as it did not hamper discovery. This proviso is the greatest compliment to his memory.

Liebig noticed neither Bacon's doctrine of prejudice nor his philosophical discovery of the bias that the theoretical aspects of observation reports introduce. He took these for granted. What should cause surprise is that Liebig had to expose Bacon's obvious weaknesses. Surely, Bacon's seventeenth-century critics were far better acquainted with Bacon's works than Liebig's contemporaries were. They repeated his experiments; they compared his texts with those of Aristotle, Porta, and Paracelsus, which were then incomparably more popular than in Liebig's days. But they usually refrained from expressing their views in public; the person who did speak up, namely, Henry Stubbe, was violently censured. True, even Boyle mentioned once in print that Bacon's work was unoriginal and unreliable, but this was taken as a small lapse. Esoteric criticism of Bacon existed through the late seventeenth century, but it was considered a duty of the Moderns to save Bacon from public disapproval, and as an act of gratitude.

Evelyn's letter to William Wotton of 30th March 1696 (Evelyn 1854, 348) was not intended for publication, much less for posterity. Wotton was preparing a biography of Boyle (that did not materialize), and Evelyn wrote to him some of his memories of this great leader:

> and what he discover'd he has faithfully register'd, and frankly communicated; in this exceeding my Lord Verulam [Bacon], who (tho' never to be mentioned without honor and admiration) was used to tell all that came to hand without much examination. His was probability; Mr. Boyle's suspicion of success.

This is but one example of the contemporary awareness of Bacon's defects and preference to keep this awareness far from the public eye: as Evelyn has put it to Wotton, Bacon should never be mentioned in public except in honor and in admiration.

The details of the errors of Bacon in natural philosophy were soon forgotten, because his works became more and more obscure with the development of science and its plain language, so that they became open to many readings. Pliny, Porta and Paracelsus were forgotten and with them Bacon's plagiarism from their publications. Nevertheless, his followers had to forget him. His doctrine of prejudice puts down everyone who is not sufficiently up-to-date, namely, all the figures of the past, small and great. Also Bacon's political conduct was much more reprehensible than his literary conduct and this became increasingly obvious. Spedding tried hard to exonerate him, but he could not convince his readers.

We need not comment on Spedding's defense of Bacon's political conduct. The significant fact here is that prior to Spedding's researches, Bacon's attitude towards his friend Essex as well as his excessive acceptance of bribe were looked upon with profound Baconian disdain. The result of all this was very odd. His views were commonplace or, in his idiom, "idols of the market-place". He won praise mainly as one who had tirelessly demanded experiments, and who had tirelessly insisted that science must rest on experiment. But no one dared go into detail. In the once-famous life of Bacon by David Mallet (1711), as well as the ones in the few early editions of the *Encyclopedia Britannica* (1768 onward), his contribution to human thought is just mentioned, as briefly as possible, with no detail at all. No wonder that his ideas were never discussed. Consider the commonplace allegation that Bacon was a father of experimental philosophy. It is very unclear. In 1831 Sir John Herschel repeated it in awe and Sir David Brewster derided it as unclear. Brewster claimed that the insistence on experimenting was old hat and that the insistence on pure experiment devoid of theory is erroneous: this is how he criticized the doctrine of prejudice. His criticism of it went unnoticed. It—the doctrine of prejudice—was either taken for granted, or ignored, until the wave of speculations followed Faraday's discoveries that were hard if not (as it turned out) impossible to reconcile with Newton's system. Before that, Bacon's philosophy of discovery was so much taken for granted that no one noticed its originality and force. Indeed, to this very day commentators either ignore it and center on his metaphysics or else they read into his works their own ideas. One who praises him for an idea that one deems false will violate the doctrine of prejudice. Yet it is the doctrine of prejudice, or, more generally, radicalism, that is important and valuable, yet false. Indeed, till today the default option is to view the ideology of science as radical.

Thus, the whole force of Bacon's impact on nineteenth century thought went as unnoticed as proverbially water is unnoticed by the fish. Those hostile to speculations started to blame Bacon for his speculations, and this blame the prestigious Mach repeated. The supporters of speculations started to blame Bacon for his extreme empiricism. Nearly everybody considered him mediaeval, a view which is common today; not only Lemmi has repeated it, but also some of Bacon's leading champions who present him apologetically. In short, the rise of the riddle of Bacon is simple. According to the doctrine of prejudice, everyone is either a black sheep or a white sheep. Bacon was officially declared the white sheep, until spots appeared on his reputation that effort to repress failed. And then he was re-examined and judged by all his critics save Spedding as entirely and unquestionably black. (Spedding judged him white.) The riddle of Bacon is solvable by showing that though his methodology is erroneous, he had a great idea—the philosophy of discovery—and that he raised many problems to which he gave false answers, but which remain very important and very difficult to solve. And he invented the modern radical version of radicalism.

Like most of us, Bacon the methodologist is neither white nor black; unlike most of us, he made history, and for the better. His ideas are mostly not very original and mostly not very interesting. But two of his ideas signify. They may have found expression in writings of some of his predecessors, and they may have appeared in mature forms only after his demise—as interpretations of his texts or otherwise. They are false but important nonetheless. The significance of Bacon is that he planted in generations of thinkers two ideas that were soon taken for granted: his doctrines of discovery and his doctrine of prejudice.

Part II
The Religion of Inductivism as a Living Force

For the several employments and offices of our fellows, we have 12 that sail into foreign countries … three that collect the experiments which are in all books … three who collect the experiments of all mechanical arts … three that try new experiments … three that draw the experiments … into titles and tables … three that bend themselves, looking into the experiments of their fellows, and cast about how to draw from them things of use … three that … direct new experiments. … three that raise the former discoveries by experiments into greater observations, axioms and aphorisms. …

For our ordinance and rites, … we place … the more rare and excellent inventions … we place the statues of all principal inventors … some of brass; some of marble … some of cedar … some of silver; some of gold.

We have certain hymns and services … and forms of prayers …

(Bacon, The New Atlantis)

The philosophers have only interpreted the world in various ways. The point, however, is to change it.

(Karl Marx, final Thesis on Feuerbach)

Epistemology without contact with science becomes an empty scheme. Science without epistemology is — insofar as it is thinkable at all — primitive and muddled.

(Albert Einstein, "Reply", 1949, 683–4)

This second part of this study is devoted to the more-or-less single-handed implementation by Robert Boyle of the vision of amateur observers contributing to the rise of a new science that Bacon had initiated. It is therefore much devoted to the role of ideology in the rise of inductivism as a social movement and on the differences between the views on scientific method of Bacon and of Boyle, perhaps even between their world-views. For this some attention to some terminological detail is necessary. Let me offer the major ones here with an apology to the reader. Following these let me offer a general view of the current literature on Boyle and contrast it with my view on him. These will hopefully facilitate the reading of the more detailed story that involves methodology and social history of science and bits and pieces from quite a few disciplines.

Quasi-Terminological Notes

"The Inductive Style"

"Style" as referring to manner as opposed to matter, to the way of presenting things, is used here in a wide sense; "scientific style" denotes here the dry and prosaic style typical to modern scientific writings as well as modes of presentation like that of scientific dialogues or of power points. My interest is in the style of scientific papers proper, especially of inductivist ones, as developed in the Age of Reason. The inductive style is of intentional recording of experiences reminiscent of early Renaissance travel narratives: the inductive style is personal yet formal. A paper in the learned press in this style opens with a description of some experimental apparatus if its discussion refers to it and proceeds with a description of some experiments, perhaps adding some reports of other observations, and then concludes with a description of the result of the experiment and ends abruptly, without a word of comment or explanation as to the relevance of the facts it describes. Alternatively, it was reluctantly permitted to add to such a scientific paper a short vague preamble and a few lines at the end that present a conjecture or some theoretical conclusion. Most important, the inductive style excludes controversy and allows reference to a theory that a paper refutes only in allusion. Nowadays papers that conform to this style or mode of presentation are rare[1]; it was very popular in the eighteenth century. The only freedom granted to a writer during the time in which this style was compulsory was to add a few lines in the conclusion. If the paper lacks these additions, then some previous knowledge of its background is necessary for the understanding of its import. If the paper includes such additions in its conclusion, the proper way to read it is to start at the end, in order to get a hint at what is the concern of the paper.[2]

The inductive style is an invention — of Bacon and Boyle, instituted in 1661, soon to become traditional. In the mid-nineteenth century, under the influence of Michael Faraday, the historical or dialectic or critical style reappeared. It requires discussions of problem and of criticism of extant solutions to it. Some academics still forbid it as it boosts controversy. It does. Some academics find controversy otiose. In some periodicals the inductive style is compulsory to these days.[3]

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[1] The best-known example of the use of the inductive style in the twentieth century is in the concluding words of the first (1953) path-breaking paper of Francis Crick and James Watson on nucleic acids: "It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material." For, this sentence alludes to a speculation, and the classical permission is to use these only very sparingly.

[2] Let me report that biologist friends of mine habitually and unawares began reading a paper from the end.

[3] Popper narrated that he challenged his friend John Eccles to submit to a leading periodical a paper in the critical style. It was returned with the demand to rewrite it. A few editors have returned papers of mine expressing disdain at my licentious attitude to speculations.

"Speculation" and "Hypothesis"

Bacon, Boyle and Faraday, used "hypothesis" and "speculation" as synonyms. I use these words as they do when I quote or discuss their views. Otherwise, I prefer to distinguish between them: a hypothesis is empirically testable and a speculation is not. Thus, for example, in my terminology Boyle's atomism is speculative, while that of Dalton is hypothetical. I use the words "theory" or "conjecture" to designate both hypotheses and speculations. The ways these near-synonyms are used vary and change. This is unproblematic as long as any distinction between them is clear to readers.

"Hypothesis" and "Fact"

Following the older tradition, I often use the word "fact" to designate statement of fact. The difference between a fact and a statement is of paramount importance in logic and in some philosophical discourses. Not here. Since I discuss here inductivism, I usually use here the word "fact" to designate statement of fact universalized, at times called "general fact" in comments on classical science and "low level generalization" in comments on contemporary methodology. A general fact then is not any observed event but its supposed generalization. There is some subtlety involved here that somehow led to many useless philosophical debates. My advice to the interested in the logical analysis of these subtle matters is to consult Karl Popper's *Logic of scientific Discovery* (1935, 1959) §§14, 22, and 30.

On the Recent Literature

When I wrote my dissertation (over half a century ago) the literature on Robert Boyle was scant. It soon began to grow in earnest; it is already monumental. It includes the tremendously learned surveys by R. E. W. Maddison in series of papers and books, the correspondence of Henry Oldenburg that Alfred Rupert Hall and Marie Boas Hall have edited (Oldenburg et al. 1969) and that of Robert Boyle that Michael Hunter, Antonio Clericuzio and Lawrence M. Principe have edited (Boyle et al. 2001), as well as the collected works of Boyle that Michael Hunter and Edward Davis have edited (Boyle et al. 1999, 2000), not to mention the many books and many more essays that Michael Hunter and others have published since, that contain much fascinating information.

Current literature discusses Boyle's religious and metaphysical writings that receive much too little attention in the present study. I praise this literature for its integration of his theology in his philosophy, especially its attention to his efforts to harmonize and even unite science and faith. For my purpose suffice it, however, to note some generalities. Boyle advocated a view that he ascribed to Philo Judeus: research is the worship that befits natural religion. Here he echoed popular sentiment: both Bellarmino and Galileo found it in *Psalm* 19.[4] To this Boyle added that revelation is inessential for religion: it is the merciful gift from the Lord to sinners, his giving them a second chance. In his *Things above Reason* he expressed his unwavering rationalism, saying, it is unthinkable that the Good Lord would make unreasonable demands. He also said, belief is not open to manipulation: I cannot control it. He dissented from Descartes, however, ascribing to the soul moral sentiment (the expression is his) in addition to thinking. Natural religion belongs to thinking and revealed religion to the moral sentiment, as the Bible is a text in morality, not in reason. Hence, the expression of good will is in charity and in religious activities; science must be in the hands of (wealthy) amateurs.

Hunter is today the leading Boyle scholar. To the little extent that he shows interest in the social history of science, he tends to follow Steven Shapin (1994) whose philosophy differs greatly from the one offered in this text (Agassi 1997). As to Hunter's view of Boyle's methodology, Rose-Mary Sargent says of it adroitly (Sargent 2003, 52):

> Without argument … Hunter presents Boyle as having advocated an unproblematic and thoroughgoing empiricism that reflected an 'aversion' to reason.

Hence, the less said of it, the better. His views on the sociology of science as well as on scientific method insure that he and I will remain not very interested in each other's observations on them. Let me nonetheless comment on one idea of his. He dismisses the view of the early Royal Society as fully homogenous. In my view for what it is worth, the Society was remarkably homogenous. The greatest difference of opinions there was about superstitions — such as those of Joseph Glanvill — that is hard to dismiss or consider unimportant (Debus 1967); it show great unanimity among its Fellows, since the rules of repeatability kept all superstitions out and helped distinguish them from science. This is far from trivial. For one thing, it renders observations of colors scientific, contrary to tradition. It also renders scientific many observations about dreams and mirages. For another thing, this same ruling clearly renders pseudo-scientific behaviorism, a twentieth-century innovation that appeared as the paragon of science.

[4] The first half of *Psalm* 19 reads:The heavens declare the glory of God; the skies proclaim the work of his hands. Day after day they pour forth speech; night after night they reveal knowledge. They have no speech, they use no words; no sound is heard from them. Yet their voice goes out into all the earth, their words to the ends of the world. In the heavens God has pitched a tent for the sun. It is like a bridegroom coming out of his chamber, like a champion rejoicing to run his course. It rises at one end of the heavens and makes its circuit to the other; nothing is deprived of its warmth. The law of the Lord is perfect, refreshing the soul.

Regarding differences of opinion in the early Royal Society, the main item on my agenda is the disagreement between Boyle and almost all other Fellows that seems to me significant: they wanted him to establish a secular college and he staunchly refused. Also, most of them hoped for inductive certitude; he did not, as his skepticism was unqualified. Yet he had to accommodate for their views, as he needed badly their active cooperation as experimentors. On these matters the current literature has taught me very little.

Homage to Robert Boyle

I never swore allegiance to custom.
Boyle, Seraphick Love, letter dedicatory

And then I have to remind myself that the subject of my study is the same as the subject of many demanding scholarly publications. I share with them the view of Boyle as a shy and reclusive rich aristocrat, a frail, rather hypochondriac bachelor who lived for decades in his sister's home, a pious scholar and a leading scientific researcher. I do not share with them their view of him as a Baconian, although I am aware of Bacon's having influenced him deeply. The common assessment of Boyle as superstitious I deem a gross exaggeration.[5] In particular, my endorsement of the traditional view that alchemy is superstitious does not prevent me from asserting, as I do, that Boyle's (and Newton's) variant of it is rational. The gulf between the received image of Boyle and mine may be seen from the intriguing discussion in (Principe 1995, 392) about the young Boyle: he

> wanted his reader to do something … his writing was intended not as a pedantic or even as a didactic exercise but rather as a spur to action, to a way of life or a way of thinking. Before his conversion to natural philosophy Boyle had become quite accomplished in this way of writing, as the 1648 *Seraphic Love* witnesses. The level of exhortation or encouragement, then, is one barometer of Boyle's departure from his early moralist style.

To this Principe adds a note:

> This is not to say that Boyle's mature works are wholly devoid of exhortation or a desire for readers to act; however, they do lack the forcefulness and overtness of the early works.

Needless to say, what Principe calls exhortation seems to me Boyle's hallmark, pertaining to experimental philosophy much more than to theology. I view Boyle as the father of a movement of amateur researchers performing simple and easy

[5] See the alarming review of Boyle's *Works* (Dear 2002). "What on earth are we to make of Boyle? For the simple truth is that he was something of a prize fool" says David Wootton (2009), in his review of Hunter's *Boyle: Between God and Science* that echoes the sentiment expressed in that book.

experiments[6] in accord with Bacon's exciting vision.[7] I fancy him (as no one else does) as one who felt the weight of the world on his shoulders (as few people ever had, such as Pericles, Plato, Alexander, Caesar, al-Farabi, Maimonides, Cromwell, Pestalozzi, Churchill, Russell, Einstein, Pope John 23rd). He designed and fashioned a new movement of amateur researchers. His peers wanted him to found a secular college; he refused on ideological grounds. They wanted from him inductive certitude; he was a staunch skeptic. He developed what today we would recognize as a correspondence school for scientific empirical research, a school whose customers were the most unlikely crowd: middle-aged, poorly educated aristocrats. He wrote like this: let us make experiments with saltpeter because it is important for the following reasons. … Purchase it at a drugstore; here is a list of synonyms for it, so that your druggist will know what you want. We have to purify it first thus: …. . Now you will have a reasonably pure sample and we can start working. He would say such things as, if you want to know whether the color of a stone is original or painted, break a corner of it and see if it is of the same color all the way. He argued for the objectivity of colors (contrary to Galileo, who said, you might just as well say that the tickle is in the feather), saying, with my eyes closed I can see the curtains around my bed having any color I like, but when I open them I cannot see them but as red. We should remember that Newton contested the view of Descartes about billiard balls, and that the Royal Society Fellows met with public ridicule for their interests in fleas. Bacon conceived of the society of amateurs; Boyle fashioned it.

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[6] "In science the successors stand upon the shoulders of their predecessors; where one man of supreme genius has invented a method, a thousand lesser men can apply it. … In art nothing worth doing can be done without genius; in science even a very moderate capacity can contribute to a supreme achievement" (Russell 1918, 41). Russell's view of art, incidentally, is too Romantic. What he said of science seems to me to hold also for art.

[7] Thomas Birch, Boyle's first biographer, said this explicitly (*Works*, 1744, 1, 13; quoted in (Wilson 2008, 231)):

> He set himself to phylosophise, and to persuade the nobility and gentry of the nation, who had the means and leisure to pursue such sorts of studies, to follow his example. … it … would make them better Christians, but likewise more useful members of society.

The Royal Society of London official website holds a very different view. It depicts its first phase as a part of Gresham College, http://royalsociety.org/about-us/history/gresham-college/ , thus tacitly denying that amateur research played a significant role in its formation. It even denies that amateurs made a scientific difference:

> Banks was in favour of maintaining a mixture among the Fellowship of working scientists and wealthy amateurs who might become their patrons. This view grew less popular in the first half of the 19th century and in 1847 the Society decided that in future Fellows would be elected solely on the merit of their scientific work.

This is a misleading suggestion that the Society had no Fellows who were amateur researchers of note.

Chapter 10
Philosophical Background

This is a description of the background to classical physics, not a history of physics or of methodology. In the late eighteenth and early nineteenth century, researchers misinterpreted the history of the philosophy and of the science of their own time, and they misled later historians of science: later historians viewed these interpretations as first-hand evidence. Consequently, the largely unsatisfactory picture of classical science is almost unanimously received (Agassi 2008, 166). The misleading texts were always detached from their authors' social background. Consequently, questions regarding the social background of science remained unasked. In the twentieth century the sociology of science became popular for reasons that had to do with contemporary science. This led historians of science to study the social background of science. The sociology of the rise of modern science remained hardly studied. Some Marxist scholars touched upon this matter, but their intent was only to reveal the socio-economic background to it, and from a Marxist viewpoint. Gideon Freudenthal and Peter McLaughlin have called this "the Hessen-Grossman-Thesis" (Freudenthal and McLaughlin 2009, 1).[1] Thus, the famous Marxist crystallographer-turned-philosopher J. D. Bernal took for granted that mediaeval science was superior to ancient science (Agassi 2008, 222–3); this renders the Renaissance ideology incomprehensible.

The result was strange. Scientific organizations began then. How? Gordon Tullock has rightly noted (Tullock 1966, 5) that this question is surprisingly absent from the relevant literature that should discuss it—perhaps because it does not follow Marx's

[1] Freudenthal and McLaughlin report there that the Hessen-Grossman thesis is that science is an economic product. This displays the vagueness characteristic of the (pseudo-)Marxist literature: as it stands it is hardly open to dispute, as obviously all products have economic aspects, but it comprises a hint at a much more informative thesis. It is that all characteristics of (art and) science are basically economic. What this means is far from clear since economic theory firmly ignores the value of any product, making do with its having value enough to create demand; economics centers on demand, not on its rationale. Marx discussed the value of science, viewed its utility either a major motive, which may be true, or the only one, which is not. Once the Hessen-Grossman thesis is not allowed to vacillate between these two options it vanishes (*vide op. cit.*, Index, Art. *Motive*); in this it shares the same characteristic as all myth: it systematically vacillates (Lévi-Strauss 1966, 75).

J. Agassi, *The Very Idea of Modern Science*: *Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_10,

footsteps. Class-wise the Royal Society was aristocratic; ideologically it was Baconian-Boylean. The economic basis of science was vital: philosophy "requires as well a purse as a brain", said Boyle (*Works* 1999, 1, 5).[2] Yet the middle class replaced the aristocracy; it retained the ideology.

10.1 Inductivism Classical and Modern

The theories of scientific method and the research programs that Boyle and Newton had issued were tremendously influential. Their history is inextricably intertwined with the history of the doctrine of prejudice and especially with the attitudes of John Locke and later of his devotee Isaac Watts. Inductivism started as a popular and enthusiastic movement. It became the creed of a sect with taboos, with rewards and punishments, as well as with social pressures[3] on dissenters and deviants. Even researchers of standing like Joseph Priestley, Luigi Galvani, and Humphry Davy were unlucky enough to suffer clashes with received dogmas and pay heavily.[4] The classical version of inductivism that ruled the scientific scene prior to the crisis in physics (*ca.* 1900) differs from its modern version that modern inductivists reiterate now. Confusing the different versions of the dogma causes much trouble. Classical inductivists were very sensitive to problems. They did not much discuss induction: they took it for granted. It was part-and-parcel of training for research. The demand for strict inductions went together with the requirement to present theories as if they had factual foundations. The first researcher to break this taboo systematically was inductivist Michael Faraday in 1840 or thereabout.

Strong reasons supported the inductivist taboos. These show that, unlike today, in its heyday inductivism fulfilled important functions and assisted the progress of science. Although a false theory of scientific method, it was a positive driving force, even if sometimes blind and rather ruthless, and it fulfilled vital tasks. Refusal to recognize its value would be both incorrect and unjust.

The first task of inductivism (that Boyle undertook consciously as his personal major task) was to make science a living social movement. There was nothing new in the insistence on experimenting; "the new philosophy" or "the experimental philosophy" was new in its missionary tendencies, in its attempt to stimulate the general literate public, in its successful intention to become the faith of the intelligent

[2] Does this expression allude to Shakespeare's *Cymbeline* (Act 5, scene 4)? I cannot say.

[3] The legitimation of social pressure in science began when Boyle nastily threatened Thomas Hobbes, as well as Henry More, no minor figures, with public criticism of their ideas to force them to stop saying whatever he deemed distasteful. Thus, incidentally, when he deemed it necessary he could waive his proverbial shyness and reclusion.

[4] Galvani went on a pilgrimage to the Holy Land and returned as a dependent on his family fortune; Priestley settled in the New World; Davy left home for the Continent never to return.

ordinary citizen, in its demand of all members of the scientific fraternity to perform research in their spare time, no matter how minor or insignificant. Even this, Boyle taught, is better than nothing, as it may grow and flourish. Thus it came to pass that almost all great experimenters of the golden age of classical science began this way, including Cavendish and Priestley and Lavoisier, and even Charles Darwin.

Boyle saw his major task in winning the larger literate public to the new creed. In the tradition that began in the High Middle Ages and dominated until the activity of the Royal Society began, science was scholarly; active empirical research was almost totally limited to technology; knowledge was almost totally limited to scholarship in the church and its universities. The exception, individuals who performed experimental researches proper, were marginal people who functioned (on and off) as court-magicians—dabblers in medicine, alchemy, and astrology combined—and who had also access to some mystical literature. Their reports were largely too confused to count. Confusion reigned. It is the topic of the very opening of the first great dialogue of Galileo (1633), where the old-fashioned Aristotelian complains to the modern party of Pythagoreans, accusing the Pythagoreans of confusion worse than the Aristotelians. The modern party (to which Galileo frankly belonged) admits the charge and promises to speak clearly. This is why we consider Galileo the father of modern science.

The situation altered rapidly. From the mid-seventeenth to the mid-nineteenth century, science was confined to amateur researchers. The universities did not expand, and the number of researchers among them did not grow, and their empirical research was no part of their job description as it is these days, in the post-World War II ear. In the Age of Reason, in the seventeenth and eighteenth century, most of the leading researchers were amateurs. From the mid-seventeenth century to the mid-nineteenth century science progressed on the lines that Boyle had designed, supervised, and influenced, using to that end his legislation for the Royal Society and the influence of his very popular writings. The organizations for scientific research then were all imitations of the Royal Society of London.

10.2 Metaphysical Views, Classical and Modern

The rise of the popularity of Newton's theories (around 1700) comprised a revolution whose significance is second only to those of Copernicus (around 1550 or even 1600) and of Planck and Einstein (around 1900). Until 1900 it was considered obligatory to endorse Newton's metaphysical views. He tried to follow Boyle, whose program was to explain all the phenomena by assuming new versions of ancient doctrines: (1) matter and motion (Descartes) and (2) atoms and the void (Gassendi). Newton defended his introduction of his new concept of central forces into physics with the claim that he hoped to have forces eliminated by explaining them along Boyle's program. In his introduction to his *Principia,* he laid down the following program: assume (a) matter and motion, (b) atoms and the void, and (c) central forces; and then, ultimately, (d) explain these forces so that it will be possible to describe the laws of nature with no reference to forces.

Deviations from this program appeared first in works of Ørsted, of Wollaston and of Faraday—all in 1820, over a century after Newton's death.[5] André-Marie Ampère (and Peter Barlow) at once tried to restore Newton's program. Faraday rejected it publicly in the 1840s and announced a new program—to explain every physical and chemical phenomenon (gravity included) by assumptions about some fields of forces instead of the standard assumptions about matter, and no central forces. Fields of force, he suggested, should replace central forces: they fill space and models of their behavior should display the properties that Newton's theory had successfully describes.

My emphasis here is on the metaphysical clash between the old and the new. Viewing science as what leaves no room for intellectual diversity and considering science evenly growing with the steady accumulation of knowledge with no deletion and no corrections, inductivist historians gloss over differences of opinion within science. In line with this, some of them suggest that already Pierre Simon Laplace introduced fields of force well within the Newtonian framework, if not even Ruggiero Giuseppe Boscovich before him. Not so. The generally received framework prior to 1820 that continued to be widely endorsed until 1905 was very different from Faraday's fields of force.

Inductivism forbids speculations and programs as they prejudice research. This did not prevent its adherents from accepting Newton's speculations and program. Surprisingly, awareness of this contrast was common. Researchers often claimed to have overcome it by avoiding commitment to all speculations. By the doctrine of prejudice this is insufficient, since it says, the hope to stay uncommitted is mere self-deception. Indeed, when Faraday claimed that he too did not commit himself to his own speculations, peers took it to be an insufficient excuse. They judged illegitimate his very readiness to take the liberty and speculate. They knew that he was imitating Newton here. This they brushed aside: Newton had a special status and deserved special dispensation: he was the only researcher permitted to speculate freely.[6] In brief, this means something very simple: the strict prohibition to speculate included occasional permission to speculate just a little, and at the speculator's own risk. This permission depends on many prior performances of much empirical research. Newton did enough empirical work to justify any amount of speculating. When Faraday's speculative works finally won approval, it was with the same excuse: he speculated after he had performed much valuable observational work. This is a distortion of history, but then it is a mere excuse.

[5] Ørsted began his researches late in the eighteenth century, but with no deviation from Newton. He realized right away that his experiment deviates from Newtonian mechanics and he went into shock (Agassi 2008, 186–92).

[6] After Faraday died his heir, John Tyndall, admitted that Faraday had offered speculations, but he "did not rate them highly" and anyway, geniuses have special dispensations; they may speculate (Tyndall 2002, 40, 64, 92).

Put this way, the speculations lose their value as programs and seem related somehow to the results of prior researches. As results of observed facts, speculations can hardly clash. Wherever they do clash, they are allegedly insignificant, for they do not rest then on indisputable information (as disputes about them show). The result of this inductivist approach leads historians like Edmund Whittaker to try hard to present both Newton's and Faraday's programs in modified ways intended to eliminate the clash between their ideas.

10.3 The Doctrine of Prejudice

Boyle recommended the writing of experimental essays that record new facts: he wished to increase the number of experimental researchers so as to accelerate the development of science. The inductive style that he prescribed (following Bacon), the presentation of facts independently of any theory, grew into a tradition guarded by a powerful taboo. Unlike his contemporaries, Boyle rejected Bacon's doctrine of prejudice; Newton nevertheless generated the powerful taboo in question on its basis. He could do this with relative ease, since Boyle usually abstained from criticizing Bacon openly. The result was that the views of Bacon, of Boyle, as well as those of Newton, were welded into a confused mix that was—and still is—ascribed to them all. That includes the ascription to all three the same doctrine of prejudice. The reconciliation won the approval of the celebrated John Locke, who had the highest credentials as a research assistant to Boyle and as a friend of Newton. The confused mix is easily found presented in the latest, leading, and rather confused histories of ideas and of science, as well as in biographies of researchers—from that of Boyle (Hunter 2009) to that of Faraday (Pearce Williams 1965).

10.4 The Moral Code of the Fraternity

> … to listen to everyone; to silence none; to honour and promote those who are right; these have given science its power in our world and its humanity.
>
> Jacob Bronowski, *A Sense of the Future*, 211

Bronowski's demand "to honor and promote those who are right" may make us uneasy: we do not know who is right, we should lamentably confess. It is nevertheless a fair characterization of the code of science, even if somewhat idealized, as understood in the days of classical science, perhaps even today. Better "promote those who are right" than promote power seekers, yet it is much more difficult to follow and so it is hardly a realistic demand. It is not a matter of wording: only essays that looked right were admitted for publication in the *Philosophical Transactions of the Royal Society* and similar platforms of the scientific fraternity. This was of the main purpose of the use of the inductive style, according to Bacon and according to Boyle—even though not Bacon but Boyle came up with the idea of a scientific periodical—the first periodical ever—in order to have scientific essays replace scientific books. He deemed the stream of new discoveries steady enough to found a periodical to publish them. He took a great risk anticipating such a great and unheard-of success. Bacon and Boyle were hardly interested in the freedom of speech, since within science it was taken for granted. Rather they were interested in the truth. And, Boyle said, report of observations of facts as they were observed are true (Mother Nature does not lie)[7]; theorizing is fallible (all error is human).

Claiming that "we promote those who are right", as Bronowski asserts, we imply that we do know, that we are in the right. How can we judge? Even if we deem factual reports true (and let the devil take care of sense illusions), an observer may be indebted to a mistaken theory, say, in the case of a discovery due to efforts to refute it. Also, those who tried unsuccessfully to make the same discovery, may have contributed to it, as the successful discoverer may have learned from them what errors to avoid. From the early days of modern science, it progressed rapidly and won public appreciation of its progress. It soon claimed universal recognition. It became the creed of the intellectuals, the hope of the political philosophers, the ideal of the American and the French Revolutions, the pride of the lovers of wisdom—of the upholders of the faith in humanity.

All this was inductivist; inductivism was not without its downside: the inductivist Fraternity sustained its high critical standards to which it owed its progress by absorbing uncritically myths and taboos, and in doing so by meting rewards and punishments. In agreement with Bacon it pictured itself as a successful Utopia, nearing its final stage of perfection. This was the magnificent but far from perfect society of researchers, who tended to conceal its less attractive characteristics.

These defects were the target of Faraday's efforts at a reform. He was comparatively lucky. The success of his scientific career, despite the agony that he suffered, reflects the peculiar character of the society in which he lived: it gave him the highest rewards together with the severest punishments it had in store—high acclaim plus strict isolation. He won all possible praise and honors for his factual discoveries and a conspiracy of silence about his radical ideas. Fortunately, he was strong enough to break through the silence around him. There is still hardly any mention of him as a theoretician and a philosopher as opposed to him as an experimentalist (Maxwell and Einstein were exceptional this way). The speculative Faraday was a victim of the doctrine of prejudice, to which he himself adhered, that an almost unanimous resolution that he should be known to posterity only as an empirical discoverer, as an experimentalist (Agassi 1971, 320).

[7] (Quine 1988): "In the matter of their fallibility, my position is as follows. The observation sentence itself is an occasion sentence, indisputable on the occasion of its assertion. Its dated report is a standing sentence, and theoretical; hence fallible."

10.5 Conclusion

The characteristics of the Fraternity as I see them are these. Most of its members were amateur researchers and true lovers of wisdom; inductivism and the Boyle-Newton program were the universally received creed and its problems coincided with the problems of the physical sciences of the day; the accepted inductive style was a stereotype; the doctrine of prejudice prevailed; and lastly there existed an inductivist high discipline.

Chapter 11
The Social Background of Classical Science

The College Gresham shall hereafter
be the whole world's university.

Ode to Gresham College (Anon)

In the Age of Reason—the inductivist era—practically all researchers were amateurs whose interest in discovery was motivated by curiosity and by the love of truth, often also by the love of fame,[1] but by no financial interest. This fact is overlooked by the relevant literature that often proposes the opposite impression; so much so that Thomas S. Kuhn declared the typical twentieth-century scientist as the paradigm, thus implying that all researchers were always professional.[2] They were not. They acted in an international organization centered round national and local societies and academies, with no ties to universities.[3] The traditions and customs of this set of organizations were intentionally fitted for the purpose of recruiting all possible amateurs and directing them to be self-trained researchers. The organizations were open and their members eager to recruit.

[1] The expression "love of fame" is due to Bacon who used repeatedly as an expression of contempt for those who publish conjectures instead of taking the trouble to verify them first, thus showing that their love of fame was stronger than their love of the truth. In his autobiographical note David Hume echoed this bravely, admitting that among his motives for publication was his "love of literary fame", as it was his "ruling passion" (Sabl 2006). Interestingly, he approved of it even though he endorsed Bacon's doctrine of prejudice.

[2] Kuhn said this regarding professionalization, not regarding specialization that he rightly located in the early nineteenth century (not seeing that they came together). The case of periodicals is simpler: he knew that in antiquity there were none, yet all he wrote on them (Kuhn 1996. 30, 50, 177) suggests the opposite.

[3] Some researchers were academics—most important of them were Newton and Boscovich. Yet they were amateurs since their job descriptions were traditional, not including research; these days the demand for research is stated in most academic contracts. The nearest to an academic research job proper in the Age of Reason was that of James Watt in the University of Glasgow as an instrument maker. The oldest research institutes that employed professional researchers are the Greenwich Royal Observatory (1675) and the Royal Institution of Great Britain (1799),

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_11, © Springer Science+Business Media Dordrecht 2013

11.1 Researchers as Amateurs

To begin with researchers in the Age of Reason were almost all people of independent income. The rest were university and secondary school personnel, physicians, engineers, navigators, and even some mathematical toolmakers. Were Captain James Cook and Captain William Bligh professional scientists? It is hard to say. This is characteristic of the society of amateurs: today wherever science enters the picture scientist and researchers are employed.

This is understandable. What is puzzling is the far-reaching reform of the academic system all over the western world that took place soon after World War II with no public debate about it. This was an intended and implemented plan, it seems to me, and I ascribe it to Harvard president James Bryant Conant, who was popular as an academic, a diplomat, and a soldier. He introduced the reform in Harvard University almost single-handedly, putting it at the top of the American academic system and thus on top of the academic system of the western world and he put that system in the service of the Cold War whole. It was too easy. A sworn liberal, he nevertheless forced members of Harvard faculty to cooperate with the notorious House Un-American Activities Committee and he excluded from the faculty communists (Hershberg 1993) as well as individuals who had no doctoral degree or who would not yield to publication pressure.[4] And he instituted peer review[5] as the measure of the recognition of the academic quality of a publication with the intent to both prevent the lowering of standards to circumvent the pressure and allow administrators to judge the quality of a scholarly paper; and he introduced the scholastic aptitude tests as entry exams for university education.

Many academics follow Kuhn and take Conant's system to be the best possible. It is not. Phillip George offered a different picture as he depicted the character of the amateur movement of the early classical scientific movement (George 1952, 314):

> The number of authors who contributed papers to these early volumes of the *Transactions* is striking evidence of the magnitude of the scientific movement in the seventeenth century. … . In … 1700–50 about 800 contributed papers. … This … puts a different emphasis on the growth of the scientific movement … the major problem is the emergence of a group of people cultivated in science, numbering at least 1,000 between 1665 and 1700. … A body

the Pasteur Institute (1885), Kaiser Wilhelm Institut (1911), Niels Bohr Institute (1921) and the Princeton Institute for Advanced Study (1930). The first research universities were probably the *grandes écoles* that appeared soon after the Revolution; in the USA it was Johns Hopkins University (1876).

[4] Publication pressure appeared a decade or so earlier and even the term was coined then. Yet it was marginal. Publication pressure became standard very visibly after World War II and it soon became insurmountable.

[5] To begin with, peer review was innocent. Today it is too often a tool for wielding power; it has extended to the market of popular trade books. Publishers will not publish a popular text that a peer condemns: the academic system uses the market as perks for its better members and publishers reluctantly cooperate. When a publisher requests an assessment of how a book will do in the market, reviewers declare how it should do; all too often the publisher yields.

of men to spread scientific ideas and ensure the continuity of the movement from one generation to another was of paramount importance.

George has rightly raised as fundamental the problem of the rise of the amateur movement and its institutional embodiment: how was interest in science kindled and how was it cultivated, sustained and institutionalized? As George noted, the universities did not offer adequate science training and anyway only a relatively few researchers had studied there.

The decline of the amateur movement was prior to the establishment of regular university research laboratories. The literature on the rise of laboratory training institutions in the West is mostly recent and it is fascinating. Here suffice it to mention some well-known facts. Contrary to what most intellectual history books say, academic education was not research-oriented until the middle of the nineteenth century. The first university laboratory in England was the Cavendish Laboratory in the University of Cambridge that James Clerk Maxwell inaugurated in 1874.[6] He said then, students are lucky that the laboratory is poor, as they will have to make the instruments themselves and thus learn something. Obviously, he wanted to keep alive the tradition of the training in laboratory research that was mainly the result of the initiate's private initiative. This instruction was available in the writing of Boyle and his peers and by then these were over two centuries old and quite outdated on many counts. Until about 1820, researchers were aided by books in the mode that Boyle had designed for amateur self-training. His own output was very popular until then, and for this reason. The scientific literature of the time rapidly changed its character and style, but it still aimed at amateur readers and their style was easy and popular. After about 1850 leading researchers spent time giving popular lectures to the general public, i. e., to people with no scientific pretense. This heralded a new era.

Exceptions abounded, but as hardly significant. Newton's *Principia* and Laplace's *Mechanique Celeste* however were very significant and yet they did not aim at average readers. They received significant popularizations (although popular science was recognized only since the mid-nineteenth century). In the Age of Reason Newton's *Opticks* was more popular than his *Principia* and Laplace was a pioneer in the writing of high-standard popular science. Writers regularly tried to simplify their mathematics as much as they could in order to meet the needs of the average researcher whose mathematics was scant. Scientific papers were written for genuine amateurs in easy language.

The first move in recruitment of researchers must be the arousal of interest in science and the encouragement of any effort at experimenting and of mastering the experimental method. This the scientific movement achieved in various ways. The major influence was Bacon's mythology. To be rational was to be active. And one way of being active was to observe and to report on one's observations. Nowadays, unusual experiences—of strange countries, of encounters with rare people, of novel

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[6] Some experiments were performed in universities a bit earlier, but not in an officially designated laboratory.

political situations—would seldom count as scientific, and would be reported to the public more as the popular mix of entertainment and information. *The Philosophical Transactions*, however, did not hesitate to publish papers about the wonder child Mozart and about Sir Hans Sloan's voyage to Jamaica. Editors of the scientific literature could hardly refuse to publish any paper that (a) reported only or chiefly information, (b) reported at least one previously unknown item that (c) appeared reliable. For reliability repeatability was required, but Mozart was not repeatable; the tests of his talent made to expose fraud were, and he passed them all.

By today's standards hardly any of the papers of the classical scientific literature can claim scientific status. (The rest, however, are just terrific.) Much of the information then hardly related to any problem; it soon sank into oblivion. But the tradition of publishing anything informative was important. And one can say that in this sense classical science was in a very definite way inductive: it included a jumble of statements of facts. Inductivism was a myth that got people interested in science. True, this myth could not turn the interested into a researcher proper. This is why both Bacon's and Boyle's contributions to the development of the Fraternity were essential. The one aroused interest that the other tempered it with a little discipline.

Even today the learned press includes much that is obviously redundant. Yet the rationale for this is new: it is not to encourage research but to encourage appointment committees to hire, raise salaries, grant tenure. This requires rethinking today's structure of the commonwealth of learning. In the Age of Reason the driving forces were those that Bacon and his followers designed and developed—idolization of science, high praise for great researchers, and a deep contempt for dissent. Much as some of this attitude is commendable, overall it is not: contempt is dangerous even when it is for a good cause.

The signs of deterioration appeared at once: scientism, the idea that nothing counts but science. Admittedly, science is one of the great triumphs of humanity thus far. It surely is not the only one, though, yet a large portion of the classical rationalist literature suggests it is. Already Descartes suggested that art is but an ancillary to science. The poet John Dryden, a Fellow of the early Royal Society, advanced the view that poetry is subordinate to science and should take the form of a special kind of Baconian history of nature. Perhaps this view is too extremist to be representative. Yet it was popular and influenced European *belles letters* at least to the early nineteenth century, and not only among those whose poetry was nothing but pop science rhymed. Artists under the illusion that they were doing or serving natural philosophy might have been poor artists anyway. Surely, not John Constable, the great painter. His illusion that he was doing natural philosophy increased his love and interest in the landscapes he painted so superbly (Gombrich 1960, Pt. 1, Ch. 1). But regardless of whether the effects that the rationalist enthusiasm had on art was good or bad, art is no science, not necessarily dependent on science, and definitely not inferior to it. Demeaning the arts also demeans the sciences. Thus, a typical book on rhetoric opens with "All art is founded in science, and the science is of little value which does not serve as a foundation to some beneficial art" (Campbell 1776). This sentence (unintentionally, for sure) manages to insult first the arts and then the sciences.

Contempt is distasteful. Interestingly, for a long time, within the scientific world, both atheist contempt for religion and religious contempt for vulgar religion were painfully present even though suppressed. When open atheist critique of established religion finally began, it was virulent. Surely it was prepared long before. There is hardly any argument that Tyndall, the nineteenth-century high priest of "scientific materialism", used that is impossible to trace back to eighteenth-century Tom Paine's friendly *Age of Reason*. But as the unity of the Fraternity was more important than the doctrine of prejudice, these were off the agenda. For well over a century religious disbelief found no militant expression as long as it might have split the Fraternity.

Still another factor was the Fraternity's moderate utilitarianism the belief that though science does have a purely intellectual value, it is also necessary for the decrease of drudgery and misery of humanity. Applied science is a by-product of "pure" science, though it was very desirable.

11.2 Researchers as Experts

> Our credulity is grosser than that of the Middle Ages, because the priest had no such direct pecuniary interest in our sins as the doctor has in our diseases.
>
> (Bernard Shaw, St Joan, Preface)

> In the progress of the division of labor 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 … becomes as stupid and ignorant as it is possible for the human creature to become.
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> (Adam Smith, The Wealth of Nations, Bk. 5, Ch. 1, Pt. 3, Art. 2)

Consider research as a profession that nowadays is taken for granted. *Graduates' Jobs* that *Political and Economic Planning* published in London in 1955 takes this for granted and as commonplace:

> The present inquiry was prompted by general concern felt on two counts. First, the need to provide industry with the brains it must have if it is to become increasingly efficient and second, the problems posed both for industry and for the universities by the fact that since the war a much higher proportion of the nation's most able children now goes into universities.

Recruiting and training of both scientists and researchers is institutionalized in many ways. Industries, government offices, and the military suffer insatiable need for both scientists and researchers; at times they grant scholarships to youths who show ability and willingness to study in return for undertaking to repay in service by performing some dull science-based jobs or uninteresting research. Industries create special courses for training researchers tailored to their needs and schools to house them. Universities are by now adapted to the requirements to increase the proportion and the number of graduates who go to work in science-based industries. Local authorities open polytechnic schools to such ends. Educators and educational authorities, politicians and sociologists, all harness their expertise in efforts to create institutions and regulations intended to raise the number of employees in science-based industries.

A fair expression of the current situation is a 1990 talk by Canadian Prime Minister Brian Mulroney (on the Inaugural Canada-Wide Marshall McLuhan Distinguished Teachers Awards http://www.mcluhanmedia.com/mulroney.html.) He could say this. We are a rich country; we excel in taking care of the high quality of life of our citizens; we can afford to increase our spending on education and culture; and we should do it. Regrettably, he did not. He spoke of the need of the Canadian economy for more professional training. Regrettably, Canadian academic response to his speech was favorable: it represents the spirit of the new professional ethos, with accent on research. There is something admirable about all this, of course: in a liberal mood, social services should not dictate tastes and purposes but stand at people's service the way they find fit. Yet, politically speaking, the mood that it expresses is of replacing democracy with technocracy, and that is contrary to the liberal mood. The need then is to improve the taste for democracy and for knowledge for its own sake and to popularize it: if the way to achieve this is by promoting research, then so be it; but to insure democracy, products of research should be widely accessible.

Adam Smith found intellectual specialization less dangerous than industrial specialization; yet the latter danger is easier to meet than the former. This is due to the popular view that no opinion is worth consideration unless it belongs to some expert-specialist-professional. Bertrand Russell viewed specialism as a necessary evil and the reliance on experts as reasonable—even though only when they all agree. For, he was unable to provide non-specialists with criteria for decision that would make them generally knowledgeable and thus independent of all expert specialist judgment. Quite generally, some people judge better than others do, and we should prefer the view of the better judge. Now these days the better judge is usually the expert, and the expert almost always a specialist and a professional. This is commonsensical, yet it puts the autonomy of the individual at risk and this risk invites innovative methods for safeguarding it, especially when guilds of specialists safeguard mutual support against outside criticism.

Let us backtrack. How did professionals come to be reliable? How come we accept today the general reliance on some experts as a sufficient reason to trust them? It was not always like that. In the Age of Reason people once relied on professionals who, we agree, were obviously[7] thoroughly unreliable. How did we manage to improve this state of affairs? The widespread answer is Baconian: having

[7] Voltaire took the unreliability of medicine for granted (*Candide*, end of part IV), in the tradition reaching through Petrarch to Cato the Elder and Pliny the Elder. It easy to distrust experts; it is hard to dismiss them (Trone 1997).

emptied our minds of prejudice we saw at once that these pseudo-experts were full of prejudices, and then we naturally brand them charlatans. Even among expert historians, this refuted view is popular. Not all astrologers were charlatans—not Thales, Brahe, Kepler, and their likes. And a course of mental hygiene is insufficient to train people to detect impostors. The better answer is this. When people rely upon professionals, they are entirely unreliable; they became more reliable in the modern world when people and governments alike became suspicious of their powers, tested them and supervised them, determined to dismiss their advice if they failed the tests, made them accountable.

Professionals wish to appear as experts. To this end, they make their views appear unanimous; they gloss over their past mistakes; they sub-divide their subjects to make almost each of them a monopolist in some sub-branch (or sub-sub-branch).[8] Mutual criticism becomes increasingly difficult.[9] Specialization allegedly leads to expertise, to learning more and more even if it is about less and less; this is so only under some controls; otherwise it may easily lead to narrowing the field without thereby raising proficiency. Researchers still differ from shamans; this is so because the critical spirit is still alive despite specialization. But if specialization goes much further, then the critical spirit may suffocate.[10]

In an interesting letter to the editor of the *Manchester Guardian* (November 21, 1955) geneticist H. Graham Cannon FRS says,

> There is a rule in the Royal Society that those who contribute papers to its proceedings must supply a short abstract outlining the main findings in the paper. In his last presidential address before going out of office the late Sir William Bragg made an eloquent plea that contributors to the scientific discussions should make their abstracts intelligible, at least to Fellows of the Society. The effects of this appeal have been negligible …
>
> Now, at a meeting of the Parliamentary and Scientific Committee held in the House of Commons, we are told that it is unreasonable to expect scientists to express their complicated ideas in such a way that the ordinary person can understand them …

Educator R. D. Waller responded (*Manchester Guardian*, December 5, 1955):

> … when work in a new scientific field is introduced to a University Senate an effort is made to explain its nature and purpose to colleagues who are not researchers, and there is a considerable element in the lay public capable of taking in explanations at that level. … anybody who has really wished to do this has always managed to find out how to do it. Professor Cannon … has been … one of the most successful popular lecturers …

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[8] The most hilarious case was when under the instruction of the Vatican Cardinal Hans Küng became an academic and joined the faculty of theology in the University of Tübingen: his peers there complained that by just having joined them he violated their unanimity, thereby putting to question the scientific character of their discipline.

[9] To block criticism, competing views, say, of the child's intellectual growth, go to separate sub-departments.

[10] The critical spirit was suffocated in Vienna and Budapest as Ignaz Semmelweis was denounced for his just criticism of his peers in the medical faculty; fortunately for us Joseph Lister and Louis Pasteur saved the day.

What is peculiar to this discussion is that it took place. Indeed, in the ensuing decades very little appeared on it (Feyerabend 1970; Sassower 1993). Here let me make do with two brief observations.

Arguments about the popularization of science are commonsense—social and extra-methodological. Briefly, the public has the right to know what goes on in the laboratory because we pay for it and because what happens there is important for us all. The recognition of the need to popularize science should make the public employ experts to do it. Similarly, experts should explain their works to the public. For, when they will be unable to communicate with the inexpert, science will very probably cease to be open and so it will die out or become something else. The public and the experts need each other—for criticism: amateur criticism of experts must be useful or else research will suffocate. Yet as long as professionals are experts and amateurs are dilettantes, this is limited. At least we should not accept this as a matter of course: all good things deserve careful monitoring so that they stay good. Although the ability of popularizers to explain technical terms is limited, it is remarkably helpful, especially when it comes with their background problems. The greatest obstacle to efforts to popularize science is the taboo on informing the public on scientific problems (before they are solved) and on scientific disputes (before they are resolved).

The inductivist tradition lost ground in the mid-nineteenth century when (under Faraday's influence) it became admissible to discuss problems openly and even include the word "problem" in the very titles of scientific papers. Still, the default option until today is different as long as the taboo on mentioning unsolved problems holds. The damage due to this reticence was then smaller in the inductive era, as researchers enabled their readers to repeat their experiments, thus helping them to feel problems. When more explicit statements were necessary, researchers reported that some past theories did not rest on facts. (Whatever resting on facts may mean, a theory cannot do so and be untrue. And a hint that a theory is untrue is a hint at a problem: what theory should replace it?) Researchers tried to present their theories with illustration in accord with the myth of induction, and this helped making their presentation more problem-oriented. Researchers in the inductive era helped their readers since they appreciated them as active participants. They usually wished their public to take part in experimenting and discovery.

As a prominent example we may take one of Britain's greatest and most skilled experimentalists, Joseph Priestley who discovered oxygen. He was a priest, a theologian, and an educator. He asked his friend Ben Franklin, another amateur researcher, to write a science textbook for his school. Franklin challenged him to do that himself. Reading the literature in preparation for that, he had to perform the experiments described there. The rest is history.

The myth of professionalism—the identification of the professional with the expert and the expert with the specialist—steps in whenever the myth of inductivism is unmasked. Thus, although amateur natural science started its decline in the early nineteenth century, the myth of the validity of the experts view as such did not rise until the twentieth century, until after the overthrow of Newton's theory. For, with this event the only serious example for the myth of inductivism was gone.

The realization that the problem of induction is insoluble set Michael Polanyi on the road to the philosophy of expertise. And he was a great expert, being a professional physician turned professional physical-chemist turned professional sociologist turned professional philosopher. In brief, he declared that researchers are no different from artists: they cannot articulate their expertise but, inspired, they can transmit their expertise to apprentices. Thus, they sustain a community of experts and its traditions, he explained (Polanyi 1966, Ch. I: Tacit Knowledge). This makes them trustworthy, he concluded. He never claimed that experts are infallible, only that they are the best available.

11.3 Researchers as Inventors

> … Bacon … said a lot about the practical possibilities of science, which he regarded as a sort of rational alchemy. … these ideas came to Marx and Engels through Feuerbach … they believed that science and industry were fundamentally the same. …
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> (H. B. Acton, The Illusion of the Epoch, 1955, 253)

How do we identify discovery? And invention? And how do we differentiate between them? Polanyi said, leading researchers identify the one and patent testes the other. This answer is unsatisfactory, as it does not allow for expert consultations, much less for rules about them. The guild approves of experts in different ways: academic appointment committees appreciate discoveries and patent officers appreciate inventions. How do they do it? Polanyi said, there is no point trying to answer this question. Here is my answer nonetheless: a discovery is recognized as it refutes a received theory; an invention is less easy to recognize, as many litigations prove. Each litigation decides on a problem *ad hoc*. They all come to show that the useful idea in question is new in the sense that its useful application was earlier absent.[11]

Truth and utility differ. They are two aspects of the same theme, said Marx. He raised the question, which of them is more basic? Are theories true because they are applicable or are they applicable because they are true? This sounds silly, since false theories have true consequences that may be useful. Marx—or some of his disciples—said, truth is the by-product of utility (in pre-socialist societies). Both Bacon and Marx characterized science as both utility and verifiability. Bacon urged people to search for truth, promising them utility as a by-product; Marx said—or so it seems—as the motive for the search is practical, truth is its by-product.

[11] A court in the USA used this idea as it backed a patent for the simplest application of Lavoisier's find that vegetables rot by consuming oxygen: it deemed new the very decision to use it. The situation is still unsatisfactory: as it is often all too easy to go around a patent, industry often prefers trade secrets to patents.

Now some truths did emerge as by-products of practical research, especially in industrial laboratories. But practical interest alone never suffices for the growth of science. Marx drew attention to the significance of the economic background of individual researchers as well as of the scientific tradition. The Marxist view of science laudably asks, why did science develop when it did, not earlier nor later? The Marxist answer is economic: the demand (need) for discoveries generates their supply. "The historical role of the rising industrial capitalists" was progressive. The example *par excellence* for this is the decline of ancient Alexandrian science. Hero's steam engine had no customers as the ruling class then had no interest, as it employed cheaper slave labor. Watt's steam engine developed magnificently because the rising capitalist class as its enthusiastic customer.

Not so. There is a long way from a discovery to its application as an invention, and no guarantee (Agassi 1985). With a little more luck and perhaps a few more scientific advances, an application of Hero's machine might have ousted slavery. The steam engine was known before the scientific revolution and it was never forgotten.[12] Many people tried to improve it since then.[13] Watt found a steam engine in the university of Glasgow that granted him his unusual employment to support him as a bright young philosopher. He improved it. The story of how the steam engine ever came to the university is intriguing. Here is a most interesting comment by Sadi Carnot on the discovery in general (Carnot 1986, 63):

> If credit for a discovery belongs to the country in which it is entirely developed and improved, we cannot in this case deny that credit to England. … It is natural, of course, that an invention should be made and, above all, developed and perfected where the need for it is most keenly felt.

Carnot's statement shows that the Marxian view in this respect was in the air. (Marx was bold enough to make it a principle of a system.) The first application of the steam engine—in the seventeenth century—was of an invention of Thomas Newcomen. As Bacon fused discovery and invention, his disciples encouraged attempts to make inventions. Hooke seems to have had a hand in raising Newcomen's interest in the steam engine and in encouraging him. The engine was very inefficient and only a few replicas of it were in use to pump water in deep coalmines where coal was cheap and water a nuisance. One of these replicas was disused and found its way to the University of Glasgow. There was no economic motive for that as neither engineers nor capitalists frequented the university.

Capitalists took no interest in Watt's improved engine. His patron (a professor he was, and a Baconian) encouraged an adventurer, John Roebuck by name, to invest in it; he ended in bankruptcy. The next investor was more successful, since Parliament granted the engine a monopoly—contrary to Marx's view: competition became a mark of capitalism later—not as a historical necessity but as the outcome of the plea of Adam Smith that won popularity. There also was no interest when George Stephenson put the engine on rails. The inductivist enthusiasts encouraged inventors

[12] Hero's *Pneumatics* was printed in 1575 and Porta's *Natural Magic* in 1560. Arago's story of Watt's discovery of Hero's effect as a boy (Arago 1839) is sheer fantasy.

[13] (Dickinson and Vowles 1943) relate the early days of the steam engine (15–24).

and used the scientific organizations as tools for propaganda until capitalists rose to the challenge.

The invention of the safety lamp accords better with the Marxist description. There was a demand for a safety lamp, and it invited research.[14] Marxists see this everywhere. Had Watt not supplied the engine, Marxists suggest, someone else would have done it—historical necessity created the demand that was bound to bring about the supply; eventually. Here Marxism replaces the inductive necessity that thinkers from Bacon to Herschel discussed with a social necessity, referring to the socio-economic basis of science. The demand for invention, Marx said, rests on the historically necessary economic situation. Any theory of the necessity of science renders accidental the individuality of any single research: if one researcher had not done it, another would. This is questionable: the results of research are unpredictable, much less their utility. Research is the search for the truth; researchers are happy to do it for love; only incidentally, as Bacon has suggested, science helps create technology, and this helps progress but at times it also impedes progress (Agassi 2003, 239).

The popularity of the view that science is utility led to a sharp decline of amateur natural science (Johnston 1851, Vol. 2, 241–59, esp. 243). Researchers employed in industry were few and their studies were restricted to matters of little intellectual value. Science was in danger of disappearing. What kept it on going for a while were a few factors: the love of learning, the national pride, the readiness of industries to employ researchers, and the eventual readiness of the universities to do the same.[15] This took about a century and a half, starting with Napoleon's foundation of the *École Polytechnique* that led to the rise of professional science. He did not care about the fruits of research, as he declined Robert Fulton's inventions (the steamboat, the submarine and the torpedo). He wanted science not for technological ends but for political ones.

Thus, the first drive for industrialization was not greed but enthusiasm for science—Bacon's naïve vision. The inventors were first self-made upper-class amateurs of all sorts, yet the inventors who finally counted were technicians of "the lower walks of life". The Fraternity gave them enthusiasm for novelty. From time immemorial, some rich people helped and encouraged the education of the poor and sent some of their children to schools. The success of the Fraternity brought about the recognition that learning includes discoveries and inventions as well as improvements of techniques. Self-made innovators counted then: the printer Benjamin Franklin, the clockmaker John Kay, the barber Richard Arkwright, the engineer George Stephenson and the researchers Humphry Davy and Michael Faraday. These were all lower-class self-made, encouraged to study like thousands of others before

[14] It may be objected that supply creates its own demand, to use the wording of Say's law, so-called, that J. M. Keynes introduced. This is a misreading of Say. In any case, Say's law does not hold in Marx's framework.

[15] David Budworth (at times assistant director of Britain's Technical Change Centre) reports the failure to bridge between university and industry (Budworth 1981, 2). "A good deal of this failure has been caused by taking action on the basis of illusions, not least illusions about science and technology."

them but differently (unlike, say, Johann Friedrich Böttger, the Dresden alchemist of an earlier generation): a powerful Fraternity offered them support.

The Fraternity was more than the club that the Royal Society was in its early days even though the inductive tradition allowed for no organization of research.[16] The rich then offered the poor researchers patronages. The creed of the time was that of the Royal Society, and it was not utilitarian. Utilitarianism appeared later and helped the rise of the class of trained engineers. These joined research much later, with the institution of the faculty of engineering. Technological publications appeared first with hardly any intellectual claims. Remarkably, the first of these was *Engineering and Science*, founded in the USA in 1937 by the Caltech Office of Marketing and Communications. Yet already in the mid nineteenth century, researchers made increasingly significant contributions to industry, starting with the electric telegraph, galvanization and organic chemistry, especially pharmaceutics and finally electric dynamos and motors. Amateur science and especially dilettante and semi-dilettante science vanished almost completely (and the debunking of Bacon as a dilettante appeared) while professional industrial scientific research just began. When industrial research got under way, researchers did not fail to demand their share in the profit. Until then research was hardly the business that blooms today.

When Bacon invented the system of rewards for researchers he did not think of money, perhaps because he imagined a non-monastic monastery (as it was later called) or perhaps because he was under the influenced of Thomas More, whose *Utopia* his *New Atlantis* echoed. However it may be, it was of great consequence that Bacon promised fame and gratitude to inventor-discoverers rather than money.

Bacon's successor—Robert Boyle—made a special point of restricting all scientifically recognized information to the publicly available. He saw that this caused financial hardship for those who lived on trade secrets and he felt unease about his having greatly reduced the tradition of trade secrets. The greatest reward for inventors that he offered was the recognition of their service in contributing useful inventions (whether their usefulness was realized or not).

11.4 Researchers as Dilettantes

> They are very seldom the same sort of people, those that invent arts and improvements in them and those that inquire into the reasons of things: this latter is most commonly practiced by such as are idle and indolent, that are fond of retirement, hate business and delight in speculations …
>
> (Mandeville, Fable, Part 2, Dialogue 3)

[16] A few commentators have suggested that Bacon had inaugurated organized research—perhaps as a solution to the riddle of Bacon. This was impossible, as it requires research programs and these he condemned as prejudices. The most outspoken idea of Bacon about research cooperation is his *New Atlantis*, and there no two researchers working on the same project are mentioned as cooperating in any way.

Poet Joseph Addison said, he suspected anyone he met who exhibited enthusiasm about science and ignorance about all else of being a Fellow of the Royal Society (Miller 2008, 4). This observation is needlessly nasty, but shrewd all the same. Although Fellows of the Royal Society were not all ignorant, possibly average Fellows then were not very broadly educated. Yet they contributed significantly to the advancement of learning. It is almost universally taken for granted that the Royal Society and its likes contributed much. This seems too obvious to discuss. Consequently, its daring is underestimated. A scientific academy is able to contribute in a number of ways. It publishes periodicals and books, organizes lectures, connects researchers in meetings, conferences, and more; it propagates science and at times proposes projects of research and offer money to defray costs. All this the Royal Society of London did from the start barring offering money. These actions were once much more significant than they are today, so that today the Royal Society is less significant, although it still is the leading scientific society. It is the role of scientific societies as such that was once much more significant than today (Agassi 2008, 180–94). They had a particular task that is no more. Its main task was to respond to the backwardness of the universities. It boosted intellectual progress while these strongholds of intellectual heritage stayed strong and stagnant. It advocated advanced theories: Copernican astronomy and Cartesian and atomistic metaphysics; above all, it displayed a liberalism that it hardly articulated. Also, under the direct influence of Bacon's *New Atlantis* the early Royal Society had a visionary idea of the ideal secular research college. In its embryonic state and in its formative years the Royal Society ran ordinary meetings, lectures, and public demonstrations of experiments. It aspired to become a college.

The Royal Society quarreled bitterly with the Aristotelian universities.[17] Culturally, the quarrel was ideological, between Ancients and Moderns. It continued for about two generations, yet formally it ended almost at once as Boyle received the title of Doctor of Physics in Oxford as early as 1665. This solitary acceptance of his of an honor was an act of reconciliation, for, he proudly declined bigger honors, including a peerage, a bishopric, the Provostship of Eton and the Presidency of the Royal Society. The reconciliation generated some professorships to Fellows of the Royal Society. The universities did not thereby become fortresses of progress, but they came to tolerate modern science—not as a compromise but as capitulation that expressed itself in mutual indifference.

In the classical era the Fellows of the Society were very much as Addison viewed them, amateur enthusiasts. They had little knowledge of science, much less of its problems or methods, they had laboratories of sorts in back kitchens or in garden shacks, perhaps in basements or attics, where in solitude they observed some of the beauties that nature revealed to her devoted but dilettante worshippers. If they found anything new, they would communicate their discoveries to friends, usually in private letters. If they were lucky and bold, they would report these at meetings or during

[17] To repeat, Sprat wrote defiantly in a radical mood that the Society had accomplished in 6 years more than others had in 6,000—since Creation (Sprat 1667, 154)!

discussion periods after some lectures. If they were very lucky, their letters would appear in the *Philosophical Transactions* or the editor might even ask them to write up their finds as scientific paper. This would be the excitement of a lifetime, but no material benefit. (This way Pushkin's 1835 story "Egyptian Nights" describes amateur poetry.)

Example: Benjamin Franklin narrates in his autobiography that in 1752 he sent his description of his famous kite experiment to a slight acquaintance who was a Fellow of the Royal Society and who then disappointingly sent it to *The Gentleman's Magazine*, whose editor published it as a separate issue—much to Franklin's annoyance. This story should convey the feeling of solitude that researchers had then, the slowness of the tempo of research, the true love of wisdom for its own sake, the dilettante character of the work, the concealed prophetic enthusiasm that characterizes those amateurs, and finally the immense prestige value that the Royal Society had among them.

To show that without the dilettante amateur the picture of the history of science is incomplete, here is Macaulay's description of the Baconian fervor of the Fellows of the budding Royal Society (1949, Chap. 3, State of England in 1685):

> All classes were hurried along by the prevailing sentiment: Cavalier and Roundhead, Churchman and Puritan were for once allied. Divines, jurists, statesmen, nobles, princes swelled the triumph of the Baconian philosophy. Poets sang with emulous fervour the approach of the golden age. Cowley in lines weighty with thought and resplendent with wit, urged the chosen seed to take possession of the promised land flowing with milk and honey, that land which their great deliverer and lawgiver had seen from the summit of Pisgah but had not been permitted to enter …

The immense success of the Society called for a reaction and the reaction included derision of its Fellows—as ignorant enthusiasts, to echo Addison. Perhaps Samuel Butler's verse, *The Elephant on the Moon* (1759), is the most familiar example, unless it is the earlier (1676) successful play of Thomas Shadwell *The Virtuoso* that ridiculed a fictitious Fellow of the Society, one Sir Nicholas Grimshack, who stayed for many decades in light-hearted discussions of the Society.

Indignant William Wotton commented (Wotton 1705, Conclusion)

> Yet the sly insinuations of the *Men of Wit*, That no great things have ever, or are ever like to be performed by the *Men of Gresham,* and, That every Man whom they call a *Virtuoso*, must need be a *Sir Nicholas Gimcrack*: together with the *public Ridiculing* of all those who spend their time and Fortune in seeking after what some call useless natural rarities; … have so far taken off the Edge of those who have opulent Fortunes, and Love to Learning, that Physiological Studies begin to be contracted amongst Physicians and Mechanics. For Nothing wounds so much as a Jest; … How far this may deaden the Industry of the philosophers of the next age is not easie to tell; …

Fortunately, "the next age" did very well, while opponents continued to mock at hard working, enthusiastic Grimshack. Addison complained (Stimson 1948, 128):

> I would not have a scholar unacquainted with these secrets and curiosities of nature … but certainly the mind of man, that is capable of so much higher contemplations, should not be altogether fixed upon such mean and disproportionate objects. Observations of this kind are apt to alienate us too much from knowledge of world, and make us serious upon trifles, by which they expose philosophy to the ridicule of the witty, and contempt of the ignorant …

> Studies of this nature should be diversions, relaxations and amusements; not the care, business and concern of life.

Researchers in Addison's circles were rather odd. But this is no reason why a poet should tell a researcher that pure contemplation is superior to pure experimenting, especially since neither exists. Yet so it was. Addison's view was not new. Hobbes, who preceded him, responded bitterly to Boyle's unfair assault on him. The peak of his *cri de coeur* was a sharp, general denunciation (Hobbes 1680, 53–4):

> Every man that hath spare money, can get furnaces, and buy coals. Every man that hath spare money, can be at the charge of making great moulds, and hiring workmen to grind their glasses; and so many have the best and greatest telescopes. They can get engines made, recipients made, and try conclusions; but they are never the more philosophers for all this. 'Tis laudable, I confess, to bestow money on curious or useful delights; but that is none of the praises of the philosopher.

This is a criticism of Boyle's self-appointed task of recruiting everyone to the new Experimental Philosophy. It is reasonable yet profoundly erroneous. Some of the enthusiasts attacked genuine problems and became genuine researchers, though in a small way—Hobbes was in the right when he viewed his own output as more valuable, yet in the wrong when he dismissed theirs.

Hobbes and Addison dismissed research as a passing fashion; this shows that research was indeed a fashion, that there was embarrassingly excessive enthusiasm about it. Nevertheless, the fashion was not passing; it worked. The enthusiasm survived as its owners adopted Bacon's doctrine of prejudice that identifies the impartial with the disinterested. Bacon had judged enthusiasm as conflicting with the calm disinterestedness of the good researcher. Indeed, Leslie Stephen has observed (Stephen 1876, 370, n. 45) that "a hatred of enthusiasm was strongly impressed upon the whole character of contemporary thought" during the high tide of rationalistic enthusiasm and over-optimism (see also Heyd 1995). It worked. This is why the style of scientific papers of the period had a deliberate dryness and bold dilettantism. In his chapter on the Royal Society Isaac Disraeli notices this (*Calamities and Quarrels of Authors*):

> The *Philosophical Transactions* were … accused of another kind of high treason, against grammar and common sense. … . Sir Hans Sloan … and most of his correspondents, wrote in the most confused manner imaginable. A wit of a very original cast … took advantage of … the meanness of their style … and their vanity that prided itself on pretty discoveries, and invented a new species of satire. …
>
> The new species of literary burlesque … consists in selecting the very expressions and absurd passages from the original he ridiculed, and framing out of them a droll dialogue or a grotesque narrative, … replete with the keenest irony, or the driest sarcasm. …

Disraeli's description is vivid, fair, and dignified. All the same, he too missed the point. The inductivists imitated the dilettante style of Bacon and Boyle both in order to encourage the genuine dilettante and in order to make induction—they did not know how. They had the authority of Bacon, Boyle, and Locke to justify their activities as true learning and Newton's glorious success as ultimate proof. The following passage from Locke's *Conduct of the Understandings* (Sec. VII) seems fairly characteristic:

> Perhaps it will be objected, that to manage the understanding, as I propose, would require every man to be a scholar … . To which I answer, that it is a shame for those who have time and means to attain knowledge, to want any help or assistance for the improvement of their understanding that are to be got … Those methinks, who, by the industry and parts of their ancestors have been set free from a constant drudgery … should bestow some of their sparetime on their heads, and open their minds by some trial and essays in all the sorts and matters of reasoning.

Following Boyle, Locke demanded less scholarship and more small and simple contributions, especially from the well off. Here is an assessment of the situation, taken much later, from the fourth edition of *Encyclopedia Britannica* of 1810, Art. Science Amusement and Recreation:

> … scientific recreations must be regarded as entirely modern as previous to the era of Lord Bacon, philosophers were much more attached to rigid demonstration and metaphysical reasoning, than experimental illustration. Much may be found on these subjects in the works of Lord Bacon and Mr. Boyle, but the earliest collection of scientific amusement which deserves notice, is the work of Jacques Ozanam, entitled *Recreations Mathematiques et Physiques*, published in 1692.

This is an expression of an established movement, just before its decline. In its early days, its spokespeople were dead serious when they were busy defending its new experimental philosophy as against their critics, in a famous battle that was soon christened the battle between the ancients and the moderns. Their severest, most forceful and most hostile critic, Dr. Henry Stubbe or Stubbes was interested in new experiments and even cooperated with Boyle, and he was not very defensive about ancient texts. Yet he attacked the Royal Society most severely for its radicalism and he was right. In the classic *Ancients and Moderns*, R. F. Jones expresses less appreciation of Stubbe than of the balanced and scholarly classicist Meric Casaubon, who was more defensive of ancient texts, since he saw the movement as materialistic-utilitarian and thus as bearing the seeds of atheism despite itself. He did not object to experiments, of course, or to any researches. Yet he approved of them only when he viewed them as recreations and amusements; taken seriously, they would oust older virtues, he feared. For Casaubon, then, advocating experiments as recreations and amusements is putting them down; not for Ozanam: he was following the spirit of Bacon and Boyle, who had promised in return for the investments of small effort that anyone can afford some great results that should change human life on this earth. Casaubon feared that these results would generate frivolity. His concern was thus *élitist*. In the last resort Bacon and Boyle were egalitarians[18] and Ozanam understood them well. Of course, his contributions were partial and amount to little without the efforts of the many great popularizers of science.[19]

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[18] This was daring, as is evident from Boyle's having written an essay in praise of tradesmen (Boyle et al. 1999, 6, 467).

[19] Strictly speaking popular and professional science are twins—born in the mid-nineteenth century. Speaking not of professional and amateur but of expert and inexpert, renders popular science very important ever since Galileo—both for the generating of expert amateurs and for the raising of the educational standard of citizens: average citizens of a modern country today know more practical medicine than experts in the mid-nineteenth century did.

All this, I am afraid, is misleadingly incomplete. What is missing is this.

The dream of an ideal secular college vanished—perhaps even before the Society received its Royal Charter. The function of an ideal college was confined to the Society's early years. The true function of the Royal Society that it carried out successfully for two centuries was to be the recreating and organizing body for amateurs as well as to propagate and promote science in all of its manifestations.

The Society has changed (http://royalsociety.org/about-us/history/):

> The Government recognised this in 1850 by giving a grant to the Society of £1,000 to assist scientists in their research and to buy equipment. Therefore a Government Grant system was established and a close relationship began…

Before 1850 the Society was a proud amateur club that would not accept a grant—from the government or form any other source. In the second half of the nineteenth century. Darwin represents the amateur; Kelvin had as his major source of income his patents and the business of the Transatlantic Cable Company (in addition to his professorship). He represents the small group of professional researchers, the physicist-engineers. The Society for Telegraph Engineers appeared in London in 1871 and later became the Institution of Electrical Engineers; the Institute of Electrical and Electronics Engineers appeared in New York in 1884.

Chapter 12
The Missing Link Between Bacon and the Royal Society

12.1 The Rise of the Royal Society

Historians who write on Bacon's Utopian college view it as an inspiration for the early Royal Society. They offer two versions of this inspiration as to the precise role of the Society: as an educational institute for training or as a research institute. Historians who advocate the version of the society as an educational institute view as the core of the Society the group of educational reformists that included Samuel Hartlib (the much-loved person who tried to prevent the civil war), Jan Amos Comenius, Sir William Petty, and later also Robert Boyle and John Beale. Historians who advocate the version of the Society as a research institute view the core of the Society the science study group that Boyle labeled "invisible college … or as they term themselves, the philosophical college"; it included John Wallis and John Wilkins, and later also Boyle, John Evelyn and Beale. Possibly the early Royal Society endorsed both ideas as it emerged as a union of these two groups. This depends on the ideas that these groups brought with them to the Society.[1] The idea of the educational reformers was Bacon's radical hostility to all established education. To repeat, the Society advocated radicalism but had to behave moderately.

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[1] A third precursor was Gresham College, the only one that was an institution proper (founded just before 1600). The parties to the current dispute overlook it—because their dispute is more ideological than institutional. Thus, Weld mentions in his *History of the Royal Society* many precursors that today historians utterly ignore. Thus, not Gresham College (or its several professors) but the informal meetings in it comprise the obvious though still overlooked precursor. Francis Johnson is different (Johnson 1940). The criterion that he offered (424) of the significance of members is of their contributions to science proper. No contribution to science occurred within the walls of Gresham College, yet Johnson appreciated it, as it "was a center of scientific activity in London from the beginning of the seventeenth century". He found in this story sufficient ground for his claim that Gresham College is the true precursor of the Royal Society.

Founding Gresham College around 1600, just like founding the University of London in 1830 over two centuries later, required money and hardly anything else. Founding a philosophical society was hard in other respects but it cost almost nothing. Fellows paid membership dues that included subscription to its *Philosophical Transactions*; this was it. The first grant the Society received was in 1850.

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_12,

Historians of the rise of the Society agree that the precursor groups were closely knit, that they drew their inspiration from Bacon's writings, that he had a vision of the research college in New Atlantis, and that he had some ideas about educational matters that deserve attention as they influenced Comenius, Leibniz,[2] and others. Which of these two ideas of Bacon's was more influential, then? This problem is not serious, yet it lies at the root of the discrepancy between the two versions. The *Dictionary of National Biography* subscribes to both. Under Art. Petty, Sir William, it says that Petty's educational letter to Hartlib provided a scheme for a college that the Royal Society served as its realization. This is evidently an exaggeration. Under Art. Evelyn, John, the same *Dictionary of National Biography* says that Evelyn's letter to Boyle served exactly the same purpose. The information is true; its weight is under dispute: soon after Boyle received Evelyn's historical letter, Boyle and Wilkins, the core of the Invisible College, called the meeting that founded the (not-yet-Royal) Society. The same *Dictionary of National Biography* also states, in Art. Wilkins, John, that, more than any other single person, it was Wilkins who led to the founding.

A balanced and well-considered judgment concerning this dispute over the foundation of the Society is extremely hard to come by. Probably none of the current views is quite right. The problems that any of these views raises are too difficult and too artificial and none of the participants in it is quite disinterested. (Nor do I claim this for myself.) Attention to the aims of the Society may help set clearly the limits of the dispute. It will place it on the first decade of the existence of the Society. For, whatever the initial motive for the founders of the Society was, it soon dropped its educational motive as well as its academic research motive: the Society was neither a school for research nor a research institute. It was a club of amateur self-made researchers. Both parties to the dispute extend it to more than its early years. They are in error; it stems from playing down the amateur and dilettante character of the Society—not to mention the misreading of Bacon's works. He had two different though not mutually exclusive ideas. (Hall mentioned both; Hall 1975, 174.) One was Bacon's vision of the Solomon's House that is a research institute. The other was his amateurism, his injunction to start experimenting here and now. Erroneous though his view of experimenting was, it was very useful. And the founders of the Society, whether they were better or worse researchers, whether they were interested more in education or less, they were all grateful to Bacon for his having urged and encouraged them to experiment. They were all Copernicans and Cartesians, and they wanted to be active researchers. Not all of them knew enough mathematics and astronomy. Bacon provided them a proper alternative. True, he was hostile to Copernicus, but this is a minor point; the major point is that he prevented them from stagnating into a school by telling them that all the novelties must unite as small parts of the one magnificent Utopian whole science that will change the world. He showed them how to do it. Ellis' criticism of this part of Bacon's teaching is quite valid; Liebig's ridicule of this same part is likewise quite just; they are both not to the point—not in the least.

[2] The famous view of the university as a school for teaching universal knowledge that Leibniz advocated is rudimentarily but explicitly stated in Bacon's writings. They are as scanty as his ideas of Solomon's House. Nevertheless, his Utopian enthusiasm excited readers to develop both. See below.

The proportion of amateur scientists in the Society grew surprisingly within the first 3 years of its foundation (when special regulations rendered membership easy to acquire), and it was still very high in 1830, when Charles Babbage complained about its social composition. He said, too many of its members were amateurs and mostly dilettantes. This is the decisive point. If "the new Philosophy" is an injunction to experiment, then, indeed, it was anything but new. This, we may remember, is the core of Brewster's critique of Bacon. Additionally, as far as "the new philosophy" was identified with Boyle's Cartesian-Gassendian philosophy it had nothing whatsoever to do with the Society's indebtedness to Bacon, who opposed metaphysics. But the "new philosophy" was very new nevertheless. Only a generation earlier Gilbert sneered at the multitude and declared publicly "philosophy is for the few" (Gilbert 1600, Author's Preface). "I think, my Kepler", the wise Galileo wrote (August 19, 1610), "we will laugh at the extraordinary stupidity of the multitude". Bacon, the ignorant dilettante, found the right path to the heart of this "multitude" and he drew the best out of these barely educated people. He was the first to tread the path that the Society followed. Members of that amateur club were deeply grateful to him for his having encouraged them and boosted their faith in their experimenting.

Not two trends go back to Bacon, but three. There is the obvious dream of a Utopian laboratory in the research-institute. There is the dream of a Utopian college—where novices become researchers. And then there is the dream of a loose society of amateur researchers. He did not even dream of a club for them. He did not even ask, how they should pool together their empirical findings. These ideas belong to the missing link. The missing link is Robert Boyle. Had the parties to the dispute about the origins of the Royal Society investigated the missing link, they could develop a more sociological, more accurate, and a more interesting image of the pre-history of the Royal Society.

The nearest to this that I have found is Philip George's paper on the rise of the amateur tradition as reflected in the Society's *Philosophical Transactions*. It concerns the way that the society of amateurs got started and the way it lasted. Let me discuss Boyle's contribution to this process, regardless of who started it and how big were the contributions of others. John F. Fulton tried to connect the admiration that Boyle won from his peers with his having been an amateur, proud of his status as an amateur. He was the most esteemed philosopher of his age (Shapin 1994; Agassi 1997) although scientifically both Hooke and Newton were more successful than he was. This is explicable by the fact that while they were considered as great pupils he was regarded as a teacher—not the esoteric teacher who lectures in his academy, but the amateur teacher who trained the army of amateurs.

12.2 Boyle's Spirit

> 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!
>
> (Malthus, *Principles of Political Economy,* Introduction)

Bacon died almost entirely isolated. His only friend who survived to see the foundation of the Society was Thomas Hobbes, and he quarreled with them hard. How then did the Baconian tradition start? His books were popular in England and on the Continent even more so. (Hegel's myth of British Empiricism and Continental Rationalism was not yet born.) His first great follower was Father Marin Mersenne, a leading international figure of French philosophy, whom Bacon impressed deeply. Harcourt Brown describes Mersenne's group as an early "Invisible College" (Brown 1967, 41 and 45). He also quoted (Ch. 2) an interesting correspondence between Mersenne and Theodore Haak, whom Wallis described as "a German palatine and resident in London who I think, first gave the occasion and first suggested the meetings" in London that Boyle later labeled "the Invisible College". The idea of the reformed college—educationally or scientifically—seems to have been then the strongest influence on the learned readers of Bacon's texts. Some of them started to experiment, but this did not amount to an experimentalist movement proper as yet.

True, a new informal institution did appear and then became somewhat less informal and a Royal Charter gave it kudos and permanence beyond dreams. True, the problem for which its members founded it they successfully solved early in this process. Historians of the Royal Society tacitly and uncritically advocate the view that this was somehow sufficient for the creation of the whole enterprise of a durable, widespread research network. This is untenable. The Society had only 12 chartered members. How could such a Society (with or without a Royal Charter), popularize science or advance it in any way merely by organizing itself into a sort of fashionable scientific club and run a constant publication of scientific novelties?[3] After all, the Royal Society was not the first scientific club: nothing like its impact appeared in the similar unofficial ones scattered then all over Western Europe and the formal ones, including the Academy of the Lynx to which Galileo belonged. No one ever tried to discuss the question, how the Royal Society grew, recruited new members, became popular, etc.[4] Only Philip George has raised the problem (George 1952), and as yet no one took up his challenge.

The Royal Society was a traditionalist institution, even by British standards—perhaps because its Fellows were convinced that they acted purely rationally and not because of any precedent. Perhaps it was the recognition that only the aristocracy was able to contribute amateur researchers. Indeed, the middle class and then the working class joined as soon as they could. The Royal Society began with Robert Boyle as its intellectual leader whose works were very popular over a long period before they sank into oblivion. He evidently took it for granted that rational social thinking is conservative—perhaps because he deemed the civil war ample proof for that. His father, who came from "very inconsiderable beginnings", ended as an aristocrat and the richest man in the realm and a staunch conservative Royalist. He

[3] "In the seventeenth century this Royal Society was in no sense a professional organization in its origin but rather a gentleman's club for the discussion of scientific matters" (Stimson 1939, 40).

[4] The best evidence for this is the publication of the seminal works of Antonie Leeuwenhoek of Delft in *The Philosophical Transactions of the Royal Society* in 1673.

sent his son Robert to Eton when he was almost 9 years old. There the Provost was conservative Sir Henry Wotton, who died 3 years later. It is hard to suppose that Wotton had left a great impression on the boy, but significant evidence shows that he did—politically and intellectually. A two-sided explanation for this is possible. Wotton, by his friend Izaak Walton's report, was an able and dedicated teacher. In addition, presumably, the sons of the Earle of Cork received favorable treatment due to their rank wherever they went. Personally, too, they were Wotton's favorites: he could not but find Robert impressive both as a gentle soul and as a talent. As Robert was a shy lonely person, Wotton's attention probably left its great impression on him. What is important, however, is that Wotton opened for him the door to scientific research.

Wotton was a genuine dilettante lover of wisdom. He published a translation—or rather a free adaptation—of *de Architectura* by Marcus Vitruvius Pollio[5] and a short memoir. He wrote some poetry, biography and history; he tried his hand in experimenting. Only a few poems—two of which Cambridge history of prose and poetry praises sky-high—were finished and the rest was partial and of questionable value. His posthumous publication (Wotton 1651) is unfortunate. He admired Kepler; he admired Bacon too, and not only because they were cousins.[6] A dilettante experimenter, he was Bacon's first English disciple. Boyle deemed him a good educator and popularizer of Bacon's experimentalism. Some information on Wotton comes from letters that Robert Boyle received from his close friend John Beale (his senior by nearly two decades). Beale was a pupil in Eton before Boyle entered, as he had to leave when he was 19. Decades later he wrote to Boyle telling him that Wotton had given him unpublished manuscripts of Bacon to read and these had freed him (Beale) of scholasticism. Beale reminded Boyle there of his promise to rewrite Bacon's dubious *Sylva Sylvarum* as a projected similar work called *The Promiscuous Experiment*. He did not. Why? Possibly, Flora Masson's gossipy 1914 life of Boyle (that almost ignores his researches) includes a hint at an answer: Boyle's sense of debt to Wotton made him a Baconian. This is not serious and hardly explanatory. I suggest that both Beale and Boyle were grateful to Wotton and to Bacon and knew that *Sylva* is full of errors and superstitions. Unlike Beale, Boyle would not fully suppress his criticism.

All this is secondary. More significant by far was Boyle's leading purpose: to fulfill Bacon's dream as he understood it and create a society of amateur scientists. He may have learned from Wotton's example how unhappy is the result of purely dilettante research in the purely Baconian fashion and how powerful the amateur's love of wisdom can be—especially when supported with a little guidance. One way or another, Boyle

[5] It is not clear what made Wotton care for Vitruvius. Perhaps the following two details explain it. He was banished form the court of Queen Elizabeth because he said an ambassador is "an honest man, sent to lie abroad for his country": it did not put her in good light. And he probably saw in Vitruvius an expression of ancient virtue: "commodity [= usefulness], firmness and delight", as he put it in his translation.

[6] Bacon sent three copies of his *Novum Organum* to Wotton, who dutifully sent one of them to Kepler. The reticence of Kepler on the book may be indicative, but it need not be: there are many possible explanations for the reticence of a busy researcher regarding a book by an unfamiliar author.

deliberately played the dilettante and he urged others to join him, adding to the amateurism he propagated a few simple rules that sufficed to turn the collection of facts, apparently similar to that of Bacon, into scientific material—at least partly so.

Only R. F. Jones noticed (Jones 1982, Ch. 5, 166) that Boyle "was the missionary *par excellence* of the experimental philosophy". Alas, he too missed major points of the story. First, Boyle did advocate an experimental method, but his method differed from Bacon's. Second, Boyle aroused interest by describing some simple but important experiments and by teaching his readers how to perform them and invent variations on them and invent similar experiments. The main significance of Boyle's works is that they teach names of chemicals, ways of disposing of impurities, the difference between a mixture and a compound, ways of manipulating simple chemicals, ordinary methods of precaution against burns and other dangers, the method of testing other people's theories by comparing their results with facts, methods of devising very simple hypotheses and simple experiments, methods of simplifying other people's discoveries, writing a scientific paper, etc., etc. and then, later, he added, you do not need the best instruments to perform experiments (*Works* 1999, 6, 31–2). And all this he wrote with the utmost simplicity in a mixture of reportage, narration, anecdotes, and the occasional mention and admirable elucidation of some interesting metaphysical idea. He repeatedly apologized for the simplicity of his work and the allegedly low standard of his experiments and with the greatest humility he appealed to the patience of his readers and their goodwill and goaded them to experiment.

His works were very popular. His faithful friend Beale repeatedly reminded him that his works were too difficult. Today it looks differently, and commentators still complain about the excessive details of his descriptions. They are just ignorant of his intention to bridge the gap between writer and reader sufficiently to make them volunteer to perform experiments. His works looked untidy and confused—not only because he was a very untidy person, since he was capable of an amazing self-control and discipline when he thought it necessary. He evidently thought it better to publish frequently and sloppily. He was the founder, organizer, and teaching staff of an unofficial adult education correspondence school, a school that taught some metaphysics, physics, methodology, chemistry, mineralogy, physiology, medicine, and everything else he could learn by himself, and, above all, the art of publishing a paper as soon as possible.[7] The repeated complaint that he was prolix and superficial is true but unwise.

[7] A passage form Boyle's early *Certain Philosophical Essays* (1661), Proëmial Essay, first paragraph (*Works* 1999, 2, 10) reads:

> "Nor should I … have ever prevail'd with my self to present you so early these Discourses, since by keeping them longer by me, I might easily by second Thoughts, and fresh Experiences be enabled to correct and enrich them, did not the frequent and dangerous distempers to which my very sickly Constitution has of late render'd me obnoxious, make me justly doubt, whether or no, if I should long forbear to write, Death would not sooner come than the expected Maturity of Age and Judgment. And though I had no such Considerations to move me to make haste to render to you the ensuing Discourses, yet this would suffice to engage me to present them to you with all their present defects; that if I should keep them till I can make them less unworthy of you, I must keep them till you are grown past the need for them."

Or as Faraday has put it, "Work; finish; publish" (Thompson 1901, 267).

The amateur movement was taught in combination both the mechanical philosophy and, by illustration, how to perform an experiment, especially chemical and biological. The enthusiasm for the New Philosophy included an enthusiasm for the theories of Descartes and of Boyle. In 1671, at the height of his influence, he took pride in his contribution to the experimental method in the mechanical-corpuscular style (*Works* 1999, 6. 594–5). He made there a complex claim. He was the first to apply the new philosophy to chemistry. (He was the first to interpret chemistry in the light of the new philosophy, to use a modern idiom.) Thereby he made chemistry intelligible to amateur readers. He presented to them the subject-matter clearly and in an intentionally amateur presentation. He thus offered them good impressions of the study of nature. He thus managed to "persuade a great number of differing sorts of readers" to make "simple experiments".

It is scarcely possible to consider such sophistication as Boyle's output displays a variant of Bacon's doctrine, yet this is the current view of it. Whereas Bacon claimed fame for his having freed us his readers from scholasticism by his demand that we empty our minds, shun metaphysics, and start some small-scale research, Boyle's parallel claim was that he replaced scholasticism with the mechanical metaphysics and taught people how to interpret phenomena in its light, thereby coaxing, not demanding, that his readers repeat his experiments and vary them. He offered his readers recipes for conducting scientific research proper by reporting to them regularly the recipes that he himself was following. His was a true leadership. His research program survived in variation (together with Newton's, and as a competitor to it) until Planck and Einstein appeared.

12.3 Boyle's Views on the Spread of Science

At the age of 20, Boyle complained that the "Invisibles" were too few:

> … their chiefest fault, which is very incident of all good thing; and that is that there is not enough of them.

His earliest famous work, *Seraphick Love*, is propaganda for research, pledging to do something about the paucity of researchers. The title of *Seraphick Love* suggests that it is theological. Many commentators say that it is.[8] Yet in it he stated (Sec. 2) what he later repeated in many ways: the universe is a temple, research is worship and the researcher is a priest.[9] We can learn only a few of God's infinitely many attributes, and one of them is the material universe.[10]

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[8] This error is understandable: Boyle's verbosity that was so useful then is intolerable now. In order to read it I had to write down a précis of it first. Only then could I find that it is not really theological.

[9] The idea that the universe is a temple, Boyle says, he borrowed from Philo Judeus. The idea that the researcher is a priest is his own, and it enhanced Philo's idea by infusing it with a practical suggestion.

[10] This idea is usually ascribed to Spinoza, Boyle's younger contemporary, as he was the first to elaborate on it. Berkeley repeated the same idea in a variant in his *The World as Divine Visual language.*

> And I must need acknowledge … that when with bold telescope I survey the old and the newly discovered stars and planets … and when with excellent microscope I discern … the … subtlety of nature's … workmanship; and when … by the help of anatomical knives and … chymical furnaces, I study the book of nature, and consult the glosses of Aristotle, Epicur, Paracelsus, Helmont and other etc.

> This is what philosophers call by the scholarly name the physico-theological proof of the existence of God, a name that is due to Boyle and is typical of his humorous compounding of words.[11] Boyle considered this proof as rationalistic refutation of atheism. But he also used it, as he used all his other ideas, as a vehicle for propaganda; as almost any idea he touched upon, this proof too is a vehicle to propagate the idea of experimenting. *Seraphick Love* is Boyle's old-fashioned recommendation to his reader the sublimation of sexual love into divine love, plus the new recommendation to translate the latter into deeds, into experiments, thus entering the promised land, the "celestial Canaan", etc. (Sec. 26).

> Yes, Lindamore, in that blessed condition, our wills being perfectly conformed unto our maker … and because our personal capacities are too too narrow, to contain all that joy, we are … in a manner multiplied into … many happy persons …

The great success this sentimentality had when it was published (in 1659) shows that it struck the right chord. Poet Abraham Cowley adopted the metaphor of Canaan, the land of promise, in his famous ode to the Royal Society. This is a mere gloss. What matters is that in the end of *Seraphick Love* Boyle promised his readers to return to them to teach them the nature of the duties of a "Seraphick lover". This he did throughout his long career as a successful author, repeatedly presenting science and religion as partners. As he put it in his *Usefulness of Experimental Philosophy* (*Works* 1999, 3, 199–200),

> … The two chiefe advantages, which a real acquaintance with Nature brings to our Minds are, First, by instructing our Understandings, and gratifying our Curiosities; and next, by exciting and cherishing our Devotion.

And so it proceeds at a great length telling about the joy of Archimedes when he ran shouting "eureka!", citing Aristotle's "elogium to natural philosophy" and Seneca on his love of science, and declaring research to be "sacrifice of praise" and scientists to be priests. Boyle expands there on Bacon's idea that knowledge has power and quotes him to say that although a little philosophy may distract from religion, more philosophy brings the mind back to it. This is an example of the way Bacon influenced Boyle from the very beginning of his recruiting work: not in matters methodological.

Much of Boyle's propaganda that includes some Baconian arguments in its earlier stages sounds, as R. F. Jones has rightly suggested, like interpretation or even mere repetition of Bacon's ideas. Obviously, this is how Boyle intended them to sound. And behind his propaganda he hid his own thoughts, both because he had

[11] "Hermetick thoughts" (scholasticism), "Hermetick language" (German), "superstructure", "Protestant prejudices". "Catholick matter" (universal primary matter), "Chymico-physical doubts", and "Cosmical suspicions", are some other examples.

decided to appear superficial and because he meant to conceal his deviations from Bacon's teachings. This appears strongly in two theological works that he published anonymously, *Things Above Reason* and *Advice about Things Transcending Reason*. The former work concludes with the assertion that his skepticism and his admission of the limitation of science only make research ever more valuable (*Works* 2000, 9, 393). The latter offers some commonsense rules. First advice: do not be quick to offer assertions: assertions are often results of some choice between competing options, a choice that rest on the need to be consistent and so they involve judgment that is better rendered more intelligible (397). This experiments help improve (399).[12] Second advice: do not be quick to offer denials or even to suspend judgment'[13] seek information in experiment to help you with that (401). Let me skip the rest.

Here Boyle explicitly abandoned Bacon's doctrine of the empty mind. He did not abandon its part that recommends wariness about the publication of hypotheses. He did abandon Bacon's idea that artisans are better informed and greater discoverers than scholars are. He did not identify applied science with pure science; he saw it as a vehicle to arouse interest. He viewed genuine researchers neither as scholars nor as artisans. He persistently called them "curious" or "curiosi", "interested", "lovers of truth" and "virtuosi". Science is the pursuit of truth, "intellectual satisfaction", "devotion". Although Boyle shared Bacon's appeal to develop amateur research, he greatly deviated from his teaching, even regarding the theory of intellectual gratification. Casual as Boyle's works were, his correspondence (e. g., Oldenburg's letters to Spinoza) show that he was very careful and sincere in wording his views, especially when his views deviated from those of his acknowledged masters—Bacon, Descartes and Gassendi. (In that correspondence, Spinoza demanded of Boyle to admit explicitly that in certain passages he was critical of Gassendi; Boyle declined.)

Boyle deviates from Bacon's view that science is the love of wisdom and of power to the view that science is the love of wisdom, no matter how satisfactorily its power can be employed. John F. Fulton rightly laid a great stress on the fact that in Boyle's earliest works, as well as in his very last ones, he declared that he was no Professor, no lecturer or doctor or gown-man. His wealth allowed him to devote his time to the pursuit of pure wisdom. But he would not declare that others, perhaps less fortunate than himself, should avoid earning their living by research. Yet in opposition to making research mainly a profession, he laid stress on his own amateurism and curiosity as the motive for his work.

This also explains the odd conduct of the individual who lived austerely and died a bachelor, who was exceptionally open-handed, who made large contributions to missionary and church charity funds, and who saw during the last years of his life

[12] It is amusing that this great asset of the search for knowledge as means for improving judgment has dropped out of epistemology early in the twentieth century as the study centered on choice given evidence instead of the search for it. Abraham Wald's decision theory brought this idea back to the fold, but philosophers ignore it.

[13] A conservative, Boyle rejects the strict demand for the suspension of judgment as politically radical (398).

the marked decline and near dissolution of the Society to which he devoted his life: he expressly left no money to the Society.[14] He wanted the Society to be of a certain character. (For details see Appendix C.) Of all the leading founders of the Royal Society, he was the only one who was never enthusiastic about a Solomon's Houses and the only one who directed all his activities towards the foundation of an amateur research movement. He probably thought the organization of the movement, the Royal Society, a sufficiently significant tool to invest in it much work. But most of his activity was personal, not institutional, perhaps because of his extreme individualism, perhaps because he feared that organizations might deteriorate. Whatever his motive was, in his profound skepticism mistrust was among them—mistrust in everything within this world but for the human spirit.

[14] In his will he left much money for charity and for missionary work, not for the Society. In the clause where he left for the Society his valuable collection, he mentions "mineral (except jewels)", clearly to avoid any possible misunderstanding and to emphasize that he wanted to leave for it no money and nothing of financial worth.

Chapter 13
Boyle in the Eyes of Posterity

13.1 The Eighteenth Century

Boyle was a most realistic teacher. No scientist in history, not even Newton or Einstein, gained so much respect during his lifetime as the modest Boyle. His authority was unquestioned; as one who aroused enthusiasm, he exceeded Newton, Einstein and Bohr. It is characteristic of him that under doctor's orders, and in order to be able to publish his works regularly, he put a board in the front of his house stating which morning and afternoon in the week he did not receive visitors (*Works*, 2000, 14, 363; Maddison, 9, 1951, 1–35 and 11, 1954, 38–53). Praises and tributes paid to him by contemporaries, even if greatly exaggerated, are most remarkable. His works were republished for about a century. A Latin edition and an epitomized English edition of his works appeared soon after his death and two English editions of his works followed two English and two Latin editions of his philosophical works in the eighteenth century.[1]

Dr. Johnson remarked (*The Rambler*, 106, 23 March 1751),

> … The authors of new discoveries may surely expect to be reckoned among those whose writings are secured of veneration: yet it often happens that the general reception of a doctrine obscures the book in which it is delivered. … we seldom look back to the arguments upon which it was first established … It is well known how much of our philosophy is derived from Boyle's discovery of the qualities of the air; yet … very few have read the detail of his experiments. His name is, indeed, reverenced; but his works are neglected…

How serious this comment is I do not know, since the complaint that impressive works suffer neglect is ubiquitous. Evidence shows that Boyle was widely read and highly esteemed in the eighteenth century. (Jean-Baptiste le Rond d'Alembert, the co-editor of the *Encyclopédie*, called him as "the father of experimental philosophy".) His reputation declined when all his theories became obsolete. His works on hydrostatics

[1] The complete editions of Boyle's *Works* were subsidized by special benevolent funds (Hunter 1994, 2).

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_13,

and on the elasticity of the air were superseded in his lifetime, but this did not hurt his reputation. The contributions of Priestley and Lavoisier did, as they left his chemistry far behind. Works by Dalton and by Gay-Lussac on gases had a similar effect. Inductivism is merciless to theories found insufficient.

13.2 Herschel's Unfair Comment

In 1831, as Brewster debunked Bacon Herschel debunked Boyle; he did so authoritatively, setting the tone for a century (*Preliminary Discourse*, 1831, 115):

> The immediate followers of Bacon and Galileo ransacked all nature for new and surprising facts, with something of that craving for the marvellous which might be regarded as a remnant of the age of alchemy and natural magic …. Boyle in particular, seems animated by an enthusiasm of ardour, which hurried him from subject to subject … with a sort of undistinguishing appetite; while Hooke (the great contemporary, and almost the worthy rival of Newton) carried a keener eye of scrutinising reason into a range of research even yet more extensive. As facts multiplied, leading phenomena became prominent, laws began to emerge, and generalizations to commence; and so rapid was the career of discovery, so signal the triumph of inductive philosophy, that a single generation of the efforts of a single mind sufficed for the establishment of the system of the universe, on a basis never after to be shaken.

Today we take for granted Einstein's view of Newton as the greatest and the most imaginative physicist ever (Einstein 1950, 220); not so the inductivist Herschel who was ambivalent about the imagination and about genius: since anyone can use the induction machine and since just in Newton's days the data for Newton's law of gravity became available to all, his admiration to Newton had to be lessened (Agassi 1981, 193). He met this unpleasant situation with an expression of disdain at other researchers of Newton's time for their neglect of a golden opportunity: they did not grasp it as Newton did, thus proving their not having been sufficiently ready for the magnificent task and so not worthy of it. This is a thin reason for the admiration of Newton, and so Herschel elaborated. Of all people Boyle should have not failed to grasp the opportunity, as he had generated and applied a general program of empirical research. So Herschel came down on him hard. He was and had to be especially unfair to him. He accused him of attention to "alchemy and natural magic" (see quotation above), forgetting that Newton was equally open to this charge (Dobbs 1991). Boyle's usual calm left him twice and then he admonished Glanvill and Stubbe for propagating beliefs that Herschel described contemptuously.[2] They were in error but this does not justify Herschel's derision: everything looked to the adherents of the new philosophy new, surprising and marvelous. Boyle's refutation of many folk beliefs made ordinary items glitter. His impact was so great for the very reason that made Herschel ignore it: it concerned not the sun and the moon and the stars—not universal

[2] Disraeli defended Stubbe gallantly (*Calamities and Quarrels*, "The Royal Society") by being unnecessarily harsh to Boyle, whose criticism of both Glanvill and Stubbe clearly displays his impartiality.

gravity—but many small things of everyday life and Herschel had no patience to see all this.

To gain the acclaim of posterity is not the chief aim of research. The inductivist system of praise and reward to discoverers may very well function as valuable encouragement, but a system of rewards, whether of contemporaries or of posterity, whether of fame, position, or remuneration, as any system of reward, is inevitably coupled with a system of penalties. Mildly dangerous as it may be, dangerous it is. And though the system is much less dangerous when controlled by scientific institutions than by states, we should beware of them too. It is not achievement but effort that counts, and all research efforts are voluntary. Against Herschel's disdain for Boyle's alleged lack of theoretical achievement, we may consider Boyle's Baconian statement concerning his efforts (*Works*, 1999, 2, 20–1):

> I know also that the way to get Reputation, is, to venture to explicate things, and promote Opinions … whereas by the way of Writing to which I have condemn'd my self, I can hope for little better among more daring and less considerate sort of men, … than to pass for a Drudge of greater Industry than Reason, … But I am content … to contribute ev'n in the least plausible Way … and had rather not only be an Underbuilder, but ev'n dig in the Quarries for Materials towards so useful a Structure, as a solid body of Natural Philosophy, than not do something towards the Erection of it. Nor have my thoughts been altogether idle … in … attempting to devise Hypotheses … but I have …found that what pleas'd me for a while … was soon after disgrac'd by some further or new Experiment …
>
> I have notwithstanding all this on some occasions adventur'd to deliver my Opinion, not that I am very confident or being less subject to err … but because … I scruple not to run the … venture … to hazard the being sometimes mistaken, than not to afford Inquisitive Persons their best Assistance towards the Discovery of Truth.

This moving passage should suffice to show the superiority of Boyle's sober attitude to Herschel's disdain—concerning both method and effort. The aim of Boyle's paper from which the above quotation is taken was to establish a tradition of writing experimental essays. Since at the time experiment was still in low esteem and since the number of people engaged in it was very small, it is readily understandable that he advised against the publication of scientific papers unless they contain new information and against the publication of hypotheses prior to having tested them, and even then to publish them only in addition to new information. Boyle put this demand in a way that would be most acceptable to his readers who were (like Herschel) ardent believers in Bacon's doctrine of prejudice. Boyle rejected this doctrine; but as he agreed that information is superior to theory, he accepted some of its consequence. Against Bacon's demand for proof (that Herschel took for granted) Boyle emphatically declared no hypothesis provable.

Boyle did not look for a criterion for distinguishing between theories sufficiently and insufficiently founded, since he deemed sufficient data necessarily unavailable; rather he distinguished between a theory that is ripe and one that is unripe for publication. He saw this as a major methodological problem. Like all his problems, it was both theoretical and practical. He could not solve it to his own satisfaction. He offered instead examples of the best modern theories in his field he knew: those of Galileo, Gassendi, Descartes and himself. He had invented many hypotheses, he reported, some of which he refuted himself, others of which he never published, and some others which he did publish, "adventuring" to take the risk of being found in error—out of consideration for the benefit of humanity, no less. This is a noble attitude, much as it is open to valid criticism either in accord with Herschel's inductivism or in accord with Popper's anti-inductivism, not to mention Boyle's complex private hypothetico-deductivism and public inductivism that made him institute the inductive style in scientific publications that he developed. This was the result of his inability to solve satisfactorily an urgent methodological problem, his inability to offer a criterion to decide that a paper is publishable, an inability rooted in his semi-Baconian theory of discovery that prevented him from seeing the value of some refuted theories.

On this point Herschel's position was more extreme than Boyle's. His objection was not that he had published conjectures but rather that he had not offered a demonstrated theory. He mentioned here Boyle, Hooke, and Newton, as ones who could do it. His work from which I quoted the accusation of Boyle is a milestone in the history of inductivism (Agassi 1969). The changes were great and many. They covered many aspects, and involved many factors. One of the changes was the termination of the direct influence of Boyle's writings.

Herschel hardly bothered to try to face Boyle's skepticism. In the nineteenth century, very few thinkers denied the certainty of Newton's mechanics after it became established, and perhaps no one but Solomon Maimon said so openly and with no qualifications (Maimon 1793). In this dogmatic atmosphere, where all ideas were valued according to their ability to resemble or to rest on Newton's, there is no room at all for skeptics such as Boyle. As Maimon was a skeptic, the commonwealth of learning ignored him. Faraday shared Boyle's skepticism, and so did a few of his close friends. He had to suffer for daring to doubt the absolute certainty of Newton's theory even if only by implication.

No wonder that Herschel's history of science was deemed a model and he was often quoted as an authority. True, some historians of chemistry simply could not ignore Boyle. But they did their best to fit their impressions into Herschel's scheme. True, historians of chemistry, even today, have a much higher regard for Boyle than their colleagues who write on the history of physics or of science in general. Thus, the greatest praise from a historian's pen that I could find is of a highly respected historian of chemistry (Thorpe 1894, 18–19, 1902, 1911, 20–1):

> It is impossible to exaggerate the importance of Boyle's labours, they served to give a marvellous sharpness to the notions of that time … The work exhibits Boyle's character as an investigator, his quick perception and receptive mind, his great power of co-ordination, his insight, his logic, his patient care and scrupulous accuracy. It exhibits, too, his weakness; for it must be admitted that it is wanting the grasp of principle and faculty of generalization which we see in the work of the illustrious author of the *Novum Organum*. It lacks, too, the *Forscherblick* and power of divination so characteristic of the genius of Newton. But to say that Boyle is only inferior to Bacon and Newton is to assign him one of the first niches in the Walhalla of the heroes of science.

Herschel's view of Boyle was influential and it still is; his comparison of Boyle and Bacon as researchers is popular among historians of chemistry, as it rests on their prejudice that researchers have the duty to theorize whenever sufficient data are available. He took it for granted that in Newton's time this was so, and concluded that any able researcher among Newton's contemporaries who did not reach for the theory of universal gravity lost a unique opportunity.

13.3 Who Discovered Boyle's Law?

Herschel's idea that Boyle was interested only in facts received a very strong confirmation from two esteemed nineteenth-century historians of science, Friedrich A. Lange and Ferdinand Rosenberger. Boyle's most important theoretical contribution was his theory of the elasticity of the air that says, the density of air is proportional to the pressure exerted on it. They claimed that he had only found the facts from which this theory is adducible; its discoverer is one of his assistants. They offered it as a statement of fact that is Boyle's own testimony. Historians of science endorsed this claim unanimously; no objection to it prior to mine was ever recorded.[3]

The story became popular in accord with Bacon's doctrine of prejudice: followers of Bacon have endorsed it just because it renders Boyle more Baconian than Bacon: it presents observations as chronologically prior to theory and Boyle as a true empiric who refused to employ his induction machine even when the attempt to do so would have led him to glorious results. In accord with Bacon's view,[4] an assistant was better at creating a theory than Boyle—even on the strength of Boyle's own observations: Boyle himself allegedly refused to take interest in a theory and was perfectly satisfied with a "just history of nature".

Boyle measured three times the pressure and density of a quantity of air, and drew a table that shows that they are almost collinear. Allegedly, he had sufficient strength of mind to follow Bacon and refuse to conclude that the function is linear. This story is silly, yet it appealed sufficiently to some scholars to make them read Boyle to say so. He said something different: he was testing his hypothesis for high pressures (above one atmosphere) and his assistant, as well as other people, suggested to proceed in the same manner to low pressures (below one atmosphere). Later he suggested that varying the temperature of air raises the pressure too. He was disappointed that there was no taker (see below). Obviously, this is the image of the teacher coaxing his crowd to experiment and having less success than expected.

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[3] My "Who Discovered Boyle's Law?", 1977, republished in (Agassi 2008), still awaits the establishment's open response to my challenges. Alas, there is no rule that prevents them from raising walls of silence when they cannot answer a challenge to their own satisfaction and will not yield either.

[4] The popularity of Bacon's doctrine of prejudice can be seen from it use in science fiction. The best example I know is the terrific 1957 *The Black Cloud* of astronomer Fred Hoyle: at the very end of the story, just before departing, the black cloud informs humanity of some great ideas; thoughtlessly, the choice of target is a great scientist who consequently goes mad rather than the innocent gardener who would absorb the new ideas effortlessly.

The historians of science who misread Boyle were able and knowledgeable and critically-minded. Yet it was very difficult to get my correction of their error published. One editor was ready to publish my findings on condition that I rewrite the paper in the inductive style—with no polemics and letting the facts speak for themselves. I found this amusing, as my point (or rather Galileo's and Kant's point) is that the facts refuse to speak. Still, there is one difficulty that explains the whole discussion and my difficulty in stopping it. Usually historians take the idea of priority as self-understood. The discussion of the priority dispute that Galileo had a few decades earlier is to date confused because it involved questions of standards when there was no convention about them. Descartes made no acknowledgements for philosophical reasons and amazingly he is still accused of plagiarism (of Snell's law). The conventional rules of acknowledgement are still those that Boyle determined—with one minor change. He published his study of the elasticity of the air before the implementation of his rules of priority. Nor are his rules clear enough on the matter of making a conjecture and testing it. Until today, in some cases we ascribe priority to the individual who invented a hypothesis, in other cases to the one who confirmed it, and we do not discuss the matter. The experiment extending Boyle's law to varying temperatures that he proposed was conducted only well over once century later. A few decades later Faraday suggested recognizing the priority for the invention of a question. There was no taker. This is no complaint. Faraday's project, like Boyle's project, was a success on a large scale. In Boyle's immediate vicinity he had less success than he expected and this frustrated him. Faraday was much more frustrated. As there is no induction, there is no guarantee of success in science: stupendously successful as science is, it may also frustrate, and in many ways. We may leave this at that.

13.4 Modern Views on Boyle

Except for Burtt (see below), all commentators describe Boyle as "a good Baconian" (Krook 1955). The dominant Boyle scholar today is Michael Hunter, the commentator (Hunter 2000), biographer (Hunter 2009) and editor (with Edward B. Davis) of 14 volumes of his *Works* (1999–2000), and (with Antonio Clericuzio and Lawrence M. Principe) six of his correspondence (2001). His intriguing essay "The State of Boyle Studies" (Hunter, editor, 2003, 1–18) says, the three eighteenth-century editions of Boyle's works reflect interest in him. The feeling "that Birch's capacious volumes" of his *Works* "had provided all that needed to be known about him" thwarted the publication of further work on him in the nineteenth century: he was thus "not ignored but taken for granted". This is not true. Boyle was then decidedly underrated. The impression that Hunter conveys of Boyle's nineteenth-century image does not exactly chime with extant information. I do not know why he beautifies it.

Renewed interest in Boyle began with E. A. Burtt's trail-blazing 1924 *The Metaphysical Foundations of Modern Physical Science*. He wished to reassess classical physics after Einstein had reduced Newton's influence (Agassi 2008, 264); to this end he studied the framework of classical physics. This made him recognize

Boyle's contribution. He said (Burtt 1924, 155), "although not commonly recognized as such, Boyle was a thinker of a genuine philosophical calibre". There was no response to this. In 1932 the impressive Boyle bibliography by the polymath-physiologist-bibliophile John F. Fulton appeared. It contributed to the Boyle trend once it got started—decades later. The immensity of Boyle's output presented to him a glimpse at Boyle's forgotten popularity and surprised him. He wrote then a solitary paper on Boyle as an amateur (*Isis*, 18, 1932, 77–102). It is still ignored.

As Hunter reports it, for a quarter of a century or so after World War II, a few professional (!) historians of science started the ball rolling, especially Marie Boas Hall who wrote a few studies on Boyle, one of them monograph-size. Hunter offers no explanation for its appearance[5] but for the dearth or response to it at the time. "Boyle did not receive as much attention … as he might have done" because he "fitted badly into the view of the Scientific Revolution, which, as Roy Porter has perceptively pointed out, was typical of the prevailing historiography". Moreover, under the influence of Alexandre Koyré, most of the energy of professional (!) historians of science was drawn to Newton. This is understandable, since Boyle's output was "at best diffuse, at worst miscellaneous, bizarre and apparently trivial." In the 1970s things changed as the fashion moved from internalism (the study of the growth of knowledge by reference to its inner logic) to contextualism (the study of the growth of knowledge in its broad context) due to the influence of the studies of Robert Merton of the 1930s (that poses the rise of Protestantism as a major cause in the growth of seventeenth-century science). Strangely, this explanation does not hold for the learned papers and biography of Boyle that R. E. W. Maddison published the 1960s, since Maddison reported only facts.

Hunter's report (Hunter 2003, 2, 16) on Roy Porter's essay (Porter 1986) is untrue. What it includes that is possibly relevant here is that radicalism is false. It does not refer to fashions in the historiography of science of the period (between World War II and 1970).[6] Yet essentially Hunter is right: the resurgence of interest in Boyle was (not internalist but) contextualist, its context being scientific revolution and their place in their socio-political contexts. The social role of Boyle in the process came up then regardless of his science.[7] To this I add, they misdescribed this role.

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[5] The only explanation for the rise of interest in Boyle is that of Westfall (1986): the impact of the studies of Burtt and of Koyré, as well as the founding of the *Journal for the History of Ideas* raised interest in the scientific revolution. My memory of the time is vivid. The main reason was the rise of professional history of science that is due to the expansion of the universities then and the success of the Soviet space program to beat that of the USA. Also Conant's program in Harvard had something to do with that. As to the *Journal for the History of Ideas*, its founder, P. P. Wiener told me it was his response to the view of the import of metaphysics that he learned from Émile Meyerson and his inability to publish on metaphysics in the relevant press as it suffered from positivist bias.

[6] Porter said, in some sense scientific revolutions possibly do occur. He said, historians who find it useful to speak of scientific revolutions should feel free to do so.

[7] Contextualism is not new. To take the simplest example, Macaulay observed early in the nineteenth century that Restoration England favored any development that took intellectuals away from politics and that this explains why the Society for Improving Natural Knowledge received the Royal Charter that boosted research.

Although Hunter began his study of Boyle as an externalist (challenged by the enormity of Boyle's literary remains), even when discussing his personal characteristics he (rightly) said, "I would place predominant stress on his significance as an experimenter" (Hunter 1999, 262). Query: what is specific about this? Answer: "Boyle's career resembled that of a modern research scientist to a far greater extent than was the case with most of his contemporaries" (263). This is true, yet it holds for Huygens and Newton more forcefully. Perhaps Hunter noted that his point is weak. For, he went on and specified Boyle's contributions in an internalist fashion: even though Bacon preceded him in advocating inductivism, his advocacy is a contribution too (Hunter 1999, 262):

> … it was Boyle who gave a new sophistication to the philosophy and practice of experiment. His innovations did not stop with trying to establish a clear rationale for experiments, particularly in his seminal work, *Certain Physiological Essays* (1661), in which he laid down parameters for conducting, validating and interpreting experiments, even including two essays reflecting on the significance of 'failed' experiments. Equally unprecedented and remarkable was the ingenuity that he displayed in devising and executing trials calculated to illustrate the characteristics and functions of all kinds of natural phenomena. He also went to great trouble to give a detailed, step-by-step account of the exact procedures adopted, thus enabling his readers to follow as closely as was practicable the experiments as they were actually carried out. This unprecedented practice, and its rationale, are surely two of the key things to be explained about Boyle.

This is correct. It is the only passage that I have found that refers, however obliquely, to Boyle as an originator and a teacher. Elsewhere Hunter overlooked this. Boyle's other scholars follow suit.

A few biographies of Boyle are available. Of these, the first[8] is by the editor of his *Works*, Thomas Birch—an uncritical and incomplete presentation of information (1744); more information is available in the biography by Robert Maddison (1969) that is even more orthodox Baconian (Westfall 1970).[9] The one by Flora Masson (1914) is gossipy and haughty; it only touches on Boyle's intellectual activity. The first modern life of Boyle is that of L. T. More (1944). He set himself a high standard. The opening passage of his preface says,

> … his biographer, to do him justice, must have a broad knowledge of mediaeval and modern science, and be conversant with the general history of the seventeenth century.

In a footnote More says (182) of E. A. Burtt that he "seems to show a very considerable ignorance of the then state of physics." More's own picture of seventeenth-century physics is clear:

> The laws of motion were but imperfectly known and very limited in their application, the weight of the air was in dispute; heat and cold were generally believed to be bodies;

[8] I do not count Richard Boulton's 1715 life of Boyle. It is merely a (boring) dissertation on Boyle's theology with a few pages in its opening and in its conclusion. That conclusion is largely borrowed from Bishop Burnett's funereal tribute. The only new item in this biography is its surprising allegation that Boyle received his education in the University of Leiden (Boulton 1715, 6–7).

[9] "Maddison is wholly explicit about his purpose. In the preface, he renounces any attempt to analyze the content of Boyle's work or to discuss his significance in the history of science."

> Newton's *Principia* had not been published, and it was not until a century later that Laplace claimed to have proved "the phenomena of gravity represent quite self-sufficient mechanical operation".

This, I hope, shows that More was steeped in the doctrine of prejudice. He found no precursor to modern theories. Also, he clung to Newtonian physics long after it gave way to quanta and relativity. And to support his view that "Boyle was undoubtedly credulous" (219) he quotes Herschel.

Boyle's credulity drew much attention in recent decades. Before that, Newton was the target of such studies. They signify because they refute Bacon's doctrine of prejudice and perhaps also because they support the proposal tacit in some learned books of some revered obscurantists in favor of some prejudices and superstitions. The means to stay clear of superstition is Boyle's ruling that scientifically all and only repeatable observations count. He applied this to keep the supernatural out of the scientific discourse, and this is as it should be.

Herbert Butterfield deserves special mention here, as he alone discussed Boyle's seminal essay, "The Unsuccessfulnesss of Experiments". Unfortunately, he missed Boyle's point completely. Apart from his surprising and quite erroneous claim that Boyle's atomism is Baconian, his observations on Boyle are matters of interpretation. He (Butterfield) did not take this essay of Boyle as offering a solution to a practical methodological problem; he considered it a mere expression of Boyle's defensive attitude to Bacon (128):

> He valued Bacon even where Bacon has most been despised by modern writers—namely, in his natural history. … where people jeered at Bacon for reporting experiments that had proved fallacious, he … discovered that, for example, Bacon was correct if you assume that he used purer sort of spirits of wine than was usually employed a generation later. Even apart from this he was interested in those anomalies or impurities … in materials employed by chemists, and which explain why so many of their experiments were vitiated. He wrote about the whole range of accidents which in given cases prevented experiments from producing the correct result or even a uniform result.

Butterfield's report is mistaken. It is not that "people jeered at Bacon" for a report on a fake experiment concerning the density of alcohol and that Boyle defended him by an excuse. The refutation of that experiment of Bacon's is Boyle's: in his report on it he explicitly claimed priority for it (*Works*, 1999, 1, 261). Boyle also said Bacon's error is "(questionless not for want of Judgment or Care, but of exact Instruments)" (262), which is a jot too generous but not culpable.

Boyle's essay (on "Unsuccessfulness of Experiments") is a plea to the new amateurs not to publish results prior to careful repetition, as the concern of science is with regularities. The first essay in the same tract is one "On Experimental Essays in General" that is a plea for the publication of successful experiments without delay. The one on unrepeatable experiments is the second; it is a plea that they should not be reported. Butterfield saw in it only another proof of Boyle's adherence to Bacon's empiricism; there is indeed much truth in this. Boyle understood very well the danger of unrepeatable information about magic: it abounds and can easily distract and overburden the scientific literature. Boyle was passionately interested in the supernatural, yet it is hard to say whether it was reasonable or not: he could not judge it but he tried and failed[10]; therefore he kept this apart from science. He pleaded with his peers that they do likewise.

The demand for the repeatability of empirical information is obtainable from the writings of Galileo and of Descartes. Boyle's contribution was that he instituted it. Before that he stated it sharply and at length and discussed it—also at length. His institution of the demand for repeatability in the Royal Society made it a must in the whole of modern scientific literature to date.[11] It is possible to try to explain the failure of an unsuccessful experiment, Boyle observed. It may then be possible to test the conjecture that explains it; this test may very well turn a failed experiment into a success and the conjecture into a scientific hypothesis.

This discussion of Boyle went further than those of Galileo and Descartes. He also added a demand to avoid discussing irreproducible reports in order to prevent controversy. We should not reject a report of some weird falsehood; for science it suffices to exclude it as irreproducible or untestable or unsuccessful and therefore suspend judgment on it. Here is Boyle's explanation. Take for instance Dr. Finch, a respectable individual, a good scholar, a friend of mine and of yours, my dear Philaretus, and he has told us about a blind man able to sense with his fingers the colors of ribbons. Boyle regretted that he could not himself test this information: he would gladly investigate the possibility that the blind man smelled rather than sensed the dyes, for example. Also, the story raises an objection: our theory shows that black is the absence of color, while, from the report of Dr. Finch it sounds as if the opposite is true, since, according to it the blind man reported his sensation of black as more violent than that of white. Boyle thus gently dismissed the story, until the case will be reproduced, tested and confirmed, thus refuting our theory. In the meanwhile we shall stick to our theory, no matter how sincere is our belief in Dr. Finch's story. Nobody can be offended. There is a similar report about white gold, and likewise Boyle refused to adjudicate on it and expressed the wish to test it. Thus, it is important to avoid both the affirmation and the denial of reports on unrepeatable experiments. Here the suspension of judgment is the right response.

This presents Boyle as far from credulous. Yet the current literature says he was. Now if we view credulity as the tendency to believe in statements of fact that we have not observed, then we are all credulous. Any man is a lunatic who argues from his not having seen a woman giving birth to conclude that his having been born to a woman is doubtful. If we view credulity as a belief in a false theory, then, we know that all past generations were credulous without exception. We may then conclude inductively that probably we too are mistaken. (This is the only induction I would admit.)

[10] Whenever an experience becomes repeatable it ceases to be viewed as miraculous or enchanted and it becomes scientific by definition that Boyle instituted. It is odd that he found it hard to abide by.

[11] Although nowadays every researcher knows that reproducibility as a condition for the scientific status of an observation report, almost all methodologists—with the exception of Karl Popper and Mario Bunge—overlook it and discuss the scientific status of generalizations instead. Here let me overlook this sad fact.

If we view credulity as the tendency to believe in theories that have no sufficient foundations in fact, then we are all credulous. If we view credulity as the tendency to endorse statements not well tested, then we need to specify, and then again most of us would count as credulous. For this is really the hope we have in the progress of science; scientific theories are never too well tested—there is always a possibility to try to test them and supersede them. But if we view credulity as the lack of a critical attitude, if credulity is readiness to endorse any story, or a higher disposition to endorse stories the better they fit for some purpose, then Boyle was one of the least credulous thinkers, all the valid evidence that diverse scholars accrued that show him credulous notwithstanding.

13.5 Conclusion

Views of posterity on Boyle are almost unanimously inductivist. They are judgmental, pro or con, depending on their view of him as a good inductivist or poor. These are almost entirely arbitrary. The significant exceptions are E. A. Burtt and John F. Fulton; they occupy here a marginal place as do those who discuss his output from specific angles and thus happen to have raised some interest in him.

Those who have discredited Boyle most, Herschel, Rosenberger, More, Butterfield, and to an extent even Hunter, display inductivist muddle. Behind their condemnation often stands Laplace's famous view that Newton was not only the greatest but also the most fortunate: there is only one universe for its laws to discover and one stage of knowledge with sufficient accumulated data. Herschel deemed Boyle sufficiently able to join Newton and Hooke in the attempt to go for it; he found it incredible that anyone could miss such a glorious opportunity and dash instead from one experiment to another.

Boyle genuinely admired Bacon. He often implied but seldom asserted his methodological dissent from him. His peers followed Bacon—for reasons good and bad—and he found antagonizing them on this a threat to his consciously and carefully devised self-selected role as a reformer.[12] As time went by, this became increasingly a cause for confusion.

Butterfield's discussion of Boyle's methodology is representative at least as far as the view on Boyle's attitude towards Bacon is concerned. He joined the majority in identifying Boyle's views with those of Bacon. The *Encyclopedia Britannica* expresses this openly: "Boyle's greatest merit as a scientific investigator is that he carried out the principles which Bacon preached in the *Novum Organum.*" Hunter's

[12] Steven Shapin praises Boyle without reference to his opinions or research (Shapin 1994)—as one who won trust as a researcher and as a gentleman. His credibility, Shapin seems to suggest, rendered all natural science credible. What Shapin has systematically overlooked is Boyle's having acquired and maintained his credibility the hard way. For, a major factor here was methodological and compromise about methods. This conflicts with Shapin's view.

way of paying Boyle a general tribute is to compare the empiricism of these two great thinkers. To get a better image of Bacon and of Boyle without overlooking the difference between them, it is necessary to decouple assent from admiration. One who praises thinkers on the ground that they are right merely indulges in self-flattery.

Chapter 14
The Inductive Style

A scientific paper is supposed to be innovative, to comprise a contribution to the stock of human knowledge. The trouble is, we do not know what innovation is, what the stock of human knowledge is, and how the one augments the other. The discovery of the New World is a paradigm case; should we ascribe it to the first humans who crossed the Bering Sea, to the first Vikings who crossed the Atlantic Ocean, to Christopher Columbus, or to Amerigo Vespucci? Each of these options rests on a theory that is hardly articulated, much less open to critical assessment.

Western-type universities require that doctoral dissertations be assessed as contributions to human knowledge, yet they offer no criterion of novelty. This is no censure. As we seldom ask for a criterion of beauty when we enjoy art, we may well treat scientific novelty that way, as Michael Polanyi has repeatedly suggested. How does the system operate without criteria? Polanyi suggested that the system relies on expert opinion in a manner that defies all criteria. He was in error—on both facts and philosophy. Boyle certainly could not rely on a criterion such as that articulated by Polanyi since the experts he knew were no good.

Boyle invented what we now deem the height of common sense: he treated scientific papers as open letters and suggested that they should comprise mainly observation reports; these should be informative: they should include material that is new to their unspecified receivers. This seems reasonable but possibly it misses a significant *desideratum*: the intended readers are supposed to be interested in the information offered. To this two answers are possible. First, readers can choose. This answer involves technicalities: the cost of providing information, the problems of storing it and more so of retrieving it. All this was beyond Boyle's horizon. The second answer is very much within his field of vision: Bacon had said, in research excess information is impossible. Still, it is possible to consider this matter practically. Take for example Boyle's posthumous (1692) questionnaire (Daston, 2011, 89) to travelers (*General heads for the natural history of a country great or*

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_14,

small drawn out for the use of travellers and navigators), concerning weather, flora and fauna, and local customs.[1] It was published repeatedly, Fulton reports, by instrument makes. (It is the kernel of the tradition that current inductivist *Notes and Queries* maintains; it began in the mid-nineteenth century—soon after Boyle's writings lost their popularity.) Assent to information generally seems easier than assent to a theory. Bacon went further and said, all information should be on record, as filters are prejudicial; misinformation will be filtered out in the stage of theorizing, he promised. This is fantasy.

14.1 The Discussion of Style

The subject of scientific style is only seldom discussed. The only extensive study of the seventeenth century concern in matters of style is the already mentioned work of R. F. Jones. He discusses hardly more than the attack on figurative and metaphoric presentations. There is also Disraeli's incidental discussion of the attack on the confused style of the eighteenth century, quoted above. The main source of this confusion is Boyle's theory of circumstantial description.

Let me quote Petty's lecture (Petty 1674).

> May it please your Grace,
>
> I am commanded by the *Royal Society* to Print the Discourse which I made before them, … Because … the Society are content, that this Exercise pass for a Sample, *Pro tanto*, of what they are doing; for that the same may be conceived to consist of three parts, *vis*. The *first* being an endeavour to explain the Intricate Notions, or *Philosophia Prima* of *Place, Time, Motion*, Elasticity, etc. in a way which the meanest Member of adult Mankind is capable of understanding: The *second* being, to excite the World to the study of a little Mathematicks, by shewing the use of *Duplicate Proportions* in some of the most weighty of Humane affairs, which Notion a Child of 12 years old may learn in an hour: And the *last* being, without Chymerical Speculations, to consider such points and properties, even in Atoms (such, whereof perhaps a Million do not make up one visible *Corpusculum*,) as may give an intelligible Account of the Nexures, Mixtures, and Mobilities of the parts of the Universe.

The Society had recommended the lecture for publication as exemplary, mainly from the stylistic point of view. It is so simple that even a child can understand it. It refers to facts and does not defend refuted theories.

An interesting suggestion of Hooke for a shorthand scientific jargon is similarly indicative. Here is Oldenburg's reaction to Hooke's suggestion, in a letter to Boyle of January, 1665–6

> Mr. Hook has also ready (having shewed me and others) a method for writing a natural history, which, I think, cuts out work enough for all naturalists in the world; and intends, I hear, to print it ere long … Mean while, I wish most heartily, both yours and his were publick,

[1] The editors of Boyle's *Works* (1999, 5, xli–xlv) notice that this work is largely a compilation of items from works of Boyle and of other Fellows of the Royal Society published in their *Transactions* in the 1660s.

> considering the great good it would do to philosophy, most men not knowing what to enquire after and how.

Oldenburg's letter alludes to a paper by Boyle that he (Oldenburg) wanted to publish together with Hooke's work. He was presumably referring to Boyle's brief "General Heads for Natural History of a Country, Great or Small", published in November, 1666 (*Works* 1999, 5, 508–11). The "heads" are not names of projects for research or of a classification; it is questions put forward to travelers. Boyle's *Heads* was reprinted, Fulton said in his bibliography, was republished repeatedly by instruments makers. This shows how good a teacher Boyle was.[2] What happened to Hooke's proposal is not clear. Oldenburg reports that he saw it completed and that Hooke intended to publish it. The obvious conjecture is that Hooke did not publish the work that Oldenburg referred to because Boyle interfered (he could: he paid the salaries of both of them). Why? The work is sketchy and his prescription there on writing scientific reports is especially brief (Hooke 1705, 63):

> The next thing to take care of is the manner of Registering. And this, as it ought to be done as fast as Experiment is made, and as soon as the Observation or Circumstances occur, because of the Frailty of the Memory, and the great significancy there may be in some of the meanest and smallest Circumstances, so ought they afterwards to be several times reviewed and examined, and ranged into a better Method, and abbreviated in the manner of Description, so that as nothing be wanting the History, so nothing so be superfluous in the words.

Nothing here Boyle could object to. What he would object to survives in another of Hooke's manuscripts (Weld 1848, 147)[3]:

> And till there be a sufficient collection made of Experiments, Histories and observations, there are no debates to be held at the weekly meeting of the Society, concerning any Hypothesis or principle of philosophy, nor any discourses made for the explicating any phenomena, except by special appointment of the Society or allowance of the President …

This is more of a Baconian prohibition than Boyle would tolerate. And yet Hooke's arguments are all from Boyle's stock, and the chief one is the need for as total circumstantial descriptions as possible. Clearly Boyle was ambivalent.

Much more relevant evidence for this appears in the introduction to his last publication, his *Experimenta et Observationes Physicae* that includes bits and pieces written decades earlier. Although supposedly a paradigm of the Baconian style, it is Boyle's most explicit criticism of Bacon's methodology.

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[2] Incidentally, much later, when amateurism was over, it was easy to misunderstand Hooke's "heads". In the Introduction to *Encyclopedia Metropolitana* (1845), for example, Samuel Taylor Coleridge considered Hooke's "heads" a classification and ridiculed it and its going into strange details. His elder contemporary, the famous philosopher-physicist André-Marie Ampère wrote "heads" too, and these comprise a classification—of possible problems for possible future researches more than concrete ones for specific people. (Thus, one of his heads is "cybernetics" that waited about one century before it came to use.)

[3] Hunter denies this the attribution of this document to Hooke (Hunter 1989, 175), for circumstantial rather than contextual arguments; see also (Hunter 1995, 6, 173).

The introduction is a letter to Oldenburg, [4] then dead for over a decade. A paralyzed author on his deathbed, writing an introduction in the form of a letter to a dead friend, would write only on things nearest to his heart.

> Sir,
>
> Being at length come to a Resolution, I have already done something more than barely entered upon that way of Wiring that you and I have more than once Discoursed of together; and wherein you *particularly* (though not only *you*) among my Learned Friends, have wished to see me Engaged.
>
> 'Tis not, that I am insensible of the Prejudice which the things I deliver are like to sustain, by the disadvantageous Dress wherein they must appear, in the way of Writing I have pitch'd upon; which being for the most part plainly Historical, and set down in the order Wherein they chanc'd to come to hand, denies most of them, not only the usual Ornaments of other Books, but the allowable Advantages, that Method, elaborate Discourses, neat *Hypotheses*, and subtil Disputes, are permitted to bring even to philosophical writing.

The expression "plainly historical way of writing" describes the inductive style. Despite Boyle's repeatedly expressed dislike for "subtil Disputes", here is a complaint of his that the inductive style inhibited his wish to engage in a dispute. This is odd. His assertion that Oldenburg had urged him to enter upon the style is immediately cancelled by his admission that he had "pitched upon" himself, indicating that Oldenburg was a mere supporter of the style that is Boyle's. After his repeat of his standard discussion of circumstantial description and his urging his reader to take up various hints that the book includes and perform more experiments he then added (370–71), he "now and then propose some Conjectures and Opinions, whose proof I do not insist on." How familiar were Boyle's intended readers with all this? Evelyn's letter to William Wotton—who had intended to write Boyle's biography—may provide the right answer: Boyle's style was poor though it improved in his later years, Evelyn reported. Now Boyle was always blamed for "being too prolix", mainly and frequently by himself. Here is a quotation from a letter of February 1665–1666, from John Beale that testifies for the view that Boyle's chief aim always was simplification.

> I must remember that when first you bestowed your bookes upon mee, you blamed mee for not returning my Censure. If I had neede of an Apology for such freedome as I here intend, Hence I could frame it. But your Vertue is more solid than to require smooth handling; And you cannot exchange your leysure for a compasse of soft words.
>
> 1. Therefore to begin rudely with Generals; I shall tell you that, which you will hardly beleeve. Persons of noe ordinary Capacityes do find your [last book] … difficult. your assiduity upon the argument, with exemplars before your eyes, hath made all things soe easy to you, That you will scarce apprehend, Howe it can be intricate to another. …

[4] The editors of Boyle's *Works* take it for granted that he wrote this letter when Oldenburg was still alive (*Works*2000, 11, pp. liii–liv). In the middle of the letter to Oldenburg (p. 371), Boyle requested of his readers that they should ask Oldenburg to testify that he (Boyle) had performed all of his experiments carefully and reported them candidly. Obviously, the insoluble problem of circumstantial description irked him to the very end. He feared that some of his experiments would prove unrepeatable due to the incompleteness of his descriptions of their circumstances. As Bacon received much ridicule then for his poor reports and as Boyle was very sensitive to ridicule, this is not surprising.

2. I engage for you, That … you are as plaine & ample, as can be. And whatever is of it selfe intricate, It comes often in your way, & is layd downe in variety of good expressions. But whilst you take too much care to secure our Understandings, & to helpe us with much of your patience, … [those who do not repeat your experiments] are more apt to languish than to feel life in your argument. …

To conclude, both in content and in style Boyle was much more thorough, careful, profound, and of a wide outlook than any of his friends except for Newton. His ability and modesty explain his tremendous fame. "He was modest almost to a fault", as Bishop Burnett declared. Historians, even of chemistry, tend to consider his works trivial and unimportant. His prescription for the inductive style in 1661 led the way for the inductive style to develop for two centuries. In spite of its critical tendency and much simplicity, the inductive style is over-complicated and dogmatic. As Beale has points out, it conceals the life of arguments. Boyle's style could have improved gradually into the critical style. Newton's notorious hostility to criticism, his authorization of Boyle's style, and his unequalled success, made it hardly possible to alter gradually and peacefully the style that he prescribed. All this commentators[5] regrettable tend to ignore;[6] it is relevant for a more enlightened future judgment of the inductive style.

14.2 The Inductive Style Versus the Argumentative Style

Tradition recognizes three leading styles, the deductive, the inductive, and the dialectical or critical or argumentative or historical. Each of these categories is split in many ways, yet traditional discussions are limited to simple paradigm cases. In particular, the styles of encyclopedia essays and of surveys are very important yet they hardly enter the discussion of styles, which is regrettable. Inductive surveys are unavoidably apologetic and thus they are very reader-unfriendly. Critical surveys can be most reader-friendly. They are then of the greatest importance. All this will be ignored here as the present discussion contrasts the three leading styles.

Papers in the deductive style are mathematical and they begin with axioms.[7] Papers in the inductive style follow Boyle's description. A paper in his style opens—after a brief, not obligatory introduction—with a description of instruments if any are used in the reported observations, and then reports experiments and their result, so that amateur readers can repeat them; one may add to this a brief coda with some

[5] Historians of science are mostly inductivists or else mostly indifferent to style. Literary historians do better but are regrettably rather intimidated by scientists.

[6] An exception is a passing remark in Justus von Liebig's tirade against Bacon (Liebig 1863, 253 note): he equated there the style of the inept English critics of his own work with that of Bacon. "It is not necessary to say that these experiments have no connection with any reasonable question whatsoever", he added.

[7] Some mathematical texts skip the axioms. In probability this is understandable (Popper 1959, Appendix *4).

speculation. The argumentative style includes first the general theoretical and experimental state of affairs when the research started, and the problem intended to solve, the set of extant solutions to it and critical discussion of them; only then comes the innovation if there is any, whether the author's new solution if there is any or the author's new attempt to criticize a solution. This style does not demand a faithful historical presentation of the research, since one can start one's study with one problem and then go to a more general one or a more specific one, or even shift to another problem.

In his early "Proëmial Essay … Touching Experimental Essays in General", Boyle dismisses the speculative or deductive style of writing accepted then. Instead, he advocates the empirical or inductive style of writing. This is a pity. He wrote against arguments but he ignored the argumentative or critical style. Significant works of his followed neither the deductive style that he rejected nor the inductive style that he advocated but the argumentative or critical style. The obvious examples are William Gilbert's *De Magnete* of 1600 and Galileo's two magnificent *Dialogues* of 1632 and of 1638. This did not escape Boyle's attention; he wrote some of his early works in dialogue form, namely in the argumentative or critical style, not in the inductive style. This holds especially for his early *Sceptical Chymist* (1661) that is so celebrated that many consider it his *magnum opus*. He even makes there some remarks on the critical style. In his unavoidably argumentative *A Defense of the Doctrine Touching the Spring and Weight of the Air* of a year later he argued against publishing criticism. He never gave the critical style the consideration it deserves. The speculative or deductive style is so abstract that it comes with no background. Notoriously, this makes it difficult to read. Documents written in either the inductive or the critical style are historical. All historical documents are selective. The inductive style does not allow discussion of the rules for selection, and so it does not supply readers with background information and that limits it greatly. The critical or argumentative style invites background information, thus rendering the document less limited. Let me elaborate.

What facts are relevant to what kind of writing is a matter of interpretation. Boyle's chief problem when he discussed (or rather taught) the method of writing a scientific paper regularly touched upon this. How detailed should an observation report be? His point always was that experimenting and observing was the scientific activity most wanted—so that this is the chief activity to report. The inductive style is in the character of a chronological narrative. Boyle explicitly demanded that one should write in this manner. Robert Hooke, his follower, assistant, and colleague, wrote,

> On Saturday morning, April 21, 1667, I first saw a Comet.

Notwithstanding the beautiful freshness that the inductive style once had, and despite its obvious adequacy for some ends, for science it is inadequate. Compare reports in this fashion on the observation of a meteorite and that of a comet. Are these of equal value? Bacon said, yes: he declared valuable all reports of new information. Boyle agreed: no information is so insignificant to be negligible. The question then is, how detailed should information be? (The more detailed one renders more variants of it new!) We cannot record all the details of the situation in which an experiment happens. And, to repeat, from the inductivist viewpoint, the more detailed a report is, the more difficult it is to repeat it, through the good offices of a good theory (whatever it is) the problem vanishes. We use common sense then and ignore details that we consider irrelevant to its repetition. This commonsense is a hypothesis and it may be erroneous and at times it is, as the progress of science shows us to our surprise.

Boyle declared research papers publishable if and only if they contain new information. What makes an item new Boyle did not ask. Whatever its novelty is, however, in case it is a refutation of a received opinion, then we do know that it is new. For, otherwise the advocates of that theory would have desisted from having it published, as both Bacon and Boyle have noted. We may go further: viewing some given item of information as an argument for or against a new theory puts it in a new light and so it may become new. Still, the supposition in this discussion is that the scientific community can face such a situation with ease. Hence, novelty within a scientific tradition may differ from novelty elsewhere.

What then is Hooke's 1667 observation of a comet to us? It was no novelty, since many people observed comets earlier. Comets were more important than meteorites. Descartes, who never saw one, had explained their motions relying on old records. Later observations of these motions had to be more accurate, because they served as arguments for or against a theory: they were test statements. Without this background information a student who reads Hooke today will fail to understand him.

Boyle played down all this, as he took very seriously the sensitivity of most of his peers to open criticism: as they found it unpleasant, he wished it toned down. Although the tendency to ignore refuting information may once have been strong, nowadays the situation is different. Today to be clear about physics one must mention some experimental results as refutations of some theories.[8] Boyle knew that already then. This enabled him to write friendly criticism. Consider his eminent *Defence of a Doctrine*. His response to his critic (Francis Line) is nice:

> I hope there is not in my answers anything of asperity to be met with; for I have no quarrel with the person of the author or his just reputation … which things I represent for the defence of what I think the truth, and not to offend my learned adversary, who shall have my free consent to be thought to have failed rather in the choice than in the management of the controversy.

Boyle also offered some unfriendly criticism, however. Newton, too, was at times civil in debates, his hatred of controversy notwithstanding. This is not very pertinent: the matter at hand here is of standards that determine only the default options. Exceptions show the prevalence of ambivalence; they are better overruled.

To keep some sense of proportion, we should recognize that the hostile style that many seventeenth century controversialists adopted rendered it difficult to

[8] Thus Michelson's experiment is pointless unless it is a refutation of the ether theory and the observation of Bothe and Geiger is likewise useless unless it is a refutation of the Bohr-Kramers-Slater theory.

advocate free controversy. Consider however Boyle's *Defence of a Doctrine*. The problem situation that existed before he wrote it is briefly but fully reported by a summary of the views of Boyle and those of his critic Francis Line, and a contrast between them. Only then comes Boyle's answer to the criticism. Line, incidentally, conceded defeat.[9]

This makes the *Defence of a Doctrine* a possible paradigm; Boyle did not choose it. Its merit is now easier to see then at the time, though, since it is less bound to its time than the inductive works of Boyle: it presents a problem and a suggested solution that has its thrill for any reader in any time. And the impossibility to understand a paper written in the inductive style without prior knowledge of the problem-situation that it addresses is precisely that this style precludes it. Although Boyle intended to advocate the style that is easiest for amateurs, he advocated a style that possibly makes it easy for amateurs to write but not to read. The inductive style is no longer obligatory across the board, yet public discussion of the problem-situation in which a paper signifies is still sorely missing. The assertion that lack of expertise blocks understanding of a scientific publication may be illusory: good vulgarizers offer background information and the better ones include problems. And good middle-school science-teachers may open the door to simple science reports.

Is the inductive style suitable for current situations? This question is open. Yet to concede to inductivists is to forego discussion of it and to bar by default friendly controversies such as the Einstein-Bohr classical controversy. Whether such cases sufficed in Boyle's days to try to promote civil controversy is hard for me to say. As the Royal Society was quite conservative in its politics, it made a number of compromises, some of them much less honorable than the willingness to forego civil disputes: they should have supported women and opposed witch-hunts (Ehrenreich and English 2010, Preface).[10]

To conclude, Boyle's inductive style was a reasonable solution to a genuine problem. It was workable and it worked for long; it is now admittedly outdated.

[9] Line, a professor in the casuist tradition, was reluctant to allow his peers to continue the debate on his behalf after he had lost it (Reilly 1962, 227): he "felt that his *confrère* was showing undignified obstinacy, and… since his error was obvious, he should be obliged to desist from further dispute. He wrote … 'it is to be feared that the dispute would give occasion to those who wish us ill, to say that we are such that once we have asserted anything, we will always defend it, even though we have been convicted of error'." This indicates that possibly Boyle deemed his peers more sensitive than they were. Newton's sensitivity, however, goes the opposite way. Without his reinforcement, Boyle's demand to suppress controversy would not have persisted for as long as it did.

[10] The literature suggests that Boyle was as superstitious as Joseph Glanvill was. It overlooks the difficulty that the Royal Society had about accepting Glanvill that R. F. Jones discussed in detail. It also ignores Boyle's correspondence with Glanvill (Prior 1932). Glanvill's conduct was more honorable than Boyle's. Since Boyle insisted that the Society limit its studies to natural knowledge and since he declared scientific evidence acceptable if it is acceptable in court, it is clear that he was not happy about witch-trials yet in public he ignored them.

14.3 Reporting on Experiments and Writing Systems

The "Proëmial Essay … Touching Experimental Essays in General" is an introduction to the 1661 *Certain Physiological Essays* that is a series of essays on chemistry, physics, medicine, and methodology. Boyle wrote that essay a few years earlier, when he was about 30 years old.

The essay begins with an apology for the haste in publication. Boyle prefers to experiment than to write, he admits, and to retain manuscripts in order to improve them. But for the benefit of humanity he will not keep his treasures to himself. Then he explains his use of the style of an essay:

> … it has long seemed to me one of the least impediments of the real advance of true natural philosophy, that men have been forward to write Systems of it, and have thought themselves oblig'd either to be altogether silent or not to write less than an entire body of Physiology…

Bacon and Boyle used the word "system" the traditional way, to designate a general view concerning nature, such as atomism, Cartesian mechanism or Aristotelianism. Like Bacon he considers systems obstacles, or rather the over-readiness to write one. Unlike Bacon, he declares this an impediment for technical reasons. Briefly, he wanted to teach his readers to write short experimental essays lest their discoveries be forgotten. Boyle claimed explicitly that the writing of short essays is not the custom and that he was consciously seeking a way to establish a new custom. He spoke here with authority, which is rare in Boyle's works:

> Nor am I so rigid as to be unwilling, that, from time to time some very knowing writer should publish a system …; For such a Work may be useful … not so much by gratifying the Intellect … as because … these Writers … must either bring New Experiments and Observations, or also, must consider those, that are known already, after a new Manner, and thereby make us take notice of something in them unheeded before; and … the curiosity of Readers … excited to make tryals of several things, which seeming to be Consequences of this new Doctrine, may … either establish or overthrow it.

Boyle took seriously only two systems (of Descartes and of Gassendi), yet he also argued in favor of systems in general: they gratify the intellect, help classify facts, and, may lead to the search for new facts by inviting tests.

This is not a sufficient criterion of goodness of a system. Boyle required anyone who had a hypothesis whose immediate consequences are testable to test it oneself and then to publish the results of the test with or preferably without the hypothesis tested. This evidently reflects Boyle's view that regrettably people were then more ready to conjecture than to experiment. This is why he feared that publishing testable hypotheses would not do: he feared that no one would test them. He thus had no intention to legislate for coming generations; he wanted his peers to volunteer as empirical researchers. Had he been as Baconian as they were, he would have advised them simply to avoid theorizing and to start experimenting. This he decidedly refused to do; in many of his publications he urged his readers to theorize. But he was afraid that at the time encouragement for theorizing was also encouragement for foregoing experimenting. In his view the ratio was clear: the need is for many experiments and very few theories; which he found hard, taking for granted that it is easier to conjecture than to experiment.

It is hard to judge whether Boyle's view of his peers is right. Admirably, he never advised anyone to do what he would not himself do and exemplify. And he had no criterion that commands the consensus, except for the validity of repeatable observation reports. And even this, we will see in detail, he found problematic. Nevertheless, he could more easily perform and repeat experiments than develop a criterion for the goodness of a system. Indeed, the system he advocated was syncretic and hardly impressive, as he could not handle the difficulties that wedding Gassendi to Descartes raise. Let me ignore all this. Suffice it to notice that, nevertheless, and as if by miracle, he did develop a methodology, and one that proved most significant fairly soon, when Newton applied it.

There remains the general problem. Was Bacon right on this? Is it easier to imagine an experiment than to perform it? Is this comparison of thought and action at all reasonable? I have no idea. This, however, is obvious. Ever since Bacon declared the use of the imagination due to laziness and an original sin, he won agreement as a matter of course: armchair experience is the indolent substitute for the genuine action. Woodger is alone in having objected (Woodger 1967, 49).[11] Yet practice agrees with Woodger, and as a matter of course. Researchers usually design experiments and supervise their technicians' work; researchers who receive Nobel prices do not share them with their technicians.[12] Obviously Bacon found it much easier to devise an experiment than to perform it, evidently feeling much more at home in the armchair than in the lab. A philosopher, I share this feeling, yet I can repeat with ease such a beautiful, earth-shaking experiment as that of Einstein and de Haas, but I would not be able to design it were I to live a thousand years. This does not speak against Bacon, though; he reflects the ethos of his times. Max Perutz for example better reflects current ethos: he has received a Nobel Prize (in 1962) for an experiment that many researchers armed with the proper equipment now repeat with ease as a part of their routine. Boyle could not imagine such a boon, yet without him it might not have happened.

14.4 Boyle on some Systems

Boyle discouraged writing books. He discussed this diplomatically. Bacon's prohibition on systems is a corollary to his doctrine of prejudice that Boyle had rejected. This is not to deny it all merit, since science and prejudice are opposite

[11] See also Conner (2005, 330). Conner praises Boyle's egalitarianism and is amused by Boyle's "making experiments by others' hands". See also Hobbes (1680).

[12] Boyle did give credit to some of his technical assistance; he did not institutionalize this and he was seldom followed. The familiar if atypical study by Julius Roth (1966) is different. It deals with improper use of hired hands who are only partly technicians and mostly they are research assistants who often take initiative. Employment of research assistants is no different from purchasing research work—privately or in industry—and so it is of no concern here, be its output of high quality or low. Unfortunately even Kant agreed (and added that action makes perception scientific).

poles, but Bacon's version is too extreme. Hence, it is an error to take his prohibition too rigidly, as some systems are commendable (*Works* 1999, 2, 12):

> And that you may know, *Pyrophilus*, what kind of Writing I mean, I shall name you the learned *Gassendus* his little *Syntagma of Epicurus's Philosophy* and the most ingenious gentleman Mons' Des-Cartes his *Principles of Philosophy*. For though I purposely refrain'd … from transiently consulting about a few Particulars, yet from serious and orderly reading over those excellent (though disagreeing) books, or so much as Sir Francis Bacon's *Novum Organum,*[13] that I might not be prepossess'd with any Theory or Principles till I had spent some time in trying what Things themselves would incline me to think; yet beginning now to allow myself to read those excellent Books, I find by the little I have read in them already, that if I had read them before I began to write, I might have enrich'd the ensuing Essays with divers truths which they now want and have explicated things much better than I fear I have done.[14]

This passage has intrigued many a commentator who took this text literally as a confession that he avoided "reading over" works of Descartes and of Gassendi in order not to be prejudiced by them, and thus as evidence that he was a good Baconian abiding by the doctrine of prejudice both in theory and in practice. This, they said,[15] despite his having been famously expert about these thinkers and despite his careful, detailed, astute dissent from their views. These commentators also managed to miss the humorous manner in which Boyle refuted Bacon here: he avoided reading him too for fear of endorsing his views as prejudices: the doctrine of prejudice is inconsistent because it is itself a prejudice. Boyle took care here of Bacon's observation that dogmatic people see only confirmation of their systems: the scientifically minded Boyle proposed to put these systems to empirical tests.

Relying on this passage, Boyle's biographer L. T. More reported (p. 235), "It was not until 1657/8 … that he reluctantly put himself to read" these books. The reluctance is of the Baconian biographer. He noticed that the above passage is marked with modesty and that Boyle had not said that he had never opened these books before. In response to More, let me quote a letter (of May 8, 1647) from the 20-year old Boyle to Samuel Hartlib:

> … Gassendus, a great favorite of mine, I take to be a very profound mathematician, as well as an excellent astronomer, and one who collected a very ample treasury of numerous and accurate observations …

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[13] The editors of Boyle's latest *Works* add a remark here: Bacon was Boyle's preceptor, namely, teacher. They notice that he claims to have written but never published his opinion about *The Partiality and Uncertainty of Fame* (*Works,* 2000, 8, 92 note). As he explained, he had changed his view of the quest for fame and it became greatly different from that of Bacon.

[14] Buchdahl rightly observes: "As Dr. Boas Hall points out, Boyle's writings are immensely long-winded and repetitious, and only ruthless pruning can enable the true brilliance of his thought to emerge" (Buchdahl 1966, 83).

[15] Examples: Birch in (*Works*, 1744, 1, 35); (Cajori 1929, 78); *Britannica* 14th edition, 1954 Art. Boyle. Marie Boas Hall happily disagrees: "His familiarity with Bacon, Descartes, Gassendi … and indeed all the scientists of his day is amply attested by frequent references in his works, despite his often quoted disclaimer" (Hall 1952, 418, note 6). Hall (1987, 112 and 119) mentions Boyle's atomism and ascribes to him "a truly Baconian mistrust of dogmatic systems"; see also Hall (1992, 27): "the Royal Society's declared opposition to premature theorizing which its members tended to equate with speculation; a position which Boyle himself often publicly proclaimed."

In this letter, from the pen of a lad 20 years of age, addressing a reputed polymath, a world-famous "intelligencer", a leader and a scholar, the shy Boyle uncharacteristically offers his views uninhibited. He willingly advises the leading expert on education and on scientific matters, on his fields of expertise. It testifies to Boyle's familiarity with the research scene.[16] At the time, Gassendi' s *Syntagma* was not yet published. Presumably Boyle studied it when it appeared—in 1649—as he was a bookworm from his early days. (In 1653 Dr. Petty reported that in spite of his very weak eyes he "read twelve hours per diem or more".) In 1660, at any rate, he was sufficiently well read in Descartes and in Gassendi to discuss their views brilliantly. This did not stop him from repeating in 1666 his 1661 complaint that he had not yet read properly *the Syntagma* (*Works* 1999, 5, 295):

> … I hope I have been benefited by those I have consulted, and might have been more so, by the learned Gassendus's little, but Ingenious *Syntagma Philosophiae Epicuri*, if I had more seasonably been acquainted with it.

This is obviously a repeated expression of admiration, humility, propaganda and playing the dilettante, the way he did already in his early (1661) "Proëmial Essay":

> But of such Writers the number is … so small that I shall not need to make many Exceptions … I am very sensible of my being far from having such a stock of Experiments and Observations, as I judge requisite to write Systematically; and I am apt to impute many of the Deficiencies to be met with in the Theories and Reasoning of such great wits as *Aristotle, Campanella*, and some other celebrated philosophers, chiefly this very thing, that they have too hastily and either upon a few observations or at least without a competent number of Experiments, presum'd to establish Principles, and deliver Axioms.

It is this sort of paraphrases on Bacon's texts that contributed more than anything else to the usual misreading of Boyle as an adherent to Bacon's inductivism, all evidence to the contrary notwithstanding. In the misleading texts usually his criticism of Bacon is thinly veiled. Thus, whereas Bacon condemned Aristotle as "hasty to deliver axioms", Boyle spoke of him as of a man of "great wit". Bacon used the concept of wit usually in the sense in which he claims that Induction needs much diligence but little wit. By contradistinction Boyle demanded to leave theorizing to men of wit, but he did so a few pages after the passage just quoted. To sharpen the irony, Boyle associated Aristotle, Bacon's *bête noire*, with Tommaso Campanella, whom his readers held in esteem. To make it quite clear that Boyle's remark about the lack of sufficient empirical foundations holds not only for Aristotle's theories but also rather as a general phenomenon regarding theories, Boyle continued thus:

> For it very rarely … happens than that Theories that are grounded but upon few and obvious Experiments are subject to be contradicted by some such Instances, as more free and diligent Enquirers … are wont to bring to light … And … in divers of Philosophical Theories, that have been formerly applauded … whilst they are look'd on with … weak … light … a full light of new Experiments and Observations … let upon them, the Beauty of those … Structures does immediately vanish.

[16] Leading Boyle scholars—John T. Harwood (1991), and Michael Hunter (1994, Introduction and *Works* 1999, 1)—use newly discovered early Boyle manuscripts as evidence that his interest in science began later than previously assumed. This letter to Hartlib disproves this.

14.5 Thinking and Experimenting

> And truly, *Pyrophilus*, if men could be perswaded to mind more the Advancement of Natural Philosophy than that of their own Reputations, 'twere … easie to make them … set themselves diligently … to make Experiments … without being over-forward to establish Principles and Axioms …

Boyle used here Bacon's phraseology, except that by "establish" he meant publish, as he rejected Bacon's demand to prove before publishing, nor did he forbid premature conclusions:

> Not that I at all disallow the use of reasoning upon Experiments, or endeavouring to discern as early as we can the Confederations, the Differences, and Tendencies of things: For such an absolute suspension of the exercise of Reasoning were exceedingly troublesome, if not impossible. And, as in the Rule or Arithmetick which is commonly called *regula falsi*, by proceeding upon a conjecturally-supposed Number, as if it were that, which we enquire after, we are wont to come to the knowledge of the true number sought for; so in Physiology it is sometimes conducive to the discovery of truth, to permit the Understanding to make an Hypothesis in order to the Explication of this or that difficulty, that by examining now for the Phenomena are, or are not, capable of being salv'd by that Hypothesis, the Understanding may, even by its own Error be instructed. For it has been truly observed by a great Philosopher, that truth does more easily emerge out of Error than Confusion.

The great philosopher on whose authority Boyle permitted making hypotheses (even prior to any experiment!) is Bacon, who deemed premature theorizing sin. He quoted Bacon's permission to the intellect that Ellis deemed bankruptcy, we remember. Boyle declared impossible Bacon's demand for "an absolute suspension of the exercise of reasoning", contrasting it with his permission to err. This became standard throughout the Age of Reason—but only under stress. In the meanwhile, Boyle's approval of Bacon's permission to the intellect and of his argument for it—better error than confusion—was used both in general and as evidence that Boyle was a follower of Bacon (Sabra 1981, 181) and commentators on Boyle still use it this way—flippantly.

More seriously, when are systems permissible? When we are ready to try to test them and agree to withdraw them upon their refutation. Is this all that we require from a system? No:

> … I wish … That men, in the first place would forbear to establish any Theory, till they have consulted with … a considerable number of Experiments in proportion to the comprehensiveness of the theory to be erected on them. And … I would have such kind of superstructures look'd upon only as temporary ones; which though they may be preferr'd before any others, as being the least imperfect, or, … the best in their kind that we yet have, yet are they not entirely to be acquiesced in, as absolutely perfect, or incapable of improving Alterations.

The development of European thought would have been quite different had philosophers taken better note of this passage and similar ones; they would then consider science tentative rather than knowledge grounded *a priori* or *a posteriori*—as if these two options were the only available alternatives. Tradition ignored the option that the *Sceptical Chymist* has offered; today students of his philosophy ignore his skepticism and saddle him with Baconian inductivism.[17]

Boyle did follow Bacon, Descartes, and Gassendi; since they disagreed on methods, he was bound to be critical of some of their methodologies. A genuine rationalist, he rejected important parts of the methodologies that aimed at certainty. Theories, he said, are not as arbitrary as Pyrrhonists had said: conjectures that explain as many known facts as possible are not *ad hoc*. They do not explain sufficiently many facts: they cannot, since factual knowledge is limited. Hence, all theories are provisional. This is no ground for despair, Boyle added, since we may try to improve them.

14.6 The Inductive Style

> I think that if I could engage you, *Pyrophilus*, … to cast … Observations and Reflexions into Experimental Essays, I should thereby do real Learning no trifling service … I must beg leave to represent to you this great Conveniency of Essays, That as in them the Reader need not be clogg'd with tedious Repetitions of what others have said already, so the Writer, having for the most part the Liberty to leave off when he pleases, is not oblig'd … to teach others what himself does not understand, nor to write of any thing but of what he thinks he can write well.

Boyle's aim here is to make easy for amateurs to report. A reporter of facts may ignore others' works. This, let me comment, may make experimental essays easy to write, not to read. Boyle viewed the important readers as "men of wit", supposed to erect systems. Reporters of facts may have their own systems but, Boyle continued, they are not obliged to mention them. Reporting facts in a repeatable fashion will do, as others will repeat them and vary them and publish their observations. This is the whole background to his considerations of style. The first rule is one that Bacon advocated and Boyle illustrated: avoid rhetoric. This does not mean that scientific papers should be positively dry and lack style:

> for though a Philosopher need not be solicitous, that his style should delight its Reader with his Floridness yet … he may very well be allow'd to take a Care that it disgust not his reader by its Flatness … Thus, … though it were foolish to colour an enemal upon the glasses of telescopes, yet to gild or otherwise embellish the tubes of them, may render them more acceptable to the Users…

Decorate the sides of your telescope, not its eyeglass. Use "exotic words" and long sentences only when they help readers; use the vulgar style in order to be better understood. And play down theories. For, although experimenting presupposes them, presenting them is difficult, and reports of new facts should be simple and easy to write.

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[17] The possible exception is George Sarton, whose study of Boyle and Bayle as skeptics (Sarton 1950) is difficult to assess. He constantly referred to the contrast between skepticism and dogmatism. It is traditional: rationalist tradition deems skepticism and dogmatism Scylla and Charybdis. Boyle and Bayle opposed this tradition.

14.7 Encyclopedia of Facts or a Just History of Nature

> The next thing, *Pyrophilus*, of which I am to give you an account, is, why I have … deliver'd my Experiments and Observations, which may seem slight and easie, and some of them obvious also, or else perhaps mentioned by others already. … many of the Particulars, which we are now considering, were in my first design collected in order to a Continuation of the Lord Verulam's *Sylva Sylvarum*, or Natural History. And that my intended centuries[18] might resemble his, to which they were to be annex'd, it was requisite that such kind of Experiments and Observations as we have been newly speaking of, should make a considerable part of them. … I confess I think myself beholden to him that first makes me take notice of what I might easily have known, but heeded not before; it not seldom happening, that we are prejudic'd … . But I digress …

Despite Boyle's apology for writing easy stuff, his writings were not easy to follow, as comparison of his writings with those of his peers may show and as Beale's letters to him confirm. Here again Boyle urged those who would not experiment to describe some familiar facts: he suggested scientific activity could be fun for anyone. Also, he advocated here Bacon's idea of a just history of nature, but for a different purpose and in a different manner. It worked: peers endorsed his proposal unanimously for two centuries—until Faraday found it impossible. Boyle advocated describing facts clearly but only alluding to their agreement or disagreement with some theory. This became impossible in Faraday's time, as he needed an explicit and careful debate about alternatives. Peers were ready to allude to the fact that Faraday's discoveries support or undermine some theories; but they did not know how to describe these discoveries without using the concepts he invented (especially "lines of force" and "fields of force") yet using a given terminology is itself an allusion to some agreement. The result was that Faraday's prudent contemporaries did not describe the facts he discovered (at least not as often as they wanted to); instead, they mentioned them. He repeatedly suggested that they use his terminology without commitment; the unwritten code of allusion defeated him.[19]

Boyle's last quoted passage is historically significant. Much that he wrote and more that he and his followers exemplified is in its wake. He had many readers, and the better ones read him carefully and followed him. They also followed his manner of allusion and valued his having found a practicable method that appeared Baconian. His central idea here is fairly Baconian anyway: an encyclopedia of known facts is required. Discussing the role of small facts, Boyle quietly shifted his scrutiny from the questionable role of information as support of a theory to its obvious ability to undermine a theory.[20] Even this obvious logical fact leads to a hard question: why do the unprejudiced need refutations? They do not; but we are all prejudiced, unawares, Boyle said; we can become aware of them by refuting them. Again, Boyle spoke in the old

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[18] *Sylva Sylvarum* comprises ten books, each with 100 brief chapters.

[19] A letter of the Astronomer Royal on Faraday (Agassi, 1971, 315) is telling. So is de la Rive's obituary.

[20] Amusingly, Herschel did the same almost two centuries later (Herschel 1830, Ch. 7). The asymmetry between support and undermining is logical and although many, from Bacon to Popper, have repeated it, some bamboozlers deny it and win applause of the ignorant.

Baconian idiom but injected a new meaning into it. “Diligently turn our eyes to Observations” is a Baconian idiom that at times meant looking for refutations and at times looking for new facts. This displays the prevalent ambivalence towards criticism.

Boyle declared this a digression, perhaps in fear that he had gone too far in his criticism. He followed Bacon, though, on the demand for an experimental encyclopedia that may be of use both for discoverers (to save the trouble of looking for facts already discovered) and for theoreticians (as bases for theoretical superstructures). Encyclopedias then had the form that Bacon had prescribed, but for a different use.

> But I digress; and therefore must step back into the way, and tell you that the reasons, why I first designed the Narrative of what I have try’d and observ’d for a Continuation of Sir *Francis Bacons* Natural History, you will meet with in my … intended Continuation … that treat of Promiscuous Experiments: and the reason why I have since declin’d that succinct way of Writing is, for the sake of *Pyrophilus*, that I might have … a greater Liberty to insist on and manifest the Reasonableness of such Animadversions, as I thought reasonable for a Person, who, though a great Proficient in other parts of Philosophy, is but a Beginner in Experimental Learning. … I have often made use of seemingly slight Experiments … because … I thought their Easiness or Obviousness fitter to recommend them, than depretiate them; and I judg’d it somewhat unkind … to refer you most commonly for proof of what I deliver’d, to … intricate Processes … unless you be already, what I desire my Experiments to make you, a skillful Chymist. … I was also hopeful that the Easiness of divers things inviting you to make tryal of them, and keeping You from being disappointed in Your Expectations, the success of your first attempts would incourage You to make tryals also of more nice and difficult Experiments. …

Bacon’s *Sylva Sylvarum* was a posthumous publication; Bacon’s reason for writing it is not obvious. Boyle always stated his reasons for his publications. He aimed to publish *The Promiscuous Experiment* to serve as aid for beginners. He found its form constraining, however, as he wanted freedom to comment on his information. He viewed *Sylva Sylvarum* as a good prompter, but otherwise as not commendable. Nevertheless, in a way his ideal presentation resembles Bacon’s *Sylva*. This supports Herschel’s censure of Boyle for his imitation of Bacon and the praise that current Boyle scholars lavish on him for the same reason.

Both attitudes are faulty, since Boyle rejected Bacon’s doctrine of prejudice. Instead of purging the mind of all opinions, he planned an encyclopedia of facts partly to refute some theories. This his peers considered but an interpretation of Bacon: we should clean our minds, and some facts may help for this task. This, however, is travesty: Bacon denied the possibility that Boyle advocated of cleaning the mind in stages through research. He had said, prejudices resist refutations as the case of Aristotle illustrates: all his experiments were useless as he approached them in prejudice (*Novum Organum,* 1, Aph. 63). Boyle rejected this silently.

14.8 Boyle’s *Promiscuous Experiments*

Boyle had intended to publish an encyclopedia of facts, his *Promiscuous Experiments*. As he wrote almost every day on various matters, the task of producing this work was for him slight. He prepared it but did not publish. His friend John Beale repeatedly exhorted him to do so, noting that it was easy for him to perform the task. Their friendship had begun due to the spell of Henry Wotton who introduced each one of them to Bacon's writings. Beale said he owed Bacon "some aid for his kindness and solicitude" for his having freed him from scholasticism. He feared that progress would eclipse Bacon. He therefore urged Boyle to publish an imitation of Bacon's *Sylva Sylvarum* and quote harmless passages from that admittedly obsolete work as well as from the equally obsolete *Novum Organum*.

Boyle misled generations of readers, including scholars who specialize in the study of his work, by his refusal to publish open criticism of Bacon's views. Beale wanted him to go further; he tried to exploit Boyle's traits, strong and weak. Boyle's continuation of the *Sylva*, he suggested, would free him of worry about his manuscripts and the need to express theoretical disagreement with predecessors; the mere presentation of facts would suffice to show progress, since "one well chosen experiment is worth more than the largest volumes of the old style". Criteria for choice and similar matters did not concern Beale; all he wanted was to rescue Bacon from oblivion—in gratitude. Boyle hesitated: he saw the danger in Beale's proposal. He was uneasy. He published something like it, years after Beale's demise and a few months before his. It is a collection of facts. In its introduction, he compared his own work and that of Bacon. We all continue some part of Bacon's work, he said there, engaged in experiments and propagating the experimental philosophy even to people of limited abilities. In another way, Boyle continues, Bacon's work is objectionable: it is too uncritical (*Experimenta at Observationes Physicae, Works* 2000, 11, 373)

> … since (at least in our age) no Writer, that I know of, has so early and so well, both urg'd the necessity of Natural History, and promoted divers parts of it by Percepts and *Specimens*, as the illustrious Lord Verulam; I shall not scruple in the way or manner of Writing these short Collections of mine, to make use somewhat frequently of his Authority and Examples, but without confining my self to either. … you will find that some of the Particulars, that the following Treatise consists of, are single, and as it were, independent ones; upon which account they resemble those, which in the Verulamian *Sylva*… are called experiments solitary …

Boyle mentioned then stylistic points borrowed from Bacon's *Sylva*. For, Bacon had wished his *Sylva* to serve as a stylistic example. Those who disagree about this, unwittingly saddle him with plagiarism. Worse: even as an example, Boyle added, *Sylva* is deficient as its author was credulous. He had demanded reliance on one's own senses and on nothing else, yet here he had swallowed "unverified reports or vulgar traditions". This is as close as Boyle ever came to express his displeasure with Bacon: he was trying to follow Beale's request.[21]

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[21] (*Correspondence*, 2001, 3, 139–40): "The Continuation of Lord Bacons *Sylva*, or Promiscuous Experiments wilbe a Pandect, to receive all your scattered papers, & to reduce them into a disorderly Order as falls out oft-times to be better than the best Methode, for all uses, occasions, & for immortality. Here you may annexe to any piece that is published, reexamine, enlarge Here you enter polychrests, which cannot belong to one Head, nor confind to fewe heades. Here you relieve Lord Bacons *Sylva*, & his *Novum Organum* which oft times wants your ayde. Here you shewe, Howe much you have Advced both beyond my Lord Bacons Votes, & beyond Des Cartes his sayes, or Imaginations."

"Sir I have strong reasons to urge, That as soone as they amounted to a Century they deserv'd to be abroad." For thus you may empty your deskes often; & be lesse overwhelmed with your owne

14.9 Boyle on Attempts to Create some Theories

> Perhaps you will wonder, *Pyrophilus*, that … I should speak so doubtingly and use … expressions, as argue a diffidence of the truth of the Opinions I Incline to, and that I should be so shy of laying down Principles, and sometimes of so much as venturing at Explications. But … having met with many things of which I could give myself no one probable cause, and some things, of which several Causes may be assigned so differing … I am so sensible of my own Disability to surmount those Difficulties, that I dare speak confidently and positively of very few things, except of Matters of fact. And when I venture to deliver anything by way of Opinion, I should … speak yet more diffidently … 'Tis not that I at all condemn the Practice of those Inquisitive Wits, that take upon them to explicate to us ev'n the abstrusest Phenomena of nature: For I am far from censuring them, that I admire them when their Endeavors succeed, and applaud them ev'n where they so but fairly attempt.

In brief, Boyle admired attempts to explain, to invent theories, even the failed ones. His diffidence about the truth of any theory is in contrast to Bacon's (*Works*, 3, 249):

> Of the impediments which have been in the affections, the principle whereof hath been despair or diffidence, and the strong apprehension of the difficulty, and that men have not known their own strength … That this diffidence had moved and caused some never to enter into research …

Boyle's declaration of his diffidence; he declared his strong apprehension of the difficulty and of his own weakness. Again he clearly alluded here to Bacon: in defiance of Bacon's hostility to the effort to explain, Boyle advocated it. Bacon had said (*Works*, 3, 226),

> The error is both in the deliverer and in the receiver. He that delivereth knowledge desireth to deliver it in such form as may be soonest believed, and not as may be easiest examined.[22] … Glory maketh the author not to lay open his weakness, and sloth maketh the disciple not to know his strength.

This is Bacon's explaining error by his doctrine of prejudice: deliverers (teachers) seek fame; receivers (disciples) lazily follow. Boyle rejected the very problem: he said he knew no way to certainty and he allowed both deliverer and receiver to err.

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abundance; Thus you may enforce our dullnesse to apprehend our Worke, & the sooner you will see us worke under your commands.

(*op. cit*, 208): "Your Pandects, or promiscuous Experiments doe seeme to promise a larger bulke than any of the other. And I would earnestly disswade from publishing more than a Century at a Time; for all the reasons above renderd, & more especially for the immense extent of the importance of a small number of those collections in the severity of your choice; & they may easily overwhelme an ordinary Industry, & confound Memory. Then these are in your Thoughts I hope you will sometimes caste your eye upon Lord Bacons *Novum Organum*, & give him some ayde for his kindnesse & sollicitute. By thirty of your experiments, you may lifte his head above the waters, & save him from … [oblivion]."

[22] Again, Bacon's inconsistent brilliance stands out. Here, in an aside, he throws away the great idea of Charles Sanders Peirce about the advantage of the most refutable hypothesis and the demand of Karl Popper to speak in a way that renders the vulnerability of what we say obvious. Yet Bacon said this while insisting that the expectation that people will ever behave this way is bound to bring about utter frustration.

Hence, he concluded, whenever "I venture to deliver [!] anything by the way of opinion" (as opposed to a matter of fact), I shall speak "yet more diffidently" (despite Bacon's demand). Boyle was the first, perhaps the only, Modern who commended sincere attempts to theorize, even when they result in failure. Defying Bacon, he declared he did not care that much about fame (*Works*, 1999, 2, 20), let me repeat:

> I know also that way to get Reputation, is, to venture to explicate things, and promote Opinions; for by that course a Writer shall be sure to be applauded by one sort of men, and be mention'd by many others; whereas by the way of Writing to which I have condemn'd myself, I can hope for little better among the more daring and less considerate of men, … than to pass for a Drudge of greater industry than reason, and fit for a little more, than to collect Experiments for more rational and Philosophical heads to explicate and make use of. But I am content, provided Experimental Learning be really promoted, to contribute ev'n in the least plausible Way to the Advancement of it; and had rather not only be an Underbuilder, and ev'n dig in the Quarries for Materials towards so useful a structure, as a solid body of Natural Philosophy, than not do something towards the erection of it.

Here Boyle agreed with Bacon: science is the outcome of humble labor. Also, he appreciated theories but these require experiments; and even though builders of theories are acclaimed, he "had rather not only be an under builder, but even dig in the quarries for materials" for the sake of promoting experimental science.

Thirty years later, John Locke, Boyle's celebrated disciple, responds in his famous "Epistle to the Reader" of his celebrated *Essay Concerning Human Understanding*:

> The commonwealth of learning is not at this time without master builders, whose mighty designs in advancing the sciences will leave lasting monuments to the admiration of posterity, but every one must not be a Boyle or a Sydenham and in the age that produces such masters as the great Hugenius, and the incomparable Mr. Newton, with some other of that strain, it is ambition enough to be employed as an under-labourer in clearing the ground a little; and removing some rubbish that lies in the way to knowledge …

The commonwealth of learning still preaches humility, but now from a position of strength: fame is for the few, observes Locke, but there are many lowly tasks for the rest of us. And, as Bacon had promised, the lowly way is the true way to real fame. The under laborer or the under builder whom Boyle described as the performer of experiments, Locke describes as the performer of methodological or epistemological functions. We may thus contrast the quotation from Buffon, who used Boyle's idiom in order to attack speculations, and the continuation of the above-quoted passage from Boyle that Buffon imitates (Hazard 1954, 143):

> Let us continue to add to our experience and avoid all theorizing, at least until we have thoroughly mastered the facts. We shall find out easily enough some day where to put our material, and even if we are not lucky enough to complete the building, we shall at least have the satisfaction of knowing that our foundations have been well and truly laid. And perhaps—who knows?—we may have progressed beyond all our expectations with the superstructure? [23]

[23] The metaphor of foundations is from the *Novum Organum*; that of superstructure is from Boyle's "Proëmial Essay".

Boyle (*loc. cit.*):

> Nor have my thoughts been altogether idle and wanting to themselves, in … attempting to devise Hypotheses … but I have hitherto … found, that what pleas'd me for a while … was soon after disgrac'd by some further or new Experiments, which at the time … was unknown to me, or not consulted with. … I have had opportunity to observe how many … Doctrines, after having been for a while applauded and even admir'd, have afterwards been confuted by the discovery of some new Phenomenon in Nature … For I have found it happen … in our Theories built on either too obvious or too few experiments, … may agree very fairly with this or that or the other Experiment; but being made too hastily, and without Consulting a competent number of them, 'tis to be fear'd that there may still after a while be found … their Inconsistency of which will betray and discredit them.
>
> I have notwithstanding all this on some occasions adventur'd to deliver my Opinion, not that I am very confident of being less subject to err … but because I would manifest to You, that I scruple not to run the same venture with those incomparably better Naturalists, that have thought it no disgrace in difficult matters rather to hazard the being sometimes mistaken, than not to afford Inquisitive Persons their best Assistance towards the discovery of truth.

This is Boyle's final word: the ambitious tend to publish too many theories; one should test one's theories and humbly keep them private unless the public, rather than the author, might benefit from their publication. After all, a theory is only a conjecture and even if it is confirmed it is still open to refutation. Still, it may be right to take the risk of being found mistaken for the sake of truth. And then it is no shame to suffer refutation.

14.10 Methodological Tolerance

How satisfactory was the mechanical philosophy? How satisfied was Boyle with it? An ongoing discussion in the literature concerns the first question. My discussion here concerns mainly the second. Boyle was then working on his vacuum machine and this led him to his study of the elasticity of air and to his famous law of it. He could not say whether his hypothesis abided by the mechanical philosophy and he noted that some researchers habitually preferred to ignored any deviation from it at any price. This is regrettable: the empirical study of elasticity should continue regardless of the question of the possibility of its mechanical explanation. Advocates of the mechanical philosophy, Boyle observed, had to explain mechanically the elasticity of steel springs, and when they do, they will thereby have also explained the elasticity of air. This is a general methodological issue. Many of his contemporaries, he charged, despised some observations just because their reporters did not interpret them mechanically. This probably alludes to earlier background information. A short time before writing this complaint, in 1655 to be precise, he published anonymously a medical tract (that he had written in 1647, when he was 20); it was entirely overlooked, perhaps because he had written it with no reference to the mechanical philosophy (Rowbottom 1950).[24]

[24] Boyle did not publish his voluminous output under his own name before he was 30 (Shapin 1993, 336).

In his "Proëmial Essay" Boyle discussed tolerance, while affirming loyalty to (the atomistic version of) the mechanical philosophy. He suggested the possibility to postpone discussing the mechanical background for a hypothesis. He justified tolerance by reference to levels of explanation. In his system it is hardly necessary for facts but it is required for theories. As a new fact appears, it is for the mechanists to make it compatible with the mechanical philosophy or declare reconciliation impossible and discard mechanism. As a philosophy is of our making, it is less warrantable than a report of a given fact. Boyle's argument is simple: the assumption that all facts are explicable mechanically and atomically does not contradict all non-mechanical hypotheses: a non-mechanical hypothesis may even follow from mechanical principles. Yet the mechanical philosophy may be false. Boyle's idea of its status is quite different from that of Descartes, who had declared the intuition of the essence of matter clear and distinct and that any idea clearly and distinctly intuited is true. For, otherwise God deceives us. Boyle's response to this was sharp: Descartes blames the Lord for his own mistakes. Boyle rejected all views of scientific certainty (particularly the doctrine of essences[25]). What remains of Descartes' philosophy *sans* its claim for certainty is the task of science: to pose satisfactory explanations, to explain the obscure by the obvious, the unknown by the known, the not understood by the well understood. The most understandable or comprehensible, according to Descartes, are the mechanical principles. Now, giving up certitude allows giving up the idea that the known is superior to the unknown. The appeal of the mechanical philosophy as obvious is thereby much lessened. This appealed to Boyle. His chief concern was to keep any experiment superior to all theory. He therefore charged some mechanical philosophers with intolerance. They ignore medical and chemical facts and theories that have not been interpreted mechanically although some of the most important theories that they advocate, even within mechanics, were not yet interpreted mechanically. (He took credit, we remember, for having interpreted mechanically many chemical facts.)

The mechanical philosophy assumes that a body can influence another body only by pushing it, by coming in contact with it. The mechanical philosophy requires all explanations to assume impenetrable matter and its motion. Many great philosophers advocated this philosophy with small variations. Descartes had employed it most convincingly, impressively, and extensively. One of his important ideas was his theory of the mechanical model. To explain a phenomenon in terms of a mechanical model is to imagine a mechanical structure that behaves in a way that corresponds to the phenomenon to be explained. If the explanation is successful, then presumably the structure it postulates does exist. The most celebrated example of this is Descartes' vortex theory that assumes swarms of hard particles whirling in the sky and causing planetary motion. Newton shattered this model. Smaller but still lovely examples are those that

[25] Boyle criticized essentialism as anthropomorphic. He said, for chemists the essence of ice is its ability to melt but for physicians the essence of water is its ability to freeze.

explain the conduct of a geyser. Yet dolls with built-in clockwork mechanisms that under his influence appeared in eighteenth-century Europe, scientifically valueless though they were, contributed no less to the popularity of the mechanical philosophy. These dolls could write sentences and perform other tasks usually performed only by humans, thus exciting the imagination to suggest machines that imitate human conduct (robots) like E. T. A. Hoffmann's story (1817) famous as *Coppélia* (1870).

Boyle did not quarrel with the mode of explanation that Descartes had suggested. He objected to Descartes demand to limit all explanations to mechanical models. He allowed other kinds of explanation—at least temporarily. He went so far as to claim that nobody had followed rigorously Descartes' demand, not even Descartes. Historically, this may be Boyle's greatest contribution to science: he was the only (somewhat older) contemporary of Newton whom Newton deemed his superior. And so Boyle could encourage him. And he did. Newton had to deviate from Descartes' demand that he deemed inherently correct, at least temporarily. And Boyle argued in favor of this move precisely.[26]

The situation in mechanics was unsatisfactory in two ways, Boyle observed. First, theoreticians failed to explain mechanically many known phenomena. Second, some philosophers have used successfully some non-mechanical hypotheses. Archimedes' law that has a great explanatory power, Boyle remarked, assumes a theory of gravity that is not yet mechanically explicable. This reference to gravity may be an allusion to Galileo's hypothesis that explains the mode of falling bodies, but not mechanically. Galileo had expressed displeasure at the theory of force of the great Gilbert (Galileo 1953, 406). Boyle commented that Galileo's theory too is non-mechanical. The mechanical orthodoxy rejects even successful hypotheses that are not mechanical. This forbids "intermediate causes" and prevents gradual progress in the hope that radical progress is in the offing. The advantage of discussing intermediate hypotheses is that they help unify problems. Assigning elasticity to air both explains many phenomena and connects the behavior of steel with that of the behavior of air into one general problem. (The mechanical theory thus acquires its value as a regulative principle, to use Kant's idiom.) "There are so many subordinate causes" between facts and the universal mechanical theory "that there is left a large field, wherein to exorcise men's industry and reason." Creating hypotheses follows two factors: the urgent demand that they should explain and the principles by which they should abide eventually. Hence, the mechanical theory should not be a criterion for rejecting hypotheses—especially, Boyle added, since mechanism is itself entangled in difficulties, as the problem of the origin of motion shows.

[26] There is more to it than tolerance: there may be serious criticism of Descartes. It is hard to adjudicate how serious was Boyle's occasional inclusion of gravity among the mechanical affectations of matter and his refusal to engage in a debate with Henry More about "the cause of gravity in general". See his *Hydrostatical Discoursse Occasioned by the Objetion of the Learned Dr. Henry More*: in his *Works* 1999, 7, 139–84, 148. Obviously, Newton knew this text.

14.11 The Usefulness of Hypotheses

Methodologists often discussed the need to exclude some sort of hypotheses *a priori*. Boyle did so too. His injunction to publish only tested and confirmed hypotheses did not satisfy him, since he did not wish people to adhere to his suggestions come what may, and since even confirmed theories may be too arbitrary. The obvious demands for restrictions he found poor but valid. It is advisable, he agreed, (a) to avoid arbitrariness and excess speculation, (b) to offer hypotheses that are economical, simple, beautiful, convincing, and non-*ad hoc*; and (c) to provide some evidence supportive to the chosen hypothesis. Nevertheless, hypotheses that abide by some of these rules, especially the non-*ad hoc* or the confirmed, he declared too important to disregard. This is my reading of Boyle, that is not the received one, I am afraid. In this reading, Boyle's position is admittedly hard to back. The difficulty it comes to solve is genuine and one that Boyle had to struggle with. William Whewell was the first to argue in detail—almost two centuries later—that the exclusion of *ad-hoc* hypotheses is necessary. Still later, Karl Popper showed it to be a part of a broader requirement, namely, to admit only the most highly testable hypotheses. His methodology explains the exclusion of *ad hoc* hypotheses from science as necessary and sufficient, and it does this in a non-*ad hoc* manner (Agassi 1975, Ch. 8). Boyle said, we want our hypotheses explanatory and thus non-*ad hoc*. He could not elaborate.

The theory of explanation belongs to Galileo and to Descartes. They viewed explanation as deduction of the explained phenomena from the explanatory hypotheses; they viewed a satisfactory explanation in physics as mechanical. Mechanism gave way to Newtonian dynamism: a satisfactory explanation is by a hypothesis that follows Newton's scheme, said Hermann von Helmholtz (1847). Twentieth century methodologists ignored all metaphysical limitations and demanded to stick to the methodological rules only. The Whewell-Popper requirement for testability stands; it does not harmonize with the (verificationist) idea about testability as the assurance that if the hypothesis is false the test will refute it. It is better to relativize the Galileo-Descartes requirement, I say: a satisfactory explanation is non-*ad hoc* and preferably in accord with the best metaphysics available (Agassi 1975, Ch. 9, Appendix).

A major part of Boyle's difficulty was his fear that no one would bother to test a theory, no matter how testable it may be: he viewed dogmatism as the opposite of the wish to test. He discouraged mechanistic dogmatism. He allowed for intermediate hypotheses to that end. He encouraged curiosity about facts, allowing for the hope that they would serve induction but not encouraging it. To be able to deduce more information from principles, he added, is to test them. No one before Boyle and no one except for him before Kant, so emphasized the active role of the human intellect in the process of forging science.[27]

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[27] It is difficult to stick to this obvious claim. For, classical observation theory—sensationalism—is inherently passivist. According to Bacon, since Mother Nature does not lie, utterly passive observation cannot err. This is his doctrine of prejudice, of course. Locke too insisted on a theory of perception that views it as pasive.

To begin with a seemingly casual but repeated stress on the practical aspect of the demand: Boyle's objection was not to thinking *ad hoc* theories, but to publishing too many of them. He clearly opposed all demand to restrict thinking. Discussions of the publication of wild hypotheses should center on public needs. Boyle declared bravely and perhaps against Bacon, saying that knowledge and power are distinct. But a slight comparison will show that he had changed the terminology. Since Plato and until Einstein, knowledge was distinguished from opinion by its claim for certainty. Descartes declared comprehensibility a criterion or a guarantee for certainty. Boyle replaced knowledge by comprehensibility but he did not find in this replacement any guarantee. He argued for it as the cause for intellectual satisfaction—although Bacon had deemed satisfaction a major inducement for the development of prejudices. In one way Boyle did agree with Bacon more than with Descartes (and rightly so): Bacon had claimed that intellectual satisfaction with the theories we have at hand may lead to stagnation. Boyle concluded from this that satisfaction is not enough: we need power too. Now by "power" Bacon meant control over nature and thus utility. By the very same word, Boyle meant simplicity or non-*ad hoc* character, i. e, explanatory power. Bacon had suggested utility as the "mark" of science, the distinction between scientific and speculative systems; he objected to the criterion of intellectual satisfaction. Boyle interpreted Bacon's idea by misrepresenting his view—claiming that Bacon meant to say that science may be usefully applicable, not that it must be. Perhaps he was right: it is always hard to interpret an inconsistent writer.

In Boyle's writings, however, this change is surreptitious of the meaning of "power"—from control power to explanatory power. It makes his discussion rather cryptic, not to say obscure. Although he recognized the value of utility, he tacitly rejected Bacon's identification of science with utility. Some non-mechanical hypotheses, he said, are "able to do many things … some, of them strange, and more of them useful to human life …" It may sound far-fetched to claim that by "strange" Boyle meant new, but this seems to be the case, because the continuation of his argument against mechanical dogmatism rests explicitly on the assumption that the useful non-mechanical hypotheses could lead to the discovery of new facts. Boyle said, non-mechanical theories gratify only poorly; mechanists agree, he added, that we must observe facts; but then we must allow for non-mechanical hypotheses that help us observe and discover. Thus, even the unwise theories of Paracelsus are valuable, as they helped perform some "truly admirable" experiments. Now as many historians of science still pass in silence over Paracelsus if not dismiss him with contempt, Boyle's courage is notable. It took centuries before another significant theoretician had a good word for Paracelsus; that was Faraday.

Admirably, Boyle declared the system of Paracelsus in a way superior even to that of his great hero Descartes; he admired some of the discoveries of Paracelsus even though he explicitly and perhaps rightly judged him more of an imposter than a philosopher. He adhered to Descartes' theory of comprehensibility (although denuded of his claim for certitude), and he wanted to render each comprehensible theory non-*ad hoc* so as to "free it from all imputations of barrenness".[28] His recommendations then are these: researchers should try to render their theories non-*ad hoc*; anyway, they should be tolerant towards them, preferably but not necessarily in accord with the mechanical principles.

The demand that theories should be made non-*ad hoc* thus takes precedence over the demand that they should follow mechanism. Boyle's objection to the traditional program, the program of assuming "hidden qualities" or essences to explain the observed phenomena, rested on the fact that this program condemns its adherents to be content with *ad hoc* theories. The non-*ad hoc* character of some mechanical theories is a major merit of mechanism. The best summary of this view is in Newton's *Opticks* (towards the end) where Boyle's argument backs his own deviation from mechanism:

> To tell us that every Species of Thing is endow'd with an occult specifick quality by which it acts and produces manifest affects, is to tell us nothing. But to derive two or three general Principles or Notions from Phaenomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from those manifest Principles, would be a very great step in philosophy, though the Causes of those Principles were not yet discovered.

Occult explanations are worthless, Newton said, as they are *ad hoc*. Non-*ad hoc* explanations are valuable, he added, even when they are not fully satisfactory.[29] Evidently, this does not close the discussion of this matter: *ad hoc* hypotheses can be irksome: when the wish to avoid them is at times very hard to gratify researchers may take rain checks (Agassi 1975, 189). Surprisingly, until today most methodologists repeatedly ignore this important and living demand to avoid *ad hoc* explanations.

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[28] Descartes claimed that even if for some reasons the certainty of his principles is discarded, their fruitfulness is unquestionable, as they brought about theories like his optics. Boyle, who rejected Descartes' optics, here claims that the mechanical principles can be useful but that they are barren as yet. This shows again both his implicit mode of criticism and his usage of "power", "usefulness" and "fruitfulness" not in their Baconian senses.

[29] Some methodologists totally ignored demand to avoid *ad hoc* assumptions. Surprisingly, most of those who utilized this demand in their methodological theories did not discuss it. I may mention perhaps Whewell as an example. He used the non-*ad hoc* character of hypotheses as his criterion for their scientific status, i. e. for their certainty. As certainty is elusive, it may perhaps be less surprising that his arguments for his demand for non-*ad hoc* character are complex and overlooked. Still, at least he could repeat Newton's argument that we want information about the world and *ad hoc* hypotheses give us none. Yet Whewell's problem was different. He wanted to explain the refutation of Newton's optical theory in a manner that dismantles the threat of a similar fate to Newton's mechanics. His explanation rested on his criterion: he showed that Newton's optical theory was *ad hoc* but not his mechanics (and not the wave theory of light). Whewell's explanation is somewhat *ad hoc* (just as his construction of the optical True Ladder of Axioms is). Russell's simplistic attitude is perhaps best: *ad hoc* hypotheses are arbitrary. This does not clash with Popper's sophisticated view of it as the contrary to testability.

14.12 Civilized Argument

> A Senate or a Monarchy may indeed command my Life and Fortune; but as for my Opinions, whether of Persons, or things, I cannot in most cases command them my self, but must suffer them to be such, as the Nature of things I judge of requires; and therefore, the thinking all things done with Wisdome, that are done by Men in Power, is too great an Impossibility to be a Duty …

(Boyle, *Occasional Reflections*, Sec. 4, Disc. XI, *Works*, 1999, 5, 145–6)

Boyle never wishes to impose his own authority, since he recommended discarding any view upon learning of valid criticism of it, as he would naturally do: authority is not philosophical.

> For *Aristotle* spoke like a Philosopher, when to justify his Dissent from his Master *Plato*, he said among other things, That for the sake of Truth, men (especially being Philosophers) ought to overthrow their own tenets. … And though for a man to change his opinions, without seeing more reasons to forsake them than he had to assent to them, be a Censurable Levity and Inconstancy of mind; yet to adhere to whatever he once took for truth, though by Accession to more light he discover it to be erroneous, is but a proud Obstinacy, very injurious to Truth, and very ill becoming the sense we ought to have of human frailties. … And certainly, till a Man is sure he is infallible, it is not fit for him to be unalterable.

Here we see Boyle's conservative alternative to Bacon's radical doctrine of prejudice: hold on to your views, be critical about them and relinquish them upon refutations. To date this opinion is not easy to dissent from (Popper 1962, 120; Pera 2006), but we should nevertheless avoid guarding it against criticism, to say the least. This follows from the last statement in this quotation, the assertion of Boyle's unflagging fallibilism. It is as characteristic of him as its contrary is characteristic of most of his current commentators.

There is hardly any idea that was more influential in the Age of Reason than radicalism. The presumption of a complete break with the past dominated the intellectual life of Western Europe in the eighteenth century, as well as the hope to start completely afresh, this time in the best way. It transformed philosophical, scientific, and political thinking—and largely even theological views. Boyle rejected it as well as the quest for certitude. He denied the very possibility to free ourselves of our own prejudices in any way but by becoming aware of them through criticism. He considered it desirable to insist on one's past views, not dogmatically but until they be refuted. This idea, too, he repeated sufficiently often (e. g., *Exc. Theol.*, Sec. 5; *Works*, 2000, 8, 94) to deserve more consideration than it received. I do not deceive myself: this discussion of mine will not alter the situation significantly. Let me quote one more passage nonetheless, this time from the end of his most famous work, his *Defence of a Doctrine* (*Works*, 1999, 3, 82),

> And though being very far from being wedded to my Opinions, I am still ready to exchange them for better, if that shall be duly made out to me, (which I think it possible enough they may hereafter be;) yet peradventure the Reader will think with me, that the Examiner has not given me cause to renounce any of them, since the Objections he has propos'd against me have been sufficiently answered, and since the *hypothesis* he would substitute in the room of ours …

Boyle the teacher said, do not be ashamed of error, provided you did your best to avoid it and provided you easily admit it when refuted. Boyle the realist legislator noted that too many of his contemporaries did not follow this advice. And so he ruled ("Proëmial Essay", *Works*, 1999, 2, 25):

> You will easily discern, *Pyrophilus*, that I have purposely in the ensuing Essays refrain'd from swelling my Discourses with solemn, and elaborate Confutation of other men's Opinion, unless it be in some very few Cases, where I judg'd, that they might prove great impediments to the Advancement of Experimental learning; and even such Opinions I have been wary of meddling with, unless I suppos'd I could bring Experimental Objections against them. For it is none of my design to engage my self with, or against any Sect of Naturalists, but merely to invite you to embrace or refute Opinions as they are consonant to Experiments, or clear Reasons deduced thence, or at least analogous there unto, without thinking it yet seasonable to contend very earnestly for those other Opinions which seem not yet determinable by such Experiments or Reasons. And indeed, … I would endeavor to destroy those curious but groundless structures that men have built up of Opinions alone, by the same way (and with as little noise) as light destroys the picture in a *camera obscura*, by just reporting of new facts to the contrary.

He urged researchers to invent theories, considering facts necessary for refuting some and for creating better ones. Yet he had trouble posing Experimental Objections. It is hard to object to his avowed desire to avoid controversy. It is easy to declare science a permanent debate, as I do, yet doing so we should also admit that joining in the debate is utterly voluntary. It is thus hard to oppose Boyle's wish to refute theories quietly. His support for this with the remark that he was not for or against any "sect of naturalists", however, invites explanation. Bacon had condemned important philosophers, especially those from whom he lifted some ideas, and he did so meanly, often by ascribing to them the ambition to establish sects in the wish for fame. Distancing himself from Bacon on this (see note 13 above), Boyle developed a reluctance to argue.

The fear of sectarianism is rather naïve. True, sects cling to immutable dogmas while non-sectarians may argue freely and rationally. Yet there is no sharp division between sectarians and rationalists. Sects shift their grounds surreptitiously and change their dogmas (Mises 1957, xiii). Bacon's followers too did so: inductivism comes in many mixed versions (Bunge 1960). The more dogmatic a sect, the less ready it is to admit having made a change. Inductivists were peculiar in one respect: eager to acknowledge discoveries, they had to change many views, and quite rapidly. There was also the lucky fact that Newton's ideas proved so durable. Inductivists had three techniques for concealing changes. One: denounce forcefully refuted views to show you never held them. Second: following Boyle's tradition, hint at refuted views instead of stating them explicitly and clearly first. Three: hint at a new hypothesis, claiming priority for it if it becomes the received opinion and denying having asserted it otherwise. Faraday exposed this last technique as dishonest (Agassi 1971, 23).

Boyle was extremely popular. He was always open to criticism. He was grateful to Spinoza for his criticism; he encouraged Beale to criticize him, and he probably avoided public discussion of his ideas because he gained little from it and feared hurting others. He discussed criticism not theoretically but practically, with the concern of the commonwealth of learning in mind and in the hope to encourage experimentation. He criticized mainly outsiders: Aristotelians, alchemists, Line or Linneus, Hobbes, and even his close friend Henry More the celebrated Cambridge Neo-Platonist (*Works*, 1999, 7, 139). This selectivity rendered the Royal Society a kind of a closed club: its Fellows were reluctant to criticize friends. A complaint by Henry More against his friend Boyle needs airing. I do not know how serious it is. Boyle had criticized him in response to his public branding of Descartes an atheist; Boyle first criticized him in a private letter. More's complaint suggests that possibly Boyle had exaggerated people's sensitivity to public dispute; perhaps Boyle was himself somewhat over-sensitive. At least this is what More claimed in what seems a very candid letter to a close friend (Nicolson 1930, More to Conway, May 11, 1672; Fulton 1933, 101):

> Mr. Boyle does not take my dissenting so from him in publick so candidly as I hoped, which I am very sorry for. For he is a person that I have a singular honour and esteeme for as I do cordially and simply declare in the book in which I meddle with his notions, and thought Philosophy had been free. If any body had confuted any thing of mine with such circumstances as I have done, I cannot perceive that that would have given me the least disgust in the world. If he were mistaken I would with the same kindnesse demonstrate his errour to the world, and lett them judge.

Perhaps much as Boyle saw the need for civil argument and its benefit, he did not clearly see its fundamental difference from bickering. Perhaps his idiosyncrasy played a role here (as was the case with Newton). "My humour", he said, "has naturally made me too careful not to offend those I dissent from." Here is this statement in context (end of his *Defence of a Doctrine*, *Works*, 1999, 3, 14):

> To conclude, I feel no cause to despair, that whether or no my Writings will be protected, the Truths they hold forth will … establish themselves in the Minds of men, as … formerly much contested Truths have already done, My humour has naturally made me too careful not to offend those I dissent from, to make it necessary for any man to be my Adversary upon the account of Personal Injuries or Provocations. And as for any whom either Judgment or Envy may invite to contend, that the things I have communicated to the World deserved not so much Applause as they have had the luck to be entertain'd with; that shall make no Quarrel betwixt us: For perhaps I am my self as much of that mind as he …

This is a lovely sentiment, but it leaves unanswered a serious question: what distinguishes between useful argument and futile disputation, between serious arguing and mere quibbling or bickering? Boyle did not know, as we still do not. He made all his discussions as fruitful as he could. His method of experimental arguing was novel. "I was at first astonished", said Christiaan Huygens, "to see that he has taken the pain to write so big a book against so frivolous objections … but having begun to peruse it, and seeing that among his refutations many new discoveries and observations not yet seen, I wish it has been bigger" (Rigaud 1841, 92). The *Defence* thus was surprising. Boyle continued in it a tradition that Gilbert and Galileo began; with it he started the tradition of experimental arguments that was revolutionary. His wish for arguments to be less explicit is less valuable, and fortunately also less significant. It was the influence of Newton more than that of Boyle that perpetuated the inductive tradition of hinting at criticism. This tradition is somewhat justified by the inability to accept criticism with good humor, as a footnote from Disraeli's *Calamities* (140) testifies:

> So sensible was even the calm Newton to critical attacks, that Whiston tells us he lost his favour, which he had enjoyed for twenty years, for contradicting Newton in his old age; for no man was of "a more fearful temper". Whiston declares that he would not have thought proper to have published his work against Newton's *Chronology* in his lifetime "because I know his temper so well that I should have expected it would have killed him; as Dr. Bentley … told me, that he believed Mr. Locke's thorough confutation of the Bishop's metaphysics about the Trinity hastened his end". Pope writhed in his chair from the light shafts which Cibber darted on him … Dr. Hawkesworth died of criticism.

14.13 Boyle on the Method of Quoting

Boyle's discussion of civil argument ends as a discussion of the method of quoting as his aim was to discourage quoting errors as well as superstitions. Reports of wonderful but unrepeatable events are typical of the Middle Ages. His proscription of these had two significant consequences. One is that quotations were almost entirely avoided. Even the great authority, Newton, was surprisingly seldom quoted, least of all by researchers. (They often alluded to his authority but never openly relied on it.) The second is that editors of scientific journals preferred to publish full translations of significant papers rather than quotations or abstracts.

Boyle's other aim was to encourage those who could write scientific reports but who did not know how to decorate them with quotations. He asked the more learned to conceal their knowledge so as not to discourage the less learned. This is another argument in favor of the dryness of the inductive style. It is simpler, I think, to help people overcome their shyness and learn the easy art of decoration.

14.14 Circumstantial Descriptions A: The Problem

Next Boyle passes almost immediately to the discussion about circumstantial descriptions. This is practically the last point of his "Proëmial Essay" and I will discuss it in some detail. I do not claim that I fully understand Boyle's attitude towards this problem. My interpretation of his view concerning it is conjectural. He repeatedly expressed his regret that he did not describe the circumstances of his experiments more fully; he added justifications for this omission repeatedly, although his descriptions are always profuse and seldom terse.

A concise report of a crime may be inadmissible as evidence in court. The court demands detailed statements, often by adding circumstantial details. The result of cross-examination usually increases these details. How many and what kind of circumstances must witnesses supply for their evidence to be acceptable in court? This is a legal problem. How many and what kind of circumstances must researchers propose for their experimental results to be acceptable for publication? This was Boyle's major problem: as he legislated for the whole commonwealth of learning, he had to offer some guidelines, but he could not go beyond the one practical guideline: what courts accept as eye-witness testimony is accepted if two independent sources testify to it and if it is taken to be repeatable. Boyle regularly deviated from it (save in a few of his early works, perhaps), because the inductive style often conflicts with it. Nevertheless, to this day it is the only rule of method that the scientific community adheres to with no dissent—as necessary although not always as sufficient.

A report on an experiment should include all and only those circumstances that suffice to enable readers to repeat it. This is problematic, we remember. How serious is the problem? Boyle had to be regularly well informed about efforts of his readers to follow his instructions for experimentation, and when they had difficulties he immediately met them with newer and more helpful publications. This leads us to two practical aspects to the task of making readers follow a description, and of ensuring the adequacy of its repetition.

First, since the days of Locke, the received inductivist view is that in principle it is possible to communicate all our data in terms of sense data universally understood. All other terms then are only their shorthand expressions, readily understood or decipherable with the aid of an ideal dictionary. This view was not contested. Not even by advanced methodologists like Whewell and Duhem, historians like Butterfield, epistemologists like Russell, and the Würzburg and Gestalt schools of psychology. It is still very popular despite ample refutations. moreover, it is irrelevant: it does not touch upon the practical problem involved, Boyle's problem that pertains to repeatability: what details of an experiment should an observer include in the report on it? (Observation reports hardly ever refer to sensations.)

Butterfield raised this very problem anew. Unknowingly. His story is about his own schoolteacher who failed to reproduce an experiment in the classroom. This is common. The teacher reads and obeys the instructions carefully but something goes wrong nevertheless. It may be due to poor training that does not result in the acquisition of the knack of following instructions. What do trainees learn? Among other things, they learn the missing instructions. Inexperienced experimenters may do something that no trained experimenter would do, because their instructions did not tell them not to do it. No instruction can be complete. This is the variant of Boyle's problem (Polanyi 1966).

This is not a purely technical matter. It is methodologically intricate. Boyle's service was in giving it a practical solution. After a few years of part-time elementary self-training under Boyle's tutelage, readers found themselves familiar with up-to-date problems and even able to try to make minor contributions. This seems easy yet it is very difficult to carry out, especially with no tradition for the instructor—Robert Boyle—to fall back on. The methodological significance of the situation is this. There are contradictory experimental reports. Bacon would say, believe none of them: trust only your senses. Polanyi would respond, this overlooks the skill of the experimenter. Butterfield's story illustrates Polanyi's response. Two views compete here: according to Polanyi, research is the task of the gifted, the skilled, the artist-scientist, the *connoisseur*; according to Bacon and Boyle, everyone can chip in and contribute. Things would be easy to discuss were Polanyi to say that expertise is obvious. He did not; he said, the expert is the acknowledged expert. This is calling in the expert *ex machina*. It is possible to do so in the days of reliable expertise, not in Boyle's days. And to get reliable experts it is necessary to re-raise the problem in another context. Take William Gilbert, who was then surely a gifted researcher and a recognized expert physician but not a recognized expert researcher. Why should people believe his experimental reports rather than those of his opponents, the now forgotten but then recognized experts, or even Gilbert's admired predecessor Battista Porta (1560)? Gilbert claimed that he had performed his experiments in the presence of witnesses. He notices that this claim was unconvincing, and so he requested of his critical readers that they repeat the experiments:

> We have set over against our discoveries and experiments larger and smaller asterisks according to their importance and their subtlety. Let whosoever would make the same experiments, handle the bodies carefully, skillfully and deftly, not heedlessly and bunglingly; when an experiment fails, let him not in this ignorance condemn our discoveries, for there is naught in these Books that has not been investigated and again and again done and repeated under our eyes.

This is admirable but hardly a solution. It is no use branding those who succeed in repeating an experiment skillful and those who fail inept. Indeed, even the acknowledged expert Polanyi failed for decades in his efforts to convince experts about his scientific theory (of adsorption and of catalysis). But the main point is not whether to consider people inept or skillful, nor whether to suspend judgments, but rather to train systematically to repeat experiments successfully and to distinguish between an inability to repeat an experiment and the impossibility to do so. According to Polanyi, only those endowed with certain talents can learn to repeat experiments, especially difficult ones; therefore, testing is a matter for the élite, for the *connoisseurs*: belonging to the élite is a condition for the ability to pronounce judgments in science. Boyle took the opposite view and undertook to train every diligent and sincere person to be able to repeat a repeatable experiment and perhaps to improve upon it.

A part of the opening of Boyle's already mentioned posthumous 1691 *Experimenta at Observationes Physicae* is "A Letter that may serve as a Preamble: To my Learned Friend Mr. H. Oldenburg (Secretary to the Royal Society)"—who was dead since 1677. Having apologized for the lack of sufficient circumstantial descriptions in his observation reports he added (*Works*, 2000, 11, 371–2):

> To these things perhaps, so favourable a person as *Mr. Oldenburg* will add, that the Characters, which learned Writers, English and Foreign … have been pleased to give of the diligence and sincerity employed in setting down the *Physico-Mechanical* Experiments, and those of some other Writings of Mine, may permit me to hope, … that, after having been divers years vers'd in making Tryals and Experiments, I have made them with some care and wariness, and mentioned them faithfully, where I have not done it amply; upon hopes it may be taken in good part from a Person in my present condition, that way never a Professor of Philosophy, nor so much as Gown-man; to have made shift to make the Experiments and Observations he communicates, and set them down truly and candidly,

> without fraudulently conceding any part of them, for fear they should make against him. And tho perhaps you will easily believe, that in divers of the Experiments which I have but briefly mention'd, I have been as diligent an Observer of Circumstances, as I was wont to be when I made those, which have had the luck to be taken notice of for being fully related; and tho it may be also, that some Scrouples or Objections, which my brevity may in part occasion, were not unforeseen by me, and might have been avoided by a more copious and diffus'd way of writing; yet I purposely declined such a way of delivering things, not only for the reasons above mentioned; and because I suppose them that may pursue these Papers, to be acquainted with my formerly Published Writings, and to have either from them, or otherwise, understood the way of making such Experiments as mine; but also because … I should not … reap the Field so clean, as …

In this passage Boyle expressed his concerned with repetitions of his experiments by others. It says, some people had successfully repeated Boyle's experiments and they need less circumstantial descriptions now, and even can take his hints in order to make more discoveries on lines that he was suggesting. If so, then the solution of the problem depends on expertise. Boyle had trained people systematically to be able to repeat experiments. His trainees should be competent to judge the repeatability of an experiment. And if they fail, let them not blame him for fraud. This is a practicable but not a sufficient solution: not all of Boyle's trainees became skillful. Polanyi's claim seems to stand that repeatability is decided by the repetition of the skillful.[30]

The repeatable fact here is that here and there experiments appear that are not repeatable, we do not know why. Boyle often said, repetition ensures repeatability. It does not. He was rightly uneasy about this. He said, the fuller the circumstantial description, the more its repeatability by the diligent and sincere is ensured. Even so, he knew it is impossible to write fully circumstantial descriptions. Such was his alleged discovery of the philosopher's stone: he left its detailed description to his closest friends, Locke and Newton, to check it for its repeatability. His circumstantial description of it was so detailed that it baffled Newton, no small alchemist himself. Boyle's worry was that the inability to repeat some of his experiments would make some people consider him a fraud.

As full circumstantial description of even a grain of sand is impossible (Harris 1968, 288); and so, any effort to repeat any experiment may fail for want of vital detail. Knowing this, Boyle requested his readers to believe then that he had tried hard to be careful in observing and recording, that any failure to reproduce some of his experiments was not due to faking, since he had no vested interest, being no professor of philosophy and no academic but simply fallible.

Let me have one brief paragraph to discuss this question analytically. To know a general observation or to deduce it from a known theory is to know under what circumstances it holds. To view a general observation on the supposition that received opinion is true is no different except that the circumstantial description of

[30] An ambiguity is possible here that is better avoided. The demand to develop a skill to a reasonable level as a condition for some tasks is often reasonable and is not necessarily *élitist*. Not so when the demand is to listen to the skillful, as these are the élite. In a discussion with me in Boston Colloquium (Agassi 1981, 192) Polanyi admitted that his view of science is somewhat romantic as he admired leading thinkers as having abilities above those of ordinary mortals.

the general observation is tentative and subject to improvement with the growth of knowledge. Inductively this is not so: with no theory there is no way to distinguish between relevant and irrelevant circumstances. And so, to be on the safe side, all that is possible to do is to add details as they *may* be relevant. And then, the more detailed a report is, the more difficult it is to repeat it.

Boyle was tilting against windmills, and it seems he knew it. Knowing that one cannot record all the circumstances of an experiment, he claimed that in his earlier writings his descriptions had been fuller. Not so: in his later writings readers get lost among the multitude of details. As an example let us consider his description of pressure-cooking in his posthumous *Experimenta at Observationes Physicae*. He added to the exact dates the exact menus that he had tried out, the number of slices into which he cut an orange, the seasoning of the meat, etc. There is no limit to such thoroughness. The *a priori* or commonsense knowledge as to what circumstances are relevant and thus to be recorded breaks down here as the discussion concerns research that goes beyond commonsense. Often theory guides research. Boyle feared that (as Bacon had observed) a theory and an observation may supports each other spuriously and become dogma. This does not hold for an observation that refutes a hypothesis. Whewell discussed this almost two centuries later. Boyle did not think of that; even Whewell viewed it as a mere preliminary to real progress. Boyle desperately raised the degree of the detail of his circumstantial description, but it was hopeless.

Boyle also hit upon a different solution, independently found by Gilbert before him and by Duhem and by Quine after him: replace a repeatable scientific report with a reliable historical report about a single event, of an experiment performed at a certain time and place (Quine 1988). Boyle stated quite explicitly in his *Excellency of Theology* (that like other theological works of his is a methodological tract) that in science we must put up with mere "moral certainty", i. e., with the sort of certainty of evidence upon which, say, a judge will pronounce an accused guilty. It is wise to listen to (at least) two witnesses before pronouncing a verdict—in court and in science alike. A fact refutes a hypothesis only if it is repeatable; repeatability is forever hypothetical. The conjunction of two incompatible hypotheses is undoubtedly false. Which of the two is false, however, is not known, Duhem observed. Why should we choose the fact over the theory and not the other way around? The problem of the veracity of the statement of fact is solved at least partly by considering it a historical statement and by assuming witnesses candid. Repeatability remains problematic, however, with a shift from one kind of repeatable fact (experiments) to another (reliability of witnesses). This leaves the status of statements of general facts hypothetical, and the problem unsolved as to why they are preferable to the theories that they clash with. Still, under the impact of Gilbert, Bacon, or Boyle, the legal approach did enter the scientific tradition, as the Statute of the Royal Society of 1663 indicates (Weld 1848, 2, 527):

> Two or more Curators shall be appointed … of every Experiment or natural Observation, that cannot conveniently be made in the presence of the Society. …
>
> In all Reports … the matter of fact shall be barely stated, without any prefaces, apologies, or rhetoric flourishes, and entered so in the Register-book of the Society. And if any Fellows shall see fit to suggest any conjecture … the same shall be done apart; and so entered into the Register-book, if the Society shall order the entry thereof.

This is an imitation of the procedure in courts of law. Facts are registered in one book, conjectures in another. Viscount William Brouncker, the first president of the Royal Society proposed this rule; Boyle seconded him.

This legal solution clashes with another legal solution of Boyle. Repeating another person's experiment and finding results different, Boyle decreed, we remember, should not lead to the conclusion that that the other person is a liar.

The problem is real and unsolved. Modern chemists would consider many of Boyle's fullest circumstantial descriptions as lacking in important details. (These they could easily add, with the aid of newer theories and a pinch of commonsense.) This problem is only a weaker variant of the problem of observation. That problem arises from the theory that we see things in the way some theory prescribes. The problem of circumstantial evidence arises from the fact that theory directs observation as Galileo and Bacon have observed. Yet the two variants of the problem differ in thrust. Seeing no solution to the problem of observation may lead to the view that theories are irrefutable whereas seeing no solution to the problem of circumstantial evidence may lead to the view that refutation is tentative as it is inconclusive because the refuting evidence is refutable too.[31]

Boyle's suggestion sounds commonsense: the more detailed a description of an experiment, the more its repeatability is assured. It is no less commonsense that the more detailed the description of the experiment, the harder it is to repeat (unless the details are dictated by a theory, to repeat). But then, commonsense also tells us where to draw the line. The trouble is that in the discussion at hand the search is for the assurance that commonsense cannot provide. This is what makes this discussion perplexing: commonsense certitude is inferior to philosophical absolute certitude, and even Boyle at times confused the two—when he discussed the repeatability of observations that he demanded to prefer to a theory when the two are in conflict. He would have been happier had he not worried much about repeatability. Einstein's and Popper's suggestion is better that life depends on observed regularities and science comes to explain them; that were observation reports not repeatedly corroborated, life would be different and with it science would too; wishing to know why a given observation is repeatable, invites the search for a testable scientific explanation of it.

This is the key to the proper attitude towards this matter: no solution is ideal; solutions are often custom-made to meet local needs *ad hoc*; and this leads to critical examinations in the hope for improvement. One reason Boyle admired Bacon is that he found facts to report everywhere he looked and he had no idea whether they were repeatable and if so whether they were of any value. His first motive was to charm his readers in the hope of making them experiment and report even papers of poor value—any start is right for a start. He taught them to relate what they observe to whatever theory that engages them. One example for this that seems to me delightful is Boyle's observation that violins improve sound as they age. He said, this fact shows that the wood of which the violins are made keeps changing, even though very slowly. He put this in his essay about the impossibility of complete rest (*Works*, 1999, 6, 200).

[31] Compare Popper (1959, §30) with (Musgrave 1976) regarding the refutability of all refutations.

14.15 Circumstantial Descriptions B: Recent Solutions

The incompleteness of circumstantial descriptions render "general facts" hypothetical as they are universal statements that assert that a repeatedly observed event will always recur under the circumstances that its report specifies. Thus, some circumstances are necessary for the repetition of the result, while others are not. The problem is, then, why do we declare a hypothesis false on the strength of an alleged statement of fact that is but a hypothesis?

These days, inductivists usually worry about a different version of the problem. They take it for granted that the problem of induction is that of the choice between hypotheses, and that empirical information renders this decision easy; yet they know that this decision rests on the decision to let facts determine the outcome of the choice. The need to decide on the choice of hypotheses seems reducible to the single decision to rely on factual evidence. This reduction does not work, as the problem of choice of hypotheses becomes the problem of choice of data. Methodologists will have to admit that choice is not only of a theory in the light of evidence but also of the evidence. This requires an explanation and a further justification: either a criterion for choice justifies the choice of the evidence or else all evidence is acceptable. This is not the only point in which, in a sense, Bacon went further than his followers. He explicitly demanded the acceptance of all evidence without preference, arguing that otherwise we get into trouble, and promising that false evidence will cause no harm as it will all be weeded out in the process of induction (*Novum Organum*, 1, Aph. 118).[32] Clearly, the preference for repeatable observations that already Gilbert suggested (implicitly but with ample argument) and that Boyle supported explicitly is the only alternative to Bacon's ruling. Sadly, most philosophers ignore Boyle's rule, although it is the only one on that all researchers endorse, because it does not insure certitude nor any surrogate for certitude.

To modern inductivists who claim that any statement of general fact is a hypothesis to be confirmed by evidences concerning singular facts there seems to be no room at all for the selection of evidence. Therefore they hesitate between stating that one must trust only one's senses and stating that only skilled scientists can give reliable evidence. That selecting evidence happens is an empirical fact. Likewise, sometimes science recognizes evidence of non-experts and sometimes science ignores the evidence of fully qualified researchers (DeMeo 2001).[33] The only other theory of selection extant is that usually researchers test the statements of general facts and endorse them tentatively as long as they are not refuted.

[32] This is the requirement for total evidence. The literature on it is extensive, yet most of it adds nothing to the initial observation on it by Charles Sanders Peirce. Unusual are McLaughlin (1970), Fitzhugh (2006) and Cloos (2010). Still, Bacon was the most consistent yet his promise is invalid. This insures the insolubility of the problem.

[33] Michael Polanyi used this as empirical refutation of inductivism. He declared that no alternative to it is possible and that therefore science has to rely on expert leaders—even when they cannot explain their judgments (or else the explanation would be an induction surrogate). He stressed that expert pronouncements are authoritative even though they are fallible.

That science endorses tentatively some statements of general facts is obvious. Refuted general facts are translated to more adequate expressions thereby made more precise, both as to circumstances and as to quantities. Improvement is not automatic, however; tests invite it. That statements of general facts undergo tests is hardly disputable. One of the methods of testing statements of general facts is known by the name of the method of variation; the method of varying the circumstances systematically. Bacon opposed this (*Of the Interpretations of Nature*, Ch. 17; *Works*, 3, 246):

> That those that have been conversant in experience and observation [Leonardo?] have used, when they have intended to discover the cause of any effect, to fix their consideration narrowly and exactly upon that effect itself, with all circumstances thereof, and vary the trial thereof as many ways as can be devised; which course amounteth but to a tedious curiousity, and ever breaketh off in wandering and not knowing. And that they have not used to enlarge their observation to match and sort that effect with instances of a diverse subject, which must of necessity be before any cause can be found out.

This passage of Bacon is interesting. It claims that the method of research by varying circumstances is endless, and suggests that instead of deliberately varying them we let them vary themselves randomly. Boyle adopted the spurned method of following one's curiosity, in spite of his endorsement of Bacon's objection, since he knew no better method. But he pretended to have adopted Bacon's random method that involves no anticipation. As a result of the inductivist style, the method was employed but hardly mentioned, though it was alluded to with hefty hints. The first inductivist who discussed explicitly the method of variation of circumstances is John Stuart Mill, who, however, had an implicit theory that fully solves our problem: assuming tacitly that the number of possible circumstances is finite, he explicitly suggested varying all of them, one by one. Except that since he did not list them, his recipe is not executable.

John Maynard Keynes said (*Treatise on Probability,* 1920, 271),

> The assumption that every event can be analyzed into a limited number of ultimate elements, is never, so far as I am aware, explicitly avowed by Mill. But he makes it in almost every chapter, and it underlies, throughout, his mode of procedure. His methods and arguments would fail immediately, if we were to suppose that phenomena of infinite complexity, due to an infinite number of independent elements, were in question, or if an infinite plurality of causes had to be allowed for.

Keynes made one step further and assumed explicitly what Mill had assumed implicitly, namely, a "limited complexity in the problems which we investigate". The view that there is a limited variety of circumstances solves the problem by legislating it out of existence. Researchers see a complex world and they declare it simple by making testable hypotheses concerning its simplicity and testing them. Mill and Keynes assumed a metaphysical assumption that allows the conduct of real investigation if only it were specified in advance.

Keynes discussed (368) the correspondence on this matter between Jacob Bernoulli and Leibniz. Bernoulli had posed a question: is it possible to decide *a posteriori* about people's life-expectations by observing long sequences of life-records in the same sense in which we have a priori knowledge concerning the probability of a die to fall on a given side.

> Leibniz's reply goes to the root of the difficulty. The calculation of probabilities is of the utmost value, he says, but in statistical inquiries there is a need not so much of mathematical subtlety as of a precise statement of all circumstances. The possible contingencies are too numerous to be covered by a finite number of experiments, and exact calculation is, therefore, out of the question.

Keynes's text continues thus (369):

> The view of Leibniz, dwelling mainly on considerations of analogy, and demanding "not so much mathematical subtlety as a precise statement of all circumstances" is, substantially, the view [that Keynes adopted].

This may mislead the unfamiliar with the Keynesian system. For, the expression "dwelling mainly on considerations of analogy" belongs to the Keynesian system, not to the Leibnizian one; it means, employing the principle of limited variety. Thus, by curtailing the multitude of circumstances, Keynes claimed, he was able to do what Leibniz had thought impossible, since Leibniz could not endorse this principle: his (Leibniz's) principle of the identity of the indiscernibles clashes hard with Keynes's principle of limited variety. Keynes declared that he followed the demand of Leibniz to present a "precise statement" of all the circumstances, after simply declaring them to be few. This is not so. To declare their number small will not do; the need is to have a list of them. Leibniz never agreed to so limited a view of the world.

The next inductivist solution to the problem at hand is the most radical ever suggested. It seems to imply that even singular statements of fact are hypothetical, and are thus incapable of serving as legitimate solid foundations for induction. The solution is not to accept even singular statements of fact of any sort or kind save ones about the state of mind of their observer. These so-called "Protocol statements", roughly of the form "Otto speaks-thinks: there is a table in this room". (Otto Neurath was the originator of this idea.) This is a dud: the conjectural character of "there is a table in this room" only increases with the preface "Otto speaks-thinks": it is easier to test a statement about this room than about what Otto Neurath speak-thinks (Popper 1959*,* §26).

A decade and a half later Rudolf Carnap said (Carnap 1950, 230),

> One of the principles of the methodology of induction says that, in testing a law we should vary as much as possible those conditions which are not specified in the law. This principle is generally recognized, and scientists followed it long before it was formulated explicitly. The theoretical justification for this methodological principle must lie in a theorem of inductive logic to the effect that by following the principle, that is, by distributing the test cases among a wider variety or different kinds, a higher degree of confirmation is obtained.

For example (575),

> Suppose that one physicist tests the law by making experiments with one hundred specimens, all of the same kind, and finds the results positive. Suppose that another physicist does the same with one hundred specimens taken from various kinds and finds likewise positive results … Then we should say that the second physicist had made a more thoroughgoing examination of the law and therefore has more reason than the first to believe in the law … or in a prediction … of a future instance of the law.

This is a claim about testing a hypothesis by varying some circumstances not mentioned in it. Which of these variations is the first to take place? Carnap's claim is that the testing proceeds "by distributing the test cases among a wider variety of different kinds". This claim is wild. Imagine that two researchers confirm Galileo's theory of the inclined plane by rolling a ball down an inclined plane, one of them repeating one experiment one hundred times and the other doing the same but with different balls of different colors. By Carnap's claim, the conduct of the second researcher is superior to that of the first. In fact they are wasting their time (Popper 1962, 389). In this game "no one can win against you, but must always lose; then it would be better not to play" as Galileo has observed (Galilei 1953, Fourth Day, 439).

14.16 Circumstantial Descriptions C: Boyle's Example

One of the most important Renaissance theories (due to Paracelsus, I think) was that there is one inflammable matter and it bursts into flames when heated. This is an important and very interesting theory. Boyle refuted it. The theory may be stated thus: under any circumstances, if inflammable matter is heated to a given temperature, then it burns. Or, there are no circumstances under which sufficiently hot inflammable matter would not burn. One way of testing this theory would be to vary at least one of the circumstances under which we normally make fire. One of the circumstances accompanying the occurrence of fire was the presence of air. The vacuum was known before Boyle, but he was first to think of varying this factor, air, with respect to fire. This idea was not obvious at all. Varying this newly detected circumstance makes sense only under the suspicion that this move will refute the old theory (Popper 1935). How did Boyle come to suspect that the newly discovered factor is connected with the old theory of inflammability?

I do not know. Inductivists conceal the history of their discoveries. Let me offer a conjecture, then. The rise of water in pumps raised an old problem. The solution, *horror vacui*, met with its classical refutation by Galileo and by his follower Torricelli, who conjectured that the suction of the pump is the result of a push caused by the weight of the air, and that therefore it should be equal to the weight of the mercury-column that it is able to raise. To test this hypothesis Pascal showed that the height of the column falls when it is carried up to high mountains. Boyle built his air pump to test this view. He may have wanted to test the theory also by using another pump that was newly discovered—the heart. He asked an assistant to put his hand in the vessel gradually emptied of air. The result, the swelling of the assistant's blood vessels, is mentioned in Evelyn's diary. Next, I conjecture, Boyle concluded that as ordinary pumps cannot work *in vacuo*, therefore the heart too will stop under these conditions. Indeed, he put into the vessel a bird that owed its survival of the experiment to the tender heart of a lady spectator. The obvious conjecture is that the bird suffered not from heart-failure but from the lack of air. The rest of my story is from Boyle's introduction to his work *Flame and Air*.

Oldenburg had asked Boyle to continue experimenting on respiration and on the vacuum.[34] By the extant theory, the soul dwells in the heart, and is a "vital flame" (*Works*, 1999, 7, 79). "This among other considerations made" Boyle study "the relations betwixt flame and air." What are the "other considerations" is a mystery, except that they connect with theories of his friends, to which his experiments were relevant.

> Yet the nobleness of the Question now under debate, and their pertinency to it, will possibly keep them … from being useless.

What their theoretical significance is he preferred not to discuss:

> And that also they may be the better kept from being unwelcome, I have chosen to make my self a Relator of matters of fact, without ingaging with either of the Litigant Parties in a Controversie, wherein I am the less tempted to be partial, because I have not formerly declared my opinion about it, and at present, I see, in *either* side, Persons for whom I have no small respect and kindness.

Not wishing to partake in a controversy between his friends, he preferred to ignore it. Hence, he offered his general facts not as arguments and with no context (p. 83). He offered his conduct as an example: ignore theories and report facts historically and faithfully and cautiously; if you repeat the experiments you will discover for yourself their use as arguments.

Now to Boyle's experiment.

The first step is to vary the circumstances under which Boyle had conducted his earlier experiment (that show that fire in a vacuum is impossible). In varying degrees of vacuity, different inflammable materials are successively heated by various ingenious ways. In these experiments, in which all the other conditions were as favorable to the theory of inflammability as possible, one circumstance is varied: the presence of air. With air the attempt to make fire succeeds; without air it fails.

An argument in defense of the theory is that the vacuum prevents the creation of a flame, not a burning flame. Without mentioning this argument Boyle answers it: he introduces burning materials into the tube and pumps air out of it, thus snuffing out the fire. This variation fails to refute the general fact that flames cannot burn in vacuo, and it also removes a possible defense of the refuted theory. Another defense: the extinction of the flame may be due to smoke, for we do know that smoke does choke fire. Indeed, Boyle repeated the same experiment with alcohol, as it emits no visible smoke. It is impossible to appreciate the significance of this set of experiments without knowing their background as explained here. More background material is pertinent. Leonardo, the first to contemplate building submarines, held that fire and respiration consume air. Cornelis Drebbel, the early seventeenth-century creator of the first submarine, made a simple experiment to test this: a candle covered with a vessel turned on a surface of water is soon extinguished and the water surface inside the turned vessel rises. As late as 1646 Sir Thomas Browne said that all fire is extinguished by smoke. Boyle's refutation of this theory was therefore

[34] This is a red herring that again shows Boyle's eagerness to involve people in experiments.

an essential step in the history of the chemistry of the air. Today it is easy to refute Boyle by using the fact that it is the oxygen in the air that was active.

To return to Boyle's work, under-water flames refute the claim that fire requires air. Boyle tried to make flames under water, but failed to reproduce the experiments that "eminent Writers, both Ancient and Modern, tell us without scruples" (102). By many variations of circumstances he then found that certain flammable stuff kindled before it is thrown into water will continue to burn. Boyle now argues explicitly about this new discovery, but he did it in a roundabout manner (103):

> It is probable, that most men will conclude from this Experiment, that Air is not so absolutely necessary to the duration of Flame, as some other of our Tryals seem to argue; and that there ought to be a difference made between ordinary Flames, and those that burn with an extraordinary vehemency. But my design being … rather to relate Tryals than debate hypotheses, I shall only add, that it may be pretended on the behalf of the opinion, that this experiment seems to disprove, that, not to mention the Air that may lurk in the Pores of the Water, or that which may be intercepted between the grains of the Powder whereof the mixture consists …

This illustrates a brute fact: Boyle tried to avoid arguing but he failed. Trying to test the general fact that clashes with the theory, he found a seeming refutation of the general fact. He then suggested an *ad hoc* modification of the general fact—a distinction between ordinary and extraordinarily vehement flames. Also, he suggests an *ad hoc* hypothesis not as a correction of the general fact but in effort to bring the seeming refutation of the theory into a complete agreement with it. Both of these new hypotheses are testable and their tests might change their status. Boyle offered a hint about a possible way to such tests. This renders his puzzling conduct acceptable, especially since he often gave his readers half-digested experiments to pursue. Nevertheless, his presentation is here questionable, and his excuse for it—as due to his preference of experiments over hypotheses—rather lame. He knew this; returning to it later on he said (p. 113),

> The event of this Tryal seems to contradict the inference, that probably you have drawn from the foregoing experiments,

that flames cannot burn *in vacuo*,

> but yet it may not be unworthy of our inquiry, whether this way of Tryal be as proper to give satisfaction to the curious as …

To wit, beware of too much *ad hoc* tinkering.

The argument is carried out by varying circumstances, but not emphatically so and the reason for the choice of specific circumstances to vary is pronounced. On one occasion Boyle contrasted very sharply two experiments involving pulling a trigger, once with the gunpowder protected from air and once not, with all other circumstances kept the same in both experiments. It is easy to analyze the purpose and order of the experiments as parts of an ongoing debate, but the very need for such analysis makes reading of the text heavy going. Let me pick up only one point. Does not a refutation of the hypothesis that all flames expire *in vacuo* revive the hypothesis that flame a can burn *in vacuo* as in air? Evidently not: the two are not the only alternatives. Refuting the hypothesis that all flames expire *in vacuo*, leaves the hypothesis that all flames nourished by alcohol die *in vacuo*, which is still a refutation of the hypothesis with which he had started. Therefore, even the modification of the general fact after its refutation, the fact that is behind this modification is also inexplicable by the original hypothesis. Thus the original hypothesis gains nothing from the refutation of the general fact that refutes it. These two important points are Boyle's but his reluctance to write argumentatively prevented him from stating them sufficiently clearly.

With the development of science it became increasingly easy and trivial to devise and to perform possible refutations of known hypotheses. How difficult it was to refute the theory in Boyle's time is hard to imagine just because it is easy to do so nowadays. And this is so not only because of the development of technology. It is also because the new theories and their refutations provide many more ways to provide independent refutations of the theory of inflammability. Seeing the difficulties that Boyle surmounted prevents judging him inefficient. His task was truly heroic.

Our initial problem was, why is a hypothetical general fact preferable to the theory with which it conflicts? This is a serious problem since possibly the refutation of a general fact may revive the hypothesis that it refutes. Yet the refuted theory is not necessarily revived when its refutation is refuted. It is possible to defend a theory against a refuting fact by claiming that it is unrepeatable or that it is an error due to some unknown circumstances. This is why researchers try varying circumstances. Refuting a general fact this way only alters its circumstantial description, not its ability to refute. The alternative policy, the right to overlook refuting evidence is possibly at times right—this is why researchers subject them to tests—yet overlooking refuting evidence regularly will return the scientific tradition back to scholasticism. This then solves our initial problem—not by putting it to rest but by allowing it to drive research forward.

This shows that Boyle's problem has no clear-cut solution. This is why researchers habitually use some new theories to correct some old evidence. Already then there was such choice, even if less wide than today. In such cases, some researchers use one option and others use another and there is no *a priori* telling who is right. This observation is very much in line with Boyle's character. The secretary of the Royal Society Henry Oldenburg described him this way to Spinoza (Letter 11, 3, April, 1663, 785):

> Our Boyle belongs to the number of those who have not so much confidence in their reason as not to wish that the phenomena should agree with their reason. Moreover, he says, that there is a considerable difference between superficial experiments, where we do not know what Nature contributes and what other factors intervene, and those experiments in which it is established with certainty what are the factors concerned. …

14.17 The Expert and the Curious

> The whole of science is nothing more than a refinement of everyday thinking.
>
> (Einstein 1950, 59)

Historically, the inductivist movement comprised amateurs, and the method of induction was the method of circumstantial description that helped the movement in various respects. The method is not the best, but it worked. It worked mainly thanks to Boyle's sagacious simplification and even oversimplifications of the problems of the day. Inductivist descriptions were still too rambling, but he created standard modes of communication, of using ordinary language in a not too cumbersome a way of adding to the long description interesting remarks and observations.

With new problems, new challenges for new effort were necessary. No one took up the challenge. Sir Humphry Davy, the first holder of a professional researcher position in England other than that of the Astronomer Royal, initiated a more economical, professional style. He gave a professionalist argument against the method of circumstantial description (*Phil. Trans.*, 1821, 3, 48):

> To describe more minutely all the precautions observed, would be tedious to those persons who are accustomed to experiments with the voltaic apparatus, and unintelligible to others; and after all, in researches of this nature, it is impossible to gain more than approximation to true results …

This argument is very popular today: to understand a researcher you must undergo extensive training, and researchers addressing the ordinary public will not be able to express themselves fully. This argument is only an attempt to transform a methodological problem into a technical one. It is an error, since the problem concerns not the way of describing a fact, but its role in a dispute. What is required is the explicit, clear statement of the problem involved, an outline of its solution under discussion sufficiently detailed to see its relation to the factual description offered and the role or the purpose that this description serves; it becomes then relatively easy to describe the circumstances within which the described fact take place. This is not the received view. The received view is that you have to be an expert in order to see why in one experiment the law is more evident than in another; you must be a skilled and gifted experimenter to know how to present the circumstances that you intuitively selected under the guidance of a long scientific training.

The most explicit expression of this view is probably due to Michael Polanyi. His *Logic of Liberty* argues against the authority of the state. In the chapter "The Scientific Community" there he asserted that the state's authority over science will stifle it. He said (Polanyi 1951, 57), scientific research

> is the art of making certain kind of discoveries. The scientific profession as a whole has the function of cultivating that art and of transmitting and developing the tradition of its practice …
>
> Professional scientists form a very small minority in the community … Clearly science can continue to exist on a modern scale so long as the authority it claims is accepted by a large group of the public. …

Polanyi's description of scientific activity contains much truth, and it constitutes an argument against the present state of science: it comprises a challenge to anyone who values science to try to change the tradition by rational means. The task is to change what Polanyi has described. We have to decide what is true and what is false in his description. Science is becoming increasingly authoritative; there is a tendency to render it both a technique and a provider of beliefs to feed the public. Polanyi totally ignored the amateur tradition of science. Here is a passage from the Preface to Boyle's *Sceptical Chymist*, whose message both ousted the profession of alchemy wherever it arrived and introduced a new movement of amateur chemists (*Works*, 1999, 2, 211), where Boyle (named here Carneades, after the Cyrenean academic skeptic)

> hopes, … that by having … drawn the Chymists Doctrine out of their Dark and Smokie Laboratories, and both brought it into the open light, and shewn the weakness of their Proofs, that have hitherto been wont to be brought for it, either Judicious Men shall henceforth be allowed calmly and after due information to disbelieve it, or those abler Chymists, that are zealous for the reputation of it, will be oblig'd to speak plainer than hitherto has been done, and maintain it by better Experiments and Arguments than Those *Carneades* hath examin'd: so That he hopes the Curious will one Way or other Derive either satisfaction or instruction from his endeavours. And as he is ready to make good the profession he makes in the close of his Discourse, he being ready to be better inform'd, so he expects either to be indeed inform'd, or to be let alone. For Though, if any Truly knowing chymists shall Think fit in a civil and rational way to shew him any truth touching the matter in Dispute, That he yet discerns not, *Carneades* will not refuse either to admit, or to own a conviction …

Boyle emphatically contrasted the alchemist whose secrets he exploded with the curious who was both a rationalist and independent. Philosophy, Boyle said (in his introduction to *Spring of the Air*), "demands a purse as well as a brain." "It must be acknowledged", repeated Priestley over a century later (Priestley 1781, xii), "that in these studies mere genius can do nothing without the aid of wealth". This raises a problem. Our purses are now fuller than in Boyle's time. Facilities and leisure are incomparably larger than they were then. Why are there no private laboratories nowadays? Why do people today rely exclusively on professionals? Is it because the tradition of professional science ousted the amateur tradition? Indeed, the only argument by that Polanyi could employ in effort to answer this must be from tradition. And it is self-defeating, since regrettably it leads to the end of the tradition of plain speaking that Boyle valued: it requires radical change.

The liberal tradition of the commonwealth of science does not fit comfortably into Polanyi's description. Perhaps the clearest expression of this was written in 1661 by Dr. William Croon or Croone, F. R. S., a Gresham College professor, to Dr. Henry Power of Halifax, in efforts to recruit him to that Royal Society (Mulligan and Mulligan 1981, 330):

> … this Company does not take upon it selfe to assert any one Hypothesis but every man is left at present to his Freedom; for they believe that to make any Hypothesis, and publickly owne it, must bee after the triall of so many exp'iments as cannot be made but in a long tract of time.

The same argument by which Polanyi claimed that beliefs that science prescribes are at bottom matters of faith, Boyle used and Croone repeated (slightly less carefully, as he said it in a private latter), to explain why science does not prescribe beliefs—or rather it should not. Boyle and Polanyi shared the view that induction does not impose belief; it can then rest only on authority. Polanyi contrasted only two authorities, of the state and of science. In Polanyi's view science claims authority and required submission. Boyle opposed all authority in science. Here is a part of his introduction to his *Sceptical Chymist* (*Works*, 1999, 2, 209).

> … I am troubled, I must complain, that even Eminent Writers, both Physicians and Philosophers … have of late suffer'd themselves to be so far impos'd upon, as to Publish and Build [theories] upon Chymical Experiments, which questionless they never try'd … . And indeed it were to be wish'd, that now, that those begin to quote Chymical Experiments … for each Experiment they alledge, name the Author or Authors upon whose credit they

> relate it; For by this means they would secure themselves from the suspition of falsehood … and they would Leave the Reader to Judge of what is fit for him to Believe of what is Deliver'd, whilst they employ not their own great names to Countenance doubtful Relations; and they will also do Justice to Inventors or Publishers of the true Experiments … .

This is Boyle's proposal for the introduction of a custom that is today taken for granted, after his having criticized the received custom. His aim was to increase the critical aspect of empirical learning, not its authority. He advocated his proposal saying it leaves the beliefs of readers to their own judgments.

During the crisis in physics, as inductivism became less dominant, confusion spread, and with it professionalism developed as well as contempt for amateurism—even for popular science.[35]

Inductivism presents a theory as if it rests on experiments that anyone can perform. This is not the case. It takes great efforts and ingenuity to simplify an experiment to render it repeatable by the inexpert, and too few take the trouble. (Science correspondents are at times terrific, but their task is very limited.) Inductivism was critical and anti-professional, because it emphasized facts and simple experiments. But due to its empiricist *naiveté* it was soon discredited. Theories are always abstract. It takes ingenuity to "reduce them to facts" as Faraday put it, to devise simple and diverse tests. Modern scientists are less critical than classical inductivists because they are satisfied with tests that are not very severe and simple: professionalism rests on the decline of scientific standards. Professional science is a part of modern life and is not subject to discussion here; what is under discussion here is professionalism and its possible dangers; as the idea that professional researchers should have narrow, specialized education, it is difficult to commend except as education : it is better narrow than absent.[36] Regrettable though narrow professional specialization is, it is here to stay. Contrary to Boyle's fear that professional research will create excessive bias,

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[35] H. Graham Cannon noted this in a wise letter to the editor of the *Manchester Guardian*, of 16.12.1955:

"As specialization becomes more and more pronounced, so the possible audience capable of grasping the problem concerned becomes smaller and smaller. The crash will come when scientists as a whole find that they are quite unable to grasp the developments in their ancillary subjects."

"I consider strongly that the fault lies in our own university departments of science. We turn out honours graduates—too few it is stated—but I maintain it is too many who are already specialists on graduation and may even have been specialists right through their school days. It is the placing of research before anything else that is the cause of the rot. How often in making a university appointment in the faculty of science is the question asked—can this man teach? Practically never. It is always—what is his specialist branch of research? Teaching ability is the last thing to be considered, and this is possible simply because the only interest in teaching is taken[to be?] the curriculum (if I can be called such) of the special honours degree. The general honours degree, that is, the degree in science covering a wide field of interests, rather than specialization in one particular science, is treated with scorn and contempt by the majority of science teachers. Until this misguided attitude is abandoned, there is little hope of producing research workers whose knowledge of science as a whole is wide enough to enable them to express the significance of their results in any except the ghastly jargon of the specialist scientist."

[36] Aldous Huxley described narrowly educated researchers in his novels, for example in his 1939 *After Many a Summer Dies the Swan*. He contrasted there the broad and narrow education of old-style and new-style researchers. Some critics read the novel as lamenting the absence of culture in the USA. They thus misread even its story line.

we may institute diverse means for overcoming bias of any sort on a regular basis. Prior to that, however, is the need is to communicate all achievements—first to the people whose interest in them is greatest and then in steps to the public at large. There are some obstacles to this and we have to tackle them before we plan a proper system of spreading knowledge. One of them is the new version of inductivism that is still defensive of science. Another is the taboo on discussing scientific problems openly prior to their solutions. Still another is the fear of the authority of the professional researcher. Remembering the authoritarianism and obscurantism of Boyle's times, and the problems that he struggled with and magnificently overcame, we cannot but admire him and his rules for the Royal Society. Yet these are long outdated. The new authority of science must be checked and new needs taken care of.

Boyle prescribed research programs for generations to come. He was tremendously successful, but not always. Here is one obvious example of a failure of his (*Works*, 1999, 6, 376–7):

> I had observ'd, not without some wonder, in the Inquisition into the Nature of the Air, that they have not, that I know of, so much as attempted to discover, Whether the Air either in the utmost or in the intermediate degrees we can bring it to, does retain a constant and durable Elasticity?

And more significantly (381),

> I cannot but little wonder, that among so many, that have to observe the nature of Cold, and the Condensation of the Air by it, I have not yet met with any that have had the curiosity to measure that condensation…

This last experiment that he failed to challenge his peers to make, has all the possible merits. It is unproblematic, as it is a variant of what he taught them to do, and it is very promising, as it is a generalization of his celebrated law and as he has reported that cooling air raises its density a bit. It is also very intriguing, as he said, because it raises a question that should throw light on the theory of heat: why does the cooling of air raise its density? The person who performed this experiment was John Dalton. Why? Because he had a problem and a solution that he wanted tested. His problem concerned not heat but diffusion: the discovery that air comprises mostly nitrogen and oxygen raised the question, why are these elastic fluids not separated like water and oil? Whatever Boyle's theory of heat and cold was, he did not raise curiosity the way Dalton did.

Boyle could do it too, but its description would be argumentative and so he preferred to skip it. All he said was that the theory of cold current then was "imperfect". This may mean that the theory failed to answer some pertinent questions or that it provided answers that were known to be false. This episode still awaits reconstruction.

14.18 Conclusion

Boyle's inductive style has its merits—as a mode of civilized argument that leaves readers to judge for themselves—as well as its drawbacks—prolix circumstantial descriptions and lack of background information, especially lack of mention of the problem under discussion. The greatest drawback of the style is extra-stylistic: its similarity to Bacon's style of the just history of nature. The main lesson to draw from the inductive style is that we do have standards or traditions of clarity of exposition and that we should assess them critically. Boyle consciously designed a style as a way of maintaining critical standards. The style was not subject to these critical standards. It was taken for granted that Bacon's inductivism is the sole justification of the inductive style, and that inductivism and even its style are responsible for Newton's stupendous success. This is a strong recommendation, of course, to emulate Newton's style. As the basis of empirical science, inductivism was considered beyond criticism. The problem of induction, as it is called, was hardly discussed. Until the crisis in physics, even the people who felt most the need to solve the problem of induction never thought conceivable that the theory of induction is false, and that science does not progress by the application of the principle of induction.

Inductivism was a myth and then the inductive style was a taboo. The taboo was broken. Even believers no longer adhere to it. But other taboos were instituted in the tradition of science instead. Tradition, being of our own making, is open to critical examination and possible revision—although not with a complete freedom. Like Boyle, we may discuss the advantages and disadvantages of the existing traditions and try to suggested changes. The aim of criticizing and changing the tradition should be to increase smoothness of communication and to improve critical standards.

Chapter 15
Mechanism

Mechanism is an ancient metaphysical doctrine modernized. The main problem it was meant to solve is that of change.[1] This is how Descartes understood and applied it, and this is what made Boyle advocate it. Mechanism comes in diverse versions, and it is not clear whether the systems of Newton and of Leibniz qualify as variants of mechanism or are too remote from the original for that. At times a few theories appear that are fairly similar to each other, yet they belong to different groupings or families of theories and it is not always clear where to draw the line between families of theories: it is often difficult to judge when two similar theories are variants of each other and when they belong to different families of theories. Thus, some consider the theories of Newton and of Leibniz mechanical and others deem them too different from Descartes' theory to count as mechanical[2] and they call Newton's and Leibniz' theories variants of dynamism (Čapek 1961, 96). As long as there is no measure of proximity of ideas, any answer to this question may count as reasonable. Much later, Faraday, Maxwell, and Einstein suggested a new metaphysical system, according to which atoms are not material but characteristics of fields of force. This surely is dynamism proper.

The two initial and central versions of mechanism are the atomist and the continuist theories, and the atomistic one offers an idea of change that is easier to comprehend as it takes all change to be rearrangement of atoms. Descartes was a continuist and Gassendi was an atomist. Boyle followed the latter. He labeled his theory the mechanical or the corpuscular philosophy. The scientific import of metaphysical systems is that they purport to be research programs. (This is not contested; Bacon's negative opinion

[1] Marie Boas Hall rightly found here a problem (1952): it is as theoretical as they come, yet it was viewed as factual. She explains—rightly—that this was the anti-scholastic aspect of mechanism. We must add a no less important aspect of it, namely, anti-occultism. Newton's opposition to all assumptions of occult qualities blurs the difference between the Aristotelian and the Neo-Platonist traditions between which Harry A. Wolfson was at pains to distinguish. Galileo declared his aim to be the denuding of the latter tradition of its murkiness, i. e., its magic (opening of his first dialogue).

[2] Leibniz objected to Newton's system because it is not mechanical. Leading historian of science Dijksterhuis declared it mechanical, refusing to discuss what the mechanical world picture is (Dijksterhuis 1969, 4, note 2).

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_15,

of it is dominant but not unanimous.) Being speculative, metaphysical systems are inherently unfounded and in need of critical assessments. The central traditional problem of the philosophy of science is whether science begins with assured facts and proceeds inductively or with demonstrated metaphysics and then proceeds deductively. Both answers are false: science begins with wild speculations: scientific research operates with both theory and facts; it pitches them against each other dialectically. Scientific theory then may but need not conform to any metaphysical worldview.

15.1 Boyle's Program

Following Descartes and Gassendi, Boyle endorsed both mechanism and atomism, or the atomistic or corpuscular version of mechanism. He tried to apply his philosophy to all the phenomena that he could observe. He discussed this program widely in a number of his works. The most famous of these is his "Excellency and Grounds of the Corpuscular or Mechanical Philosophy", appended to his *Excellency of Theology* (*Works*, 2000, 8, 103–16).

This text begins with a contrast between the mechanical and the Aristotelian views. Boyle sought a comprehensible theory, one from which empirical accounts follow, that is to say, one that has explanatory power. He said repeatedly that a comprehensible theory offers intellectual gratification. He endorsed Descartes' ideal of explaining the unknown by the known, the familiar and the unfamiliar by the intelligible. In particular, he recommended trying to avoid *ad hoc* hypotheses.

Although Boyle was aware of the paucity of empirical conclusions from his theory to facts, he pointed out that past success allows for hope for further future success. This is no inductive generalization from the past to the future. It is hope, a challenge to continue this way and see what happens. Boyle concluded this discussion by expressing the wish to be fair to his opponents. Following it he admitted the reasonableness of their rejection of his corpuscular philosophy as insufficient. He took this as a challenge that he partly met by arguing that his principles are the fewest, most primitive, simplest, and most comprehensive. Thus, success in applying them is the same as the discovery of non- *ad hoc* empirical explanations. He tried to show, then, how these few simple principles suffice for research and how they permit a picture of a complicated and varied world, just as a few letters enable us to write many words (to use Aristotle's simile). Assumptions on matter and on motion should (hopefully) suffice for explaining the variety of observed facts.

This may look to us trivial, as we are so used to Newton's mechanics.[3] This is regrettable hindsight. The problem of the mechanics of rigid bodies was alive then; Newton solved it and thus justified the standard treatment of real bodies as

[3] Milton (1981, 175) declared that the ancients had no concept of a law of nature, so that Popper was in error translating "aitiologia" to mean causal law. Milton viewed Newton's idea of laws of motion as a neologism. To the extent that this is right, obviously, Boyle's discussion of the role of the laws of motion was far from trivial to his readers. This Milton also noticed (182, 195).

ideal point-masses. Even this solution justified only his own treatment. Euler closed the discussion satisfactorily decades later. Newtonian mechanics has corollaries that were vital clues for him in his construction of his system.[4] But Boyle raised here another problem: what is the methodological status of the hypothesis that matter is atomic? He tried to see his program as the program of geometrizing natural philosophy.[5] He was much concerned about the fact that he had deviated from Descartes' geometrical program that he had introduced indivisibility into a geometrical system of infinite divisibility, and he often declared that this was the major problem in his system. Newton admitted that atomism is an additional assumption. Leibniz imposed this admission on him. Leibniz claimed his system had no need for this addition as it was the assumption that atoms are monads (the Greek word for points): a point is an ultimate geometrically given. But then, to explain the fact that atoms fill space Leibniz had to make an additional assumption, the assumption of repulsive forces. It might seem that Leibniz had to make more assumptions than Boyle did, but perhaps this is not the case: it is hard to judge these matters.[6]

Descartes had assumed only motion (the laws of motion). Matter he identified with space (this is his plenism), and space was given[7]; Boyle, a vacuist (anti-plenist), assumed both matter and motion; Leibniz assumed both force and motion. Leibniz then had to show that his principles too allow the observed variety. He could not, and he developed the strange idea that every atom includes an image of the whole universe, and they seem to interact out of some pre-established coordination. I need not go further into all this. I mention it in order to contrast views in order to present later on Newton's response to Boyle.

Boyle deemed the principles of matter and its motion unproblematic. These two are essential, since, as the ancients have shown, the explanation of variety needs at least two principles; these should be the most basic possible. To these Boyle added atomicity as the easiest way to explain diversity. He showed less concern for the legitimacy of his atomism than for the need to unite the mechanistic sects. He suggested that this is possible due to (a) his (methodological) tolerance for non-mechanical hypotheses, (b) his program to construct mechanical models to explain these, and (c) his arguments for the legitimacy of proposed explanations that embrace the Cartesian assumption that matter is "infinitely divisible".[8]

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[4] Boyle's influence on Newton occupies an extensive literature. The relevance of Boyle to it is still unstudied. For more details see Ben-Chaim (2001) and Wojcik (2000).

[5] A superficial examination of the remnants of Boyle's correspondence and some of his work would undermine the currently popular view that he was mathematically illiterate. See also (Anstey 2000 Introduction).

[6] All this is too fuzzy; since Leibniz's atoms are points, abstract set theory plays havoc with it.

[7] Descartes implied that geometry is reduced to arithmetic and arithmetic to logic, so that it is given for free.

[8] The most forceful discussion of this is in Boyle's *Defence* and in his *Things above Reason*. One may suggest that his discussion of continuity shows him uncertain about his atomism. Not so. Of course, he was skeptical about all of his principles, but he had no second thoughts on atoms; he challenged the Cartesians to use their principle of continuity instead of arguing that it is possibly true. Which move is *a priori* valid, of course.

The rest of "The Excellency of the Mechanical Hypothesis" is an attempt to discuss the possible interpretations of the chemistry known in Boyle's days. This is a major portion of the essay. Boyle argued first that the then accepted chemical interpretation is inferior to mechanistic one, as it is not sufficiently wide. He then showed that the mechanical hypothesis is insufficient (p. 109) and considered what is missing: explanations. The few chemical explanations extant do not abide by the mechanical hypothesis as yet. He tried to envisage what sort of explanations he expected to fill the gap between his principles and his chemical theories. In particular, he emphasized, we may assume as many chemical elements as we wish, since their diversity still awaits a mechanical explanation. This idea runs contrary to the programs that dominated chemical researches then, beginning with the system of Paracelsus that assumed only three elements. This is not to say that Boyle was too permissive. In particular, he objected to the many spiritualistic hypotheses extant—as *ad hoc*.

The main point is to try to benefit from atomism but to be ready to violate it. Since it is general and fertile, however, it is hopefully reconcilable with any discovery. It is also useful (namely, non-*ad hoc*) and intelligible (namely, non-*ad hoc*), so that there will be no need for more "catholick principles" (universal principles). Boyle was nevertheless ready to abandon this philosophy, except that he viewed it as the foundation of geometry and geometry is vital for research: he viewed the mechanical theory as hardly more than a mathematical outlook developed so as to be able to solve the problem of diversity and change. Boyle interpreted Bacon's ladder of axioms as a hierarchy of hypotheses. Into the idea of a hierarchy he also read permission to have hypotheses in the hope of reducing them later to the mechanical principles. The initial acceptability of a specific hypothesis then is conditioned on its ability to solve a problem without being refuted. The mechanical program then is the task to try to apply this mechanical philosophy to ever more phenomena but not to use it as a proscription on good (non-*ad hoc*) hypotheses.

Here is Boyle's summary of this work (115–16; also his *Forms and Qualifies*, in his *Works*, 1999, 3):

1. The Principles of matter and motion are the fewest and most primary.
2. Matter comprises particles of differing sizes and shapes in different motions; these should suffice to explain the rich variety observed.
3. The small variety of atoms and their motions should explain the great variety of chemicals and of our sensations of them.
4. These principles, matter, motion, size, shape, posture (the position of a particle relative to other particles), order, and texture, being simple, clear, and comprehensive, are applicable to all the phenomena. Assuming immaterial principle or agent may be not intelligible and have no explanatory power, because explaining how spiritual things may work on material things is more difficult than a mechanical account of the phenomena. We cannot imagine how spiritual principles produce changes in material things without the help of mechanical principles. Thus, the human soul is able to produce changes in the body only within what mechanics permits.
5. And physical things too operate on physical things in accord with the same mechanical principles; an alternative to the mechanical principle should be at

> least as universal and as simple. The number of hypotheses it employs should be small and as versatile. Thus, the fear that a new physical hypothesis will overthrow or make useless the Mechanical Principles is unreasonable. It is like fear that there will be a language for which the letters of the alphabet are insufficient.[9]

Boyle advocated principles—matter and its general properties (being universal and indivisible) as well as motion and its general properties (action-by-contact as the cause of deviations form inertia). These furnished him with a picture of the world, with a worldview. But there are too few explanations. For centuries, thinkers repeatedly alleged that their principles were established. This allegation would mean that the principles explain all known phenomena, which is false. At best, principles interpret all known phenomena; they do not explain them. Explanation is much superior to interpretation. All previous worldviews were unsatisfactory. To be satisfactory an interpretation should be wide and involve minimum assumptions. A good interpretation is also a program, a program for building mechanical models to explain the variety of chemical elements, etc. The principles he advocated go back to antiquity and are common to a few great late Renaissance thinkers, especially Galileo and Descartes. The contribution of Boyle is that for the first time he tried to apply them to chemistry—both as an interpretation and as a program. This, he said, is his claim to fame. To this I would add his idea of methodological tolerance.

15.2 Newton's Program

Here is Newton's presentation of his program in the Preface to his *Principia*:

> Our design, not respecting arts, but philosophy, and our subject not manual but natural powers, we consider briefly those things which relate to gravity, levity, elastic force, the resistance of fluids, and the like forces, whether attractive or impulsive … [Having explained gravity, celestial motions and tides in this way,] I wish we could derive the rest of the phenomena of nature by the same kind of reasoning from mechanical principles [!]; for I am induced by many reasons to suspect that they may all depend upon certain forces by which the particles of bodies … are either mutually impelled towards each other … or are repelled … from one another.

[9] This is a good example of a theory that is so commonsense that it is unnoticed or taken as facts. Today the supposition behind the Hebrew alphabet and its derivatives and its shortcoming are all too obvious. It does not even take account of the diversity of dialects and of pronunciations—especially by non-native speakers. The discarded idea is that all sounds are either consonants (stops, voiced and unvoiced) or vowels, and that it is possible to enrich the alphabet by adding to its list of consonants and vowels—with diphthongs as pairs of vowels. Today we view some sounds as gliding, as between consonants and vowels, so that not all diphthongs are combined vowels. The difference between consonants is insufficient as it does not distinguish between the explosive and the implosive (as in d and dt) and that it does not allow for intonations and for the tonality that is so characteristic of all Chinese dialects. And it is debatable whether the southern African clicks are consonants.

Newton's methodological remarks are notoriously laconic; they require careful examination both because of their high interest and because of their great, enduring (and inductively hardly permissible) influence. Newton advocated the inductive method, "the same kind of reasoning" as the one that he had applied. In his view all physical phenomena are explicable by assuming only central forces (equal and opposite forces of mutual attraction or repulsion acting at a distance). He presented his program as a program. He considered his view true and he wishes others would try to apply his program. He hardly argued for it. He did his best to conceal its revolutionary character, and he presented it as subsumed under Boyle's methodology.[10]

In the quotation above we have met the expression "mechanical principles". Newton gave it a new sense. This is admissible: everybody may use any term in any sense. But it does not add to clarity to change a term tacitly. Since the publication of *Principia* it was unclear whether Newton viewed force as a new item to add to the mechanical principles, or whether he introduced it *pro tem* in the hope to explain forces mechanically. It seems he was undecided about it.

15.3 Newton's Theory of Force

> All these philosophers, our predecessors, discouraging of attraction on the basis of a few vague and indecisive experiments and of reasoning from recondite causes of things; … are world-wide astray from the truth and are blindly wandering.
>
> (Gilbert 1958, 11)

Why did Newton offer his first law of motion, his law of inertia? In the standard way in which it is misconstrued (Harman 1985, 122, 129, Schmiechen 2009, 592; Bechler 1992, 287) it is redundant, as it follows from the second, which states that the acceleration of a body is proportional to the force acting upon it. For, if the force acting on a body is nil, so is its acceleration; which is the first law of motion. He must have had good reasons for having worded it separately. Now, some nineteenth century interpreters of Newton's mechanics, especially the Cartesians among them, considered his second law of motion—that the force acting on a body is equal to the change of its quantity of motion—a verbal definition of "force". They took it to say, the word "force" is shorthand for "the product of mass and acceleration". They took for granted the reality of inertia but denied the reality of forces.[11] They had to explain the third law of motion. Moreover, taking the second law as a definition requires a rewording of Newton's first law. The new interpretation of the second law renders

[10] Thus, Newton presented the calculus as a part of traditional geometry and covered his tracks, as Laplace noted.

[11] Not Ernst Mach. He saw that the abolition of forces requires a reinterpretation of the first law. So he declared it a rule of coordinating motions with the system of the fixed stars, whatever this is. This demolishes all statics; it also leaves the third law still unchanged and so quite problematic for him.

the first law to mean this: whenever a body's quantity of motion is constant its velocity is constant. This is definitely not a law of motion (but of the constancy of mass), much less the law of inertia. No doubt, the wording would require a possible and a better modification; this suffices to show the difference between Newton and Descartes on inertia.

Obviously, in the spirit of Descartes Newton worded his first law independently of whether the second law is true or not: unlike force, inertia is an essential property of matter.[12] This is confusing, since the law of inertia of Newton is not the same as that of Descartes or Galileo: in their systems inertia holds for a body unless something bumps into it; in Newton's system it holds unless a force operates on it; in statics, for example, the body in question is at rest with nothing bumping into it but with forces operating on it that should cancel each other.[13]

Inductivists often declare that the law of inertia is unproblematic. This way they obscured the difference between the Descartes' and Newton's views on inertia, especially since already Newton identified them. Newton viewed inertia as essential (*Principia*, Definition 3) and deviation from it as external[14]—as due not to another body as Descartes had suggested, but to a force, since Newton's inertia depends on the absence of outside causes, it is not essential. He said it is:

> The *vis insita*, or innate force of matter, is a power of resisting by which every body, as much as in it lies, endeavours to persevere in its present state whether it be of rest or of moving uniformly forward in a right line. This force is ever proportional to the body whose force it is; and differs nothing from the inactivity of the mass, but in our *manner of conceiving it*.

The law of inertia then is a part of the definition of the inertia that is not passive but active—a force. (The text does not even allude to the theory of collision.) Newton never tried to offer a theory of that force.[15]

The above quoted passage from Newton's preface presents as a program a general theory of force—as central—in the wish to enable others to explain elasticity (in line with his own explanation of Boyle's law). He allowed any kind of central force.

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[12] In his famous letters to Bentley, Newton endorsed Descartes' or Boyle's program. His wording of his first law makes it a hybrid. Alexandre Koyré's view (Koyré 1965, 66) that he was in debt to Descartes for all of his laws of motion wants a reassessment.

[13] This is contrary to Koyré's, "… the *principle of inertia* … holds a special place in classical mechanics. It is its fundamental law of motion; it implicitly pervades Galilean physics and quite explicitly that of Descartes and of Newton" (*loc. cit.*). It is also possibly contrary to Feynman's "The First Law was a mere restatement of the Galilean principle of inertia" (Feynman 1963, 1, 9/1).

[14] Duhem and Poincaré discussed at length the absence of inertial motion. This never brought anybody to think of the difference between the inertia in the systems of Descartes and of Newton. Planck hinted at this in his discussion of the law of energy that never holds strictly due to the non-existence of strictly closed systems.

[15] What makes this easy to discern is Einstein's theory of gravity that abides by Newton's first law in a generalized version (as straight lines become geodesics) and absorbs forces into the geometry, so that the first law stands alone. Einstein went further than that: he viewed invariance to some sets of transformation as a key characteristic of any good theory, and he took Newton's first law to be the corollary from Newton's theories being invariant to Galileo transformations (Einstein 1921, Chapter 1).

He did not demand even that mass should stay constant, let alone taking the equality of inertial and heavy mass as a model to imitate. The introduction to *Principia* appears to put one limitation: we should introduce only central forces: his program requires abiding by his laws of motion.[16] Newton's first law of motion then opens a world of possibilities before his program closes them as it requires limiting research to central forces. Newton's general theory of force that leaves room for non-central forces perhaps hardly drew any attention before 1820. Then Ørsted, Wollaston and Faraday contemplated rotary forces. Thus, although they were directly assisted (consciously or not) by Newton's general theory of force, they were violating (consciously or not) his program.

Newton introduced his theory, or so it looks, in a manner that makes it look as Boylean as possible. He had a great talent for presenting almost any idea he had in almost any form he wished; feeling the need to legitimate his ideas, he used this talent in a dazzling manner. In time disciples took notice only of his inductivist legitimation of his views, allowing other legitimation to sink into oblivion. This requires a historical reconstruction.

15.4 Newton's Attitude Towards Bacon and Boyle

Burtt was the first to analyze Newton's methodology carefully. He noted that the greatness of Newton as a scientist made very significant anything that this remarkable genius published as he became "an authority paralleled only by Aristotle to an age characterized through and through by rebellion against authority" (Burtt 2003, 203). He found Newton's metaphysics disappointing. Newton's methodology and philosophy he found eclectic and unclear. His judgment is clear (208–9): in scientific discovery and formulation Newton was a marvelous genius; as a philosopher he was uncritical, sketchy, inconsistent, even second rate. "His paragraphs on method are, however, superior to his other metaphysical pronouncements" although he "never rose, in his conception of method, to any higher degree of generality than relevant in his own practice—it is always *his* method that he is talking about. This is, perhaps, to be expected, though it is somewhat disappointing philosophically."

These lines of thought display an admirable high individualist spirit. It the boldness of a humble philosopher who dares to pronounce an unfavorable view on the scientist who was the greatest and most admired of all times—in efforts to understand his activities and inclinations. This stands out especially when compared to run-of-the-mill histories of science. Nevertheless, Burtt was in error and even (unintentionally) unfair to Newton. Here is my alternative view of the matter.

Newton's metaphysics and methodology served one purpose: to defend his scientific activity before the court of public opinion. He wanted his theory to say something about the world and to have it stood up to tests. He discussed methodology

[16] Electromagnetic forces, it is well-known, abide by these only globally not locally.

only by referring to other people's views and only in to order to show that he was conforming to their demands, thus gaining the coveted legitimacy of his work. This looks like a rather uncritical endorsement of any methodology that he was discussing. He thus endorsed Bacon's doctrine of prejudice and Boyle's metaphysics and methodology, obviously inconsistently. He was not interested in methodology, but in legitimizing his science. That he was on the defense is almost incredible, Augustus De Morgan attempted to explain this psychologically by reference to Newton's deep fear of controversy and his inferiority feelings due to low birth. This explanation is insufficient, as Newton could and often did overcome limitations.

The function of each of Newton's methodological remarks is usually the lack of readiness to take responsibility for what he said. This holds for most of his arguments—especially against Leibniz (quoted above in the discussion of Bacon's influence) and his famous letters to Hooke[17] and the most famous of his texts, the *Scholium Generale* of his *Principia*. I do not commit myself to my theories and therefore I have nothing to defend; besides, insofar as I do hold some theories, they are not hypotheses, and therefore need no defense, or, insofar as they do need defense, they were already defended; besides, insofar as my theories are hypotheses, they are only a part in the hierarchy of theories, and therefore will later be justified by further scientific development; so let us not argue, but wait and see.

This attitude is fairly rational in that it suggests the idea of presenting facts, theories, and any considerations and leaving it for readers to judge for themselves. This is what Boyle demanded; this is what Newton told his friend Bentley he was doing, and this is the way in which Faraday tried to imitate Boyle and Newton. But whereas Boyle and Faraday did accept the challenge to return to the argument when they had something new to say, Newton made it a method of arguing about his reluctance to argue. His inconsistencies are of the type that a lawyer may produce in order to show (not that the charge is false but) that the charge does not arise and invites no defense. That Newton need not have defended his theories is obvious. It is a pity that he preferred to argue about arguments instead of about theories, though it was his right to do so, of course, and there was a precedent of sorts for this mode of conduct even in Boyle's works.

Boyle has made legitimate assumptions about forces to explain mechanically later. But, right or wrong, his major point was that the mechanical framework is no excuse for ignoring a hypothesis that does not fit it; rather it should serve as a challenge for adherents to that framework. Newton argued that one should try to explain everything by assuming forces and trying to explain forces mechanically (in Boyle's sense of the word) only afterwards. But if we have already explained everything in terms of forces, a subsequent mechanical explanation of forces will not be genuinely scientific, since it will not give rise to any predictions independent of the existing non-mechanical theory of forces, so that there will be no independent

[17] "I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses; for whatever is not deduced from the phenomena is to be called a hypothesis, and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy" (Koyré 1952).

test for it. Such a mechanical explanation would only be a metaphysical interpretation of a non-mechanical theory. Thus, Boyle's mechanistic program was like a troublesome politician elevated to the House of Lords where there was no way to cause too much trouble. It ceases then being a program for scientific theorizing. This great change of status hid behind Newton's great play with Boyle's theory of the hierarchy of theories.

Boyle's theory of hierarchy is simple and logical; one can illustrate it by examples. For instance, Einstein's special theory of relativity implies Maxwell's electromagnetic theory, though in a new interpretation. Maxwell's theory includes Faraday's theory under certain conditions (of stationary currents) or as a first approximation (quasi-stationary currents). Faraday's theory includes Poisson's magnetic theory in a modified form (the elementary magnetic dipole or "Magnetic molecule" is smaller and is a current in Ampère's fashion). Poisson's magnetic theory includes the theory of magneto-static induction, which includes the general fact that the induction is given to saturation. All this is in perfect agreement with Boyle's theory that asserts that assuming the truth of a principle, the intermediate hypotheses, insofar as they are also true, should follow from the principle.

The above picture is very different to Newton's semi-Baconian theory of hierarchy. Bacon's theory of the hierarchy of theories, his *Scala Intelectus*, is only Aristotle's theory of classification plus the new maxim not to jump a rung in the "true ladder of axioms", to arrive by induction at a series of statements. This stuff fills even the wonderful works of the great methodologist William Whewell, just because it is the element of Bacon's methodology that Newton had endorsed and discussed.

Bacon demanded, we remember, the recording of observed facts. Boyle demanded, we remember, the recording only of repeatable facts, and as generalizations, but he demanded that the recording itself be historical, in a Baconian fashion. Newton used these two similarities between Bacon and Boyle to create a methodology that no one ever employed, but he pretended to have employed it.

Newton used the word "induction" to designate generalization of singular statements of fact. His theory of hierarchy refers to a partly ordered set of general statements of fact where (a) the set of objects (or experimental set-ups) referred to in an earlier statement should be a (proper) sub-set of the objects (or experimental set-ups) referred to in a later statement; (b) each statement is either deducible from the one which comes next, or (c) it is in agreement with Newton's program (or preferably with Boyle's program). This sounds ideally both Baconian and Boylean. As Newton stated, the statements of observation are certain and so are the deductions; the only uncertainty is involved in the generalization. No hypothesis is involved. These are doubtful, he admitted, but minimally so.

The greatest departure from Boyle's view, however, is this. Boyle, with his usual care, always stated that if a hypothesis is true, it should be deducible from the principles if they are true. This is far from obvious, as it relates to the very definition of the concept of "principle". Newton went much further than Boyle did. He turned the conditional into a categorical; he demanded a true hierarchy of theories. This, let me now argue, is impossible to attain.

15.5 The True Ladder of Axioms

The major point about the Newtonian hierarchy is that the lower hypotheses are less universal than the higher ones. A lower hypothesis may concern all planets, a higher one all heavenly bodies. Every law that applies to all heavenly bodies must apply to all planets, but not the other way around. Newton required that a less universal theory should be strictly deducible from a more universal theory—not a first approximation to it. Now Kepler's theory is a first approximation to Newton's theory under a certain condition (relatively massive sun), which in its turn is a first approximation to Einstein's theory under a certain condition (low velocities and weak gravitational fields). This hierarchy is Boylean and not Newtonian. It should be mentioned that Newton was aware of the difference: he stressed that observed deviations from Kepler's ellipses had been observed that had puzzled astronomers that his theory explains.

The profit from confusing the Boylean and the Newtonian hierarchies is this. Suppose your predecessor's theory follows from yours under the special condition C. A set-up which violates condition C offers a refutation of your predecessor's theory. Suppose you want to save your predecessor's feelings. All you have to do is to tack the antecedent "under the condition C" on to his theory and it becomes a part of your theory. Moreover, you will read into your predecessor's circumstantial description condition C. This way, the "true ladder of axioms" will enter the inductivist history of science and stay there until some anti-inductivists like me protest.

The methodological point is this: the more general a theory is, the more scientific it is. As more conditions qualify a theory, the more its scientific character deteriorates; it becomes more *ad hoc*, it becomes, if you will, decreasingly explanatory. Theory 1 is at first stated without qualifications. It follows from a subsequent theory 2 that theory 1 is true only under some conditions. In order that theory 2 could be as universal as theory 1, it must state that only under those conditions is theory 1 true; that under different condition it is false. The two theories are now competing: there can be a crucial test between them. Thus, there was a crucial test between Einstein's and Newton's theories; there was earlier a crucial difference between Newton's and Kepler's theories. Newton both stressed and veiled this fact. According to Newton's theory, Kepler's theory holds only in modification and only under certain conditions; but he stated both that this is so and that Kepler's theory follows from his own. This simple logical inaccuracy took three of the greatest philosophes of science to clear, Whewell, Duhem, and Popper. So great was Newton's authority (Cohen 1974, 1987; Dauben et al. 2009, 21; Shea 1982; Agassi 2008, 482 ff.; Smith 2001, 250–251).

Newton said,

> Hypothesis I
>
> The centre of the system of the world is immovable. This is acknowledged by all, while some contend that the earth, others that the sun, is fixed in the centre. Let us see what may from hence follow.
>
> PROPOSITION XI. THEOREM XI.
>
> That the common centre of gravity of the earth, the sun and all the planets, is immovable…
>
> PROPOSITION XII. THEOREM XII.

> That the sun is agitated by perpetual motion but never recedes far from the common centre of gravity of all planets…
>
> PROPOSITION XIII. THEOREM XIII.
> The planets move in ellipses etc.

The last sentence cited here provides the fullest and most satisfactory support for the unanimously endorsed interpretation of his view as one according to which Kepler's laws are absolutely true, despite his discussion of the deviations of planets from their Keplerian ellipses that he could explain and was rightly proud of. But it also shows that he clearly rejected the heliocentric hypothesis, a fact that inductivist historians also veil.

Newton did not have to assume the hypothesis concerning the immobility of the centre of the system of the world quoted above. He only made this hypothesis because it is common to all the systems prior to his, and, he added, "let us see what may from hence follow". What follows is that Copernicus' theory is not quite true: not the sun but the center of gravity of the solar system is its immobile center. He could say in favor of Copernicus that his was a better approximation than Ptolemy's by far, of course. He did not.

The matter is still confused. Here is a strong statement by Ernest Rutherford, responding to Eddington's crucial observation of 1917, from his Presidential Address to the British Association, 1923 (p. 24):

> There is an error far too prevalent today that science progresses by demolition of former well-established theories. Such is very rarely the case. For example, it is often stated that Einstein's general theory of relativity has overthrown the work of Newton on gravitation. No statement could be farther from the truth. Their works, in fact, are hardly comparable, for they deal with different fields of thought. So far as the work of Einstein is relevant to that of Newton, it is simply a generalization and broadening of its basis; in fact a typical case of mathematical and physical development. In general a great principle is not discarded but so modified that it rests on a broader and more stable basis.

This is a myth in the making. The two allegedly competing theories are incommensurable (Kuhn 1962, 62); the one is a generalization of the other that thereby broadens its basis (Newton); the one is a modification of the other and is better confirmed (Eddington; Popper). It is saddening to read such an unclear defense of such a great creation of the human mind. It is more saddening that the confusion here became very popular a few decades later as Thomas S. Kuhn made it into a cornerstone of a philosophy of science. Of course, the thrust of Rutherford's remark—and of Kuhn's elaboration on it—is quite right: whereas we may safely ignore Aristotle's physics and even parts of Newton's optics, we cannot ignore Newton's mechanics or his theory of gravity without severe loss. The right conclusion from this is that some theories that approximate current ones are not to be rejected despite their being superseded. It is not easy to admit this conclusion. The view that approximations are good enough threatens science with stagnation. It is no small matter, as E. A. Burtt stressed, that Kepler was dissatisfied with his success despite its being a better approximation to observation than anything achieved till then. Nevertheless, it remains true that, as Einstein has put it, the scientific truth is not the same as the absolute truth.

Newton's theory surely was not demolished, simply because it is a very significant scientific theory. To say, as Rutherford[18] did, that Newton's and Einstein's theories are "hardly comparable, for they deal with different fields of thought", is embarrassing. If Newton's theory needs defense, it is not against people who admit that his great theory has been refuted, but against people who insist that any refuted theory is a valueless prejudice. The ladder of axioms that is but a polite correction of the history of science and the doctrine of prejudice are harmoniously knit together into a convincing mesh. We do not need it.

15.6 The Philosophical Foundations of Mechanism

By Newton's mechanics the world comprises particles (atoms represented as point atoms) and central forces attached to them. It amounts to more than that. It requires that researchers assume nothing else; it is the claim that Newtonian assumptions suffice to explain all phenomena. While a few mechanical theories are testable and thus scientific, mechanism is an untestable metaphysical doctrine.

Although a metaphysical doctrine cannot be supported by such strong argument as can a scientific hypothesis, that is, by what Faraday has called experimental arguments, it can be supported by weaker argument of the kind that Faraday has called theoretical considerations. It is difficult to produce testable hypotheses and to test them. Sometimes it is easier to invent a lot of hypotheses and the problem is to select the one which seems worthy of effort to develop it according to some program. Sometimes it is difficult even to formulate a hypothesis, and the best way to proceed is to take some untestable speculation and to try to strengthen it until it becomes testable. The reasons for accepting a program—whether mechanistic or not—are ontological. We try to carry out a program because we take seriously the factual but untestable assumptions behind the program as possibly true. Mechanists took for granted that their mechanical speculations are true and Faraday took for granted the reality of fields of force.

The logic of programs is fairly simple. They rest on the view that science is *a priori* given, that its axioms should explain all that is known but that they do not: they are metaphysical. In the way is the traditional dismissal of metaphysical theories on the ground that they are shallow and that they may conflict with the *a posteriori* given. The shallowness of the metaphysical theories is a challenge, and so is the conflict, real or possible, between speculations and the *a posteriori* given. This leads to developing ideas and criticizing them, and this hopefully is progress. How then is argument for or against metaphysical ideas possible? One cannot test them empirically. One can, however, employ received scientific

[18] The idea is not exclusively Rutherford's; we learn from Charlie Chaplin's autobiography that he had heard it from Einstein. This signifies, even though we should ascribe its precise wording to some benevolent copy editor.

theories as arguments. Einstein, anti-mechanist though he was, in his scientific autobiography he explained why in his own youth mechanism was so very convincing (Einstein 1949, 19):

> In spite of all the fruitfulness in particulars, dogmatic rigidity prevailed on matters of principles: In the beginning (if there was such a thing) God created Newton's laws of motion together with the necessary masses and forces. This is all; everything beyond this follows from the development of appropriate mathematical methods by means of deduction. … What made the greatest impression upon the student, however, was less the technical construction of mechanics or the solution of complicated problems than the achievements of mechanics in areas which apparently had nothing to do with mechanics: the mechanical theory of light, which conceived of light as the wave-motion of a quasi-rigid elastic ether, and above all the kinetic theory of gases…
>
> We must not be surprised, therefore, that, so to speak, all physicists of the last century saw in classical mechanics a firm and final foundation for all physics, yes, indeed, for all natural science, and that they never grew tired in their attempts to base Maxwell's theory of electro-magnetism, which, in the meantime, was slowly beginning to win out, upon mechanics as well. Even Maxwell and H. Hertz, who in retrospect appear as those who demolished the faith in mechanics as the final basis of all physical thinking, in their conscious thinking adhered throughout to mechanics as the secured basis to physics. It was Ernest Mach, who, in his *History of Mechanics*, shook this dogmatic faith…

These arguments of Einstein's for and against Newton's metaphysics are not scientific. It is possible to argue that a scientific theory that contradicts a metaphysical view under scrutiny is tentative and a mere approximation to the truth, that a better scientific theory will supersede it and it will conform to the metaphysics that is now under scrutiny. This, for example, was Einstein's attitude towards the non-deterministic character of quantum theory when he advocated determinism. The argument, therefore, that takes place in a metaphysical realm is less conclusive than scientific argument proper.

Mechanism had many arguments in its favor. Some of them are the principle of causality, the principle of economy, the principle of simplicity, etc. The most significant problem related to the philosophical foundations of mechanism, is that of the status of Newton's principles, his laws of motion. Nowadays physicists agree that Newton's laws of motion are hypotheses plain and simple. So now that the problem is solved there is no need to revive old arguments. It is a popular fashion these days (started by Hans Reichenbach) to ridicule defenders of Newton like Kant, whom Einstein had criticized successfully. Yet the erroneous element in Kant's theory concerning the status of Newton's laws came from Newton himself. To see this we should remember that the problem of the status of the laws is closely linked to the problem of their origin but not identical with it. The defenders of Kant emphasize that Einstein chose to agree with Kant, and disagree with Newton on the origin of scientific ideas: according to both Kant and Einstein their origin is the mind, not in experience. The opponents of Kant emphasize that Einstein chose to agree with Newton, not with Kant, on the status of science: according to both Newton and Einstein but not according to Kant, science is not *a priori* valid but is an empirical system.

This is roughly true; in detail it is problematic, however: what was Newton's view on the status of the laws of motion? Here are two passages from Newton, often quoted with relation to another problem but they present his view on this problem too. The first is most famous; it is from *Scholium Generale* at the end of his *Principia*:

> I frame no hypothesis; for whatever is not deduced from the phenomena is called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies and the laws of motion and of gravity were discovered.

The other passage is from the penultimate paragraph of his *Opticks*:

> As in Mathematicks, so in Natural Philosophy, the Investigation of difficult Things by the Method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments and Observations; and in drawing general Conclusions from them by Induction, and admitting of no Objection against the Conclusions, but such as taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general … This is the method of Analysis: And the Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phenomena proceeding from them, and proving the Explanations.

The problem most often discussed in connection with these passages is not very important. It is, why did he assert his "*hypothesis non fingo*" in connection with his pronouncements of highly speculative statements? The reason for this is simple: (in disagreement with Bacon, not to mention Wittgenstein) he did not ban conjectures; (in agreement with Boyle) he excluded them from experimental philosophy. Thus, *hypothesis non fingo* is a kind of justification for having feigned his speculations—as extra-scientific. In empirical science we "argue from experiments", Newton says, borrowing a famous phrase from Boyle.

Newton wanted for his theory of gravity the status of experimental philosophy. To this end he used his theory of a hierarchy of theories that almost obliterated the distinction between general fact and theory. In Boyle's system, a general fact is an observation report in the naïve, straightforward sense of the word. No methodological argument can make one ignore the distinction between fact and theory that Boyle used. In Newton's use, however, a statement about forces can be a general fact, and this is surely not an observational report since no one ever observed Newtonian forces.[19] Of course, Newton might have replied that he translated statements of fact into mathematical language; but he also claims that the laws of motion are results of induction from observations. He did not state this to claim certainty for his laws of motion, but to include them in experimental philosophy. Induction, Newton claimed, is as certain as possible although it is not absolutely certain.

[19] By Locke's theory of observation, force is not observable; modern perception theory allows for perceptions that Locke would have rejected as he was determined to reject reports of observation of the motion of celestial bodies, declaring them theoretical. In any case, no one has reported having observed Newtonian forces, yet Laplace spoke of Newton's law of gravity a general fact.

In the introduction to *Principia* Newton claimed inductive status or experimental status for geometry too:

> … geometry … is nothing but that part of universal mechanics which accurately proposes and demonstrates the art of measuring …

Therefore, Burtt concluded, Newton claimed uncertainty for Euclidean geometry too. Possibly he is the only one who has exposed Newton's methodology as "a frank presentation" of "a tentative mood of empiricism". This is brilliant; nonetheless, I am afraid I cannot agree.

In the above quoted passage from *Opticks* Newton states that the more general a statement, the more certain it is. He did not argue for this statement, presumably because he did not argue for any idea that Boyle had rendered sufficiently popular. All this renders clear the distinction between the origin and the status of the principles. To use Kant's idiom, geometry is *a posteriori* discovered *a priori* valid truths. Though an inductively arrived at statement is not certain, as its degree of certainty[20] increases with its generality,[21] the most general is most certain (although, according to Boyle, the most certain possible is still not absolutely certain). We arrive inductively at the most general statement, Newton said in the end of his *Opticks*, and we then reassess it as a principle, not merely as a general fact, and proceed from it deductively.

In the *Scholium Generale* of his *Principia* Newton said, the laws of motion belong to experimental philosophy. In the introduction to Book 3 of that book, in the part where he presented his astronomical theory, he admitted that the status of the laws of motion is not inductive, that they do not belong to experimental philosophy but to mathematics:

> In the preceding Books I have laid down the principles of philosophy; principles not philosophical but mathematical; such, to wit, as we may build our reasoning upon in philosophical enquiries. These principles and forces, which chiefly have respect to philosophy … It remains that from the same principles I now demonstrate the frame of the System of the World.

The reconciliation between Newton's statements that the principles are and are not philosophical (i. e. experimental) is this: going up the ladder of axioms they are philosophical and not mathematical while coming down the ladder of axioms they are mathematical but not philosophical. I do not wish to argue how satisfactory this reconciliation of Newton's statements is, and what problems it raises. The major criticism of it is Kant's great contribution to human knowledge. It is that we need the principles before we can start to climb up the ladder.[22] He asserted that for the

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[20] Degrees of certainty signify, according to Einstein, in that they comprise default ordering of hypotheses as candidates for modification. Yet he considered the law of entropy more certain in this sense than the law of gravity.

[21] Degrees of generality increase with degrees of testability (Popper 1959, §18).

[22] Kant seems to have rejected the ladder since his concept of matter was Cartesian (*Prolegomena*, §15); he did argue (§38) that Newton's law of gravity is *a priori* valid since the area of an expanding sphere grows by the square of its growing radius. This idea makes sense only by the postulate of the conservation of force that appeared after his death.

sake of climbing the ladder, mathematical principles are indispensable.[23] The answer that Newton could give to Kant's criticism, one may argue, is that although the principles are necessary for the ladder as described in Book 3 of his *Principia*, their origin is another ladder of axioms. This is problematic, however: Newton never mentioned how he arrived at the laws of motion, nor did he indicate how one could possibly arrive at them—inductively or otherwise. Einstein saw this as the greatest breakthrough of all times.

Following Boyle, Newton declared that his forces are not occult since his theory of them is non-*ad hoc*. He meant more than that. He denied that his theory is a mathematical hypothesis, as Newton used this term (*Principia*, 2, 9) to denote a hypothesis not yet tested. So his theory was non-*ad hoc* since it had stood up to a test. This is remote from Bacon's doctrine of prejudice that dismisses all tests. Newton could allow for that—and suggest that his work follows a program that hopefully will end up successful. To repeat, in the opening of Book 3 of his *Principia* he spoke of his principles as conducive to research:

> In the preceding Books I have laid down the principles of philosophy, principles not philosophical, but mathematical: such, to wit, as we may build our reasonings upon in philosophical inquiries.

15.7 Newton's Style

The first significant deviation from the inductive style that Boyle's instituted was that of Newton in the greatest scientific text of all times, his *Principia*. The deviation was not in the least from inductivism but from amateurism. This is understandable, considering that his *Opticks* was popular whereas his *Principia* was a closed book to most of his contemporaries.[24] Here is a part of the introduction to Book 3 of that monumental book:

> … I now demonstrate the frame of the System of the World. Upon this subject I had, indeed, composed the third Book in a popular method, that it might be read by many; but afterward, considering that such as had not sufficiently entered into the principles could not easily discern the strength of the consequences, nor lay aside the prejudices to which they had been many years accustomed, therefore, to prevent the disputes which might be raised upon such accounts, I chose to reduce the substance of this Book into the form of Propositions (in the mathematical way), which should be read by those only who had first made themselves masters of the principles established in the preceding Books: not that I would advise any one to the previous study of every Proposition of those Books; for they abound with

[23] This is Kant's solution to the problem of observation: subjective experience becomes scientific, i. e., certain when worded mathematically. His example is Kepler's first law that is a proper generalization of astronomical observations made possible by the concept of ellipse (§38). The irregularities reported in Newton's *Principia* refute Kant's solution together with his philosophy of science. His criticism of empiricism, however, is valid.

[24] "… after its appearance the Principia was more admired than studied" (Whewell 1859, addition to Ch. 3, 431).

> such as might cost too much time, even to readers of good mathematical learning. It is enough if one carefully read the Definitions, the Laws of Motion, and the first three Sections of the first Book. He may then pass on to this Book, and consult such of the remaining Propositions of the first two Books, as the references in this, and his occasions, shall require.

Newton here deviates from Boyle's style, first in the first two books that are "not philosophical but rather mathematical"; here in the philosophical Book 3 he does so in order to avoid dispute.

The major results of Newton's mathematical style were two. (1) The inductive style was often ignored in texts in mathematical physics. (2) Mechanism became a prerequisite for physical study on the supposition that it is but the application of mathematics to physics. This explains why so many qualitative hypotheses were presented in a mathematical guise.

Newton's emphasis was on mathematical form, rather than on the need for and fruitfulness of mathematical physics. A mathematician who reads Gilbert's *On Magnets*, for instance, would recognize there its mathematical mode of reasoning that is strikingly similar to that of Galileo. It is interesting to compare Gilbert's mode of presentation with Newton's. The third Book of the *Principia* is entirely in a mathematical form, though a great part of it is not mathematical at all. Whereas Gilbert and Galileo tried to talk in ordinary language, Newton preferred to put his ideas in mathematical attire. Not that people endorsed Newton's theories because his mathematical language impressed them; only that as they became popular, people tended to forget that Newton intended his mathematical style to prevent controversy; they tended to view his style as the best possible. In the year 1855 the Astronomer Royal George B. Airy, then Newton's representative on earth, wrote a letter to a friend of his and of Faraday (Jones 1870, 2, 353), well over a century after Newton's demise, characteristic of the dogmatism about his program:

> The effect of a magnet upon another magnet may be represented perfectly by supposing that certain parts act just as if they pulled by a string, and that certain other parts act just as if they pushed with a stick. And the representation is not vague, but is a matter of strict numerical calculation … with precision. … I can hardly imagine anyone who … knows this … to hesitate an instant in the choice between this simple and precise action, on the one hand, and anything so vague and varying as lines of force, on the other hand.

Faraday's theory allowed as precise results as the Newtonian theory that Airy proposed, and he knew it. But he preferred Newton's program as a matter of course, especially as long as Faraday's program was not (yet) expressed mathematically.

15.8 A Flood of Fluids

One of the standard jokes against mechanism concerns the multitude of fluids with which it filled space: gravitational (gravific), electric, magnetic, and luminiferous, caloric, animal spirits, and what not.

The most important distinction is between fluids and effluvia. Etymologically these two words are synonymous. But by consent an effluvium is Cartesian (or rather Boylean)—its particles are endowed only with bulk and inertia—whereas a fluid is Newtonian—its particles are endowed with forces too. As the nothing has no property, it is possible to deduce from observation of heat that there is something that has the property of heat. Whether or not this has the property of, say, inflammability or luminosity, or both, is a problem to settle by empirical research. Similarly, we do not know if caloric is heavy or not, but the existence of caloric is no hypothesis: "Light and heat" Joseph Priestley wrote (Priestley 1796, 16), "are almost universally allowed to be substances, though no person has been able to weigh them." He said it when he was forced to say whether the fluid of fire, the phlogiston, had weight or not: as an orthodox inductivist, he wanted to say nothing that he could not observe. And yet a simple problem showed the inadequacy of Newton's program. The interaction between two electric charges is observable because electrically charged pieces of gross (regular) matter interact with each other because they are charged; how is matter charged? To be charged there must be some force linking gross and electric matter. This is impossible since electric matter interacts only with electric matter. This problem was not discussed, but it was familiar. John Davy's biography of his celebrated brother Humphry says of his galvanic decompositions (Davy 1836, 307),

> He saw in it the connection between galvanism and chemistry; he expected that it might prove a link between the ponderable and imponderable substances…

Einstein noted this difficulty (Einstein 1949, 75–7) after he had solved it for gravity: even the inertia of these fluids or its absence, as well as their modes of action, elementary points about them from the mechanistic standpoint, were not known, and no way to explain them was suggested. These days, however, elementary particles theory does offer an alternative to Einstein's program.

No philosopher before Faraday ever thought that we could dispense with fluids: that the nothing has no properties is a self-understood thesis ever since the old days of Democritus. Already the ancients had a number of fluids. Descartes introduced a few more. Boyle multiplied them (*Works*, 2000, 10, 307 ff.), with a different effluvium for each different function; heat was a fluid and cold was a fluid. Frictional electricity was one or two fluids. Allergy to cats was due to a fluid too (*Works*, 1999, 6, 304; 2000, 7, 267–71). (It would have warmed his heart to learn that allergens comprise gross matter but he would be greatly puzzled to learn today's views of gross matter as so very thin). Newton, in a private letter to Boyle that was published a few times in the mid-eighteenth century, added a few to them, and with the rise of the wave theory of light in the early nineteenth century their number increased beyond measure.

This sounds highly *ad-hoc*, unless we realize that there is hardly any scientific objection to their introduction. The problem of inquiring into the properties of a fluid, or the problems of inquiring into the identity of two different fluids, was indeed a scientific problem, presented inside the mechanistic framework. The assumption behind the flood of fluids, that magnetism, heat, etc., are properties, is no small assumption, and its refutation in some cases was of paramount importance, but all this was beyond the scope of those researchers who did well to stick to their mechanistic program. Only in 1820 Ørsted first violated the mechanistic program, and this brought Faraday to assume that force is not a property but a thing. It led Einstein much later to assume that mass or quantity of matter is not a thing but a property (of a system). This was the end of a process that began in the mid-seventeenth century with inquiries into properties of the many fluids whose existence was postulated with efforts to identify pairs of them or to abolish some of them: any achievement of this kind would reduce the *ad-hoc* character of physical theory. The study of the ether was the last stage in this process.

All these developments freed physics of much of its *ad-hoc* character. This shows the superiority of Newton's program over Boyle's program. In the year 1820, just at the end of the period a classical paper by Dr. Robert Hare appeared (Smith 1911, 132–8) with an attack on Davy's kinetic theory of heat, the theory that denies the existence of the fluid of heat. The paper contains a really beautiful refutation of Davy's theory and on the same grounds as those on which the theory was supposed to rest, namely, on energetic considerations. The end of the paper is a restatement of the mechanistic program in extreme terms—in the demand not to get rid of any fluid. Speaking of attraction and repulsion he says (Smith, 1911, 137),

> … the existence in nature of two kinds of reaction between particles is self evident. There can be no property without matter in which it be inherent. Nothing can have no property …
>
> There must also be as many kinds of matter as there are kinds of repulsions … Hence I do firmly believe in the existence of material fluids, severally producing the phenomena of heat, light and electricity.

The *ad hoc* character of the fluid view rests on its being the method of speaking of a property as if it were an object. Once we ascribe to an object two properties or more, then a non-mechanist would interpret our statement as a hypothesis as to the connection or similarity between two properties, or some similar hypothesis. This renders attempts to find new properties of the same fluid more promising than attempts to eliminate a fluid. The attempt to find more properties of phlogiston, the fluid of fire, led to its refutation. But the attempts to find more properties of caloric and electric fluids were beautiful and fruitful while remaining entirely within the mechanistic program. Here is what Maxwell said on Ohm's work (Maxwell 1873, 333.):

> Ohm, misled by the analogy between electricity and heat, entertained the opinion that a body when raised to a high potential becomes electrified throughout its substance, as if electricity were compressed into it, and was by means of an erroneous opinion led to employ the equations of Fourier to express the true laws of conduction of electricity through a long wire, long before the real reasons of the appropriateness of these equations had been suspected.

Maxwell denied that there is an electric fluid. So he rejected Ohm's theory. Therefore, according to the inductivist code, he had to consider Ohm prejudiced or misled by analogy. But he endorsed Ohm's law. Therefore, Ohm was only misled by analogy. Now if Ohm was misled, then he was misled by the false theory, not by an analogy. This is no logical subtlety; rather it is the fact that Maxwell overlooked the fruitfulness of the mechanist program. He did not see the advantage that he himself drew from the peculiar mechanist program that he and Kelvin were following. For, in his last masterpiece, his *Matter and Motion*, he strongly, and perhaps unjustly, criticized Descartes, whose writings were a source of inspiration for the both of them.[25]

Another successful development of mechanism took place then: the theory of elasticity and of the elastic ether. These theories were not exactly Newtonian since they employed partial differential equations. But the deviation was relatively small. The hope prevailed that all elastic media were composed of Newtonian particles. The continuous media came to life not before 1830. Before then, the fluids or effluvia or matters, were composed of particles, and they were distinguished mainly by the major property that made researchers introduce them. Ordinary matter—gross, bulky, ponderable—is impenetrability; heavy matter is gravity[26]; ethereal or subtle matter may be any matter other than ordinary matter. It must be elastic to transmit action, while fluids represent the various properties: electric fluids represent electric forces; caloric represents heat; etc. The word "fluid" should not be taken too literally in this context; it is a leftover from Newton's discussions.

The assumption of fluids should comprise the axiom that nothing has any properties. Researchers strove to learn more about these fluids and to eliminate some of them, mainly by identification of two fluids as one. But almost each fluid had its own problems and all of them had common, insurmountable ones. The mechanist idea progressed tremendously but it never came any near to realization. Faraday created very serious problems for it which its adherents never solved. His followers, Kelvin, Maxwell and Hertz, began in efforts to reconcile the mechanical program—Descartes' or Newton's—with Faraday's program, and then they abandoned it. Einstein was their intellectual heir and he finally laid it to rest.

15.9 The Mechanical Model

The mechanical model is an indispensable part of mechanism. Descartes was not the first to introduce it, but he was the one who presented it as a general rule of method because he realized that the paucity of the mechanical assumptions on the material world (matter and motion) and the refusal to allow any other hypothesis concerning its properties, made it hopeless to explain anything by assuming so little. He therefore permitted and even urged people to explain certain phenomena by making hypotheses of a certain kind, mechanical hypotheses—ones that are specific and often related to the structure of matter in certain circumstances. Given a clock,

[25] There is no presentation of Ohm's theory as a theory of electric fluid, nor of its predecessors, the fluid theories of Fourier and of Carnot. The discussion of Poisson's magnetic fluid theory and Ampère's electric fluid theory likewise await proper discussion.

[26] Of course, ordinary mater is heavy; within the Newtonian program this is agreeable; not within that of Einstein, who found it irksome until he succeeded in developing his general relativity (Einstein 1949, 65).

we know that it works by a push mechanism; but the specific structure of this particular clock may be unknown, and then we are permitted and even urged to feign a hypothesis concerning its structure, its mechanism.

All of Descartes' justifications for this are entirely objectionable—save the one concerning its fruitfulness. But this matters little. One point, however, deserves examination. Introducing hypotheses never requires any excuse whatever, but (under Bacon's influence) most classical thinkers thought otherwise. Descartes, who said he had proved *a priori* both the truth and the completeness of his theory, had little difficulty showing that nonetheless to explain all known phenomena he needed many more assumptions. The completeness of his theory implied, he suggested, only that any other assumptions concerning the properties of matter are impossible, but that models are welcome. There is here a serious error, and even a logical one: like the great majority of methodologists, he did not distinguish between explanation and ultimate explanation. The reasons for this are clear. Boyle was the first to show that we may profit from an explanation that has a high explanatory power even if it is not ultimate. Descartes did not know this, and he demanded the exclusion of all explanations that assume any properties of matter but those of bulk and motion. All properties save those that he claimed to have proved *a priori* to be self-evident, are not evident at all; they are hidden—occult—and so researchers should avoid them at all cost. They may assume mechanical models and nothing else, he said. Occult qualities are the qualities assumed in order to explain observed phenomena, like the greenness of the grass. Boyle and Newton said, only *ad hoc* hypotheses are occult, not explanatory ones.

Hypotheses to explain given structures were more necessary for the system of Descartes than for that of Newton. (Thus, Descartes assumed a mechanism to explain the observed motions of the heavenly bodies; Newton did not.) But Newtonians too used structural hypotheses, assumptions concerning mechanisms. There is no need to justify such a procedure, but it is desirable to have this procedure discussed openly.

Faraday discovered that a mechanical philosophy—Cartesian or Newtonian—must assume specific hypotheses concerning structures. He repeatedly stressed that ultimately this procedure, of assuming structures, is more *ad hoc* than the procedure of assuming no structures but only the universal laws. Yet the clear distinction between structures and structural laws we owe to his follower James Clerk Maxwell. In the introduction to his *Matter and Motion* that describes his abandonment of mechanism (1876). There he said that the nineteenth century view of the universe superseded that of the eighteenth century; in addition to laws governing the behavior of particles, nineteenth-century researchers assumed laws governing the behavior of structures. I do not know of any *a priori* valid reason for preferring Faraday-style theories that explain the behavior of structures to mechanistic theories that do this by assuming laws governing particles. Maxwell employed the mechanical model theory that assumes laws governing parts of a structure and initial combination of the mechanism that controls it. He tried to explain mechanically Faraday's theory (in his own presentation). He then gave up hope. The great success of Faraday's research is at least partly due to his new effort to attack the extant problems by trying to explain structures by assuming laws governing them. And Maxwell finally followed suit.

Ørsted's experiment refuted one mechanical hypothesis, but not mechanism (that is irrefutable). Most researchers were less Baconian then Faraday, and they did not allow one refutation to invite a really fresh start (that Kuhn has called a paradigm shift). The argument that mechanism is clumsy in its requirement to make many mechanical hypotheses and to construct many models is also not too strong. Any testable hypothesis is welcome, and a newly constructed model that explains a structural law may be helpful and rendered testable. The argument is also very weak that these are unnecessary since some laws govern structure. For all we know, both mechanical and non-mechanical structures may exist. True, the non-mechanical may turn to be mechanically explicable after all, as Boyle has observed; the opposite is also true. Admittedly all this seems more difficult to show than to suggest, but all the same the argument in either direction is possible.

My heart is with Faraday. It was perhaps too rash of him to lay aside the whole of mechanistic philosophy for his grand revolution on the strength of scanty arguments. But how brave it was and how imaginative! His tenacity allowed him to develop newer and stronger arguments against the mechanical philosophy. Now obviously mechanism was so much a part of physics and of epistemology that it was not easy to let it go, not even tentatively. At the time, the theory of the mechanical model was inspiring. Not many philosophical ideas underwent so thorough an investigation as mechanism did. Nor were there as many controversies about a program as about mechanism. And civilized argument cannot be but of positive value. The possibilities of the old views are more explored; the weaknesses and strengths of the new view are compared with those of the old. And arguments for and against mechanism and structuralism all went to the benefit of science.

The mechanical model and the structural law are by no means the sole options. There can be non-mechanical models. There can be a model theory that does explain a structure by assuming laws governing its parts and initial combination of them or initial structures. But the laws need not be mechanical. Besides there can be combinations of structural laws and laws governing particles. (Such are the semi-classical quantum theory and the current quantum-field theories that admit both fields and particles.)

The most important non-mechanical models in the history of science are the ones that Boyle has mentioned: Brahe's and Kepler's astronomical systems and Galileo's theory of gravity. Since Brahe abolished the crystal spheres and introduced no alternative to them, he rendered planetary orbits non-mechanical. If Kepler has offered any sufficiently detailed alternative to the crystal spheres, then it is his theory of forces. Nevertheless, their planetary orbits were more real than the equator yet they were not subject to any laws of motion. As it is, Kepler's laws have a unique position. They reappeared, with a slight modification, in Niels Bohr's model of the hydrogen atom that is also non-mechanical. (Later Bohr developed another non-mechanical model—the drop model of the nucleus.) Bohr's theory shows that much as mechanism was fruitful, much as Faraday's structuralism was ever more so, there is no *a priori* reason to stick to any program. Programs are for researchers to use as they wish; their value is heuristic and not binding in any sense.

15.10 Conclusion

Mechanism was a Renaissance theory, a return to the glorious Greek science of physics and a repudiation of stagnating scholasticism. Its greatest asset is the mechanistic dismissal of the *ad hoc* method of introducing occult qualities. The dismissal was a little misplaced: hypotheses about occult causes and *ad hoc* hypotheses are different; there is nothing especially wrong about hypotheses about occult causes, except that they usually are *ad hoc*. And *ad hoc* hypotheses are unwelcome even when not of occult causes.

Descartes presented his mechanism mainly as a metaphysical theory but also as a fruitful program that helped him to create his physical theories. And then Boyle proposed mechanism mainly as a program, the greatest advantage of which is the dislike of *ad hoc* hypotheses regardless of anything else: it left room for occult properties, forces, and anything else. He made it clear that *ad hoc* mechanical hypotheses are no less unwelcome than other *ad hoc* hypotheses—so much so that today we scarcely see what is special about mechanical models. Galileo deemed force an occult quality. Leibniz said that Newton introduced an occult property. Newton denied this but had no good argument for this. Force was soon converted into a mechanical property by fiat. This was a widening of Boyle's program. As he was a follower of Boyle, his own idea was definitely more of a program than a metaphysical doctrine. One major corollary from it is that explanations by reference to occult qualities may be welcome. This might have been a turning point in the history of traditional philosophy. Programs could have taken the place of doctrines and methodology the place of epistemology. But Newton's methodological inconsistency, his greatness as a scientist, the great success of his scientific mechanics and theory of gravity, and the subsequent authority that his views exercised, all prescribed a different history for philosophy. Locke's philosophy of *sense data* and his theory of induction on the one hand, and Kant's *a priorism* on the other, were the only philosophies Newtonians considered viable. And both philosophies viewed Newton's theory as absolutely true. Mechanism was deemed a by-product or the *a priori* foundation of mechanics. Mechanism was and continued to be a very successful program and researchers endorsed it thoughtlessly, as a matter of course; when forced to adjudicate, they took it to be metaphysical or empirical or even epistemological—as long as they did not have to worry about it and proceed with their researches.

In a way, Kant's epistemology is the only satisfactory translation of mechanism from a program into an epistemology. The view of the whole of the variety and beauty of this observed world as consisting of nothing but hard particles and forces is most imaginative as a challenge, and terribly unimaginative as a dogma.

Chapter 16
The New Doctrine of Prejudice

The doctrine of prejudice has many variants since its default version is inconsistent: its application to itself refutes it. This is no serious impediment, since it is easy to eliminate inconsistency. It is also easy to reword it—as any theory—so as to dodge its inconsistency and the counterexamples to it, and there are many ways to do so. Since the doctrine was—it still is—taken for granted by most of its adherents, they varied its wording casually. It is interesting to see how it altered through the ages and what its current popular variants are. And of course, the right place to start with is the philosophy of Locke, whose influence throughout the Age of Reason was tremendous. There is an immense scholarly literature on his work and its influence—most of it irrelevant to the present study. My discussion of his work is brief, as it concerns only the doctrine of prejudice. Locke's epigone Dr. Isaac Watts takes a much larger share of this chapter since he expressed the spirit of the age better and since he was tremendously influential. This chapter ends with an attempt to assess the doctrine in its current variant: it is part-and-parcel of current inductivism, scarcely ever given explicit expression, much less public discussion, yet it provides inductivism its air of cogency.

16.1 John Locke

Locke was the official epistemologist of the Fraternity. This makes him the most influential philosopher of modern times. He was chosen to this task as he was an admirer and a friend of Newton (and 10 years his senior). He served as *the* empiricist epistemologist. None of his statements is as famous as that nothing is in the intellect that has not previously been in the senses, although the statement is Aristotle's. All this explains his fame without ascribing to him any specific contribution. Did he make any? This question was not raised. He did make one, and it is important and explains better his high status in the tradition of the Enlightenment movement. He solved a serious problem that troubled all the participants in the Royal Society

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8_16,

and other participants in the scientific revolution. The fraternity had endorsed ideas of both Bacon and Copernicus. Bacon had disapproved of Copernicus as he had ignored the evidence of his senses. This disapproval had to be answered. Locke offered an answer, and it remained the default answer for centuries. It was his application of Bacon's theory of the prejudices of the senses to the claim that we see the sun move: we see the sun in different angles in the sky and (wrongly) conclude that it moves. This was a tremendous *tour de force*.

All discussion of observation prior to, say, 1600, rested on a theory known as naïve realism: what we see is (more-or-less) what is out there. This is scarcely credible, and for two reasons. First, the rise of western philosophy rested on the distinction between reality and appearance (taken for granted until, say, 1900), whereas naïve realism is ubiquitous and it ignores this distinction. Second, knowledge of misperceptions and of sense illusion is ubiquitous too. Nevertheless, it remains an easily corroborated claim that all discussion of observations prior to 1600 is within the framework of naïve realism. Erwin Schrödinger said (Schrödinger 1951, 81), naïve realism is very easy to refute. It is an observed fact that the sun appears as no bigger than a cathedral, he said. From the claim that the sun is so small it follows with the help of simple calculations that between east and west is about one day's walking distance. Nevertheless, naïve realism prevailed, perhaps for want of a better theory of observation.

The change came with the theory that all observations are theory-laden. Galileo found this not disturbing as he was a Platonist and suggested that the theory that enables us to observe is mathematical. He did not deny that observation is theory-laden in other ways, but he demanded that scientific observation be laden with geometry. Not so Bacon: he was ignorant of mathematics and paid as little attention to it as he could. He said, we remember, Mother Nature does not lie, and so our mistakes are in our hypotheses. When we are utterly passive, the theory that we employ in our observations is the true one, and that allows us to deduce our theory from our observation by true induction.

Locke's theory of perception is still the most popular and the most refuted theory.[1] The greatest objection to it is that there are no utterly passive observations. We should leave all this now and return to our narrative.

Locke's philosophical and psychological theories were popular as they fit received methodological views well, especially the doctrine of prejudice. The Fraternity often vacillated between Bacon and Boyle. The reason for this is clear: inductivism is too vague, and researchers stressed different aspects of it on different occasions (as is characteristic of all myth). They justified making hypotheses by appeal to Boyle's wisdom while stressing the importance of refutations and appealing to Bacon's demand to avoid defensiveness and to collect facts. When discussion was civil, Boyle's authority permitted and encouraged guessing; when discussion was heated, participants in it unmasked their opponent as prejudiced.

[1] Hume's conclusions from Locke's theory of perception refute it; they are not conclusions from a proven doctrine.

Locke started as a Baconian and became increasingly Boylean. How did his intellectual development progress and how far is a fascinating problem. Here I will only touch upon it briefly. In the early 1661 *Essays* Locke endorsed Bacon's sweeping view that even the laws of logic are inductive conclusions from experience. The major point there is the view that the laws of ethics are natural, to derive from facts by induction. The early *Essays* thus fully support Bacon's view then that every important item in the life of the intellect (including logic) is to establish inductively and that the rest is sheer prejudice. [2] Bacon's influence on him is still felt in the late *Essay* (1690), but less so. The change is mainly methodological. Locke was tempted to overthrow the doctrine of prejudice altogether. A detailed comparison of his statements on prejudice in the 1661 *Essays* and in the 1690 *Essay* will show this clearly enough. He could not quite free himself of it. In his *Of the Conduct of the Understanding*, where he praises Bacon profusely (§1), he says (§10, Prejudice),

> Every one is forward to complain of the prejudices that mislead other men or parties, as if he were free, and had none of his own. … every man should let alone others prejudices and examine his own.

Locke intended this work to be a chapter of his *Essay*. Yet whereas the *Essay* appeared in four editions in his lifetime, his *Conduct of the Understanding* is posthumous—because it is devoted to the problem of prejudice and he could not be as free about it as Boyle was, I suggest. When it was published it was authoritative and interpreted as Baconian. That interpretation is a little peculiar, to say the least. For, although Locke could not get rid of the need to warn people against prejudices, his pronouncements both in the *Essay* and in the *Conduct of the Understanding* are always non-committal in a manner found in the writings of Boyle. In Book IV, Chapter XX, of his *Essay,* for a conspicuous example, he spoke of errors, prejudices and bias and of the need to eliminate them. He thus rejected Bacon's extreme demand to rid oneself of them *en gros*. On the contrary (Sec. 18),

> notwithstanding the great noise … made in the world about errors and opinions I must do mankind that right as to say, there are not so many men in errors, and in wrong opinions, as is commonly supposed. … they have no thought, no opinion at all…

This did not prevent him from discussing the various sources or errors, prejudice, and biases. Here is a passage from the *Essay* that is of some historical interest (Bk. 4, Ch. 19, Sec. 11, §7):

> There remains the last Sort, who, even where the real probabilities appear, and are plainly laid before them, do not admit of the conviction, nor yield unto manifest Reasons, but do

[2] By the way, the *Essays* end with "Sic Cogitavit J. Locke", which alludes to Bacon's *Cogitate et Visa*, where the expression "Francis Bacon sic cogitavit" is the *leitmotiv*. The arrogance it displays was subject to few nineteenth century comments. By reversing the order of the words in the phrase and by putting it in the end, Locke gives this arrogant expression a twist: it becomes humble and sincere.

either … suspend their assent, or give it to the less probable Opinion: And to this danger are those exposed, who have taken up *wrong Measures of Probability*, which are,

1. Propositions which are not themselves certain and evident but doubtful and false, taken up for principles,
2. Received hypotheses,
3. Predominant passions or inclination,
4. Authority.

The problem here is none other than the problem of general facts or of the repeatability of experiments or of circumstantial descriptions. Probability is the status of general facts in Aristotle's methodology. An anonymous thinker who had demanded to suspend assent in the face of such statements is here deplored; this shows that Ellis was not the first to discover that Bacon condemned induction by generalization. Ellis was only the first brave commentator to say so; Locke, that paradigm of a morally and intellectually brave individual, avoided open dissent from Bacon. This was not cowardice but Boylean chivalry: Locke emulated Boyle's mode of civil argument, of not mentioning the object of criticism by name. Bacon's rejection of Aristotelian induction, Locke tacitly claimed, leads to the worst results by Bacon's own standards. Refusing general facts amounts to the inability to admit refutations, as Boyle showed, and therefore one falls into the acceptance of false principles, received opinions, passions, and authority—the monstrosities that Bacon rightly never tired of condemning.

It is hard to judge what Locke's view was on the danger resulted from following predominant passions. A century passed before Faraday noted the obvious: expect researchers to be passionate (Faraday 1839, Series 12, §§1626-8, p. 518). Locke argued against what Freud called wishful thinking, and he advised his readers that they examine their thoughts. This is an echo of Boyle's demand not to publish one's hypothesis before having tested it. Locke argued against Bacon in Baconian terms, adding to Boyle's idea some Baconian flavor. He rejected Bacon's demand for the suspension of judgment; he recommended committing oneself to a hypothesis in the right measure according to its proper probability—whatever this means. His follower, the popular Dr. Isaac Watts, viewed these two ideas as identical.[3] Locke viewed Bacon's demand to suspend assent different from the demand to believe in the probable. Hence, well before Ellis, Locke understood Bacon to have opposed probability; they both deemed chimerical Bacon's permission to the intellect to make hypotheses and weigh their probabilities. Locke advocated the endorsement of general facts. He regarded this as incompatible with Bacon's rejection of induction by simple enumeration.

Dr. Watts, who was further from Boyle than Locke, generalized Locke's idea, claiming that any hypothesis has its probability (Locke never said this) and that a

[3] Current literature on induction scarcely mentions the need to suspend judgment; this seems covered by the demand to endorse all and only hypotheses to the exact degree that empirical evidence requires.

prejudice is the degree of to which a belief in a hypothesis deviates from its true probability. Thus, in Watts' reading, Locke added a new prohibition to the one not to believe too much: do not believe too little. Do believe in a theory, Watts pronounced solemnly, according to the exact measure of its probability (given all the available evidence). True, he knew no more than anyone else did how to find this exact measure, yet he advised his readers to do so. A century later the idea of a wager as a means to measure probability came to fill this gap. Despite its repeated refutations, its popularity endures.

The subject matter of Locke's *Conduct of the Understanding* is the possible source of prejudices. Here is one example: anticipations Bacon-style (§ 26):

> … this is visible, that many men give themselves up to the first anticipations of their minds, and are very tenacious of the opinions that first possess them; … This is a fault in the conduct of the understanding, since this … is … a submission to prejudice. … This can never be allowed, or ought to be followed as the right way to knowledge, until the understanding, (whose business is to conform itself to what it finds on the objects without) can by its own opiniatrety change that, to make the unalterable nature of things comply with its own hasty determinations, which will never be. Whatever we fancy, things keep their course…

The view expressed in this passage is obvious. It sounds as if Locke was following Bacon here, but he was not. He rejected Bacon's view that all anticipations are prejudices to jettison without discussion. Yet the passage quoted here is interpretable as Baconian, and it was so interpreted. What was Locke's motive for stating it? I do not know.

16.2 Dr. Isaac Watts

Isaac Watts D. D. is scarcely known among philosophers today. Yet for a century he was the most authoritative methodologist in Britain. His *Logic* (1724) and its supplement *On the Improvement of the Mind* (1741), Dr. Johnson reports (Johnson 1825, 285), were accepted in the universities almost immediately. In the early nineteenth century, young Faraday recommended them, together with Bacon's *Novum Organum*. In 1831, Sir John Herschel's popular *Preliminary Discourse on the Study of Natural Philosophy* defined prejudice (Herschel 1831, 80) by using a free quotation from Watts, as Faraday did in 1856 (Agassi 1971, 257).

Watts wrote poetry, hymns, and tracts on theology, education, science and philosophy. Dr. Johnson thought poorly of his poetry. His biography in the *Dictionary of National Biography* derides his hymns. The same biography blames him for weakness towards non-conformists. This, his religious toleration in action is the best commendation of him. His philosophical works are collected in one out of the six huge folio volumes that comprise his posthumous *Works* (1800). It is forgotten because he preferred to remain a commentator on Locke's views, a task that Locke himself performed thoroughly enough. Dr. Johnson praised his *The Improvement of the Mind* (1741) while admitting that it contains nothing that is not in Locke's

The Conduct of the Understanding.[4] Why then did Watts rather than Locke hold the title of the best exponent of the doctrine of prejudice when Locke's authority stood highest? The reason for that is that while Locke was still careful in stating his subtle compromise between Boyle and Bacon, Watts preached the ideas boldly, demanding strength of will in purifying and improving the mind.

A preacher and a teacher, Watts was the ideal puritan, the natural preacher of hard work and of intellectual self-help. The main philosophical difference between Watts and Locke is perhaps the result of the increase of faith in Newton's mechanics and optics and perhaps a result of Watts' sweeping if shallow rationalism. While Locke attributed certainty only to the existence of God, Watts thought that science may achieve certainty too. This difference relates to a slight methodological difference. Watts demanded from Christians a rational defense of their faith. His *Logic* came to serve as a Lockean means for constructing empirical defenses of religion and of science alike.

Watts was genuinely a religious rationalist: he was a religious tolerant in theory as well as in practice. He pronounced his rationalism explicitly. He was convinced that his reasoning led him to doctrinal certainty. He therefore could not deny that the same degree of certainty may be achieved in matters of science, a view that accorded with the increasing feeling of confidence in Newton's theories. He nevertheless felt very unhappy about it. For (*Logic*, Ch.4, §9),

> since it is possible that the folly or prejudices of younger years may have established persons in some mistaken sentiments, even in very important matters, we should always hold ourselves ready to receive any new advantage towards the correction or improvement of even of our established principles.

The phrase "prejudices of younger years" depicts him as a Baconian. He had to be certain, then, as long as he was pure of heart (*Improvement*, General Rule 10):

> fix not your assent on any proposition in a firm and unalterable manner, until you have some firm and unalterable grounds for it, and till you have arrived at some clear sure evidence; until you have turned the proposition on all sides, and searched the matter through and through so that you cannot be mistaken.

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[4] Hume (1779, end of Part I): "Locke seems to have been the first Christian, who ventured openly to assert, that faith is nothing but a species of reason, that religion is only a part of philosophy, etc." Hume ignored or was ignorant of Boyle's *The Excellency of Theology, compared with Natural Philosophy (As both are Objects of Man's Study)*. Boyle placed metaphysics above religion, since it is unthinkable that God would order faith in the absurd. Whether he was religiously tolerant all the way is questionable, even though it seems he was: he viewed all successful research as inspired and considered the Day of Judgment the day when all people will correct all their errors. A future study of this should rest on the distinction between Boyle's philosophical and political stands on toleration. Here his indirect correspondence with Spinoza should help. See Colie (1963).

As to Locke, he was famously not sufficiently tolerant to trust atheists, Roman Catholics, or Jews, and he advocated this view out of political considerations, not philosophical ones: it is hard to imagine he did not know Newton was a secret Unitarian.

Meanwhile, your degree of belief in your theory should be neither more nor less than the degree of the accepted evidence (*Logic*, 2, Ch. 4, §8):

> Let the degree of your assent to every proposition bear an exact proportion of the different degrees of evidence. Remember this is one of the greatest principles that man can arrive at in this world.

The pomposity of all this is perhaps somewhat grating, but in fairness to Watts we may add that his theory of rational degrees of belief is much preferable to that of its twentieth-century devotees.

This is Dr. Watts' explanation of his version of the doctrine of prejudice (*Logic*, 2, Ch. 3, Introduction):

> Rash judgments are called prejudices … When we use the word in matters of science, it signifies a judgment that is formed … before sufficient examination…
>
> … It is necessary of a man of who pursues truth to enquire into these springs of error, that as far as possible he may rid himself of old prejudices and watch hourly against new ones.

This passage repeats Boyle's requirement to test hypotheses repeatedly, providing a dubious psychological justification for this requirement, and amounting to the view that false theories have never belonged to science, that all our mistakes are prejudices born as views too quickly pronounced. Watts put all this as a part of his puritanical demand to be infallible. It includes the demand to avoid sense illusions too: they are nothing but prejudices, Dr. Watts declared. Prejudice abounds; love for one's own hypotheses is their source (*Logic*, 2, Ch. 4, §3):

> emotional prejudices: The cure of these prejudices is attained by a constant jealousy of ourselves, and watchfulness over our passions, that they may never interpose when we are called to pass judgment of anything: And when our affections are warmly engaged, let us abstain from judgment.

Watts recommended constant jealousy, especially towards idea we like. When Faraday discovered his laws of electrolysis, where, for the first time, discrete quantities of electricity appeared, he presented the idea of the atomicity of electricity with a long overture of precaution. But, he added to his presentation (*Experimental Researches in Electricity*, §869),[5]

> But I must confess I am jealous of the term *atom*; for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration.

Dr. Watt's distinguishing-mark for prejudice is the lack of clarity of ideas in Locke's sense that is an echo of Boyle's idea of the paucity of past tests of it. Thus, as Faraday could not himself test his theory of the atomicity of electricity, and as he felt strongly about it, he refrained from discussing it; he likewise discouraged other people from doing so; and the idea thus sank into near oblivion—to be very carefully touched upon by Maxwell and to be revived half a century later. Would a different attitude on Faraday's part have encouraged more research and if so would valuable results have come earlier?

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[5] Twentieth-century commentators on this passage have misread it for want of familiarity with Dr. Watts.

Boyle would not have recognized his views in their Wattsian guise. Perhaps Watts did not even read Boyle's methodological writings. Perhaps he got Boyle's methodology from Locke and from the practice of the inductive style that he so ardently defended; he obviously identified the views of Bacon and of Boyle as a matter of course. Consider the process of testing a hypothesis for years and years and then, when it is refuted, declaring that its refutation shows it to have been a prejudice from the very start. This seems scarcely reasonable. It is the moralizing involved that gave it its perverse appeal—to the extent that it ever had any. Locke had said carefully and in a non-committal manner that few people could be as stupid as Bacon had describes the vast majority of humanity. Watts said that to avoid being that stupid we must work very hard at keeping careful and watchful eye every hour day and night.

This was the eighteenth-century ideal rationalism: never indulge in emotions, dreams, and follies; always try to improve your intellect, and act according to the right measure of probability[6]; always be on guard, always blame yourself for past errors, purge your mind of them, stay cool and calm as you judge, and take care not to commit yourself too quickly. Typical of this mood is Dr. Johnson's friend who could not be a philosopher, he reported, in spite of good intentions, because he could not suppress his cheerfulness.[7]

Watts, the lover of reason and of humanity, amateur scientist and the ideally mediocre thinker, the preacher and the tolerant soul, the puritan and the adviser, the severe and harsh educationalist and the poet, he was probably the best representative of the enthusiastic preachers of cool and calm judgment. He preached the impossible. The result was somewhat comical and rather tragic. People emotionally blamed each other for having been too emotional. Lovers of new ideas blamed each other for old prejudices, for having acquired new prejudices, for not being watchful enough, for loving novelty, and for being in love with their own ideas.

Watts' major achievement was in his having put Bacon's doctrine in simple terms and reconciled it with his view of scientific practice, goading his readers to work hard: you can never be sure that you have cleaned your mind entirely, so go on cleaning it. No one tried so hard and so sincerely to follow Dr. Watts' teaching as Michael Faraday. The positive result of this was that he was very self-critical. The negative results were two. First, his great sorrow and pain at realizing that one day all his theories might be superseded—as prejudices, of course—and that he refuted the beautiful theory of Coulomb thus exposing this great philosopher as prejudiced. Second, his earlier works are very difficult to understand because, following Watts, he refused to express his views freely. More important for Faraday was Watts' demand for self-restraint and cool-mindedness. He felt very strongly about science; he used to reprimand himself every time he was angry with a stupid or a dogmatic

[6] This is an empiricist version of Kant's Categorical Imperative. His puritanism is marked in his views on education.

[7] 'You are a philosopher, Dr. Johnson. I have tried too in my time to be a philosopher; but, I don't know how, cheerfulness was always breaking in' (Boswell 1980, April 17, 1778, 957).

critic, and to force himself to become cool and calm and answer the critic in the friendliest terms or not at all. Watts' teaching contributed to inductivist quarrels and allegations of prejudice; with great effort it could also contribute to friendly relations between contending researchers, as the case of Faraday shows. When each party found it obvious that probability was on its side, their canons told them that they should settle their differences by calculating probabilities (to the exact measure, said Watts) and thus settle all disputes. That this inane advice did not cause a rebellion is odd. A person like Faraday should have been able to see through it. He did not.

16.3 Probability and Induction

Classical inductivism assumed:

(a) A theory becomes more probable the more it stands up to tests and arguments.
(b) A theory becomes most improbable or false once it is refuted, once the results of repeated tests contradict those expected on its basis.
(c) The more probable a theory is the more probable it is that it will stand up to further tests.
(d) Therefore, theories tend to become either most probable or easy to refute.
(e) Deviation of degree of belief in a theory from the right measure of its probability is a prejudice.

By contrast, modern inductivists assume

(a) A theory is more probable the more instances of it are observed.
(b) Instances that contradict a theory decrease its probability (disconfirm it).
(c) The increase of the number of observed instances of a theory beyond limit renders it absolutely true.
(d) The more confirming instances of a general fact, the more probable it is that the next case will also be a confirming instance.
(e) Belief according to the right measure of probability on the basis of available evidence is rational, logical (Keynes, Jeffreys), or profitable (Carnap).

The great difference between the classical and the modern theories of probability is that the modern theory substitutes evidence for test just because the formula,[8] the apparatus borrowed from mathematical probability proper, has no room for the difference between test, evidence, or hypothesis, taking probability to be a measure of

[8] The statistical apparatus helps determine in the first step whether given evidence is likelier than random data; it is a test of the relevance of a given experiment to the hypotheses under examination. Some modern inductivists took this to present the truth-likelihood of the hypothesis. They took a validation that one had anything to discuss at all as the validation of the hypothesis.

some relation between two objects.[9] The other difference is that meanwhile the theory that was the example of a highly probable theory—Newtonian mechanics—is by now superseded. Before Einstein, a superseded theory was a mere prejudice. Since 1905, defending science and considering Newton's mechanics a prejudice is unbearable. This led some inductivists to deny that theories are ever refutable. This is contrary to the theory of probability: it says, the probability of a hypothesis given an instance to the contrary is nil (as is the probability of a contradiction). The justification of the denial that theories are ever refutable is not from any theory but from facts: refuting arguments are refutable; hence, judgment may be reversible.[10] The funny fact is that inductivists ignore the uncertainty of evidence when it confirms but not when it refutes. Considering then a crucial experiment, where the same evidence confirms one hypothesis and refutes another, reveals a transparent bias in action. Philosophers who preach it are pathetic no matter how respected and serious they may be.

Other inductivists emulate John Stuart Mill's way of making do without theories at all, even without generalizations. Some went further and declared that by its very meaning a generalization is the assertion that the next case will follow it the way its predecessors did.[11] The denial of refutability makes the whole thing entirely irrational or uncritical, particularly since the theory was originally designed as a barrier against our alleged natural bent to cling to refuted theories. The recommendation to do without the hypotheses rests on the hypothesis that it is possible (perhaps even most efficient) to predict future events on the basis of past experience alone. This hypothesis belongs to Moses Mendelssohn and Pierre Simon Laplace (Todhunter 1865, 344, 452, 616). This was not any opposition to theories. Laplace had two inductive formulae concerning probability. One is the rule of succession so-called, that involves no hypothesis and concerns the probability of success of a (repeatable) fact. The other concerns the probability of hypotheses proper. For, the classical inductivist researchers of the eighteenth and nineteenth centuries (erroneously) considered these ideas harmonious with their views of the universe that they sincerely hoped to be able to express more mathematically and render them more universally applicable; their aim was to produce abstract, testable theories that would stand up to severe tests.[12]

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[9] Popper called this the autonomous probability system; both Keynes and Popper had presented it this way.

[10] The paradigm case is Prout's hypothesis according to which atomic weights are whole numbers when the weight of a hydrogen atom is taken as a unit. It was refuted and its refutation was refuted by the discovery of isotopes. Of course, the difference in weight between proton and neutron further refutes the hypothesis.

[11] The verification theory of meaning, the theory that an assertion that is not verifiable is meaningless, forces its staunched adherents to reinterpret any unverifiable meaningful assertion to mean some (allegedly) verifiable one, so that "all swans are white" should mean "the next swan that I will meet is going to be white". The egocentric aspect of this attitude is not psychological but the result of taking seriously Bacon's injunction to believe one's senses only. Compare the tortured presentation of this in (Toulmin 1953, 90–2) with that of (Popper 1959, 37 note).

[12] A few commentators have ascribed this view to Popper and ignored his protests against this ascription. His view is that a theory is scientific if it is testable. Does Newton's theory retain its scientific status today? By Popper's view, yes; not by the view ascribed to him, or by that of the Enlightenment philosophers.

The claim that a theory is more reliable the more it stands up to tests did not stand up to tests. Clearly, in technology some tests of applications of theories are required by law. These tests turn up in court cases of failures and testify that the individuals who were in charge of the application were not irresponsible (Agassi 1985, 220–3). Of course technology also applies quality controls, but these apply on the supposition that the system operates well and experiences only occasional setbacks. When this supposition fails the quality control system may break down (Agassi 1985, 157–9). This, however, is not to the point, which is the search for truth rather than for utility. Contemporary inductivists do speak occasionally about this aim, and the accepted theories as those that stood up to tests, although they do not admit that not all relevant observed evidence constitutes tests. Tests, Whewell and Popper said, are experiments that may, for all we know, refute the theory; tests risk the acceptability of a theory, namely the hope that it is true.

Thus far my comment on points (a) and (b) above.

Point (c) was the source of the trouble. It is the assertion that the ability of a theory to stand up to tests in the future increases by its having stood up to test in the past. The story of Newton's theory of gravity confirmed this assertion. Newton's theory of gravity stood up to increasingly subtle and severe tests; but then it fell, dragging down with it this statement and all other versions of the classical probability theory of induction. Points (d), the doctrine of prejudice, and (e), the identification of credibility with probability, crash when point (c) does: admittedly, highly confirmed theories were not immune to refutation. What then? Herschel considered Newton's optics a prejudice though, he insisted, Newton himself was not prejudiced. John Tyndall had trouble admitting that he shared Goethe's view of Newton as prejudiced, but he did it (Tyndall 2011, 70–2). This has far reaching consequences. It is unreasonable to demand the endorsement first of a corroborated theory and upon its refutation the view of it as a prejudiced (Cohen 1952).

And so the classical doctrine of prejudice has to go: we have better views of prejudice these days. If we stick to the literal meaning of the word "prejudice" to mean judgment that precedes all possible evidence, then all opinions are prejudices simply because the fund of evidence is unlimited. If, however, we limit concern to the reasonably easily available evidence, then we may easily deem Newton's theory of gravity a prejudice only after 1917. Moreover, we would say, those who were examining the evidence even after 1917 were not necessarily prejudiced. This discussion is acceptable, yet it skips a very important stage: why does it matter what people think? Bacon said, preconceived opinions spoil research. This idea is refuted. It is therefore not surprising that today discussion of prejudice has little or nothing to do with research and has more to do with legal aspects of xenophobia. We need not discuss these here.

There is an aspect of point (c) tests that deserves notice: as the obvious ways to test a new theory are exhausted, new ways to test it become increasingly scarce. Progress may allow suggestions of new kinds of test, though. Thus, it seems, no methodological argument helps decide whether a more successful theory has better prospects for standing up to future tests. This agrees with Boyle's view. Still, it seems most incredible that a product of the human mind will cover correctly all the relevant facts. This argument opposes Laplace's formula of probabilistic induction.

On his reading,[13] research starts with a theory whose vagueness renders probable that it would agree with the phenomena; raising its precision slowly and carefully while using many observations gets it right at each step. Applying this view in order to interpret the history of astronomy, Laplace said, the Ptolemaic system rests on observations. This agrees with Bacon's view: all theories rest on experience, yet unlike science prejudices rest on insufficient experience. Every detailed discussion of induction, then, raises the same question; how much is sufficient?

To circumvent this unanswerable question, try all possible hypotheses regarding the cause of the phenomena and arrive at the true one by the process of exclusion. This proposal is impossible, yet Laplace defended it (end of Ch. 17, Concerning various means of approaching certainty):

> This means has been employed with success; sometimes we have arrived at several hypotheses that explain equally well all the facts known, and among which scholars are divided, until decisive observations have made known the true one. Then it is interesting, for the history of the human mind, to return to these hypotheses, to see how they succeed in explain a great number of facts, and to investigate the changes that they ought to undergo in order to agree with the history of nature. It is thus the system of Ptolemy, which is only the realization of celestial appearances, is transformed into the [Copernican] hypothesis.
>
> … It suffices … in order to change this hypothesis into the [Copernican] true system of the world, to transport the apparent movement of the sun in a sense contrary to the earth…

Notice that from the idea of having all possible causal explanations Laplace moves to all known ones. And notice that he thus moves from the methodology of Bacon to that of Boyle. The general idea of this passage is that of nested intervals. Our theories are made to explain more and more facts. Yet the transition from Ptolemy to Copernicus is different. Laplace's wavering between Bacon and Boyle finds its clear expression in his wavering between viewing the Ptolemaic system a prejudice due to its epicycles and eccentrics or a somewhat valid if inaccurate phenomenology.

This, however, is important to notice (for the sake of consistency). The theory of progress by modification is incompatible with the probability theory, classical or modern. In the classical age, probability theory was rather popular but the theory of modification was not. Thus, the theory of the phlogiston was anathema and only Ørsted defended it as approximation to Lavoisier's theory.[14] Laplace advocated both inductivism and approximationism, as Priestley did before him. Clearly, this is how inductivism is viable: as a myth. Under the pressure of criticism, advocates of a myth move from it to something more reasonable—but only as long as the pressure

[13] Laplace (1814, end of Ch. 8, 183). "Philosophical" here means popular in contradistinction to "Analytical" or to "in mathematical language". Isaac Todhunter censures "the absurdity of attempting to force mathematical expressions into unmathematical language" (Todhunter 1865, 498). He refers to a formula that is much easier to read than Laplace's description of it in ordinary language. Nevertheless, Laplace's good will deserves appreciation.

[14] (Ørsted 1852, 300): "The Various Changes in Chemistry", esp. 307: "Every theory in Science prevailing throughout a certain period, contains actual Scientific Truth, though frequently much obscured".

persists. The main problem concerning approximationism is, how much can one theory deviate from another and yet count as its modification? Is Einstein's theory a modification of Newton's? Einstein said, yes, of course: he deemed any theory a modification of any other, no matter how much they differ, *as long as the observations derived from the one approximate those derived from the other under explicitly specified conditions*. His approximationism is better than that of Ørsted, yet both are methodological (not metaphysical). And clearly, inductivism and approximationism are incompatible. The one includes the doctrine of prejudice and the demand start research by to collecting as many facts as possible. The other rejects both of these ideas. Yet the vacillation between inductivism and approximationism is popular. It is a myth. Why do reasonable people who endorse approximationism still cling to inductivism? What end does it serve?

16.4 Probability and Reason

Classical inductivists were rationalists. For them rationalism was a way of life. They cast their rationalism in inductive language. Those who (like Kant) distinguished probability from reason had no hesitation and relinquished probability; those who (like Laplace) did not, still viewed probability as a justification of their rationalism, of their intellectual independence. Modern inductivism is different. Its adherents cling to facts in the wish for an authority to impose on them the views that they hold anyway. They are not so irrational as to accept other people's authority, but not sufficiently resolute in their rationalism as to resume their responsibility for their acting on the dictates of a theory. They want brute facts to impose theories on them. They hardly discuss rejection of theories, refutations, or error. They admit from time to time that predictions can go wrong, but they hardly discuss this. They often say, of course they know that humans are fallible, but this is not interesting. They want confirmations as these reduce error, they say.

The doctrine of confirmation of Rudolf Carnap gained great esteem, its appalling scholasticism notwithstanding. Here is his description of his idea of decision method (Carnap 1953, 138):

> The same method may be used to make rational decisions in a situation where one among various possible actions is to be chosen. For example, a man considers several possible ways of investing a certain amount of money. He can calculate—at least in principle—the estimate of his gain for each possible way. To act rationally he should then choose that way for which the estimated gain is highest …

The principle that allows calculating the outcomes of different ways of investment (of a given sum) is what makes Carnap's vision a pipe dream.

> … the Bernoullis, Laplace and their followers envisaged a theory of inductive probability which when fully developed would supply the means for evaluating the acceptability of hypothetical assumptions in any field of theoretical research and for making rational decisions in the affairs of practical life. They were a great deal further from this audacious objective than they realized. In the more cultural atmosphere of the late 19th and early 20th

> centuries their idea was dismissed as Utopian. But today a few men dare to think that these pioneers were not mere dreamers.

A rationalist *à la* Carnap has two kinds of decision problems: of the selection of hypotheses and of the selection of actions. He seems to have considered the first problem a part of the second.[15] He seems to have contended that the end of the choice of a hypothesis is action upon it. The end of rational action is then to gain by the use of the proper hypothesis, and its rationality is in its accord with some inductive formula. Gain is a general utilitarian concept that seems to be the axis of Carnap's theory. He summed it thus (Carnap 1950, 252):

> Our problem is to find a rule which tells a man X, with the help of inductive logic, which decisions it would be reasonable for him to make in view of his past experience. Such a rule does not belong to inductive logic itself but involves the methodology of induction and of psychology. In this section four tentative forms of the rule are discussed, each more adequate than the preceding ones. The final rule will be explained in the next section.

The rule itself, in its fourth and final wording, is (269),

> Among the possible actions choose that one for which the estimate of the resulting utility is a maximum.

By "methodology of induction" Carnap, meant (262) a theory that "gives no exact rules but only advice how best to apply inductive procedures for given purposes". He presented utility as the axis upon which we base even the utility of the scientific researcher achieved by crafting good science (243):

> there are also cases in which there are good reasons for the expectation that the application of induction will become useful for the scientist, or in which useful application is already possible today.

Carnap did not say more on the application of induction to scientific research and he returned to the more general problem, the need "to find a method for measuring the (positive or negative) utility of a gain (or loss as a negative gain)", concluding thus (267):

> It must be admitted that there are some … serious problems involved in this assumption of the possibility of measuring the utility of all advantages and disadvantages for a given person at a given time on the basis of one common, one-dimensional scale. But something like this assumption is usually taken as a basis of an analysis of what is called "rational behavior" in many parts of social science, especially in economics and ethics.

This is a frank submission to received opinion. No situation appears in Carnap's voluminous *Logical Foundations of Probability*, except that of a fair bet: there is a gain or a loss of whatever the player decides that it is, and the player has the choice to play or not to play. To increase the probability of gain is to follow the (logical) rules of probability, and to do this is to act rationally. (Cheating is tacitly excluded, I do not know on what ground.) This Carnap presents as a daring thought and as the perpetuation of Laplace's Utopian dream.

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[15] Frank Knight and John Maynard Keynes distinguished between risk and uncertainty. Carnap's unspecified principle suggests no uncertainty, only risk (Runde 1998).

Laplace had claimed that he could show the irrationality of the lottery system (Laplace 1814, Ch. 16, esp. 161–2; also 28, 54). What is the good of lotteries? Where can any profit come out of them? They cheat the public by highlighting the tremendous gain of the successful person, in oversight of the total loss that must be larger than the gain.[16] Carnap claimed the opposite. All modern inductivists admit all fair bets to be rational. They are no gamblers; Carnap would probably never have considered becoming a successful gambler; he would not give up his intellectual activity for all the tea in China. And yet, he understood the formulas of the calculus of probability better than Laplace did. Had Laplace understood the formulas as well as Carnap did, he would have rejected them. Carnap had no such excuse. Likewise, Laplace was Utopian; Carnap was not.

Consider Hume's evaluation of the activity of the French researchers of his day ("The Rise of Arts and Sciences"):

> What checked the progress of the Cartesian philosophy to which the French nation shewed such a strong propensity towards the end of the last century, but the opposition made to it by the other nations of Europe, who soon discovered the weak side of that philosophy? The severest scrutiny which Newton's theory has undergone, proceeded not from his own countrymen, but from foreigners; and if it can overcome the obstacles, which it meets with at present in all parts of Europe, it will probably go down triumphant to the latest posterity.

This passage of the great skeptical philosopher is characteristic. Cartesian philosophy he deemed a prejudice; the results of tests of Newton's theory, carried out by foreigners, are less liable to be prejudicial and chimerical confirmations of it than those carried out by compatriots; genuine confirmations are gained by the "severest scrutiny", by the ability of the theory to "overcome obstacles"; excellence is a function of genuine confirmation; and, finally, past success increases the probability of future success. The greatest skeptic of his day could not seriously doubt the validity of induction and the truth of Newton's theory.

Hume was advocating rationality, not success. Had he known that probability theory does not justify his demand for rationality he would discard probability, not rationality. The essential point about his theory of rational behavior is that it embraces both moral and natural philosophy. As nowadays this ideal of complete rationality is no longer in vogue, we may underestimate the difficulty in rejecting the classical inductive theory of probability that sustains it. Since today it is an unimaginative farce, we may tend to forget how much more forceful it was in its heyday. For classical inductivists probability theory meant the basis for their demand for enlightenment, the search for truth, and the attempt to improve the conditions of humanity. Today it is an excuse for conservative politics (Agassi 1995). It was Heinrich Heine who first used the political implications of a philosophy (in any sense of the word) as the litmus test for its claim to be reasonable. This raises the obvious question: is the basis of all philosophy political as Marx taught, or is it

[16] Hayek responded to Laplace's argument, claiming that the lottery costs marginal fees and makes some individuals rich, so that the economy on the whole is better off, and it does so while giving pleasure to the players.

morality as some of Heine's followers suggested, or is it rationality, as the Enlightenment movement bravely advocated? The answer is, there is no foundation for philosophy other than, as Heine said, the inalienable right to admit error. He called himself the last Romantic, but he was the first of the new enlightenment, the philosophy of the Enlightenment Movement sans their promise of infallibility.

Appendices

Appendix A: The Riddle of Bacon Today

Twentieth century Bacon scholars ignore the riddle of Bacon, as well as the rest of the lifework of Robert Leslie Ellis, even though most of them endorse his reading of Bacon's texts. The following few pages expand on this.

Following Ellis, today's scholars do not ascribe radicalism to Bacon (Agassi 2008, 125). This is understandable. Scientific philosophers ignore their radicalism as the proverbial fish are allegedly ignore water. Radical political philosophers are painfully aware of their radicalism which they often identify it with scientific rationality. Others dismiss it unjustly. Popper was the first to present it as an important innovation objectionable as anti-democratic (Popper 1945, i, 166–7). Then, J.L. Talmon's *The Origins of Totalitarian Democracy* (Talmon 1952) is a historical presentation of it in detail that impressed scholars greatly; F. A. von Hayek endorsed it (Hayek 1960, 80–1), and made it the received view. It awaits its application as a solution to Ellis' problem.

To the extent that there is a canonic reading of Bacon's works, it is that of Ellis, since John Maynard Keynes has given it his blessing (Keynes 1920, 271). Yet Ellis' problem about it is ignored. The opposition to it is from a few writers—most significant among them is Paolo Rossi—who ascribe to Bacon Whewell's view attenuated, or a mix of Bacon and Whewell (often mixing the two hypothetico-deductivists, Whewell and Popper). The major difficulty of Ellis (his inability to ascribe to Bacon a major innovation), they all ignore.[1] There are some exceptions. These are light-weight.[2] Conspicuous among them are two Bacon scholars: Charles

[1] Keynes referred to the view of Ellis that the principle of limited variety is Bacon's contribution, perhaps as a solution to the riddle of Bacon, I cannot say: anything to do with this principle is murky.

[2] Example: Wheeler (2001): "Through a series of interpretive errors and mis-translations of Bacon's Latin originals, his science fell into an eclipse from which it has only recently begun to recover."

J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8,

Whitney (1989) and Antonio Pérez-Ramos (1988). Of Whitney I can say very little.[3] As to Pérez-Ramos, following Paolo Rossi (see below), he has dismissed as simply misguided the strictures of all of Bacon's leading critics of the last two centuries.[4] Unlike Rossi, though, he has suggested that we have a partially satisfactory view of Bacon's contribution of a new and important idea: it was the idea of secular research.[5] This is a new term, and I do not know how to read it: to separate the religiously motivated research from the secular is hardly possible (impossible, if Boyle is even partly right). The wish for secularization, Bacon's or anyone else's, is supremely important, but it was institutional, the wish to see the founding of a secular college devoted to research. (Secular colleges were not new: all traditional free universities or city universities were such. Only academic research is new.) In Bacon's *The New Atlantis* members of Solomon's House that is secular conduct research (although they also do conduct ceremonial prayers). This idea was not new: if not in antiquity, then at least in Thomas More's *Utopia* it appears clearly enough (see above, motto to Chapter II).[6]

Before commenting on the two contributions of Pérez-Ramos to the *Cambridge Companion to Bacon* (Feltonen 1996), let me report briefly on that volume. For, it may very well serve as a fair representative of the scholarly quality of current Bacon scholarship [7]: the high standard magnificently characteristic of Ellis is not that of the editor's introduction to the *Cambridge Companion to Bacon*.

In the very opening of that introduction the editor says, "for some present-day epistemologists" Bacon's view of induction is "hopelessly naïve" so that "it had nothing to do with the development of science". To this he adds: "In striking contrast, the Frankfurt School criticized Bacon for being the very epitome of the modern scientific domination of nature and humankind." Let me ignore the absence here of any contrast, let alone a striking one, as it is irrelevant to the riddle of Bacon: Ellis would have never considered Bacon's idea of the domination[8] of nature as

[3] Whitney (1986, 1989) offer a bewildering flood of references and analogies. Whitney deserves praise nonetheless for his citing Bacon to say we must start afresh.

[4] Pérez-Ramos (1993) has a rich, useful bibliography.

[5] Pérez-Ramos (1993, 141–2) ascribes this idea to Thomas S. Kuhn, to his contrast between the mathematical and experimental traditions (Kuhn 1977). This is a slight exaggeration: Kuhn hesitantly ascribed some "qualitative" novelty to the Baconian movement suggesting that perhaps it was "very small": he was in awe of Koyré's dismissal of "the Baconian movement as a fraud". Koyré was greatly mistaken here in a manner uncharacteristic of his admirable disposition to see value wherever possible.

[6] Of course, Bacon's idea of amateur research also makes it secular, and willy-nilly; commentators ignore this.

[7] Another amusing example is Zagorin (1999, 261, note 60) that censures Popper for having read Bacon's "anticipations" as hypotheses whereas they are hasty inductions. It seems to me no mean an achievement to write a book on Bacon and remain ignorant of his view that all hypotheses are hasty inductions. The view of fantasy as re-combinations of the given follows from the Aristotelian view of perception, and so it was current throughout the Middle Ages; Bacon adapted it to his doctrine of prejudice.

[8] The idea of domination of Nature seems to conflict with the idea of obedience to Her. Bacon explained; the suitor stoops to conquer.

the cause of his fame, just as he would not have accepted Liebig's solution that is the view of Bacon's *Essays* as that cause. The same holds for Thomas Fowler's claim that Bacon's message was not new but his having "shouted it from rooftops" is, we remember. Nothing will make Bacon intellectually significant the way Ellis thought he must have been except an important philosophical idea proper.

The editor observes (still on page 1) that "tremendous strides have been made in Bacon scholarship. … Anachronistic criteria for assessing Bacon's philosophy have been abandoned." He also mentions a few neglected areas of Bacon's works that are now under study (page 2): "Bacon's speculative philosophy represented a hodge-podge of ideas taken from various sources and put together in a way which would have surprised the originators of these ideas" (18). A final page of the introduction goes to the impact of Bacon's ideas. The founding of the Royal Society attained a "full translation of these ideas into action" as a "final victory of the Baconian project of collaboration,[9] utility and progress" (23). Further, "the French *philosophes* revered him as the most important propagandist for science (although they ignored the more technical parts of his philosophy)." To this the editor adds that the technical part of Bacon's output was later rejected with contempt so that "it lost its relevance".

The first paper here is "Bacon's Idea of Science" (25–46) by Paolo Rossi, the doyen of Bacon scholars of today.[10] He valued Bacon's work as a rebellion against scholasticism (223). In his *The Birth of Modern Science* (1997, 2001) he criticizes the view of science as "dry", applying this epithet only to scholasticism (Rossi 1968, 26), thus ignoring Bacon's demand that researchers better be unexciting to prevent their personal involvement. Rossi dismisses the view that Bacon was an inductivist (44–5), considering him a hypothetico-deductivist. He relied here on Peter Urbach (1987; Agassi 1989). This leads Rossi back to scholasticism that is permission to defend any thesis. This can be done (Nabokov 1962, "Commentary").

The next two papers here are on Bacon's classification of knowledge and on his methodology. The latter reports that although his view of science "went to a dead end", it is of historical importance (75). I could not see why.

Pérez-Ramos is the only contributor here who refers to Ellis, but only apropos of a point that he finds insignificant: he declares that "Ellis wrote apologetically" about the doctrine of Forms: it "is in some sort an extraneous part of Bacon's system" because it belongs "not to natural philosophy but to metaphysics" (100). Pérez-Ramos complains: commentators on Bacon's theory of Forms wrote not as historians but as philosophers.

The next paper has a promising title: "Bacon as an advocate of cooperative research" by Rose-Mary Sargent (2003, 146–71). She noted that Bacon had tried (in vain) to secure funds to build colleges and libraries. This had two new characteristics,

[9] Current literature ascribes to Bacon the view of collaboration in research (Heilbron 2003, 75). Bacon never wrote of collaborative research. At most he had a "principle of division of labor" (Zilsel 2000, 17).

[10] Rossi's attention is admirable to the rapport between science and magic prior to the scientific revolution and beyond. That was one of the major interests in his scholarly career. Particularly significant here is his attention to the secrecy involved in the magical tradition that the scientific tradition bravely opposed.

let me add: one that he thought big, as he always did; the other that he included the new idea of building laboratories.[11] Sargent noticed rightly that Bacon had inspired efforts of researchers of the next generation to construct a secular college.

The last essay here is of Pérez-Ramos on "Bacon's legacy" (311–34). He offers a solution to the riddle of Bacon. "No science can exist without a specific ethos and Francis Bacon was the first explicit and articulate theoretician of an ethics of science in the form of a message embodying values, visions, hopes, and rational expectations" (312). Now since "no science can exist without a specific ethos" and since science existed before Bacon, then Pérez-Ramos is in error: there was such an ethos before Bacon's time. So the novelty of his message is in his articulation of a new ethos. What is it?

Pérez-Ramos notes that Descartes, Mersenne, and even Gassendi had read some Bacon. He says they distorted Bacon's ideas. Also, unlike his English followers, they ignored his new "pedagogical and institutional tenets" (312–14). If these are important, then Pérez-Ramos could have offered a new solution to the riddle of Bacon by stating them. He did not.

Bacon's influence on the educational ideas of Jan Amos Comenius was important. It is not clear what it was, however. This *Handbook* does not help. Feltonen dismisses (Introduction) the claim that Bacon's critics lacked historical sensitivity. This is now the generally received view. It is indeed historical sensitivity that made Ellis seek in Bacon's works some interesting new idea to explain his tremendous intellectual prestige. The idea that renders his fame just, I claim, is his radical attitude to science, the idea that research must begin *de novo*, that is both very important (though erroneous) and a part of his influence as understood by his immediate followers.

Appendix B: Boyle's Philosophy of Religion

Boyle's first famous publication was his *Seraphick Love* (1659). I was amazed to learn that it is only a small part of a larger work (Principe 1994, 250), since it is so verbose that it is hard to read.[12] Here then is a *précis* of *Seraphick Love*. (The numbers denote sections.)

(1) Seraphic love is higher than ordinary love, and should be dedicated to God alone. Ordinary earthly love can be transformed into Seraphick love and (2) the latter is surely preferable. (3) All disappointment from earthly love should lead to Seraphick love, and this should transform into devotion (4) to which there can be no limit but complete self-denial (5) that in its turn should relieve one of the pain of'

[11] This idea he already envisioned early in his interest in philosophy; see Ch. 1, note 10 above.

[12] Perhaps this is a misconception: Principe notes (256) that the manuscript has "embellishments, qualifications, and extensions" added. Perhaps not: "Various rhetorical devices (chiasmus, preterition, personification, etc.) are added, but metaphors and similes make up the largest share."

the disappointment. (6) Moreover, earthly love can be a vehicle for the achievement of the seraphic kind, (7) for all love is unselfishness and devotion. (8) Still, only Seraphick love brings a complete peace of mind.

So far love; now to its object: (10) Being perfect, God is the best object for love, and surely better than a woman. (11) He is so perfect, that only some of his attributes are available for study, one of which is the material world that we should explore scientifically. (12) The more we learn the more we see how little we know and our admiration increases. (13) God's love is great since it is real, (14) disinterested, (15) (since all that we can give him is his) (16) and constant. (17) The differences between the different Protestant churches are small. The main thing is to assess our freedom of will. (18) God granted us this freedom and all else. In return we ought to love him. (19) But it is not with an eye on the reward in heaven that we should labor. (20) Besides, the greatest joy on earth is the intellectual pleasure achieved by studies of Scriptures and of Nature. (21) In heaven we shall have a greater joy, as there all our desires will be met, and there we shall see all the great people and Christ Himself, with whom we shall praise God. (22) For, Christ used always to converse with common people. (23) Moreover, there we shall unite with our friends and relations. (24) There we shall understand (a) the mysteries of religion (b) the passages in Scriptures that we cannot now properly interpret and (c) the seeming evil in the universe that has led even Job to question God's behavior. "I should value the having my understanding gratified and enriched with truths so noble … enough to court heaven at the rate of renouncing for it all those unmanly sensualities and trifling vanities …" (25) The purpose of creation of Man is learning of Nature and of God, which also brings the greatest satisfaction. "In God there is … such various Identity that the fruition of him both satisfies and creates desire: though that without satiety; and this without disquiet." (26) Such is the Promised Land, the celestial Canaan that we enter in a real brotherhood that increases and grows. Here comes a sharp change of tone, from high enthusiasm to earthly prose. Boyle apologizes for having taken too much time of his reader, promising compensation by "riper discourse of the nature and duties … of this love" in the hope to meet him in heaven.

Boyle wrote this work when he was 22 years old, and when he probably had a real heartbreak over some earthly love affair. This may explain the sentimentality, especially since the introduction suggests that Lindamore, to whom the work is addressed (perhaps a cousin, but unlikely: Boyle's description of Lindamore makes him too similar to Boyle), is identical with Boyle's emotional self, and that the work was intended not for publication but in order to convince his own suffering self as a kind of self-therapy. (He probably saw this as a success: Lindamore was dead, he reported in the epistle dedicatory.) He first showed the manuscript to John Evelyn in connection with Evelyn's contribution to the ongoing discussion about a suggestion to create a secular research college. He published it soon after Evelyn saw it. The great excitement that the manuscript caused Evelyn, it seems, rests on his understanding that Boyle would contribute the funds necessary for the establishment of the desired college. The society that they founded soon was then a stage towards this, and indeed, the society was initially associated with Gresham College. Boyle never intended to gratify Evelyn's wish.

Boyle's other self,[13] Lindamore, was dead, suggesting that Boyle had acquired the peace of mind he was advocating; it probably never left him.[14] It is possible to ignore Lindamore altogether and discuss the philosophy of this book alone. This is hardly useful, as Boyle's later works offer better opportunities for this kind of study—except in the study of the roots of Boyle's philosophy and its very inception. The most obvious comparison, then, especially of Boyle's theodicy with that of Leibniz (Loemker 1955), is scarcely of interest for that study. Rather, the traces of similarity with ideas of Spinoza are more useful to this end, since possibly these two thinkers share some roots in the philosophy of Maimonides. This too is of no interest for the present study. And the same goes for Boyle's possible influence on Spinoza who was almost his age (born in 1627 and in 1632). Freeing the intellect from authority may lead to at least two unusual, interesting trends, one of excessive self-assertion, like that of Paracelsus and of Bacon, and the other to an enviable peace, like that of Boyle and of Spinoza. Similarity between ideas of contemporaries is possibly better explicable by shared experiences and problems than by shared roots. The allegation that Boyle "understood the Hebrew very well and had made considerable progress in the rabbinical writings" and the assertion that "he carried his study of the Hebrew tongue very far into the Rabbinical writings" come from Bishop Burnett's obituary that flaunts a list of blunt exaggerations. To the extent that there is truth to Burnett's obituary, it may be noted, Boyle made most of his Jewish studies before he wrote *Seraphick Love*. The idea of the intellectual love of God clearly appears in that work of his, though less emphatically than in Spinoza's work. It is already Maimonidean if not plainly Socratic. As to the idea of the material world as an attributes of God and its development, I cannot say how much Boyle influenced Spinoza, for what he said of it is much less in volume and significance then what Spinoza did—which is not much either.

The comparison of young Boyle's views with those of Descartes is more interesting. Yet a better source is his work-diaries (Hunter and Littleton 2001) and his correspondence (such as his letter to Hartlib cited in Ch. 14 above). The most important aspect of Boyle's thought is his methodology, where, unlike Bacon or Descartes, he found no need to explain human fallibility and he rejected the idea of essences. As to religion, his independence expressed itself in his deviation from Descartes' metaphysics: unlike Descartes, he ascribed to the mind at least two properties—reason and emotion. The field of reason includes rational theology or natural theology, i. e. the theory that God is the maker of the natural clockwork whom we worship by learning and by implementing justice. This Boyle christened as "the Physico Theological proof of the existence of God". The field of emotion includes moral sentiment, namely, charity and revealed religion. The only demand which

[13] The editors of Boyle's *Works* mention that he describes Lindamore as a cousin whom they cannot identify.

[14] Michael Hunter, the greatest expert on Boyle by far, has presented him (Hunter 2000, back cover) exceedingly differently, "as a troubled figure, plagued by religious doubt, ambivalent about magic, and convoluted in his relations with the wider world". This is thought provoking.

rational theology can make from emotional theology is not to believe in obscurities. Here reason is above religion, since it is absurd that God would demand from us to believe irrationally. Otherwise, faith is "above reason".[15] As Bacon said, miracles come not to convince the heretic but to encourage the believer. They are not necessary, they are results of God's charity; religion (proper) is a part of the field of charity. Thus, the mind has two faculties and two religions: natural theology and justice, as well as Christianity and love and charity.

Boyle took part in Oldenburg's correspondence with Spinoza on matters both religious and scientific. Oldenburg urged him to believe in Christianity. He answered well and rationally. Then it appears, Boyle (who never corresponds directly with Spinoza though he let Oldenburg express his views) interfered and criticized Spinoza—not on his disbelief in Christianity but on his disbelief in the freedom of will, on his determinism.

On this Boyle and Oldenburg had the upper hand. Abraham Wolf, the first editor of Spinoza's correspondence, bitterly mocks at both Oldenburg and Boyle who just wanted Spinoza to become a conventional Christian. He was right in considering Boyle as a party in the dispute. Oldenburg did wish to save the soul of his beloved friend Spinoza. When Boyle entered, the dispute over miracles dropped out and freedom became prominent. Boyle took as a matter of duty not emotional religion but natural religion. To defend this view he used the idea of the freedom of the will. This encouraged his tolerance in matters religious and his use of rational argument as he moved from revealed religion to natural religion. It became emphatically present in one of his latest works, in his (posthumous) *Christian Virtuoso*, that emphasized revealed religion but used rational arguments—the same arguments as those he had used in previous works of his, or very similar ones.

Boyle's explanation of this shift of emphasis—from established to natural religion—is present in his claim that in his work he was addressing a virtuoso, one who is already a researcher. This is unimpressive. I wonder if he was not alarmed—as Bishop Berkeley was—by the religious skepticism that captured his younger contemporary Edmond Halley. Perhaps it was in the vogue among the Virtuosi then. We know little about it because of the intolerance of the time, of course. And if this is the case, it explains Boyles' reticence: he found it politic not to speak of the possible ill effect of the new experimental philosophy on religious observance (MacIntosh 2005, General Introduction, 8, 174). Be that as it may, the major point that Boyle enlarged upon is his discussion of the physico-theological proof for the existence of God. He asserted that he assumes his reader was a Christian, but of course he did not, since he was arguing for the existence of God. His attitude towards the Royal Society and the missionary institutions may relate to the two religions that he sponsored, the natural and the revealed (Christian or Anglican as you will): he left no money science, only for charity and for missions.

[15] This is not very revolutionary. In Judaism it is traditional. Galileo took it for granted. In his *Letter to The Grand Duchess* he cites Cardinal Baronio: the intention of the Holy Spirit is to teach us how to go to heaven, and not how heaven goes.

To conclude, Boyle was first a rationalist and then a Christian even though still extremely pious. His Christianity meant both charity and some historical beliefs. Although metaphysics is above religion when general ideas are concerned, the historical truths that lie in the foundation of religion are “things above reason”.

Appendix C: Boyle’s Attitude Towards Financing Research

C.1 Boyle and Money

Boyle, the son of the fabulously rich First Earl of Cork, was as closed-fisted in his attitude to scientific institutions as he was open-handed to charitable and missionary ones. His having bequeathed to the Royal Society only his scientific collection, and even then, while explicitly excluding the precious stones that it contained, is strong evidence that this stinginess towards science was with him a matter of principle.

The principle behind this attitude of Boyle is rooted in his philosophy. Descartes, we may remember, assumed the mind to have no other faculty but that of thought; sensations he considered to be bodily, and feelings to be a hybrid between the two, as it were. Boyle disagreed: he viewed the mind as having two distinct faculties, reason and emotion. And while only natural religion is rational, revealed religion, Christianity, is emotional: it was God’s charity that he graced unreasonable people and gave them a second chance of believing in Him—by appealing to them through revelation.

Boyle’s philosophy was very popular in the seventeenth century; his influence was even greater in practical matters by laying down the etiquette of the scientific community. The search for money in order to finance the cause of science ceased under his impact and gave way to his idea that science should depend on wealthy amateurs. This idea remained popular for a long time.

This changed early in the nineteenth century. In his *History of the Royal Society* of 1848, C. R. Weld expresses no small bitterness towards those who caused the failure of the scheme (1662) to secure some of the lands confiscated in Ireland for the use of science. He disliked Boyle’s refusal to bequeath any property to the Royal Society, not even the gems which belonged to his scientific collection. Flora Masson shared Weld’s feeling, but in her life of Boyle (Masson 1914, final pages) she only hinted at that.

The new attitude became explicit and not blurred by courtesy in L. T. More’s life of Boyle (More, 1949). More noticed, no doubt, the consistency with which Boyle avoided supporting financially the cause of science; he found only two documents that are exceptional. I shall now discuss them.

C.2 The Confiscated Irish Lands

The story of how some Fellows of the Royal Society tried to secure some of the confiscated Irish lands for the Royal Society (referred to above) might explain the fact on which Boyle reported (*Correspondence*, 2001, 2, May 1662) to a relative of his, the Bishop of Cork, Dr. Michael Boyle:

> … But in regard my Intention in generall, was to apply an Addition of Revenue, if my friends procur'd any for me, to good Uses, though I confess I design'd it rather for Advancement of real Learning than to any other purpose, Yet since it soe falls out that unknown to me it is cast upon Impropriations, 'tis very likely, that by the Accompt I expect of the State of them, I may *see* Cause to make the more immediate Service to Religion (by releeving the Poore in those Places, & contributing, if need be, towards the Maintenance of Ministers there, or promoting other good workes (there or elsewhere as from time to time occasion may require) … [for this 2/3 of the total sum, and 1/3 of it for missionary work in North America].

It is strange that Boyle should deviate from his philosophy and intend to support science financially. One may assume that this intention rested on the fact that the land had been procured for him for this purpose. This assumption explains why Boyle used his claim (that Hunter deems disingenuous (Hunter 2000, 75)) that he had been unaware of the transaction as permission for him to make the decision to spend the property in question for charity and missionary work instead. He meant to say, then, that since he had not been consulted, he was not bound by others' intentions.

This is my reading, not that of L. T. More. He suggested (102) that "under more favourable circumstances Boyle might have been persuaded" to use the land for the sake of promoting science. He did not say what kind of circumstances he had in mind. Rather, to support his conjecture he used the contention that a few years earlier Boyle had been persuaded to support research. To me this seems to be hardly a supporting argument; rather I see it as an additional problem. It is strange that Boyle did not keep a promise to support the cause of research at the very first opportunity he had. It is especially strange since we know of no other case in which he broke a promise, and that his moral code, and his adherence to it, were proverbially strict. Thus, the whole situation strongly points at the possibility that Boyle never made the alleged promise. A glance at the document on which this allegation rests, a letter from Oldenburg to Boyle, shows that it is very problematic.

C.3 Oldenburg's Letter to Boyle

Oldenburg's letter is of September 1657. Here is the relevant part of it (*Correspondence*, 2001, 1, 239):

> I am hugely pleased, that the council hath granted your desires for the promotion of knowledge; which I suppose to be those, that we couched in certain petition, you were pleased to impart unto me in Oxford; wherein, if I remember well, a matter of 12000 lb. sterl. was offered to purchase confiscated land and houses with in Ireland, and to commit the profit thereof into the hands of certain Trustees, for to employ in it the entertainment of an Agent, Secretary, Translators, for keeping intelligence, distributing rewards, etc., in order to the end aforesaid. I beseech you, Sir, to favour me with acquainting me with the progress of this business and, if it discomode you not too much, with what else occureth notable in England: half a word, or a word *used per antiphrasin* (since you must use the pen of a servant) will be enough to make me understand your meaning, especially if you shall please to add but a N.B. to such matters, as you shall not think fit to speak freely and plainly of to such kind of persons.

The idea of getting money from rich donors to build a secular university was in the air then. Yet the present document cannot pass as direct evidence without a discussion of a simple question: why was the explanation to Boyle necessary? Was he not supposed to know what he wanted to be done with his money? And why was it done in secret? Why does Oldenburg expect an answer *per antiphrasin*?

I conjecture that the whole thing is *per antiphrasin*. Perhaps Oldenburg spoke of a plan to sell land for Boyle, not to buy land with Boyle's money.

The beginning of the same letter may be entirely irrelevant to the above quotation, but it may also be the clue to the riddle; here it is:

> Making use here of an Italian, to teach your nephew something of practical geometry and fortification, I found he had a way of writing to others very secretly, which though he would not exchange with me for another, I have by your favour, yet he did it at last… And thinking you have it not this way, I did venture to send it here inclosed, being tried by me, and doing very well. It may be of great use, amongst others, for besieged towns, to encourage them with unseen promise of succourse, unsuspected by the besiegers, the white characters being written over by black ones, of quite another sense, which may be such, as that the besiegers, if the message should fall into their hands, would not stop, but rather be glad to let pass; which yet the besieged, by a water mention in the receipt, blot out and make appear what concerns them and their relief.

Possibly the whole letter is written in this method — the visible part of it being the *antiphrasin* of the invisible part of it. As this is the only letter of Oldenburg I know of in which address and date are absent, I asked (1954) the permission of the Royal Society to use the chemicals mentioned in Oldenburg's letter in an attempt to detect the date and the address. I was refused permission although no risk was involved.

P. S. At this stage I decided to wait for more favorable circumstances for an attempt to test my hypothesis. Having incorporated it in my doctoral dissertation, submitted to the University of London in 1956, I put it aside until I read that a collection of Oldenburg's correspondence was in preparation. I sent this note to *Isis*, in the sincere hope of clearing this strange case before the appearance of that worthy publication. It was rejected. Meanwhile, Boyle's works appeared in a new edition and many publications concerned his philosophy. So I resubmitted this note to *Isis*, with the same result, this time for the reason that it ignores the huge body of recent literature concerning Boyle and Oldenburg. The referee did not take the trouble to show that this literature is in any way relevant to my discussion. It is not.

Appendix D: Robert Boyle's Anonymous Writings[16]

The following brief study touches upon a very small fraction of the vast anonymous theological literature of modern times. The very concept of anonymity has a diversity of flavors: even the mock ascriptions of the *Song of Songs* to King Solomon or of

[16] This is an expanded and corrected version of a paper published in *Isis*, 1977, 68, 284–87.

the *Book of Splendor* to an ancient rabbi are forms of anonymity, as are the works of pseudo-Aristotle and their likes, or of the popular *Fons Vitae* and its likes. Proper anonymity on the modern sense is possible only in a culture where authorship is usually boasted of. Hence, properly anonymous works could not appear before the scientific revolution. Dating this revolution can be somewhat flexible. Some take 1600 or thereabouts as the proper cut-off date, with the burning of Giordano Bruno at the stake as the last act of its kind of flagrant medieval barbarism. Others take it to be the year 1660 or 1663, the date of the founding of the Royal Society of London and the institution of its standards of scientific protocol—standards that were adopted as a matter of course by all its daughters.

Robert Boyle, the brain behind the institution of these standards, was a very prolific writer who published many theological works, some of them anonymous.

Boyle's early *Seraphick Love*, published first anonymously and soon after under his own name, is very important. It defends natural religion and declares research to be the natural worship of God. When John Evelyn read it—in manuscript—he wept; it moved him to call for the meeting that declared the foundation of the society that later received a Royal charter.[17] The rest is history.

As Seraphic love was an anonymous publication of a beginner, it is not problematic. Boyle published anonymously an earlier work (on the need to publish freely on medicine it was), and then his *Seraphick Love*—soon to republish it under his own name. Such anonymity is obviously a mark of the hesitance and shyness of a young beginner, justified both by the ambitious nature of the work and by the fact that it has a strongly personal touches: its author recommends research as a sublimation of unrequited love. Though the personal touch almost never left Boyle and so betrayed his authorship of some anonymous theological works, the mask of anonymity of his later works is puzzling—except perhaps the anonymity of his powerful *Protestant and Papist*, written (1687) just before the Glorious Revolution (1688), when an avalanche of anonymous theological works descended on England. Both that literature and the anonymity are politically explicable, but not so the rest of Boyle's anonymous output.

The particular interest in the rest of his anonymous writings derives from the fact that most of them are theological and written in the days when theology was of supreme concern both politically and scientifically. Canons of toleration *cum* separation of religious discourse from both scientific and political ones were soon established. At least in part, the success of their implementation was due to intellectual activities in which Boyle had a significant part. And so, the study of Boyle's anonymous output may serve as a useful case study in the history of a whole genre: the anonymous theological works in the Age of Reason.

[17] Boyle published the book soon after Evelyn saw it. The great excitement that the manuscript caused Evelyn, it seems, rests on his understanding that Boyle might contribute the funds necessary for the establishment of the desired research college. The society that they founded soon comprised a stage towards this: it was initially associated with Gresham College. Boyle never intended to gratify Evelyn's wish.

Any approach to Boyle's writings has to take account of two major figures in the field, and the present note will consider only them. First is Thomas Birch, the celebrated author of the only eighteenth-century history of the Royal Society (Birch 1757), who edited Boyle's *Works* and included in his edition all of Boyle 's anonymous works we know of, except for *Protestant and Papist*, and at least one, perhaps more, anonymous work not by Boyle (see below). Second is the famous Yale physiologist and bibliophile J. F. Fulton, whose *Boyle*'*s Bibliography* is a classic, in a number of respects quite a monumental study and a model of scholarship, and generally a very careful study.

1. In 1675 a book was published containing *Some Considerations about the Reconcileableness of Reason and Religion* by "T. E., A Lay-Man" and *The Possibility of Resurrection* by Robert Boyle. Fulton was "unable to identify" T. E., which is quite surprising (Fulton 1933, 84; 1961, 86). He was able to identify the author of *Protestant and Papist* by his style (Fulton 1933, 108; 1961, 116), but not T. E., whose style is so much more Boylean. This in spite of the fact that Fulton himself notices that possibly T. E. stands for roberT boylE (Fulton 1933, 93; 1961, 98). This, incidentally, is not as arbitrary as it sounds, since it was quite customary then to invert one's name. Thus, Boyle's friend Henry Oldenburg, at times signed his name as Grubendol (see D.N.B.).
2. It is even more surprising that Fulton says about *Judging of Things Said to Transcend Reason* that "its authorship is not established" (Fulton 1933, 93; 1961, 98), in spite of the style and the fact that it is published together with Boyle's *Things Above Reason* and the interlocutors in these two dialogues have the same names and are identical, as the Advertisement to the second dialogue hints.

   Clearly, Fulton's care is due to the want of explanation for the fact that (the title is "… *Things above Reason* … By a F.R.S. *To which are annexed by the publisher* (for the affinity of the subject) [sic!] … *Things Said to Transcend Reason* … by a Fellow of the Same Society." This double anonymity is very odd and strongly asks for explanation. But if Fulton, arguing from "style and content" (Fulton 1933, 94; 1961, 99) identifies the first F.R.S. with Boyle, in spite of evidence to the contrary, he should not have escaped identifying the two authors, whom the publisher distinguishes from each other only by implication and likewise for an unknown purpose. In any case, Fulton's caution seems to be rooted in the desire for an explanation.
3. Each of Boyle's anonymous works save *Protestant and Papist* includes passages that repeat almost literally other passages published in Boyle's name. We can compare the views of T. E. with those of Boyle's *Excellency*. There are many other parallels to this. The curious will easily find them by checking in the index to the collected works for references to Bacon, Descartes, Hobbes, and others, as well as to specific doctrines characteristic of Boyle regarding the limits of reason, miracles, and so forth, and regarding expressions characteristic of his pen. It is thus very difficult not to attribute these works to Boyle. Yet the puzzle remains: why were they published anonymously? Fulton's caution is quite understandable in view of this puzzle.

4. Here, then, is my proposed solution. There exists a letter from John Beale to Boyle dated October 17, 1663, in which Beale, by Boyle's "command," criticizes an unnamed theological work. Evidently the work was then in manuscript. He offers criticism, both general and specific. His general criticism is an expression of dissatisfaction with the overemphasis that "we"—that is, Boyle—lay on differences between the Christian sects coupled with an expression of a wish for reconciliation. His specific criticism is of Boyle's theory of God's attributes, which he either rejected or misunderstood, and of Boyle's attack on Socinus, who, in his view, should not be attacked.
5. It was Boyle who asked for criticism and he hated dispute. These two characteristics do not go well together, and I suggest that Boyle was doubly careful not to enter into dispute. Perhaps Boyle asked Beale to criticize him in order to determine whether dispute was likely to arise. In any case, my conjecture is that Boyle's ideas that Beale had criticized he published separately, either anonymously or after Beale's death or both. Here is the list:

- *Protestant and Papist*[18] was published anonymously in 1687; after Beale died (probably in 1683; see D.N.B.).
- The attack on Socinus was published anonymously by "T. E., a Lay-Man."
- The view on God's attributes was published anonymously (1681) in *Judging of Things Said to Transcend Reason*. This explains the very artificial break of this work from *Things Above Reason* with which it forms one dialogue.
- In 1685, after Beale's death, Boyle published his *High Veneration of God*. Fulton's comment on it is this (Fulton 1961, 98):

> 'The loose sheets this paper consists of, having been written at somewhat distant times and places, and hastily tack'd together …' So the 'Advertisement' reads, and perusal of the text convinces one of its honesty. There is little in [it] … to recommend either the book or its author, and one finds it difficult to recognize in [it] … the hand that penned …

This work includes, in the main, Boyle's doctrine of God's attributes, which, to repeat, I conjecture he did not want to publish previously under his own name. This may explain the postponement and the hectic way of publication. (By the way, contrary to Fulton's claim, its style is distinctly Boyle's: it is easy "to recognize in it the hand that penned one of Boyle's earliest publications, his *Seraphic Love* of 1659.")

Let me also mention in this connection the fact that Boyle accepted Beale's criticism at last, and in his will he provided for a fund to organize sermons in agreement with Beale's view. The subject of these sermons was to be, in the language of Boyle's will, "the proof of Christian Religion against notorious Infidels, viz. Atheists, Theists, Pagans, Jews and Mohammedans, *not descending lower to any controversies, that are among Christians.*"

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[18] Its exact title is this. REASONS | WHY | A Protestant | Should not Turn | PAPIST: | OR, | Protestant Prejudices | Against the | Roman Catholic Religion; | PROPOS'D, | *In a LETTER to a Roman Priest*, | ==== | By a Person of Quality, | ==== | *LONDON*, …. 1687.

6. Thus, it seems, my conjecture, based on characteristics normally attributed to Boyle (a critical attitude plus a dislike of dispute), explains what manuscripts Beale's letter refers to, the anonymity of three of Boyle's works, and the background of another. It does not explain why Boyle published his *Protestant and Papist* anonymously in spite of Beale's previous death. It also fails to explain the style of this work. This latter failure of my hypothesis is very serious, as I wish to explain now.
7. Stylistically all the anonymous works attributed to Boyle are unmistakably in his own style save the anonymous *Protestant and Papist* and the *Free Discourse Against Swearing* posthumously attributed to him.[19] The *Discourse* has some Boylean characteristics; nevertheless Fulton unhesitatingly declares that it is an imitation. It looked to me a parody on Boyle's style akin to those of Jonathan Swift,[20] but it is too long and too serious for that.[21] Quite possibly Boyle added to an existing tract against swearing his advice about how to overcome the disposition to swear. The part with the advice is separate and less coarse. And Boyle did offer pieces of advice in other writings of his. In any case, the vulgarity of some of its anecdotes (especially the one about two friends accidentally meeting in a whorehouse) stands out and makes it beyond dispute that their author is not the gentle and pious Boyle. (For further discussion see below.)

   All the attested writings of Boyle are obviously friendly to the readers, presenting narratives or coaxing them, never bullying them, much less by calling them sinful and threatening them with hellfire and brimstone, and much less in vulgar language. These are characteristics of the stern[22] *Free Discourse Against Swearing*. Even in other writings of Boyle, where devils—or demons or Satan—came up, Boyle was never as vulgar as the author of *Free Discourse Against Swearing*.

   As to the style of *Protestant and Papist*, Fulton said it is typically Boylean. In a sense it is very much so. But the terseness and outspokenness are very uncharacteristic of Boyle, whose works are otherwise verbose and ornamented. The terseness of *Protestant and Papist* may be explicable as the outcome of its expedient politics, and as due to Boyle's reluctance to publish it in his own name. The strongest evidence, which seems to be sufficient, that Boyle was the author is the subtitle: *Protestant Prejudices*. Compare this title with two other simultaneous anonymous works: Protestant and Popish way of interpreting the

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[19] Strictly speaking *Free Discourse Against Swearing* does not belong here. Birch attributed it to Boyle with strange hesitation that the editors of its latest published version discuss all too briefly (*Works*, 2000, 12, xxxvii note).

[20] "A Meditation upon a Broomstick" (1703–1710): "A Tritical Essay upon the Faculties of the Mind" (1707–1711): *(Swift* 1841, 2, 284–6).

[21] Nevertheless, this happens: Samuel Butler's *Fair Haven* (1873) was a full-length caricature that misfired.

[22] The stern character of the work is clear, say in its taking for granted corporal punishment for children. It is not consistent, though. Thus, the Last Answer includes a light-hearted couplet. The editors could not identify it; possibly it is the author's own. This would deny its authorship to Boyle, as he had confessed he was blind to poetry.

Scriptures, impartially compared … and Protestant certainty; or a short treatise showing how a Protestant may be assured of the articles of his faith …

Nevertheless, although Boyle's style is unusually prolix almost everywhere, here it is terse and quick and decisive.

8. Is it possible that *Protestant and Papist* was published anonymously because Boyle was afraid to insult the spirit of Beale? I do not think so. Indeed, though Beale was intellectually against interdenominational theological disputes, politically he was distinctly anti-Catholic. As if to exclude this hypothesis, he wrote Boyle a letter on March 3, 1672, in which he expressed his (purely political) anti-Catholic sentiment while also recommending an enclosed anti-Catholic volume (for publication?). I do not know what volume it was, whether Boyle agreed to finance its publication, or whether it resembles in any way Boyle's work on the same topic of a decade and a half later.

   Boyle and Beale alike saw anti-Catholicism as political rather than intellectual, and there is almost nothing political penned by Boyle; hence the peculiarity of *Protestant and Papist*. This may seem more so if we remember that the little political he did write—an odd letter and an odd passage in *Occasional Reflections*—was, indeed, quite terse. Perhaps he thought terseness became political writing; perhaps terseness expressed his aversion to politics and reluctance to meddle in it. Be that as it may, *Protestant and Papist* is more political than theological in nature as well as in thrust; and the view that it is Boyle's is not well founded but is thus far unchallenged.

9. Since my discussion of this matter was published, much of the Boyle scene has changed. Edward B. Davis has honored me by commenting on this note in his paper on the same topic (Davis 1994). The aim of that paper of his is to offer a new attribution of authorship to *Protestant and Papist*. The received attribution, I said, was still unchallenged when I first published on the matter. Before arguing for his new idea, he said (612), he had to offer "a general review of Boyle's fifteen anonymous and pseudonymous writings." In that general review he observed that some of my attributions found corroboration in some relevant items in the Boyle manuscript collection (Stewart 1978–82). This is nice, but in itself of little import; what matters is the question: why did Boyle publish anonymously? It is better to reword this question specifically, with reference to specific works. So far, to my knowledge, this question scarcely found interest in current scholarship.[23] My answer to this question is still an unexamined conjecture.

[23] John T, Harwood, describes Boyle's practice as using a "transparent mask of anonymity" (Hunter 1994, 41). True or false, as it happens, this is no explanation. Davis suggests (613–14) that "presumably, Boyle used only his initials in order to retain some measure of uncertainty about authorship. Since Boyle liked to pretend publicly that one ought not to mingle science with theology, it seems likely that he sought to 'preserve deniability' (to borrow a phrase from modern politics)." This too, as it happens, is no explanation. My conjecture is. This hardly interests Harwood or Davis. The anonymity of Boyle's youthful anonymous writings is easily explicable by his youth; and the work in question is an unpublished early work. (The anonymity of his late anonymous writings I explain by reference to his attitude to theology.)

10. Davis objects to my claim that the attribution to Boyle of *A Free Discourse Against Customary Swearing* (1695) is incorrect since it is vulgar. Although this claim of mine now seems to me false, it still seems to me difficult to accept the attribution of the work to Boyle. I found especially vulgar its anecdote or joke about two who bump into each other in a brothel. Davis says (p. 671), "Apparently Agassi had forgotten that passage in Boyle's early autobiography, written just a year later, where he tells of visits to a Florentine bordello (from which he nevertheless emerged innocent) and of an encounter with two friars who tried to seduce[24] him." I do not see the point: it is one thing to report traumatic experiences frankly and another to tell a bawdy tale or joke. Since preaching is illiberal, it is not in Boyle's character to begin with, but his correspondence testifies that he did preach. Yet he could not, it seems to me, stoop so low as to tell a bawdy anecdote or joke while preaching.

    Davis shows that *A Dissuasive from Cursing*, the work appended to *A Free Discourse Against Customary Swearing*, is a genuine Boyle (early) piece. In his opinion, this clinches the claim that it is genuine. This is odd. It is nothing to me whether the work is from Boyle's pen or not; it just happens that Davis' argument strengthens my conjecture that the work is a parody. This is not to insist on it: it is probably erroneous. The unease of the letter from the publisher to the reader is genuine and sincere and more-or-less attributes the book to Boyle. Similarly, its "Advertisement" in it is decidedly from Boyle's pen, as is easy to ascertain. Still, I cannot dismiss my opinion that the vulgar in it is not Boyle's. Thus, the manuscript is tainted. As the letter from the publisher stresses, the manuscript had changed hands a few times and only a handwritten copy survived to serve the publisher.

    The preamble to *A Free Discourse Against Customary Swearing* ends with the statement that some devils are doomed to "endure hideous Torment unto all Eternity", which is far from Boyle's view of hell as expressed not only in his mature years but also in his early *Seraphick Love*. Nor is this a minor point, as the discussion of eternal torment raged at the time (Walker 1964). In Answer to Plea 10 that swearing is excusable as the outcome of a fit of ill temper the text compares it to the plea of an adulteress "that she never prostituted herself, but when her Fit of Lust etc." This is not the language that Boyle used anywhere else. In Answer to Plea 13 he shows contempt for prostitutes who pretend to be bashful and adds a vulgar response to a bashful visitor to a brothel. The editors mention with no comment that Boyle reported having visited a brothel and left virtuous. This story shows how unlikely it is that he should respond so vulgarly to his own bashfulness. Even the ascription to Boyle of the assertion (in the last answer) that a fine of one shilling is a "slight penalty" seems to me much too insensitive to ascribe to gentle Boyle.
11. As to Davis' question, is Boyle's *Protestant and Papist* genuine? The discussions of all other cases of Boyle's anonymous works barely impinge upon it, and, we

[24] From the narrative of Boyle, rape is more likely that it was then seduction.

remember, he saw them as merely preparatory for his discussion of this question. He has found the reason for the anonymity of Boyle's anonymous writings in their being theological and in his aversion to disputes. Here we agree. This covers the attribution of *Protestant and Papist* to Boyle. Davis asserted that I offer no evidence for this attribution. He meant additional evidence, presumably. And then he is almost right: my evidence, I had said, is poor. First, my attribution is the traditional one. Second, its subtitle is self-deprecating, unlike the titles and subtitles of similar tracts of the day. Davis found this no argument. Perhaps he is right, perhaps my regard for this characteristically English form of speech swayed me too far. Davis noted this. The book that he found similar to *Protestant and Papist* has in its title "Good and Solid Reasons" and in its subtitle "*Or, A New and Infallible Method*"; Davis notes then that this does not display exactly the same form of speech as *Protestant and Papist* does (620). Nevertheless, he saw no argument here.

Davis has offered one strong argument against the ascription of *Protestant and Papist* to Boyle, though: Thomas Barlow (who called Boyle "my confessor") referred in his own copy of the book to its author but not by name. Davis mentions an argument against his opinion: several copies of the book in different libraries have Boyle's name penciled in the title page (620). He rightly found all this inconclusive. He offered one more piece of evidence: in the whole rich Boyle archive, there is no trace of *Protestant and Papist.* This too is a strong argument, but it is inconclusive, since it is not the only case of a book with no trace in the Boyle archives (known to be incomplete).

The ultimate reason for Davis' rejection of the conjecture that the author of *Protestant and Papist* is Boyle is "the inconsistency between the writing of an anti-Catholic work and the ecumenical attitude reflected in Boyle's will" (621). This is indeed a strong argument. "Agassi also realized this inconsistency, but he interpreted the will as evidence that near the end of his life, Boyle accepted the advice John Beale had given him some twenty-five years before, not to criticize the Socinians." This is somewhat of a caricature,[25] but let it ride. What matters is Davis' argument: "The fact that Boyle had such a magnanimous spirit is the main reason why I have never believed that he wrote *Protestant and Papist*. … it is hard to see why he would have sought to voice his views publicly and thereby distress many Catholics, when he refused to do so on other occasions." This is just wonderful, but, alas, Davis overlooks the emergency in 1687 and the fear of impending new civil war. For my part, I consider this not too "hard to see".

12. Davis has suggested an alternative: the author in question is David Abercromby, he said, a Scot and a former Jesuit who around that time wrote a similar book and who was a *protégé* of Boyle and his collaborator. Let me add to this suggestion

[25] Since in the last resort Boyle's ecumenism is the strongest argument in Davis' arsenal, he could mention that Beale had appealed to this quality of Boyle. But then such a move might be too cavalier for Davis; I cannot say. Let me add that Boyle censured Henry More, of whom he thought highly, for his attack on Catholicism. This too is perhaps too much for me to expect Davis to acknowledge.

an explanation: the attribution of the book to Boyle may be due to his having helped his *protégé* publish it. An obvious serious objection to this suggestion of Davis, however, requires a response: the book's title describes its author as a nobleman ("a person of quality"); Abercromby definitely was not. The book's subtitle is odd in any case and wants explanation. Its subtitle is, *A Letter to a Roman Priest*, yet its title, *Reasons Why a Protestant Should not Turn Papist*, wants a better subtitle, such as, say, *A Letter to a wavering Protestant*. Is it possible that the book is composite? Davis ignored this option. Considering his evidence already very strong, he concluded, "It is no longer possible, then, to maintain that Robert Boyle wrote" the book in question. This is somewhat too decisive, given that the two books that he has ascribed to one and the same author have subtitles with slightly different overtones. The one is, *A New and Infallible Method To Reduce Romanists, From Popery to Protestancy*; the other is, *Protestant Prejudices Against the Roman Catholic Religion*. An admission of the possession of a prejudice, I admit, may possibly accompany a claim for the possession of an infallible method; but not so readily: a similar case has not yet come my way.

Appendix E: Boyle's (and Newton's) Alchemy

Louis Trenchard More, the author of the famous biographies of Boyle and of Newton, commented at length on Boyle's (anonymous but easily identifiable) alchemical dialogue. He said, "One could wish that Boyle had never published this paper, for it descends to the tricks and mystifications of the charlatan" (More 1941, 66). The dialogue presents—for the only time,[26] presumably—some incomplete experimental information that is therefore not open to repetition and examination. This is a serious infringement of the rules of research that Boyle, more than anyone else, made the hallmark of science. L. T. More was hardly concerned with this infringement, however. Rather, he found it an affront that Boyle had "clung to the Scholastic sciences" (61).

More presented Boyle's publication as a psychological weakness, partly in following Bacon who had explained all publication of unproven material psychologically. It was only partly so because the usual motives for such publications did not hold for Boyle. He did not seek money or publicity (if not for the reason that he thought he had too much of both). He took in his discovery sufficiently seriously (as Newton reported in a letter to Locke) to use influence to legislate (the Mines Royal Act, 1689) the abolition of the law (the Act Against Multipliers) that prohibited the manufacture of gold alchemically (presumably to discourage alchemy). Boyle's moderate political views made him fear the strong and unmanageable social changes

[26] This requires explanation, since Boyle repeated many reports that he did not know whether they were repeatable or not. He usually goaded his readers to examine the matter; here the situation is reversed: he was secretive.

resulting from new and unlimited resources of precious metals becoming open to the public. He assumed that even some rudimentary information concerning his discovery might help other researchers. He passed the information to his best younger peers—Newton and Locke—so that they could test it. He was a rationalist ambivalent about the mystic tradition (for religious reasons), not an adherent to it.

Since traditional alchemy lasted millennia, it is clearly not of one cloth (Newman and Principe 1998). All discussions of alchemy involve the demarcation between chemistry and alchemy, and, clearly, it is more historical than theoretical although in seventeenth-century studies a theoretical aspect must enter since Boyle was both a chemist and an alchemist. In addition, there is the problem of the demarcation of science since many students of the matter (such as L. T. More) took it for granted that chemistry is a science and alchemy is a pseudo-science, charlatanry, or both.

Alchemical research comprised efforts to comprehend ancient clues in the hope to find perfection: it was a magical quest. The alchemical literature is a mix of chemistry and magic proper, with its magic characterizable as a mix of traditional symbolism, mystification, and confusions (Principe 2011, 306, 308, 311; for more references see there, notes 14 and 17). Efforts of historians to clean it of its superstition and distilling from it whatever may count as scientific is thus anachronistic (Agassi 2008, 347–50). Alchemy always combined with folk medicine, astrology and other superstitions.

The alchemical literature differs significantly from other arcane ones: usually, occult literature is as explicit as its authors could make it, and its obscurity indicated their ignorance; the obscurity of alchemy was often intentional: it comprises more hints than information, since it contains trade secrets. Between chasing the Will o' the Wisp and hints at trade secrets, readers are bound to get lost. Galileo and Boyle have put an end to this. Galileo did so by viewing the obvious and common as miraculous enough to deserve study. Boyle did so by instituting the rule that only repeatable experiment is scientific. Thus, the question we should ask L. T. More is, what "tricks and mystifications of the charlatan" did Boyle perform?

In the seventeenth century the rules of scientific research split alchemy from chemistry[27]; this did not prevent chemists from seeking the ways to transmute metals. Hence, the ancient term "chrysopoeia"—literally gold-making—is now used as a better name for the researches like those of Boyle and of Newton regarding it. They were then chrysopoets. L. T. More could not show Boyle's tricks and mystifications: there was none to show (Ihde 1964, 52). Indeed, he never doubted Boyle's honesty; he deemed him gullible. We should not take seriously his judgment on this: he was disposed to view all error as evidence for gullibility. Inconsistently, he did not view Newton gullible or superstitious, only Boyle, although he was well aware, of course, of the prevalence of fraud in these matters (Malcolm 2004; Hunter 1990, 403).

[27] This holds for the modern world; pockets of less developed thinking still exist in diverse places, of course. In the eighteenth century the last important alchemist, Johann Friedrich Böttger or Böttcher, contributed to chemistry, as he invented imitation Chinese porcelain, as well as to technology, as he developed its manufacture (Meissen).

This is the received view (Hunter 1990, 388–9, 396–9) yet, obviously, Boyle was quite able to keep his balance by the use of repeatability as the requirement for any experiment that we may call decent. It is this that keeps magic at bay and that made it lose its respectability. I cannot avoid the impression that today's scholarly concern with the darker side of Boyle's career is partly a healthy response to past efforts to conceal it[28] and partly an attraction to a piquant topic for study.

Of course, alchemy was a vain hope, akin to El Dorado and the Northwest Passage. If it signifies the hope to find a theory of the unity of matter that should somehow permit transmutation, then, ironically, this is precisely the hypothesis of William Prout (1815, 1816) that was very important and that has found its realization in radioactivity and more so in nuclear chemistry. This is more relevant to the interest of Boyle or Newton than their chrysopoeia. Prout's hypothesis was refuted, reinstated by the discovery of isotopes, and refuted again when Bohr described hydrogen as a proton and an electron. He offered a new version of it, replacing the hydrogen atom with the proton. In its new version, it was then first corroborated and soon afterwards refuted by the discovery of the neutron, to be reinstated when the proton is replaced by a baryon, which is a neutron or a proton. This is alchemy of the highest sophistication; naturally, it has hardly anything in common with mediaeval alchemy. It belongs to a science that allows for refutations of hypotheses and overlooks all reports about clues. Individual researchers may still follow clues at their own risk; at most these belong to heuristic, never to science.

Appendix F: Laplace's System of the World (1796): A Methodological Analysis

The heritages of the Age of Reason are many. This study examines one of the most durable: methodology. Scientific method was held in great esteem when Newtonian mechanics was at its zenith, in the early nineteenth century. Yet to this day researchers continue to recognize its importance even if as mere lip-service. However problematic methodology was then, its claim to have contributed to the success of Newtonian physics gave it some *gravitas*. It is easy now to poke holes in it and it is of questionable value to do so. Yet this is the task of the present last appendix. My reason is that the problems that have traditionally beset methodology have become worse after the Newtonian era—because both logic and physics have made great strides since and because the great advances in methodology made since, by Karl Popper almost single-handedly, are still assiduously ignored. The best methodological study that was written in the spirit of Newtonian physics is that of Pierre Simon de Laplace, whose *The System of the World* is a renowned classic that combines his great scientific

[28] William Wotton's failure to produce a life of Boyle as intended is due to the refusal to conceal or expose Boyle's attitude to magic. (Hunter suggests this work was lost.) Birch's life hid unpleasant facts.

acumen with detailed historical knowledge and acute logical and philosophical sense. This Appendix is homage and severe criticism of this monumental work combined.

Laplace tried to explain the success of Newton's celestial mechanics as due to its truth, a truth that rests on observed facts, although also on very "natural" hypotheses and on some *a priori* truths. Newton did not arrive at his theory inductively: even his inductive presentation of his theory is not satisfactory, if only because it is too sketchy and incomplete. Laplace tried to complete the sketch. In his introduction he said this about Newton's theory:

> To arrive at this it was necessary to observe the heavenly bodies during a long succession of ages, to recognize from their appearances the real motion of the Earth, to develop the law of planetary motions, and from these laws to derive the principles of universal gravitation and to redescend from this principle to the complete investigation of all celestial phenomena even in their minutest details … The exposition of these discoveries, and of the most simple manner in which they may arise one from the other, would have the double advantage of presenting a great assemblage of important truths, and the true method which should be followed in investigating the laws of nature. This is the object I propose in the following work.

The work consists in five Books. Book 1 presents all that was known in his time about "The Apparent Motions of the Heavenly Bodies", to arrive from this information to the theory of Book 2 on "The Real Motion of the Heavenly Bodies". Past attempts (including Newton's) to base a theory on facts were insufficient as they were not up-to-date. Moreover, since the approach is emphatically not historical, there would be no possible reason to omit any part of the known facts from Book 1. Were the intention to select our facts in the light of a theory, Book 1 would be theoretical and Book 2 empirical. This would be much easier, but less convincing for readers convinced that observations precede theory or that observations should precede theory.

Here are selections from Laplace's exposition of the apparent motion of the sun (Book 1, Chapter 2, pp. 12, 13, 14 and 17):

> It is probable that the distance of the sun from the earth varies with its angular velocity, and this has been proved by the measures of its apparent diameter.
>
> From the effect of this retardation, combined with the increase of distance, the angular motion diminishes as the square of the distance increases, so that its product by this square is very nearly constant.
>
> The resemblance of this curve with an ellipse, having suggested the notion of comparing them together, their identity was ascertained; from which it has been inferred, that *the solar orbit is an ellipse, of which the centre of the earth occupies one of the foci*.
>
> The elliptic motion of the sun does not exactly represent modern observations; their great precision has enabled us to perceive small inequalities, of which it would have been almost impossible to have developed the laws by observation alone. The investigation of these inequalities appertains to that branch of astronomy, which redescends from causes to phenomena, and which will constitute the object of the fourth book.

The first passage here presents a qualitative version of Kepler's second law without explicit reliance on the Copernican hypothesis. Assuming, as Laplace claims, that it is inductively ascertained, we move now from it to Kepler's first law in a geocentric system. In the second passage quoted above we arrive, legitimately, let us assume, to the idea that Kepler's second law (the angular velocity times the square of the distance is constant) is an approximately true description of the sun's motion. This makes it simply hopeless to arrive at Kepler's first law.

Laplace's expression "very nearly constant" may express caution, especially since in the third passage quoted above Laplace declared that the apparent orbit of the sun is an ellipse. He had no choice: inductivism has no room for specific caution, as it demands constant caution *and utter* precision. Either the observed facts fully agree with Kepler's second law or they disagree. Indeed, later on Laplace admits that observations known in his time refute Kepler's first law, the assertion that planetary orbits are ellipses. Therefore, to take Laplace's program seriously and present a "just history of nature" known in his time in order to erect a theory on its basis, is to make do altogether without Kepler's laws. Hence, Laplace had to introduce here "a frivolous distinction": here we should overlook the disproving facts, as they belong to deduction and not to induction. Laplace demanded this postponement because the deviations from Kepler's laws are almost impossible to observe without the aid of a theory. This the inductive canons forbid. Laplace undertook to be not historical but systematic: to present all known facts first. For his deviation from this not to be *ad hoc* requires the examination of every given statement of fact in order to decide whether it belongs to the inductive part or to the deductive part. This requires some criterion, of which not the slightest trace appears in Laplace's present work or in any other.

This is the crux of the matter. If the success of Kepler's theory is due to its being only approximately true, the same fate may happen to Newton's theory. Inductivists repeatedly spoke of Kepler's laws as if Newton's theory incorporates them, knowing very well that this is not true: Kepler's laws are approximations to Newton's theory, not corollaries from it.[29]

We now pass to the motion of the moon (Chapter 4). Here the difficulties are even greater. After a discussion of the lunar orbit as it appears at a first approximation, Laplace dismisses the problems that more accurate observations raise (p. 31):

> The laws of elliptic motion are very far from representing the observations of the moon; it is subject to a great number of inequalities …

Laplace considered it evident that the inequalities, i. e., the deviations of lunar motions from their Keplerian orbit, relate to the position of the sun. Not so: he learned this from Newton's theory. The rules of induction demand to present only observed facts, but bits and pieces of the theory intended to obtain from the facts are thrown into the records of the facts. Bacon repeatedly warned against this as it is cheating ("anticipation"). Indeed, what Laplace deemed evident could puzzle his readers, unless they knew in advance the theory that they wanted to reach later on.

Let us skip to the end of the factual Book 1 (end of Chapter 13, p. 82):

> Independently of those general motions, several stars have proper motions peculiar to themselves; very slow, but which the lapse of time has rendered sensible. They have been

[29] William Whewell was the first to make this point, but he took it back. Duhem and then Popper repeated it. This rather obvious logical point is difficult to make, since the great Newton both asserted it and denied it.

> hitherto principally remarkable in Syrius and Arcturus, two of the most brilliant stars, but every thing induces us to think that succeeding ages similar motions will be developed in the other stars.

This is odd. Clearly, a natural history has no room for discussions of cause (or for explanations) of the phenomenon called the precessions of the equinox. The phenomenon itself was already known to the Babylonians, or at least to the Greeks. In itself, it says nothing about the existence or non-existence of the sphere of fixed stars. All that observations could ascertain then is stellar angular positions and the velocities of their changes; in some rare cases, variations in luminosity (light intensity) were on record too. Measurements of this quality may signify only with the aid of a theory.[30] All observations of the fixed stars, save perhaps the two that Laplace mentioned, should dictate the theory that all the distant stars are fixed. Their motions were obviously impossible to find without the aid of Newton's theory, and therefore, as the irregularities belong to the deductive part of the exercise, so does this discovery of motions of allegedly fixed stars. Further, even if this discovery belongs to natural history, we still cannot generalize it in the face of many more observed facts to the contrary. Laplace's readers, if they have any critical faculty, are bound to ask, why Laplace relied more on a few observations of motion rather than on those made by thousands of astronomers for millennia that show that the stars are fixed.

This matter deserves further consideration. The idea now received is slightly different from that of Laplace. The 1951 edition of the *Encyclopedia Britannica* , Art. Astronomy, Sec. "Stellar Proper Motion" reads,

> Only spurious star parallax had claimed the attention of astronomers until P. W. Bessel announced, in December 1838, the perspective yearly shifting of 61 Cygni in an ellipse with a mean radius of about one third of a second. Thomas Henderson had indeed measured the larger displacement of Alpha Centauri at the Cape in 1832–33, but delayed until 1839 to publish his result.

There is a need to distinguish between Bessel's observation and previous ones. Inductivists cannot give a criterion for such differences. Deductivists view the difference as extremely simple: Bessel's observation is a much simpler and therefore a stronger or more severe test. It rests on fewer assumptions, on less methods of approximation and on fewer measurements.

As a test, Bessel's observation is a very strong refutation of the hypothesis that all the fixed stars move uniformly. Bessel could not observe the parallax of most fixed stars. Yet the Copernican hypothesis says, all stars display it. His followers always explained the inability to see it as due to the tremendous distance of the fixed stars. Bessel refuted the older hypothesis by which there is no parallax. This refutation of the pre-Copernican hypothesis is no proof of the Copernican hypothesis. As an observation of facts, it cannot serve as a basis for generalization that is counter to other observed facts. Even the famous red shift of the color of the fixed stars would not help here. It will cause a clash between the permission to generalize

[30] The theory is of the dependence of the intensity of light on the distance between the illuminating and the illuminated bodies, and that the luminosity of stars is rather constant.

the observation of colors of stars and the permission to generalize the observation of the fixity of all or at least most of the fixed stars.

Laplace repeatedly stated that the job of observing is complicated, and that observation reports are determined not by observations alone but also by (statistical) calculations. It is legitimate to make, say, 20 observations of the position of the moon, to come in this or another way to the conclusion that none of these 20 reports is the true one and to produce a new observation report as the true one. Evidently, this 21st statement is not a proper observation report, nor is it a result of the previous 20 that it contradicts. It no doubt "rests" on them or "depends" on them. But the connection is *via* a theory. If such a method is permissible, it is in despair of collecting a trusted history of nature, for it rests on theories that 20 statements of fact based on observations give way to one that contradicts them all and rests on a mix of observation, theory, and calculation.

Laplace offered a reply to such an objection. All the theories employed in order to arrive from the set of statements to the ultimate one are *a priori* valid, as they are geometrical and statistical.[31] Indeed he rightly found it a triumph that he could use statistics to improve upon observations, not noticing that it is also the triumph of deductivism over inductivism (Book 1, Chapter 14, p. 109):

> To appreciate the accuracy of this method, it should be considered that from the errors of observation, the place of the moon as determined by the observer does not exactly correspond to the hour determined by the chronometer; and that in consequence of the errors of the tables this same place does not refer exactly to the corresponding hour which the sun indicates on the first meridian; the difference of these hours would not therefore be such as would be furnished by an observation and tables rigorously corrected.

This error, Laplace explains, can be eliminated by comparing results of observations made on different geographical latitudes. Besides, if the error may be diminished by multiplying the observations of the lunar distances from the sun and stars, and repeating them during many days, to compensate and destroy the errors of observations and the tables. One may argue that the elimination of error of observation is legitimate, as it was already Bacon who suggested it. (He never explained how and why this should be the case.) This excuse is invalid: inductivism claims authority to the senses. If the statement of observation is not accurate, it is a prejudice of one kind or another to be discarded. Correcting an observation report employs hypotheses and so it is deductivist, not inductivist.

The problems of observing the moon's latitude are not the hardest. It is easier to explain how to measure the angle of the appearance of a star by taking, say, the sea's skyline as the horizontal (which is inaccurate). But things become hopeless when measurements of tides are concerned. These involve many theories. The sea level is a complex construct: the observable water surface is permanently changing with waves the height of which partly depends on the tides.

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[31] Book 1, Chapter 15, 126: "The results of observations being always liable to error it is necessary to know the probability that these errors are confined within certain limits." Book 3, Chapter 3, pp. 257–8: "extremely troublesome in the numerous comparisons which are continually required."

Here Laplace discussed in some detail the methods of astronomical observation and says nothing about measuring tides. His description of the tides is very brief and sketchy. But, again, in the descriptive part a theoretical element is introduced into the very description of the tides (Book 1, Chapter 15, pp. 125 and 133):

> This phenomenon gives rise to reflexions, and excites a strong desire to penetrate the cause. But in order that we may not be bewildered by vague hypotheses, it is necessary previously to know the laws of this phenomenon, and to follow them in all its details.
>
> This retardation varies with the phases of the Moon.

The statement that the tide (or its observation) "gives rise to reflexion and excites a strong desire to penetrate the cause", may be true or false. All methodologies deem it irrelevant except that of Bacon, who declared repeatedly that this is the common case and exhorted his readers to resist it with all their might.

Leaving Book 1, the phenomenological part, we pass now to Book 2, and make theories that rest on the known facts.

The introduction to Book 2 (p. 159) concerns itself with real motions—whatever theses are—and proposes to "proceed, under the guidance of induction and analogy, to determine, by a comparison of phenomena, the real motions which produce them, and thence to ascend to the laws of these motions."

Before doing that it is necessary perhaps to introduce the Copernican revolution. Perhaps one can make a shortcut and arrive at Copernicanism via Newton's mechanics that will declare the center of the system to be its center of gravity. The advantage of starting with Copernicus is the advantage of the historical method. The advantage of starting with Newton is that thereby the idea of Copernicus that is mere approximation can be dispensed with and inductivists have trouble with approximations.

Laplace's first concern here (Book 2, Chapter 1) is to show that although we do not sense the earth's motion, it still may exist. It is obvious that were we placed on another planet we would see the sky rotating around it. "Is it not therefore reasonable to suppose that it is the same with that which we observe on the earth?" (p. 162). Since Copernicus still viewed the heavens this way (no longer as a *primum mobile*, though), this should shake the faith in the authority of the senses. Bacon had promised to offer a criterion with which to eliminate all delusive evidence; he never did; Laplace did not even try. Rather he has to get rid of illusions one by one before having a reliable factual basis on which to erect his (Newtonian) theory.

So at this stage Laplace does not state that the sun is the center of our system and argues that the diurnal motion of the heaven is delusive. What impedes his attempt to deprive the earth of its supreme function as the center of the universe is his refraining from offering at this stage any other body or point to replace it. This is particularly hard as the theory is that celestial mechanics is causal is implicitly understood all the way, and Laplace saw his task as merely to show that this causal character is of Newtonian dynamics. Even this is already impossible. His argument is simple and convincing, of course, yet it is a severe break from the rules of the game that he had assigned to himself in the introduction of the book.

The idea is that as we look at the sky and see all the stellar bodies move (more or less) with the same angular velocity in their diurnal motion, we conclude that one cause is common to this same diurnal motion of the different bodies. We now also assume (let us make a concession to inductivism) that the distances of those bodies from us and the motion of comets refute the theory of the spheres. There arises then a difficult problem: how, or by what means, does the earth cause this uniform angular motion on near and far bodies? The answer comes as a relief: since analytical geometry allows us to describe the motions of the heavenly bodies in the same way both in a geocentric coordinate system and in the coordinate system of the fixed stars, it seems so much more "natural" to accept the latter as the real.

This approach collapses with one question that precedes the question whether the diurnal motion is stellar or terrestrial. It is, what is diurnal motion? How do we come to describe it? Where does the concept "diurnal motion" come from?

Historically, diurnal motion was the motion ascribed to all the heavenly bodies save the two big lights—the sun and moon. Later, one by one, the discoveries of the planets appeared as their deviations from the theory of universal diurnal motion. In subsequent theories, diurnal motion was never ascribed to any element of the solar system. Each known element of the solar system had, in the classical theories of the spheres, a motion peculiar to itself. Of course, at first approximation all these motions were in agreement with the previous theory of the celestial edifice, the oldest theory of diurnal motion. But strictly speaking the pre-Copernicans did not ascribe to the inner spheres the same motion as to the outer sphere. There was a theory that these motions were connected—the theory of the *primum mobile*. Copernicus assumed that the sun is fixed, and by this he was able to explain a part of the observed planetary motion as an illusion, as due to the earth's rotation. This made Bacon oppose him—erroneously but consistently with his trust of the senses. Laplace managed to shake the Baconian faith in the senses but he refused to give it up: he took it for granted that science without foundations is impossible, that the wish to explain is insufficient and researchers also need assurance of success.

Laplace's approach is (or should be) inductivist (and not historical). He did not attempt to describe the transition from one theory to another, but rather from a just history of nature to a true and legitimate induction. What our history of nature says is in no case anything like the diurnal motion of the stellar system. Nay, more, it even says, we may remember, that some fixed stars move. Their motion is different from the one that the theory of the diurnal motion of the outer sphere ascribed to them. Laplace's presentation is confusing.

Analytical geometry helps construct the description of each stellar motion as a super-position of the diurnal and of other motions. It helps split any observed motion to two, three or more components—as long as the study remains in the field of kinematics. Analytical geometry helps choose the earth or the sun or the moon or any other heavenly body as the center of the universe. This is the great discovery of Tycho Brahe. I shall discuss it when dealing with Laplace's criticism of it. What is obvious already is that Laplace could criticize Brahe only from a dynamical point of view: kinematically, his discovery is logically true.

Even kinematically, in a way the old geocentric system is inferior to the new heliocentric system (or that of the fixed stars, which at first approximation is the same as the heliocentric). For, the new theory allows discussions of minute details to avoid repeated references to diurnal motions. But this superiority also exists for a halfway new theory—to a geocentric theory that attributes the diurnal motion to rotation of the earth.[32] Yet the ease of calculations is no guarantee for truth.

The program of Laplace's *System of the World* is his effort to improve upon Newton's *System of the World*, no less. The trivial fact that "diurnal motion" is a theoretical concept is sufficient to show how far science, common sense, and intuition, are remote from induction. (Even the number 2, anti-inductivist Einstein observed.) True, it sounds that a trivial point would raise no difficulty. This is not so: induction is far from common sense; improving theories in stages by the support of evidence is. And Bacon's doctrine of prejudice that Laplace enthusiastically supported blocks this commonsense approach due to the fear that researchers will stick to some unfinished step in the process, fall in love with their part of it, and stagnate. Now this does happen, but from this to the stagnation of science is a long distance.

In some places Laplace admits stepwise improvements to be within the method of science. But the inductive idea that guides his book is different—we should start with accurate and careful observations, and then we shall come to an accurate theory—and straight to it, without the danger of dogmatic stagnation at insufficient theory.

Let all this ride. Assume now that diurnal motion is unproblematic. We move then to annual motion. In order to show that the motion is round the center of gravity of the solar system, we have to know some dynamics that Laplace has not yet introduced. Here again he needs heliocentricity as an approximation, and again he uses it reluctantly. Concluding from the rejection of geocentrism that the earth moves, Laplace undertakes the next task of calling it by the inductive brand-name: illusion of the senses (end of Book 2, Chapter 1, p. 163):

> Every thing therefore leads us to conclude that the earth has really a motion of rotation, and the diurnal motion of the heavens is merely an illusion which is produced by it; an illusion similar to that which represents the heaven as a blue vault to which all stars are attached, and the Earth as a plane on which it rests. Thus astronomy has surmounted the illusions of the senses, but it was not till they were dissipated by a great number of observations and calculations, that man has at last recognized the motion of the globe which he inhabits, in the true position in the universe.

So the diurnal motion is both an illusion produced by the motion of the earth and an illusion of the senses! They "have been dissipated by a great number of observations and calculations", not by the theory of Copernicus, and not by the plea of Bacon for good will and for giving up all preconceived notions.

And all this is a part of an exercise whose point is to avoid all hypotheses—whether heliocentric or not. This then is inductivism sans Bacon's doctrine of prejudice: Laplace did not need that hypothesis.

[32] This theory was discussed in Guericke (1672) and McColley (1937, 210–11). It seems Gilbert suggested it (*De magnete*, book VI). This is unclear, since he never openly responded to Copernicus.

Let us see then how Laplace deduced the laws of motion from the phenomena. We begin with the condemnation of Descartes, who feigned hypotheses at pleasure. Speaking of laws of motion, Laplace says (Book 3, Chapter 6, pp. 279–80):

> Several philosophers have endeavored to determine them from the consideration of final causes. Descartes, supposing that the quantity of motion in the universe should always remain the same without any regard to its direction, has deduced from this false hypothesis erroneous laws of the communication of motion, which furnish a remarkable example of the errors to which we are liable, when we endeavour to develop the laws of nature, by attributing to her particular views.

As to the true laws of motion, at the end of the brief exposition Laplace admits them as they are the simplest and the most natural imaginable (p. 328). This does not prevent him from starting Book 4 thus (p. 1):

> Having, in the preceding Books, explained the laws of celestial motions, and those of the action of forces producing motion, it remains to compare them together, to learn what forces animate the solar system, and to ascend without the assistance of a hypothesis, but by strict geometrical reasoning, at the principle of universal gravitation, from which they are derived.

And soon after this unqualified exclusion of all hypotheses, Laplace uses a number of them, beginning with Kepler's laws. Speaking of Kepler's third law, Laplace says (Book 4, Chapter 1, p. 5):

> This hypothesis, it is true, is not rigorously exact, but … it is natural to think it would subsist also in the case of the orbits being circular. Thus, the law of gravity … is clearly indicated by this relation. Analogy leads us to suppose that this law … subsists equally for the same planet, at its different distances from the Sun, and its elliptic motion confirms it beyond a doubt.

Here Laplace admits that Kepler's third law is a hypothesis and that he made a further assumption, justified by analogy and rigorous test. And the permission to interpret the concept of analogy widely enough allows the presentation of any hypothesis as reflecting some analogy. And if analogy allows in even "not rigorously exact" hypotheses, then the restriction of not making hypotheses fades away. This is particularly so in the light of Laplace's dynamical explanation to the "not rigorously exact": the sun attracts all celestial bodies with a force that varies as the inverse square of the distance (Book 4, Chapter 1, pp. 8–9):

> We are thus conducted without the aid of hypothesis, by necessary consequence of the laws of celestial motions, to regard the Sun as the center of force, which, extending infinitely into space, diminishes as the square of the distance increases, and which attracts all bodies similarly.

Laplace does not say, as yet, that any two bodies attract each other, but only that the sun attracts all planets. This is the first alleged result of Newton put inductively. Newton showed that this theory explains Kepler's laws as rigorously exact. Historically this theory is significant, since although it is untestable, it is an indication that Newton's theory, or any similar modification of it, may explain Kepler's theory as a first approximation and thus enable crucial tests. This is the historical approach. The inductive approach is different. The just history of nature shows that

Kepler's theory is false and therefore we need not explain it at all. But, of course, inductivism is tolerable just because it is mixed with the historical approach. This is inconsistent, and the only way to overcome the inconsistency is to give up inductivism and stay with the historical approach.

The following illustrates the duality of Laplace's view. The very possibility to arrive at a theory by observations "without the aid of any hypothesis" leaves no room for the following considerations (Book 4, Chapter 1, p. 9):

> The errors to which observations are liable, and the small alterations in the elliptic motion of the planets, leave a little uncertainty into results which we have just deduced from the laws of motion; and it may be doubted whether the solar gravity diminishes exactly in the inverse ratio of the square of the distance. But the very small variation in this law, would produce a very sensible difference in the motions in the perihelia of the planetary orbits. The perihelion of the terrestrial orbit would have an annual motion of 200″, if we only increased by one thousandth part, the power of the distance to which the solar gravity is reciprocally proportional; this motion is only 36″4, according to observation and of this we shall hereafter see the cause. The law of the gravity inversely as the square of the distance, is then at least, extremely near; and its extreme simplicity should induce us to adopt it, as long as observations do not compel us to abandon it. However we must not estimate the simplicity of the laws of nature, by our facility of conception; but when those which appear to us the most simple, accord perfectly with our observations of the phenomena, we are justified in supposing them rigorously exact.

The last quotation shows again that inductivism was not a methodology but an epistemology, not a rule or a method of inquiry but merely a justification of existing theories, a *post hoc* explanation of success. The method given in the last quotation is this: deduce from theories statements compared with observation; demand higher simplicity of theory and greater accuracy of statements of fact. If the comparison fails to fit theory with observations, it "compels us to abandon" the theories. No induction.

Skipping the rest of the discussion of astronomy and of optics we reach a major issue: the problem of the tides. As Newton's theory of the tides assumes only the theory of universal gravity, the problem concerns initial conditions only. Newton made a hypothesis regarding the initial conditions, and it was a mistake (Book 4, Chapter 11, pp. 147–9). The initial conditions are partly too complex (the coastline) and partly also unknown (the depth of the ocean) so that they are not given to calculation (p. 154). Laplace took up the challenge and tried to do better (p. 151). Incidentally, his theory proved of limited use, and is unsatisfactory, if only due to his oversight of tidal friction, yet he influenced all further researches on this topic (Marmer 1922; Cartwright 2001).

Isaac Todhunter, the great historian of science, remarked of Laplace that he stated that Newton laid the true foundations of the theories of this subject and that he was in error. "I do not understand this criticism", he said (Todhunter 1873, 2, 33). He suggested, as was common in his days, that Newton should not have made a mistake, or else that his mistake could not possibly be a contribution. This is very odd. Laplace admitted that even the Ptolemaic system was a contribution, in spite of his own condemnation of it as a prejudice and an illusion. The whole of Laplace's attitude to his predecessors was thus torn between the earnest wish to acknowledge debt and the inductivist demand to condemn past mistakes wholesale. This is obvious even in Book V of his *System of the World*, his sketch of the history of astronomy.

His exposition of the history of the ages prior to the Renaissance is quite insignificant: it is too brief and modeled mainly after the outline that Bacon had laid down in his *Novum Organum* (I, Aphs. 62–92). In brief, the idea is this: the ancient Greeks were Reasoners and the Middle Ages were mainly transmitters of the little factual knowledge from the Greek to Western Europe.

More interesting is Laplace's discussion of later scientists, especially Brahe, Kepler, Descartes and Newton.

Brahe's observations are very famous. Historians of science tend to ignore his theoretical work ever since Galileo dismissed it off-hand. Laplace mentioned one immense contribution of his. Copernicus still followed the theory of the spheres, since he assumes a law of rotational inertia of all spheres; Brahe, as Kepler noted, did away with them. His system that compromised between the geocentric and the heliocentric systems had no room for spheres: the planets rotate around the sun that in turn rotates around the earth so they cannot be glued to crystal balls as these will intersect. His theory is the first that uses the old idea of superposition of motions in a way that blocks any mechanical model of spheres. He allowed a complete freedom of superposition. As a result he allowed a general relativity which differs from Einstein's principle of general relativity in the fact that his theory is purely kinematical while Einstein's one is much more general and more precise. By this principle Brahe rejected Copernicus' spheres and his principle of inertia and threw new light on the dispute between the Copernicans and traditional astronomy. The Copernican Kepler admired Brahe for his abolition of the spheres and he endorsed his readiness to do without a mechanical model.

Laplace did not hesitate to attack Brahe's relativity from a dynamical point of view (Book 5, Chapter 4, p. 272):

> Struck with the objections which the adversaries of Copernicus made to the motion of the Earth, and perhaps influenced by the vanity of wishing to give his name to an astronomical system, he mistook that of nature. According to him … we may, in general, consider any point we chose, for example, the center of the Moon as immovable …
>
> But is it not physically absurd to suppose the Earth immovable in space, while the Sun carries along the planets in which it is included?

Brahe's case is relatively simple: despite his prejudice he is admitted as a great observer, as in theory he was both prejudiced and mistaken. More difficult are the cases of the prejudiced, imaginative, and often mistaken, who nonetheless contributed important ideas. Would not such a case refute the doctrine of prejudice? Such a case was that of Kepler. Before discussing his contribution, Laplace restated the doctrine of prejudice in a modified version that allows for Kepler (not quite; in some measure of defense of Bacon, Laplace had to slight Kepler as too ambitious). Speaking of him, Laplace says (Book 5, Ch. 4, p. 276):

> The career of the sciences did not appear to him adequate to satisfy the ambition he felt rendering himself illustrious; but the ascendancy of his genius, and the exhortations of Maestlin, led him to astronomy; he had entered into the pursuit with all avidity of a mind passionately desirous of glory.

> The Philosopher, endowed with a lively imagination, and impatient to know the causes of the phenomena which he sees, often obtains a glimpse of them, before observation can conduct him to them. Doubtless he might, with greater certainty, ascertain the cause from the phenomena; but the history of science proves to us, that this slow progress has not always been that of inventors.
>
> What rocks has he to fear, who takes his imagination for his guide!

After the downfall of the myth of induction, Kepler's character and work appeared in a different light. Let me quote a few more lines from Laplace that exhibit his ambivalence towards Kepler (*op. cit*., pp. 279, 282–3).

> Without the speculations of the Greeks .. . these beautiful laws might have been still unknown.
>
> We might be astonished that Kepler should not have applied the general laws of elliptic motion to comets. But, misled by ardent imagination, he lost the clue to the analogy, which should have conducted him to this great discovery. … In his time, the world had just begun to get a glimpse of the proper method of proceeding in search of truth, at which genius only arrived by instinct, frequently connecting errors with its discoveries. Instead of passing slowly by a succession of inductions, from insulated phenomena, to others, more extended, and from these to the general laws of nature; it is more easy and more agreeable to subject all phenomena to the relations of suitableness and harmony, which the imagination could create and modify at pleasure.
>
> Thus Kepler explained the dispositions of the solar system by the laws of musical harmony. It is a humiliating sight for the human mind to behold this great man, … amusing himself with these chimerical speculations …

Laplace insisted, we remember, that inductively the planets must move in ellipses and only deductively may they deviate from their courses. here, when Laplace had to scold Kepler for being un-inductive, he forgot all this and reminded us that Kepler had been in error.

And now to Descartes (*loc. cit*., pp. 296–7):

> Descartes was the first who endeavored to reduce the motions of the heavenly bodies to some mechanical principle. … The motion of comets traversing the heavens in all directions, destroyed these vortices as they had before destroyed the solid crystalline spheres of the ancient astronomers. Thus, Descartes was no happier in his mechanical, than Ptolemy in his astronomical theory. But their labors have not been useless to science. Ptolemy has transmitted to us, through fourteen centuries of ignorance, a few astronomical truths which the ancients had discovered, and which he had also increased. When Descartes appeared on the stage, … by substituting in the place of ancient errors other, more seducing, and resting on the authority of his geometrical discoveries, was enabled to destroy the empire of Aristotle, which might have stood the attack of a more careful philosopher; … but by establishing as a principle, that we should begin doubting of every thing, he himself warned us to examine his own system with great caution …

And then Newton is the winner who takes all (*loc. cit*., p. 297):

> It was reserved to Newton to make known the general principles of the heavenly motions. Nature not only endowed him with a profound genius, but placed his existence in a most fortunate period.

All hostility to speculation notwithstanding, in the end of the *System of the World* Laplace offered for the first time his celebrated hypothesis concerning the origin of the solar system. To this kind of conduct Einstein said once, "Don't listen to his words, examine his achievements" (Einstein 1934, opening paragraph).

Laplace's presentation of his theory (354 note 7 and last page) is most remarkable. It starts with a vigorous presentation of the problem, which is a very unconventional approach even today, and he listed the phenomena that want explanation. He criticizes Newton's view on the matter (pp. 331–2); he then analyses the theory of Buffon, the only post-Newtonian cosmogony he knew (he did not know of Kant), and showed that it had no explanatory power (pp. 354, 356). He provided his famous hypothesis and showed how it could explain, or leave room for auxiliary hypotheses that would explain, the problematic phenomena. The following comment on his hypothesis shows how far his method is from the inductive method, whatever that is (p. 328):

> Whatever the true cause, it is certain that the elements of the planetary system are so arranged as to enjoy the greatest possible stability, unless it is deranged by the intervention of foreign causes.

This statement is very important and it exposes inductivism as a myth, a *post hoc* justification of some belief in some theory and a *post hoc* condemnation of some refuted view and of those who ever believed it.

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J. Agassi, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle*, Boston Studies in the Philosophy and History of Science 298,
DOI 10.1007/978-94-007-5351-8, © Springer Science+Business Media Dordrecht 2013