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The Philosophy of Science Today

Abstract and Table of Contents:

0. The Philosophy of Science has a Remarkably Low Standard

Science began in Antiquity as a branch of wisdom. Philosophy (= the love of wisdom) was distinguished from wisdom only by philosophers. Cultivators of science in its early modern times (ca. 1600-1800) called themselves philosophers, and their activity was called not science but natural philosophy. What we call today the philosophy of science includes the theories of knowledge (epistemology) and of learning (methodology), as well as the study of the principles of science (metaphysics, the philosophy of nature). The concern with epistemology and methodology characterizes the thinkers whom we consider philosophers of science or philosophers in the sense used today by philosophers and historians of philosophy. At the time the issues of methodology and of epistemology were neglected as they were considered marginal; the third, metaphysics, was deemed distinctly dangerous. Natural philosophers did not consider their work impractical; they called themselves "benefactors of humanity", as they were convinced that their activities, in addition to their intrinsic merits, would bring peace and prosperity to the whole world. But they insisted that the practical aspects of science, significant as they surely are, can only appear as by-products, not as the outcome of study directed to any goal other than the search for the truth: any other goal will render research biased and so worse than nothing.

It is not that applied science evolves all by itself, since the application of knowledge for practical purposes certainly requires efforts, including research. But research for any practical purpose need not, it was taken for granted, be a search for knowledge. To make this clear, it may be useful to contrast the classical, typically eighteenth-century view with today's view: today we recognize within science not two but three categories; in addition to the classical pure and applied research we recognize basic research, where pure research is disinterested and applied research is the use of the fruits of pure research for practical ends; basic research is pure research directed at problems which are not very interesting in their own right but which are expected to be very useful in practice. Obviously, today research claims prestige for itself because of its potential usefulness. That is to say, nowadays all research is claimed to be more-or-less basic. In the classical vein this was unthinkable, as the value of science was deemed almost exclusively personal enlightenment and personal gratification — since the search for the truth was deemed edifying.

Obviously, most of the many thousands of citizens engaged in research proper, are engaged in small tasks — which Thomas S. Kuhn has labeled "puzzles". He has presented most scientists as "normal", as professional puzzle-solvers, whose interest in their work is not directly relevant to their employment, as the puzzles are assigned to them by the leadership of the profession. And, he stresses, normal science is practical. He probably means by this that normal science is all practical, but industrial revolution, and so nineteenth-century at the earliest; more likely it is post-Hiroshima. Normal science in the eighteenth century was more for individual entertainment than for practical ends: the illustrations in the literature on the history of eighteenth-century science suffice to make this fact obvious. This is reflected in the third edition of Encyclopedia Britannica of the early nineteenth century. The article "Science" there is extremely brief, reporting that an item of knowledge belongs to the body of science if and only if it is certain, namely, proven. Though the article gives no instances, clearly, the best instances are either from logic and basic mathematics or from extremely common and undoubted experiences, though, of course, some high-powered scientific theories should count as well. Today, incidentally, it is generally acknowledged that of these seemingly most certain items, none is exempt from doubt and revision (except perhaps logic; this is still a contested matter). Next to that brief third-edition Britannica article on science is a long article on science as amusement, in which the contents of a famous popular eighteenth-century book (by Ozanam) is reported. We would recognize today the contents of this article as vaguely within the domain of high-school science, as it includes somewhat amusing illustrations of ideas from mechanics, electricity, magnetism and the like. Probably these two articles were not conceived of together and they were put alongside of each other by sheer lexicographic rules.

The picture which emerges from this description presents a concern with science which is pre-critical. It was at times purely intellectual, at time practically oriented, always with great implication for life in general, for daily life and for peace, but with hardly any concern for the problems and issues in the philosophy of science as recognized today. Today the picture is different, even though this is obscured by the fact that all the concerns of the field, epistemological, methodological and metaphysical, are traced back whenever possible to writers of the classical era, especially David Hume and Immanuel Kant, while the marginality of these thinkers at the time is glossed over .Another peculiarity, this time of Kant rather than of Hume, is also ignored. Whereas Hume is typical of his class, as he was a private scholar who was clearly concerned with the social sciences(politics and economics, in particular), and whose contributions to the philosophy of science he himself saw as marginal and preparatory, Kant was most uncharacteristic; he was a university professor, who was on the side of science, who was reputed to be a polymath proficient in 14 different branches (some of which he inaugurated as academic subjects, such as geography and anthropology): he still was primarily a philosopher, and even primarily a philosopher of science.

It is hard to examine this assessment, since the expression "the philosophy of science" is new. To repeat, traditionally the word "philosophy" designated learning in general and empirical research in particular. In the nineteenth century this label was changed for the following historical reason. After the defeat of the French Revolution, some fashionable reactionary philosophers swore allegiance to unreason in the form of fideism, spiritualism and fake orientalism, and tried to make philosophy as utterly divorced from natural science as they could. Other, more old-fashioned philosophers, understandably attempted to distance themselves from the new advocacy of unreason, and one way they did this was by naming their own views "scientific philosophy". This name usually designated mechanistic philosophy for the following historical reasons. Since the scientific revolution to date adherents of mechanism identified theology as the ideas hostile to science, and they took metaphysics to be more-or-less the same, branding all metaphysics evil. This enhanced their claim for scientific status for their own metaphysics, which was mechanistic. This way the philosophy that upheld the traditional esteem of reason centered mainly on science and on reasonability in the moral life of the individual and the nation, namely, they stuck to the traditional view of the import of small contributions to research as the devotional acts of bringing peace and salvation to the world. Thus philosophers of science and of unreason parted company. Thus, naturally, those who called themselves scientific philosophers or philosophers of science, centered increasingly on epistemology, methodology and rational metaphysics as a main tool to combat unreason without direct confrontation. The philosophers of unreason had — still have — their own philosophy of science, but this is scarcely recognized: the defenders of science then took a monopoly on the philosophy of science that has remained unchallenged.

The philosophy of science thus evolved into a specific activity defending science against its detractors. This may explain the poverty of the field today: today science has no worthy detractors to combat; no dragons to slay, no heroic deeds.

Philosophers of science defend science not only by singing its praises, but also by attempting to solve problems in epistemology and in methodology, and by seeking newer and better arguments to combat metaphysics (especially the once very popular idea that metaphysics is gibberish). They do this merely as a pious act, paying no heed to the possibility that the problems they pose are insoluble, at least insoluble as long as they are presented in the traditional manner and settings. They cling to the pre-critical, optimistic view of science in the face of the fact that the very survival of humanity is risked by scientific technology: they relegate the study of these risks to other fields, including the new field of the philosophy of technology (which is less than half a century old), as if their philosophy of science does not include the philosophy of technology and as if their philosophy of science does not credit science with scientific technology as a great achievement. They include in the field of the philosophy of science praise for science as the source of the possible and more so the actual benefits from scientific technology and its great achievements; at the same time they banish to the philosophy of technology the condemnation of science as the source of possible and even actual ill effects of the same scientific technology. This is scarcely a fair presentation.

David Stove is exceptional. The efforts to solve the traditional problems of the philosophy of science, he says, are commendable even if these should turn out to be insoluble. For, he explains (in his book against those who have given up the traditional struggle, including Sir Karl Popper and Thomas S. Kuhn), the struggle is the ongoing defense of science and thus of traditional rationalist philosophy and thus of rationalism as such. This is a charming admission, but of a position that is obviously on the defensive. Stove advocates the study of problems because traditionally they engaged rationalist thinkers. Yet the clock cannot be turned back: these problems engaged these thinkers not as a pious act but because they wished to contribute to scientific progress. Clearly, Stove's attitude is different from theirs, since it turns the attempt at a rational defense of science into sheer apologetics.

1. Public Relations for Science is Counterproductive

In the year 1600 AD Giordano Bruno was burnt on the stake — by the command of chief inquisitor St Roberto, Cardinal Bellarmino — allegedly because he said that the universe is infinite, from which the inquisitors deduced that in all likelihood there exist other worlds like ours, in contradiction to the dogma of the uniqueness of the Savior (as if He could not appear on any other planet and die as many times as was necessary). Some years later, the said Saint issued an official threat to Galileo. Science was then rightly militant. Today science is obviously triumphant; so much so that even the Church of Rome has recently changed its stand; the current Pope has officially endorsed Galileo's side in the sad controversy. However hard it was to recant, science became too strong to continue to do nothing about the case. Today science surrounds us and appears on all levels from the sublime through the mundane to the abject.

In the sublime mood science is what Bertrand Russell called (The Scientific Outlook) "Promethean madness" and what Albert Einstein considered the scientific undertaking: "tracing the Good Lord's blueprint of the universe". In the mundane world of the modern industrialized metropolis the impact of science on the intellectual, political, social and technical aspects of life is overwhelming; the impact of scientific technology is especially prominent. The abject aspect of the impact of science on daily life has attracted a certain kind of philosophers, whose hostility to reason is expressed as a hostility to science, transformed into a hostility to scientific technology — on the ground of prophecies of gloom and of apocalypse that should be blamed on scientific technology, the cause of the alienation of Modern Man. These prophets of unreason identify science with the foolish attempt to conquer and subjugate Nature and they are confident that Nature will soon avenge this treatment by devastation. They advocate replacing the harsh, indifferent western attitude to Nature with a soft, intuitive, irrational attitude, or at least to mix the two. This mixture invites very urgently the sifting of the grain from the chaff.

My quick survey of the impact of science on society as going from the sublime to the ridiculous has omitted the ridiculous. This dimension is normally absent from the literature on the matter: science is no cause for levity. The entertainment world is as much under the influence of science as any other component of our small universe, in its shaping our tastes and opinions and values and in its stupendous media technology. But science will not be taken as an object of hilarity; even as sedate entertainment it is almost entirely confined to the juvenile. This discussion raises in a fresh manner the question, what is science? The question seems to require an answer that is easy to apply to what we usually call science, including high-school science and nuclear physics and electronic engineering. This is an error: a thing differs from its model, and the two need not agree — even when the model is generally received. In the literature on social anthropology this is taken for granted. In that literature, the paradigm for the discrepancy — between a thing and the received idea of it — is the discrepancy between magic and the received idea of it: in every society that has been described by anthropologists magicians abound, and yet (unless we deem the scientists as powerful magicians as did Sir Francis Bacon in the early seventeenth century), we all agree that real live magicians never fit the characterization of magic admitted in their society (except possibly contemporary modern society). Magicians like Merlin do fit the image of the magician, but they never existed (except perhaps among modern scientists). Do modern scientists fit their mage of science?

It is not science but its image that is rather ridiculous, at least as put forward by the spokespeople for the public-relations of science. This is not peculiar to science. The practice of public relations evolved unawares and uncontrolled as a part of the advertising world of the free market, where the shortest of the short-term interests govern, so that the most cynical opportunists set the tone there. This is harmless enough when pertaining to sales of soap, but not to sales of higher things in life, be they the arts, the sciences, or religion. Whoever are the individuals who take upon themselves to express the social concerns of science, they are these days powerful individuals and they control the appointment of suitable individuals to the positions of spokespeople of science. Hence, the (semi-)official philosophy of science, the one boosted by the scientific establishment, is inferior to soap commercials. They are as remote from the Promethean madness of the search for the secret of the universe as eroticism is from the (intellectual) love of God.

The best characterization of science that can be given is in that vein: science is a quest; it is the Promethean madness, the attempt to trace God's blueprint of the universe, the search for the secret of the universe. This characterization is contestable and contested, of course. What is incontestable is that some researchers do endorse it and others do not. Does this impinge on researches? Do any opinions of researchers impinge on their researchers? If not, they look like automata, and if yes, the status of their output as objective is questionable. Can one avoid being an automaton yet retain one's objectivity?

2. Science is a Cultural Phenomenon

There are some immediate, obvious objections to the view of science as a quest, and they center on the missing object of the quest and on its trail; they are expressed in the following two questions. What will satisfy the scientific quest? Which way does one turn to be on its trail? These questions are reasonable and should be taken seriously, but they are presented as objections, and as objections they are public-relations standard issues — as I wish to argue, by illustrating the immediate socio-political implications of the objections: they are based on the assumption that science has no competitors but merely light-weight detractors who can be dismissed out of hand.

The first objection is dominant in the semi-official literature on the matter: what exactly are we in search of? Are we in search of information or of knowledge? If for mere information, will any information do? If yes, why not be pleased with the information contained in primitive lore and in Scriptures? This series of question even though obviously we do not know: we know what we look for when we look for a lost penny, but not when we look for a masterpiece while roaming in a foreign museum, much less when we seek the secret of the universe. The question is right, but we should not expect too much for an answer: anything remotely resembling a possible answer may be a tremendous excitement. But look where the series of questions ends: it ends with an insult to the competition. This is not serious; repeatedly the spokespeople of science find the scientific quest formidable and exasperating, and then they settle for the mere propagation of physical comforts that science-based technology has to offer to the modern world. (The leading philosopher of science in the previous generation was Rudolf Carnap, the famous debunker of all speculations; his magnum opus was his The Logical Foundations of Probability of 1950; it starts with the formula for finding the truth about the world and ends by giving up the task, with the excuse that science is a mere instrument.) And so, when the circle is closed and science is praised as a mere instrument, the conclusion is not drawn but remains all the same: the mere physical comforts that science-based technology has to offer the modern world are superior to the primitive lore and to Scriptures. This is hard to take seriously. Primitive lore and Scriptures do not compete with modern science-based technology as conveyors of physical comforts, but they still are very interesting and deserve attention in many ways and on many levels. This is the end of the hostile objection to the idea of science as a quest from popular lore and Scriptures: the quest is obviously not replaced by the study of popular lore and of Scriptures, but this study is part-and-parcel of the quest.

This way the discussion, which some take very seriously, is here treated as trite and dismissed. Those who take it seriously are not lost for words in the face of my treatment. Rather, they offer a forceful objections to the light way of my treatment of the matter: they see themselves as defenders of science, as advocates of Reason, namely of science, as the guide for life; they want science to offer both better technology and better education, and they show concern for the requirement that the two should go together (as the proper education of the next generation is essential for the technological challenges of the future). This they say is obviously most important, yet the competition will not agree. Admittedly, taking Scriptures as a science-substitute is frivolous, they concede. Yet, however frivolous the competition is, its hostility to scientific technology and to scientific education must be taken seriously in the interest of the well-being of us all.

This argument will not help: it seems very serious and very responsible, but it is not. Responsibility will be served if the question of education and of the place of technology in the modern scientific world were discussed not apropos science but apropos the design of a better education policy, the study of education and its purposes. And the same holds for the problems that are specific to high-tech society. Here we are discussing science, and as these arch for the secret of the universe, not the implication this has for education and for high-tech training. True, the search for the secret of the universe is shared by science, magic and religion. Do the official philosophers of science want to distinguish between the way the search is conducted in science, and alternative ways? Do they suggest that only the manner of scientific research is becoming to science and not any alternative way? Or will it suffice to compare the results of the search in the manner customary within science and along any different line? Today it is agreed that the results tell the important tale: by their fruits ye shall know them. Do we know, then, the difference in results? Of course we do: even the most ignorant among the philosophers of science have no difficulty in telling a magic text from a scientific one! This situation is rooted in the confusion between criteria of choice within a methodology and criteria of choice of a methodology (among competing ones — magic, religion, science, or any other). To be precise, the semi-official literature in the philosophy of science is not that advanced: only a few philosophers of science discuss magic proper in a manner which is up to the standard of current social anthropology, the scientific field of study which retains an exclusive claim over magic. Rather, this semi-official literature is concerned to a large extent with the unmasking of items of pseudo-science as merely pseudo, namely, not the genuine articles they masquerade as. Moreover, much of the unmasking is itself suspect, as it employs excessive scientific terminology to no obvious purpose.

3. Science Needs no Promoters

The spokespeople of science can easily distinguish a genuine lamentably all too often they cannot distinguish a manuscript that deserves publication in a scientific journal from one that is not. This is why they are not invited to act as referees in judging the merits of scientific research any more than the public-relations spokespeople of a financial concern will be asked to adjudicate on matters financial. This is why spokespeople of science are so pleased and so proud when a scientist proper joins their ranks, even though they should know better. For, a scientist can contribute to the philosophy of science without joining the ranks of the philosophers, as many scientists often do; hence, scientists become philosophers only as an admission of stepping down as scientists. Max Born, the great physicist of the early twentieth century, said that all able-bodied researchers should devote all their energies exclusively to science and permit themselves to turn to philosophy, if at all, only after retirement.

All this sounds rather besides the point, yet it is the heart of the matter: the analysis of the means for distinguishing the genuine research from the pseudo sounds a reasonable task, yet it clearly is very questionable, and probably it cannot be done: some scientists of the highest repute are not particularly good at it. Proof: young Albert Einstein was deemed a phony by many scientists, and he was treated as suspect for well over a generation — while those who took him seriously debated hotly the question, was he right? The great historian of physics Sir Edmund Whittaker, who was himself a serious scientist, was hostile to Einstein all his long life, and as late as in the mid-century, long after the heated debate had subsided, he declared that there never was any revolution in science, Einsteinian or any other. Ignoring this heated debate (and other debates too) he described the growth of science as continuous, forgetting that he had once said, everything that the printer's ink has dried on it is out of date.

To return to the items that may masquerade as parts of science. These items are, among others, magic, theology and metaphysics. What is so hopeless about all the many studies which year in and year out bona fide, philosophers of science produce, is precisely this: were these studies anywhere near the truth, then most of the problem in refereeing would disappear, and in the rare cases for which problems would persist, the semi-official philosophers of science would naturally be the experts to consult. Instead, at best these philosophers of science deliver their judgments post hoc, after the established scientists have passed their verdicts.

To be precise, one test case does exist: once, and probably once only, a philosopher of science was invited to speak as an expert on the matter at hand. It was the second "monkey trial", so-called, the court case, a few decades ago, in which a judge in Little Rock, in the State of Arkansas, USA, was called upon to adjudicate between the education department of that state and an organization of religious scientists who demanded that the official biology text-books should include proper reference to Scripture. It is a priori obvious that both parties were lamentably in the wrong: the education department was illiberal, and the other party was disingenuous as its aim was missionary. The judge had little choice but to side with the education department, simply because the spokespeople of the other party were even more inept than the one who was invited to speak for the department. The philosopher argued that the religion is held dogmatically and science is not. Even apart from the many individuals who are religious scientists, this is a naked falsehood: there are non-dogmatic religious sects and dogmatism is lamentably common among scientists, religious and non-religious, more so among science teachers, and when it comes to the curse known a science-education inspectors, it seems that for them dogmatism is obligatory, though, as with many obligations, it is at times not carefully observed. This is no complaint about the judge: he was facing dogmatism on both sides and could not but choose the lesser one. In that Arkansas court on that day, science was the lesser dogma and thus the lesser evil. Moreover, siding with the opposite party would have raised constitutional issues quite gratuitously.

This fact is alarming. The more powerful science is, the more success it brings about; the more success it brings about, the more the dangers of its abuse. Left unchecked it will be abused. This, after all, is the major lesson we learn from all science fiction, and Mary Shelley, H. G. Wells, and Isaac Asimov span yarns of science fiction take the lesson to be mere fiction with no moral to it or as fiction with a pointless moral against the abuse of magic, not a real moral against the abuse of science. The great challenge of science, which Galileo took to be what Bellarmino disliked about it, was the challenge against dogmatism and for the autonomy of the individual. Today, many observers have reported, many admirers of science admire it because it is the best (most powerful) dogma around.

We have arrived back at the semi-official claim that magic is evil: pretending to be a science, it is pseudo-scientific. This is a most parochial attitude: most magicians (and theologians, and even most metaphysicians), operated (and still do) in societies in which there is no familiarity with science, so that they do not pretend to be scientists and so they do not qualify as pseudo-scientists. Even cargo cults, the magic rituals involving wooden copies of airplanes and other modern artifacts in the hope to induce the gods to grant them to their worshippers, even they scarcely qualify as pseudo in any sense. To say of Moses the Law-giver and of Jesus Christ that they presented their theology as science defies the imagination. Only in response to the assertion of Maimonides, that Moses the Law-giver was a scientist, could anything like the charge of masquerading be launched — validly or not. Contemporary semi-official philosophers of science, however, are not interested in all this: they care little about societies overseas; they advertise science here and this task includes discrediting the competition here. They thus permit themselves at times to be agreeably tolerant of theology and metaphysics where there is no direct competition with science. Here, however, theology and magic are deemed competitors, and so here philosophers of science act as bouncers for the exclusive club of science. The leading sociologist of science, Robert K. Merton, prefers the term "gate-keepers", as he deems it less offensive than "bouncers"; it is more offensive, as will be clear when we find the answer, which should guide the bouncer, to the central questions of the philosophy of science: who is and who is not a bona fide member of the club? What is science? Is there a quality to science that sets it apart from what the bouncers consider as the competition? For, clearly, science is open and gate-keeping makes it a closed club, a closed society in Popper's senseof the word.

4. Science is not Super Magic

What is science? Science is a body of knowledge; Science is what scientists do qua scientists; Science is a tradition; Science is any empirically involved research activity; Science is a faculty in the university. All these answers are true and meet the question, yet each is highly unsatisfactory. Hence, the question was ill-put. The trouble is that some contrast science with dogma and others with magic. Does this matter? Are magic and dogma not more-or-less the same? Suppose they are; still, we can ask, do we dislike magic on account of its being dogmatic? If yes, then perhaps we like science because it is undogmatic; otherwise perhaps not. Consider, then, this wording of the question: what is the essence of science?

This question is tricky; to avoid the hoary matter of the critique of essentialism let us re-word it: what differentiates science from the competition? This question invites an in depth study of many competitors. The study of alien cultures is so highly recommended that the bouncers will not condemn it unless it favors the competition. Yet controversy about alien cultures abounds, and consequently it is hard to contrast science with the competition. Example: Claude Lévi-Strauss, who created a revolution in views of myth and of magic. Is he a member or a competitor? The question is open: the literature in anthropology still struggling with it. Let us try to alter our strategy, then. Can we look at science rather than at the competition and find there some clear-cut characteristic that sets science clearly apart from all competitors? If so, what is it?

This is the problem of the demarcation of science as semi-officially understood. The traditional answers are these. Science is a body of theories sharing certain characteristics. What characterizes them is their certitude, the ability to prove their perfection and finality, or their high probability, or at least their being reasonable. predictive power, or simplicity, etc. The up-to-date answers differ. The most popular among them is this: science is not theories but people, a prestigious social class which lends prestige to some ideas. The traditional answers are advocated by most philosophers of science and by some historians of science; the up-to-date answer are advanced by most sociologists of science and some historians of science. There is no way to decide between the two alternatives, since the disagreement impinges on the decision procedure. Tradition offers a criterion by which to choose between theories; admit that criterion, and tradition has already won. Dispute is thus viewed as a contest between groups; ask which group should/will win, and they have already won.

The traditional view has won a great boost in the twentieth century, due to the impact of modern logic, but not without some radical alterations. The scientific character of a sentence shifted: first it was deemed to be not proven but provable. And, further, as one cannot know if a sentence will prove true or false before it is compared with experience, provability had to yield to mere decidability, where a sentence is decided if it is either proved or disproved, and it is decidable if it is either provable or refutable. This doubles the number of entries: not only a proven sentence but also its negation is scientific, as the negation of a proven sentence is disproved: a sentence and its negation taken together are together decidable. Now the claim was that though generally a sentence cannot be declared a priori provable, it was declared that every well-formed sentence is a priori decidable. The justification for the relaxation of the criterion of demarcation of science to the extent of granting the negations of scientific claim scientific status was the wish to corner the competition once and for all by permitting the competition to contradict science openly. If the competition does contradict science, they will put themselves to ridicule, and if not, they will be exposed as saying nothing.

The very idea that the advocates of this view could direct the competition and decide that they may say something or not, reveals their self-image: they saw themselves debating from a position of strength: they saw themselves serving science on the supposition that it is winning anyhow. Then they met the surprise of their lives when they learned (from Kurt Gödel) that even in mathematics decidability is unattainable. In computer science decidability itself may be empirical (not in their sense of empirical, though). A computer may decide the truth or falsity of a sentence; the decision on some sentences may take too long to complete — and is possibly not performable at all. When such a task is given to a computer, it may be unknown whether it will ever be finished. If the computer finishes, the task is performable, the truth or falsity of the sentence in question is decided, and hence it is decidable; but until the task is completed it cannot be decided whether the task will or will not ever be completed, whether the sentence is decidable or not, that is to day, whether or not it can be proven or refuted.

At this juncture the story of contemporary philosophy of science gets much too involved. First, there are modified conceptions of the empirical character of science: the requirement from a sentence that may claim (empirical) scientific status is lessened by leaps and bounds. The exercise of the lessening of the requirement is curious: the input into it increases all the time, yet the output becomes less and less satisfactory, to the point that its own advocates are too unhappy about it to conceal their displeasure with it. Briefly, the idea of certitude is replaced by probability, by a limitation on the domain of the validity of the proof, and by the abandonment of the very concept of proof (which imports finality) in favor of the concept of relative truth. What all of these substitutes for the idea of decidability share is the following, incredibly fantastic idea: though a sentence is not usually decidable, its scientific character is. In other words, though finding the truth or falsity of a sentence is not generally assured, the truth or falsity of the claim that it is scientific is easily assured. This idea is fantastic, because one way or another science is linked to truth, no matter how tenuously. The fantastic idea is accepted upon faith. What accepting on faith means is not clear, but its political implication usually is: the society of the elect are known by their faith.

This is how the first characterization of science as decidability, upheld by philosophers of science, slowly degenerates ask, as curious observers, what is the root of the success of science?

5. Science is Public and Empirical

Our question has undergone some transformations. It was initially, what is science? This was replaced by, what is the essence of science? This was translated to, what differentiates science from the competition? And this was narrowed down to, what characteristic is peculiar to science? To avoid repeating the exercise we translate this into the following, final wording: what is the specific characteristic of science? (The word "specific" in the question by tradition hides an essentialist gist, but let us not be finicky.)

There are two very generally accepted answers to this last question, what is the specific characteristic of science? Had the two answers overlapped, this would be very comforting, as we would conjoin the answers; but they do not. The one is, science is public; the other is, science is empirical.

Take the public character of science first. The claim made here is that most intellectual activities are esoteric, closed to the general public, that entry is conditioned — whether on some natural gift or some specific preparation not given to all or both. Is this true? If so, then by what virtue do the philosophers of science dare exclude people who wish to be or appear scientific? More than that: as science is open-exoteric — it needs no defenders. It needs recruiting officers, talent scouts, instructors; but why bouncers? What does it matter to science that some esoteric groups appear to be exoteric and other groups have esoteric reasons to oppose science? What does it matter to science, asked Einstein, if this or that church opposes it? If it is necessary to expose and unmask those who masquerade as scientists, is it not best to do so by examining the question, how open are their clubs? Perhaps the bouncers suggest that this is not such a good idea as it may deprive them of their jobs; if so then they are disqualified from debating this question because of a conflict of interest!

The second answer is that science is empirical. Now surely Sir Karl Popper is quite right when observing, as a matter of historical fact, that in some straightforward sense astrology and alchemy and even parapsychology, are empirical as well. Leading philosophers of science are outraged by this observation, and they protest that the empirical evidence in question is highly questionable, that often it is simply lies. This complicates matters immeasurably by raising two tough questions. What evidence is not questionable? Are all scientific reports honest and all parapsychological ones lies? It has been reported that some people pose as parapsychologists and are liars; it has also been reported that some people are genuine parapsychologists and are not liars. Are the reported liars not simply pseudo-parapsychologists? Since science is untainted by people who falsely call themselves scientists (they are unmasked as pseudo-scientists), surely the same privilege should be granted to parapsychology! The question here is not about liars (as this is no sociology, criminology, or cultural history), but, what grants a theory scientific status? What characterizes science? Supposing it is empirical character, are we to alter this supposition in the light of criticism to say that it is the employment of scientific empirical evidence? This, surely, is hardly helpful, unless we know what makes evidence scientific. Whatever it is, two obvious, extreme answers are unacceptable. The one is that scientific evidence is true: history is full of (historically important)empirical evidence that is known to be false. The other is that empirical evidence is bona fide. For it is undeniable that some parapsychology is bona fide; indeed some famous individuals whose contributions to empirical science is unquestionable were known parapsychologists. William Crookes is the standard example for that.

The matter of alchemy or of astrology is even more complex: historians of alchemy and of astrology tell us that the better practitioners of these activities were bona fide, and that some of them even contributed to what is now deemed chemistry and astronomy. And we have still not said whether all the bona fide empirical evidence should writings of Galileo Galilei concerning this question. It is called the literature on theory-ladenness, and for the following reason: some empirical evidence is based in part on theoretical suppositions, and there is the need to demarcate the scientifically admissible cases from the rest. For, the theoretical suppositions in question may be false and then so is the evidence partly based on them. Suppose the suppositions in question are known to be true. On what grounds? Suppose their truth is known a priori. Then science cannot be said to be thoroughly empirical; assuming, as we often do, that all intellectual activity involves some empirical component, and that some empirical components are partly based on some suppositions, then we need a criterion to demarcate the scientific empirical evidence from non-scientific. Suppose, then, that the theoretical suppositions behind admissible empirical evidence are known to be true on some empirical grounds. Does this then constitute a vicious circle? Are all of these items of empirical evidence free of theoretical supposition? If no, then the question returns full strength. If yes, then there is some empirical evidence based on nothing but experience. Can this exist? If so, do we have an instance of it?

Philosophers of science hardly ever stay to hear all of these objections. Usually they or their seniors are in charge of(politically significant) discussions and they curtail them long before they are exposed that much. They have a strong technique to justify their impatience: no matter how abstract and distant from real life their discussion is, they sooner or later complain that their opponents are remote from real life. In real life, they intimate, science is successful. This success is analyzed. And the outcome of the analysis tells us that the success is predictive, i.e. it yields successful forecasts. Thus, if we have no proof, we have systematic probability: the earthly success of science is too systematic to be merely accidental; rather, this systematic earthly success is due to the systematic success of science in its efforts to confirm its theories.

The traditional idea of repeatability cuts across the discussion presented here; it does not deny (or endorse) the empirical evidence of astrology and parapsychology; it does not rely on (nor put down) the promise of predictive success for science; it does not promise that the empirical generalizations of science will for ever stay true (nor that they will be refuted).It rests on the suggestion that science is an ongoing investigation of great interest, regardless of the pragmatic success or failure that it may bring in its wake.

6. Science Power Worship is Hilarious

The discussion thus leads to the question, how come science is so systematically successful in its efforts to confirm its theories? What is the trick? Can it be learned? Can it be emulated by parapsychology? The answer must be, it can be learned, or else the success would not be repeatable. How can it be learned? There are two answers to this question, that of the traditional philosophers of science and that of the sociologist of science led by Michael Polanyi and his follower Thomas S. Kuhn. The one is exoteric, and so should be able to describe the formula that makes science an ongoing success; the other is esoteric and describes the knowledge of the formula as ineffable personal knowledge of the trade secret which is transmitted by master to apprentice. This is the worst aspect of the philosophy of science as currently practiced: science is predictive success or it is nothing; and if the worst comes to the worst, then scientists are better viewed as exoteric magicians who simply deliver the goods and no questions asked. But the trick is to kill time in prolonged debates and take as much time as possible getting to the worst, and in the meantime act as bouncers. The following review of the current discussion will clearly show that it is performed only in order to kill time, that the ideas which historically were once bona fide and serious, and which are still involved in the current time-killing activity, are no longer serious or bona fide.

There are two schools of thought in the establishment of the philosophy of science, inductivists and instrumentalists. Inductivism is the preferred view, as it suggests that scientific theories are provable, or at least probable. It is not clear what this probability of theories is, and it is not clear which evidence goodness of science is shown as it yields useful predictions, or probable forecasts, becomes more than a touchstone; it becomes the criterion of goodness: science, it is then suggested, is nothing but applied mathematics; its merit is practical and nothing more. This philosophy of science — instrumentalism — is anti-intellectual as it empties science of all theoretical information, and so deflates its value . In anti-scientific philosophy instrumentalism is advocated all along. In philosophy of science proper it is advocated only in a pinch. Inductivism, which is the preferred view, turns out to be but a variant of instrumentalism, though perhaps a more comfortable one. And once instrumentalism is taken for granted, the idea that science is a social class becomes its natural extension.

Assuming, for the moment, that the value of science is nothing but true forecasts does not yield the conclusion that all true forecasts are desirable. The approval of true forecasts runs against the very well known, commonsensical facts, first that some forecasts are terrible and are better not fulfilled and second that a true forecast may mislead.

There is no question that this is the case, and philosophers of science are not in the least unaware of it. They do not deny this either. Most of them, however, simply ignore it. What this oversight amounts to is clear: we are in control and there is no reason to fear that our forecasts are alarming or that we are misled by them. This is establishment talk. The world is threatened by destruction from pollution, from the proliferation of nuclear weapons, from population explosion, and from the ever-increasing gulf between the rich nations and the poor. But there is nothing to worry about. All will turn out to be well.

Query: is this a scientific forecast or a false prophecy? It is neither; it is a fad.

To see how unserious all this is one only needs see the low level of the current debates in the leading literature on the matter. The topic common to both inductivists and instrumentalists is the question, are there any items of empirical information free of theoretical bias? Is there any "pure" evidence? Or is all evidence theory-laden?

The onus here is on the party that says, "pure" evidence exists: they should offer instances. They cannot. they should offer a theory. They cannot. The only examples for this are Bacon's naive realism and Locke's sensationalism. Naive realism is refuted: the naive see the sun rise and set, and, to cite an example of Erwin Schrödinger, the sun appears as not bigger than a cathedral, which means, given some simple trigonometry, that the distance between east and west is less than one day's walk. In an attempt to replace naive realism in view of the criticism from Copernicanism, Locke revived sensationalism, claiming that motion is not perceived. It is. Sensationalism is refuted, anyway, by a myriad of experiments. This is the end of that discussion.

The difference between instrumentalism and inductivism concerns the content of theories. There is hardly any debate between the inductivists who ascribe to theories informative content and the instrumentalists who deny that and read theories as a mere façon de parler. Rather, each party struggles with its own problems. At times, when success is found to be inductively unfounded, inductivists yield to the instrumentalists. At times, when realism is in demand, instrumentalists play the same game in reverse.

The theory of induction contains two competing sub-theories, which deal with the question, what kind of evidence confirms a given theory? Both of these sub-theories violate the only rule of science universally endorsed within modern science since its inception in the early days of the scientific revolution: neither one confines its discussion to repeatable, (allegedly) repeated observations; rather, both refer to unique items of experience. In addition to this, each of these sub-theories is easily refuted by very simple arguments. A vast literature is devoted to these refutations in an effort to get rid of them. But, as usual with public-relations spokespeople in a defensive mood, these difficulties are seldom stated, and so they sound arcane.

The first of these two sub-theories of inductive evidence is instances to it. What then is an instance to a theory? What is an instance to a theory of gravity? Anything falling? Decidedly not: a falling feather disobeys even Galileo's theory of gravity. What then counts? Rather than discuss gravity, the inductivists discuss such generalizations as, "All ravens are black", forgetting that when asserted within science, these are items of evidence, not theories. What then counts as an instance? Every item that does not contradict a theory is an instance of it, since theories can be stated as prohibitions (there exists no perpetual-motion machine; no gas deviates from the gas-law equation). And then every item that is not prohibited by a law instantiates it! It sounds very counter-intuitive to admit every non-refutation as an instance, since this invites all irrelevancies into the picture. This is known as Hempel's paradoxes (in the plural) of confirmation. The counter-intuitive character of this fact is taken to be a powerful criticism of the theory, despite the obvious fact that the theory is anti-intuitionist and so its advocates should not be disturbed in the least by its counter-intuitive character. For, were it permitted to rely on intuition, then intuitions that some laws are true is strongest, and so it dispenses with the problem of induction ab initio. A vast literature is devoted to efforts to rescue the instantiation theory of induction from its (seemingly?) counter-intuitive character.

The second sub-theory of inductive evidence has for its background the musing that there is a function describing confirmation, which determines uniquely the value of the confirmation of any theory by any set of evidence. Moreover, the unique function must conform to the calculus of probability (so that the confirmation of a theory by evidence is the conditional probability of that theory given that evidence). If the function is not uniquely determined, at least all such functions must be given to affine transformations, but the inductive philosophers have not yet discussed this obvious requirement. The musing in question has two advantages. First, it identifies the vague concept of probable hypothesis with the clear concept of conformity to the mathematical calculus of probability. Second it offers a clear-cut estimate of probability — on the further musing that the probability of an event equals the distribution to which it belongs. Except that this musing has no room for distributions other than those offered by theories whose probability this musing should help us estimate.

But evidence does play a great role both in research and in practical life, the inductivists exclaim in exasperation. Indeed this is so, and from the very start; what they have promised to expose us to, however, is not this fact but the answer to the questions, how and why? They even overlook the more basic question, which is, does evidence play the same role in research as in practical life? There is no reason to assume that this i s so, since researchers take risks that they are not allowed to inflict on the general public. Yet the inductivists take it for granted that evidence plays the same role in research as in common experience, and they avoid discussing the matter, even though investigators, be they detectives or scientists, from popular fiction or from real life, do pay great attention to minute details, as they must, and then, when their search is concluded, they ignore most of the minute details and blow up the rest. How else can the small details of scientific discovery grow so large as to cover the whole of our city-scape?

When researchers, detectives or scientists, follow clues, they do so at their own risk. Hence, science is not as successful as it looks. Even in fiction detectives do lots and lots of leg-work that ends up in blind allies. But when successful, results have to be tested and confirmed, and their confirmations have to be easily repeatable. When the success in question is scientific, it matters little to the practical world what it is. When the success is claimed to be worldly, then there are legal standards for tests and for deciding whether they result in confirmations. In medicine, for example, a claim for success has to be repeated in vitro, then in vivo on laboratory animals, then on human subjects under specified controls, and then prove satisfactory by some complex standards. Philosophers of science assiduously ignore the relevance of all this in their discussions of the role of confirmation in science i n general.

7. Popper's Critique of Inductivism is Overkill

Popper's critique of the instantiation theory of induction is concerning what counts there as confirmation, and these should betaken into account when a theory of confirmation is presented: not all instances confirm theories but, at most, those which were expected to refute it and failed. This is admitted obliquely by Carl G. Hempel, the chief discussant of the matter of instantiation and its afflictions, but not openly. Yet he is not satisfied with the situation as he seeks a formal criterion for confirmation. He thus cannot fully admit Popper's (empirical) assertion that at most only failed refutations of a theory confirm it, as failure is not a formal criterion.

The more extensive criticism which Popper has published against the theory of induction is directed against the identification of confirmation as a function abiding by the calculus of probability. This is surprising, since the probability sub-theory of confirming evidence and the instantiation sub-theory of confirming evidence are, of course, but two variants of the theory of induction, and who knows how many other variants are possible. Perhaps he does soon account of its lingering popularity. For decades now he presents criticisms of this idea, and it would have been dropped from the agenda, were the advocates of this idea able to exhibit some sensitivity to devastating criticism. Moreover, had they been more open to his criticism, they could try to develop more variants: this is in accord with Popper's own view: the admission of criticism opens the road to new options. But then they might also realize that there are non-inductive options to explore.

First, says Popper, confirmation cannot be probability as it reflects the force of the evidence and not the informative content of the theory prior to evidence. Therefore, at least confirmation should be probability increase, not probability itself. (This, he embarrassingly adds, resembles Galileo's announcement that gravity is proportional not to velocity but to its increase.) And probability increase is certainly not a function abiding by the formal calculus of probability.

The point is easy to demonstrate. Here is Popper's demonstration.

Let us write " $P(h) = r$ " and" $P(h, e) = r$ " to denote absolute and relative probability in the usual way; suppose a theory h1 is absolutely probable and some evidence e1 reduces its probability, whereas h2 is improbable yet some evidence e2 (which may be the same as e1 if you wish) raises its probability, but not much, so that

$P (h1) > P (h1, e)$,

$P (h2) < P (h2 , e)$,

Deleted: yet

yet

$P (h1 , e) > (h2 , e)$.

Clearly, though h1 is more probable than h2 it is more confirmed by the evidence. The objection that this is impossible is groundless. Moreover, a model for it is easy to construct. Here is one.

Consider event E which is the next throw of a die. Take the following cases:

h1: E is not a 1.

h2: E is a 2.

e : E is 1 or 2 or 3 or 4 .

and

$e1= e2 = e$.

Now,

$P (h1) = 5/6$

and

$P (h2) = 1/ 6$,

so that

$P (h1) > P (h2)$

and

$P(h1, e) = 1/2$.

and

$P(h2, e) = 1/4$,

Now,

$P(h1) > P(h1, e)$.

so that the evidence undermines h1, whereas

$P(h2) < P(h2, e)$.

so that the evidence supports h2, yet

$P(h1, e) > P(h2, e)$.

Hence, h2 is supported by the evidence yet is less probable than h1, which is undermined by the evidence.

This elaborate proof is superfluous, as is the model for it. It is merely a tedious if striking application of the point made by Popper in 1935 and since then generally received: probability is the inverse of informative content and science is the search for content; hence, science is not the search for probability.

The more serious criticism of the identification of confirmation with probability is directed at the identification of probability with distributions. The probability of a hypothesis concerning a distribution cannot possibly be the same as the distribution it depicts, since we have competing hypotheses concerning a given distribution, and the sum of their probabilities is a fraction, but they can each ascribe a high distribution so that the sum of their distributions will exceed unity. Attempting to escape this criticism one may seek refuge in the preference for equi-distribution. This lands one in the classical paradoxes of probability. Attempting to escape this criticism one may seek refuge in the preference for confirmed distributions. This not only begs the question: it also raises the paradox of perfect evidence: the evidence that fits a given distribution perfectly both raises its probability and keeps it intact, which is absurd.

How can one go on examining the defunct option that science equals probability? Only on the supposition that science is a success story; most philosophers of science are convinced that the difficulties on the road to presenting science as a success story are marginal.

Is science a success story? Decidedly yes. What kind of the first details are clear: science is a success story in that it needs no defense and it is a success story not in their (vulgar)sense of the word.

8. Science is more than Scientific Technology

The vulgar view of science as success is the view of the scientist as a person with a powerful insight, a sort of a magician. Surprisingly, this view does not conflict with the view of science as esoteric, since it alleges that only scientific research is esoteric, not the fruits of science, which are for all to see. The idea that scientific research is somewhat mysterious does conflict with the inductivist idea that research, too, is open to all. This is the idea that science is open to a simple algorithm that can be mastered by everybody. This view of science is dismissed by Popper derisively as the idea of a "science-making sausage machine". Under the influence of Einstein it is now generally rejected as too simplistic — by all except some zealous adherents to the original idea of artificial intelligence. Today there is a vast and exciting literature on techniques to aid the process of developing ideas that may lead to discovery ("heuristic", is the Greek word for this, coined by William Whewell, the great nineteenth-century philosopher, one of the first to criticize the idea that there can be a science-producing algorithm). (There are examples of supposedly useful heuristic computer programs; they are far from having been tested and, anyway, heuristic is the very opposite of an algorithm proper.)

The idea of a science-producing algorithm proper was recently replaced by, or rather modified as, the idea of normal science, so-called, developed around 1960 by Thomas S. Kuhn. The popularity of his view rests on his conception of science, normal and exceptional. The exceptional scientist is the leader who prescribes a paradigm, namely, a chief example, and a normal scientist solves problems following it. This suggests that the real magic rests in the leadership, the scientific character of the enterprise they lead rests on the obedience of their followers, the normal scientists, and the problems the normal scientists solve are quasi-algorithmically soluble: they are not so simple that a computer or a simple mind can solve, but they are not so difficult as to defy solution.

It is easy to see the allure of this philosophy: it balances a few ideas that seemingly conflict with each other but which share the goodness of being both popular and useful for the celebrated philosophers of science; it presents science as assured but not without some expertise and hard work; it assures science its openness to a reasonable degree, so that the philosophers of science can see a little of the mystery involved — just enough to advocate it but not enough to partake in it actively.

What is missing in the concoction is the mystery — not the alleged mystery of the leaders of science who cannot and would not divulge the secrets of their craft, but the unmistakable mystery that is the secret of the universe.

It is not that the philosophers of science are not willing to praise science as the big search; after all they will say anything to glorify it. But they will use ad hoc social criteria to judge when it is advisable to praise the search as the intellectual frontier and when to present the successful results of science. Except that they claim to be philosophers, and thus bolster each move with a principle. Thus they render complementary compliments into contradictory credentials.

9. Science is A Natural Religion

There is much to do other than gate-keeping. Certain grounds may perhaps be cleared. Certain assertions should be endorsed as a matter of course or clearly dissented from, though, of course, we may also examine them in great detail if we wish. It should be admitted that traditionally science admits as evidence only repeatable evidence, though we may examine this characteristic in great detail if we wish. It should be granted that traditionally science grants only items open to public scrutiny, though we may examine this characteristic in great detail, too, if we wish. It should be granted that some of the evidence which science traditionally admits as true is later on deemed false, but not know right now why these rules are deemed obligatory. The answer to this question is simple: it is taken to be the role of empirical science to explain known facts.

More should be stressed at once: the rules are introduced not as taboos but as reasonable and as commonsense. It should be clear that one may break any of these rules, but openly and at one's own risk. The paradigm is Max Planck, who took upon himself a most unusual research project and voluntarily and as much as he could he ignored all items that he could not square with it. This was his own private road to his discovery of quantum mechanics.

Past this we are ignorant, and it is advisable to admit ignorance in many areas and open them up for genuine research that may get the philosophy of science out of its recently-acquired role as gate-keeper and bouncer and into a proliferation of researches. We do not know how empirical empirical science is, though we have the feel that some technologically-oriented researches are much nearer to common experience than some speculative studies of first principles. We do not know how a research report is judged scientific and/or deserving of publication. We know that some erroneous criteria are used, and that much latitude is exercised in the matter; but more information is needed and more deliberation and experimentation. Moreover, of late a new challenge to the philosophy of science developed, and one that enables researchers in this field to do some genuinely exciting empirical work: the traditional problems in the field, including the questions just raised, concerning the objectivity and the reliability of empirical science are clearly open to empirical study. We do not know how much of science is empirical and how much is guided by general principles, by the culture at large and even by politics — international, national, or of the local chapter of a scientific society or the local department or laboratory in the university. We simply do not know enough about how scientific activities intertwine with other activities, and we only have an inkling as to what is the minimum requirement for a society that wishes to allow it to flourish, namely, freedom of speech and of dissent and of criticism and of organization to protect and enhance them.

It is hard to say what other item, if any, is generally admitted as a basic tradition.

This invites one to scout beyond one's horizon, and seek in the past some heuristic that might be helpful. And one might start with the roots of the unbecoming hostility to metaphysics and to religion that is so characteristic of contemporary philosophy of science that induces its practitioners to undertake their task as if on faith alone. The rise of modern science is the starting point, as the heritage from that noble period is in great need of revision.

Here only one aspect of that period will be mentioned, the idea of natural religion. It is the idea that religion comprises a doctrine plus a ritual, that the doctrine is either revealed (to a selected group) or natural (to all thinking humans as such), and that the ritual of science is prescribed in accord with its doctrine. All of these items are nowadays discarded, but let us overlook this for a while.

It was the received opinion that natural religion is consistent with and supplemented by revealed religion. This cannot be admitted without some qualification on the religion under discussion, but here no specific religion is discussed. The doctrine of natural religion, natural theology or rational theology, so-called, is the proof of the existence of God. This proof is now dead. The ritual of natural or rational religion is research as worship. This has a tremendous attraction to some researchers, some of them scarcely known, others well-known, like Einstein; yet most researchers, known and unknown, consider it plainly silly. The main obstacle in this matter is not any item under consideration, but the very idea of religion as belief. The involvement of belief as a central item in any religion is a very strong item of all western religions (though not of all eastern ones). It also led to the idea that superstitions are prejudices, objectionable beliefs, or at least unwarranted ones. Yet superstitious people, whom the philosophers of science describe as dogmatic, are notoriously skeptical, though they particularly lack the ability to be critically minded about their guiding ideas.

Traditional philosophy of science took it for granted that the erroneous metaphysical systems, now better known as intellectual frameworks. It recommended to be faithful by refusing to endorse any idea unless it is proven. Then Kant proved that a proven intellectual framework is a set of synthetic propositions a priori proven. Then Russell and Einstein between them proved that such propositions do not exist, and the gate-keepers decided to oust all intellectual frameworks. These were reintroduced by social anthropologists and legitimized by the posthumous writings of Ludwig Wittgenstein, who spoke of them, somewhat enigmatically, as "forms of life". Then various historians of science of the Koyré school claimed that indifferent times science engaged different intellectual frameworks, and these were then legitimized by those who identified them with Kuhnian paradigms. This is a confusion, since Kuhn's idea of scientific paradigms was introduced in order to prevent the conflict between the scientific systems of different ages. (To use his jargon, he insists that paradigms are incommensurable. They can be compared, he stresses, but not contrasted.) The confusion, and the bouncing that goes with it, will be cleared once we notice that intellectual frameworks do compete, and that science may both use some of them and be used as arguments for and against some of them.

Obviously, a researcher may consciously and clearly follow two different guiding ideas, employ competing intellectual frameworks — from not knowing which of them is true. Taking notice of this simple, commonsense fact will free the theory of scientific research from its obsession with rational belief. Current philosophy of science is fixated on the study of rational belief without any criticism of traditional ideas of belief in general and of the particular ideas of scientific or rational belief, of religious belief, and of their interaction. The source of this fixation is Sir Francis Bacon's superbly intelligent and highly influential doctrine of prejudice. Theories color the way facts are observed and blind their advocates to contrary evidence, he said, so that they are immune to contrary evidence, so that their advocates are blinded to such evidence and thus become prejudiced; the prejudiced then perpetrate their prejudices by endlessly multiplying evidence in their favor. To become a productive researcher, then, one must give up all one's preconceived notions. This theory is magnificent even though it is amply refuted. It animates the pseudo-researches of the self-appointed gate-keepers of science. The worst of it is that philosophy of science centers on the problem, what theory deserves acceptance, where acceptance means credence, yet it is well known that we are unable to control our credence, certainly not to confine it to a simple algorithm. It is here that the roots of the erroneous view of science as a competitor of religion can be found and corrected. This is not to deny that scientific research can be a thoroughly religious affair, a dedication to the search for the secret of the universe. Nevertheless, the religious aspect of research is not obligatory.

Once this is realized, the avenue is open to the study of science as a central item in our culture and to see its interaction with other items in our culture. It is interesting to view the philosophy of science not in isolation but as part-and-parcel of our culture. What isolates the philosophy of science from the philosophy of human culture in general is the idea of the gate-keepers that any item not quite scientific is inferior. This idea is not quite philosophical. Nothing human is alien to any philosopher-of science or of any other aspect of human culture.

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N O T E S

[1] There is little literature in metaphysics proper these is not derived from physics is claimed to be either a priori proven, which is impossible, or else vain speculations. This is so not only for the philosophy of nature, but for all philosophy. Exceptions exist, however, like H.J. Paton, who said (In Defence of Reason, Hutchinson, London, 1951, p. 13), "The business of Philosophy is to be synoptic". This view contrasts with Ernst Mach's view that he needed no philosophy, since his worldview consisted of the sum total of all the scientific theories extant. Mach's view was shared by Max Born; see note 11 below.

[2] See for more details my "Between Science and Technology", Phil. Sci., 47, 1980, 82-99.

[3] For the best presentation of this image of Kant see Stanley Jaki's introduction to his edition of Kant's Natural History of the Heavens , Scottish Academic Press, 1981.

[4] There are exceptions, of course, such as Martin Heideggers Einfluss auf die Wissenschaften, A. Francke, Bern, 1949. It seems to have fizzled out more-or-less at once. See Ernest Gellner's review if it in Philosophical Quarterly, 1, 1951, 369-70.

[5] For more details see my review of David Stove, Popper and After, Philosophy of the Social Sciences, 15, 1985, 368-9.

The significant corollary of Stove's proposal is that the barrenness of the philosophical exercise is to be calmly accepted- which is the chief corollary of skepticism. This the peculiarity of the whole tradition of analytic philosophy, and is known as the paradox of analysis. A. J. Ayer's famous The Problem of Knowledge, Macmillan, London, 1956, solves the problem of (everyday) knowledge by deflating it. Ludwig Wittgenstein, On Certainty, Harper, NY, 1969, is harder to fathom, but its message seems to be one need not worry about one's uncertainty — in line with his claim (Philosophical Investigations, Oxford, Blackwell, 1953, I, 255) that asking a philosophical question is sick.

[6] See my "On Explaining the Trial of Galileo", republished in my Science and Society, Kluwer, Dordrecht, London and Boston, 1981.

[7] See the relatively short (seven typed pages) Speech of Sciences, 31 October 1992. In it the Pope repeats his hope to "dispel the mistrust that still opposes, in many minds, a fruitful concord between science and faith"; he then discusses the theological dispute between Galileo and "the theologians who opposed him"— without mentioning them by name and without reporting that Galileo claimed that there is no authority over interpretation. Later on the Pope explains: "the majority of theologians did not recognize the formal distinction between Sacred Scripture and its Interpretation". The Pope then explicitly exempts St Roberto from this charge, as if the Saint sided with Galileo, whereas, as a matter of clear and uncontested historical record, accepted even by the Saint's official biographer, the dispute was between him and Galileo, and both were extremely clear about the distinction between text and interpretation and disagreed about the matter of the authority: the Saint claimed (citing the decisions of the Council of Trent) that Galileo needed his permission when deviating from traditional interpretation, and Galileo demanded scientific freedom and the right to leave to theologians the task of reconciling science with Scriptures.
The end of the Pope's speech can scarcely be to "dispel the mistrust that still opposes ... a fruitful concord between science and faith". It is merely his (reluctant) acknowledgement of the strength of science in his own province.
[8] For the task of sifting the grain from the chaff in the claims of the ecological and the peace movements see my Technology: Philosophical and Social Aspects, Kluwer, Dordrecht, London and Boston, 1985.
[9] For more details see my "The Functions of Intellectual Rubbish", Research in the Sociology of Knowledge, Science and Art 2, 1979, 209-27.
[10] For more details see my "On Pursuing the Unattainable", in R. S. Cohen and M. W. Wartofsky, editors, Boston Studies in the Philosophy of Science, 11, Kluwer, Dordrecht, London and Boston, 1974, pp. 249-57; reprinted in my Science and Society, op. cit.
[11] Max Born, Natural Philosophy of Cause and Chance, Clarendon Press, Oxford, 1949, ("Introduction" and "Metaphysical Conclusions") conveys his hostility to philosophy less than his explanation to me of his refusal to gratify my request for his help in my struggle with the philosophical problems of quantum theory. He stated emphatically that unquestionably science always has a higher claim on one's endeavors than philosophy. The reason is that Born was convinced that philosophy either follows science or fails. See note 1 above. To offer an alternative that is not synthetic a priori seems impossible, but is not: conjectural metaphysical systems that may clash with current scientific theories may lead to their reinterpretation that may crystallized into an alternative to them. For more details see my "The Nature of Scientific Problems and Their Roots in Metaphysics", in Mario Bunge, editor, The Critical Approach: Essays in Honor of Karl Popper, Free Press, N.Y., 1964, 189-211 and my "The Logic of Science and Metaphysics", Philosophical Forum, 5, 1974, 406-16; both are reprinted in my Science in Flux, Kluwer, Dordrecht, 1975. See also my Faraday as a Natural Philosopher, Chicago University Press, 197l.
[12] This refers to E. T. Whittaker, A History of the Theories of the Aether and Electricity, Vol. II, 1953, and to his contribution to the Reports of the British Association of 1913. For more details see my Towards an Historiography of Science, Beiheft II, Theory and History, 1963.
[13] Traditionally, what matters here is the philosophers' not out of submission (or rebellion) but independently of it. Sir Francis Bacon and René Descartes were philosophers who trusted their own judgments, for better (Descartes) or worse (Bacon) but honorably. It is often unclear whether a philosopher's endorsement of the judgment of a scientist is independent; nor does it matter overmuch. It did matter when all endorsements of the established reading of quantum mechanics, for whatever reason, were all greeted with pleasure while all dissent, even if well-informed, was frowned on: this conduct is conducive to submission, of course. So the major requirement to be an honest, independent philosopher of science on the contemporary scene is to be indifferent to scientists' frowns. This is where the bouncers come in: they try to see to it that the frown is translated into social and economic pressure.

A curious exception is Imre Lakatos (in his dubious capacity as a philosopher of science; as a philosopher of mathematics he was of a different class): he expressed in print his readiness to be in charge of the screening of research projects for public finance. Gerald Holton cited Lakatos on this in his The Advancement of Science and Its Burdens, Cambridge University Press, London, 1986, 277, and dismisses him out of hand. His dismissal, it seems, is not from the love of autonomy, nor from the dislike of outside interference into professional matters, but plainly on the (undeniable and sufficient) premise that Lakatos was too ignorant for the task. See Gerald Holton, "On Being Caught Between the Dyonisians and the Apollonians", Daedalus, 103, 1974, 65-81. In this essay Holton expresses his ambivalence towards Popper, and in this vein he describes Lakatos as Popper's ignorant leading follower and heir to his chair (which is neither true nor relevant) after citing Popper noncommittally and before citing approvingly Peter Medawar with no mention of his being a follower of Popper as well. See also his The Advancement of Science and Its Burdens, p. 9 for another, lovely expression of this ambivalence.

The trouble with the previous part of this note is its confusion of science with the scientist and philosophy with the philosopher. Some scientists do express philosophical opinions, even in debates well within the domain of their researches; see for example the critique which P. W. Bridgman launched against the general theory of relativity from the operationalist philosophy. (Einstein demurred, by the way, in view of his own rejection of that philosophy.) See P. A. Schilpp, editor, Albert Einstein: Philosopher-Scientist. When a philosopher enters the field of science and tenders a critique while knowing what a critic should know, the profession simply considers the critical discussion on its merits and of course ignores the question of the credentials of the critique's author, as is a sacred rule of the scientific tradition. Therefore, though there are quite a few examples of this kind, they are seldom cited, except when the contributor is a major influence in philosophy and a minor scientific researcher.

Finally it is a curious fact that some leading scientists published terrific philosophical works that should draw the attention of philosophers yet scarcely do. Leading among them are, no doubt, Galileo, Boyle, Einstein and Schrödinger, perhaps also Maxwell.

[14] See Michael Ruse, Darwinism Defended: A Guide to the Evolution Controversy, Foreword by Ernst Mayr, Addison Wesley, Reading MA, 1982. In the original "monkey trial" matters stood quite differently: in the first case the state of Tennessee intended to prevent instruction (of evolutionism); the second case was a class action demanding to allow instruction (of creationism). The original case was of obscurantism versus enlightenment, with Clarence Durrow defending the good guy. He would thus not have dreamt of inviting expert scientists. See his autobiography. The second case was an oxymoron: a defense of science that can scarcely be called enlightened.

A curious example of the use of an expert in science occurred when Faraday introduced his theory of ionization. He introduced then a new terminology and this was taken as the focus of the displeasure which his revolutionary ideas were causing. He dispelled the displeasure by reporting that the terminology was suggested by William Whewell. Faraday stressed on that occasion that science is one thing and words are another. He also used his authority as a researcher both against the local authorities who were delaying legislating to reduce the pollution of the river Thames and against the new fashion of calling upon the dead in seances. These cases, however, were frankly matters of public relations, and they bored him soon. See my Faraday as a Natural Philosopher, op. cit.

[15] It was Samuel Butler who asked, in the end of his classic The Way of All Flesh, how do we survive the educational system? His answer is tremendously intelligent: we owe the survival of culture to the imperfections of the system of education. (This explains his attitude to Matthew Arnold, the leading educationist and educational reformer of his age.)

[16] For more details see my "Science in Schools", a discussion note in Science, Technology and Human Values, 8, 1983, 66-7. As far as I know there was no response to this note of mine, especially not by Michael Ruse, the expert witness in the Arkansas court who is criticized there (see note 14). He obviously relied on his (mis)reading of the works of Karl Popper, which he found necessary to ridicule on other occasions. This flexibility, I suppose, exempts him from the charge of dogmatism: the practice of public relations hardly invites an expression of a dogma.

[17] For more details about cargo cults see I. C. Jarvie, The Revolution in Anthropology, Routledge, London, 1964 and other editions. For details about the view of magic as pseudo-science (of Bacon and Frazer), see my "Deconstructing Postmodernism: Gellner and Crocodile Dundee", in John Hall and I. C. Jarvie, editors, Transition to Modernity: Essays on Power, Wealth and Beliefs, Cambridge University Press, 1991, 213-30.

[18] Maimonides would hardly qualify as a pseudo-scientist, since the aim of his claim that Moses was a scientist was to boost science rather than religion. See for more details my "Reason within the Limits of Religion Alone: the case of Maimonides",

[19] Perhaps the most awesome bouncer in the United States in the last three or four decades has been Adolf Grünbaum. A forceful example is his claimed that, unless causal, the study of human conduct is essentially unscientific and even irrelevant to science — whatever he precisely means (and I suppose that he wobbles between the tautologous and the absurd, but cannot tell for sure). This claim is made in the leading anthology, Herbert Feigl and May Brodbeck, editors, Reading in the Philosophy of Science, N.Y. 1953, and repeated on the first page of the volume of Contemporary Philosophy edited by G. Fløistad, devoted to the philosophy of science (Vol. 2), Kluwer, 1982.

Grünbaum has bounced out of the philosophy of science club Michael Polanyi and Karl Popper, who, I suggest, are the two greatest philosophers of science in the mid-century, and for the same crime: they reject the kind of empiricism that most physicists endorse. This is outrageous, especially in view of the fact that Grünbaum's own philosophy of science is as unclear as that held by most physicists: at times he is an instrumentalist and at time he advocates a kind of inductivism which he calls "neo-Baconinaism". Despite repeated requests he refuses to explain what it is.

[20] Ever since Paul K. Feyerabend ascribed to Ludwig Wittgenstein the opposition to essentialism, it was taken for granted that this ascription is correct and that hence essentialism is false, or at least not to be defended. A few brave philosophers did defend it nonetheless, most significant among them being Saul Kripke. See his Naming and Necessity (1972), Revised and Enlarged Edition, Oxford, Basil Blackwell, 1980.

[21] For details and references concerning the controversial status of the works of Claude Lévi-Strauss, see my Towards a Rational Philosophical Anthropology, Kluwer, Dordrecht, London and Boston, 1977, Chapter 2.

[22] Sir Francis Bacon introduced "the mark of science": alchemy promises the philosopher's stone and science proper will deliver the goods. See his Novum Organum, Bk. I, Aph. 124: "the goal and mark of knowledge which I myself set up"; "Truth ... and utility are here the very same thing"; see also his Works, 1857-74, 3, 232: "I found that those who sought knowledge for itself, and not for benefit or ostentation or any practical enablement ... have nevertheless propounded to themselves the wrong mark, namely satisfaction (which men call truth) and not operation". Unfortunately this was often read as relativist, despite clear anti-relativist remarks of Bacon, say, in his Novum Organum, Bk. I, Aph. 129 and throughout his writings, from his early manuscript Valerius Terminus onwards.

[23] The exception is Popper's criterion of demarcation which is within language rather than of language. This enabled him to afford the luxury of denying that a theory is ever provable, so that he was satisfied with refutability alone, thus ascribing scientific status to some theories and not to their negations. For more details see my "Ixmann and the Gavagai", Zeitschrift für allgemeine Wissenschaftstheorie, 19, 1988, 104-16.

[24] For more details see David Harel, Algorithmics: The Spirit of Computing, Addison-Wesley, Reading MA, 1987, 1992, Chapter 8, passages on high undecidability.

[25] For relativism see I. C. Jarvie, Rationality and Anthropology, Routledge, London, 1984. The popularity of relativism is now so great that Ernest Gellner speaks of it as a "Spectre". See his Relativism and the Social Sciences, Cambridge University Press, Cambridge, 1985, p. 82. See also there, p. 172: "My Form of Life, right or wrong.". See also my "False Prophecy versus True Quest: A Modest Challenge to Contemporary Relativists", Philosophy of the Social Sciences, 22, 1992.

It is important, both theoretically and practically, to find out as best we can, which of the regularities we observe is due to changeable local conditions and which is unalterable. The relativists cannot even pose this question intelligibly. See my Technology: Philosophical and Social Aspects, op. cit.

[26] For all this see Karl Popper, Objective Knowledge, Clarendon Press, Oxford, 1972, Chapter One.

[27] There is precious little discussion of these two points, of the openness of science and of the repeatability of scientific experiment, and these are brief, as if to intimate that these matters are both obvious and non-negotiable. Though they appear originally as one in Descartes' Discourse on Method, Sixth Part, fourth paragraph, and in the writings of Robert Boyle, such as the Preface to his The Skeptical Chymist, they usually appear as separate— if at all. The attempt to (re)unify them occurs first in Karl Popper, Logik der Forschung, Vienna, 1935, and later in the writings of Robert K. Merton.

[28] Einstein asked, in his preface to Stillman Drake's translation of Galileo's Dialogue on the Two World Systems, why did it matter to Galileo that the Church of Rome rejected Copernicanism? There are two sufficient reasons for that, I think, one that he was an obedient son of that Church, and the other that science at the time was under attack and regrettably had to fight back. This does not constrain, however, the correctness of the distaste towards bouncers that Einstein exhibited in that discussion.

[29] When Robert Boyle declared that unrepeatable experiments are unscientific, he insisted that there is no reason to call those who report them liars (Certain Physiological Essays, "On the Unsuccessful Experiment"). He gave examples for unrepeatable experiments, and one of them has turned out to be a true scientific observation. The open hostility to pseudo-science is a nineteenth-century product.

[30] Sir John Herschel suggested in the early nineteenth century that scientific evidence is bona fide. This is wonderful but no longer valid, as so many court cases testify. For more details see my "Sir John Herschel's Philosophy of Success", Historical Studies in the Physical Sciences, 1, 1969, 1-36, reprinted in my Science and Society, op. cit.

[31] This was stressed in Robert Eisler, Astrology: The Royal Science of Babylon, H. Joseph, London, 1946, which has since gained significance, despite its defects, from Derek J. de Solla Price's studies of the import of Babylonian science for the rise of Greek science. See his Science Since Babylon, Yale University Press, New Haven CT, 1962.

[32] See my "Theoretical Bias in Evidence: A Historical Sketch", Philosophica, 31, 1983, 7-24.

[33] For more details see my "The Role of the Philosopher Among the Scientists: Nuisance or Necessity?", Social Epistemology, 4, 1989, 297-30 and 319.

[34] See for all this my "Sociologism in Philosophy of Science", Metaphilosophy 3, 1972, 103-22, reprinted in my Science and Society, op. cit.

[35] For the difference between criteria of demarcation and touchstones see my Science in Flux, op. cit.

[36] The quest for "pure" evidence led Rudolf Carnap to the suggestion that there are purely observational terms which can be added to the basic (analytic) system of logic in order to form the observational language, so-called. As to the non-observational terms, the obvious choice is between leaving them undefined so as to use them to form theories and introducing them by implicit definitions that render theoretical science applied mathematics. Leaving them undefined raises the question of the validity of theories, or of the grounds for their endorsement — which is the classical problem of induction. Carnap vacillated all his life between allowing implicit definitions and dispensing with them. A llowing them one may side with the formalist view of mathematics and the instrumentalist view of science. Otherwise, as the logicist view of mathematics and the inductivist view of science are superseded, one is left with conjectures and refutations in both. See my "The Riddle of Carnap", reprinted in my The Gentle Art of Philosophical Polemics: Selected Reviews , Open Court, LaSalle, Ill., 1988.
A thin linguistic guise of the question at hand is the question, do theoretical terms signify? Moreover, this raises the funny question, assuming that "electron" signifies any electron and "space curvature" signifies a property of curved space, what is denoted by a still more abstract term (like "attraction" or "induction" or "energy")? And so odd questions filled the literature for some time, especially when it was noted that the division of language into two, the theoretical and the observational, leaves no room for a bridge between them. Further, Hilary Putnam noticed the questionable, tacit inductivist assumption that scientific theories are true, and thus he hit upon the question, what do the terms of false theories signify? Since the classical atoms (as envisaged by Democritos or Mendeleef) do not exist (atoms do disintegrate), what does the term "atom" designate? Assuming that the term designates nothing, the false classical theory turns out to be true! The matter has gone so far astray that we can find today even debates about "aboutness". Some attempts to circumvent this issue are hilarious: there is the hope that a causal theory of reference, for example, will render reference as objective as anything. Except that the instrumentalists will deny meaning to the term "cause". Nor is this all there is to it. One of the most popular topics of research in the philosophy of science in the mid-century was the study of dispositional terms. It was opened by Carnap in 1936 in his once classic Testability and Meaning, and is still not quite extinct even though it yielded no fruits and even though it was proven worthless in W. V. Quine's still classic "Two Dogmas of Empiricism" of 1950, whose target is Carnap's view of science. The rationale of the study of dispositional terms was not made clear by Carnap or his followers; they were introduced to the discussion in 1935 when Popper showed that one cannot verify a sentence with a dispositional term ("here is a glass of water"), as these lead to possibly false predictions, ("if hit hard enough this container will break; under electrolysis this fluid will turn into oxygen and hydrogen" etc.). Quine tried to put an end to the search for "pure" experience by his claim that the search is reductionist-phenomenologist, and that the radical untranslatability thesis blocks the road to any complete and satisfactory reduction. That thesis itself is trivially true, yet it raised much barren dispute. All this is covered by a variety of studies, some of which branch off rather wildly into postmodernism and other fashionable matters that cannot be adequately discussed here.
[37] See my "Sensationalism", Mind, 75, 1966, 1-24, reprinted in my Science in Flux, op. cit.
[38] The contents of some theories but not of all of them are meant as a façon de parler; this renders both theories false. For more details see my "Ontology and Its Discontents" in Paul Weingartner and Georg Dorn, Studies in Bunge's Treatize, Rodopi, Amsterdam, 1990, pp. 105-122. (This book appeared also as a special issue of Poznan Studies , Vol. 18.)
[39] For details see my "The Mystery of the Ravens", Phil. Sci., 33, 1966, 395-402, reprinted in my The Gentle Art of Philosophical Polemics: Selected Reviews, Open Court, LaSalle, Ill., 1988.

[40] The demand that all competing theories and all (relevant) information be considered is a safeguard against prejudice. It does not work, since it permits the refraining from the search for instances to the contrary. In the absence of any background knowledge, the demand that all competing hypotheses be examined nullifies their initial probabilities, since there are infinitely many hypotheses and the sum of their probabilities is unity. The introduction of any background hypothesis may easily alter this and render the problem very easily soluble. But the background hypothesis is plainly a prejudice. The literature includes a grand debate about the possibility of introducing some background material that will not count as a prejudice, except that this is never said so plainly. The debates seemingly concern the principle of simplicity (John Stuart Mill), otherwise known as the principle of limited variety (John Maynard Keynes), or of the redistribution of initial probabilities (Sir Harold Jeffreys). These do not work, but other hypotheses work very comfortably. For example, analytic chemistry works inductively very nicely against the background of the table of elements—provided its refutations are ignored. Nuclear chemistry, of course, requires different background hypothesis.

[41] See N. Laor, "Prometheus the Impostor", Brit. Med. J., 290, 1985, 681-4. See also my Technology: Philosophical and Social Aspects, op. cit.

[42] See my "The Mystery of the Ravens", op. cit.. In that essay I did not discuss the folly of the requirement that the criterion of confirmation should be formal. It clearly has to do with the theory of demarcation of science by meaning, presented above, which presents science as in principle utterly decidable and the competition as unable to articulate except by either endorsing or rejecting some scientific verdict or another. In brief, it is the idea that a formal criterion makes the life of a bouncer easy. In a public discussion at the end of a session of the Eastern Division of the American Philosophical Association in Boston some years ago, devoted to the contributions of C. G. Hempel to the philosophy of science, I said that researchers do not require license from the philosopher before they dare employ a metaphysical theory in their researches. To this Hempel answered that at least his theory of confirmation was intended to oust theology, and did so rather well.

[43] For the critique here cited see Popper's "Degree of Confirmation", 1955, reprinted in New Appendix *IX of his The Logic of Scientific Discovery, 1959 and many later editions.

[44] Popper's point is that informative content (not in the sense of information theory but in Tarski's sense) is the reciprocal of probability. R. Carnap and Y. Bar-Hillel have endorsed it. See their An Outline of a Theory of Semantic Information, MIT Press, Cambridge MA and "Semantic Information", British Journal for the Philosophy of Science, 4, 1953, 147-57. Nevertheless, Carnap continued to identify confirmation with probability.

[45] For more detail, see my "Heuristic Project", Journal of Epistemological and Social Studies on Science and Technology, 6, 1992, 15-18.

[46] For all this see my Science in Flux, op. cit.

[47] See my Radiation Theory and the Quantum Revolution, Birkhuser, Basel, 1993.

[48] For the question of refereeing see my essay on it in my Science and Society, op. cit. See also my "Peer Review: A Personal Report", Methodology and Science, 23, 1990, 171-180.

[49] See my "The Politics of Science", J. Applied Philosophy, 3, 1986, 35-48.

[50] See my "Faith in the Open Society: the End of Hermeneutics", Methodology and Science, 22, 1989, 183-200.

[51] See my review of Recent Advances in Natal Astrology, "Towards a Rational Theory of Superstition", Zetetic Scholar, 3/4, 1979, 107-20. See also my review of H. P. Duerr, Dreamtime, "The Place of Sparks in the World of Blah", Inquiry, 24, 1980, 445-69.

[52] There are two erroneous claims for incommensurability here: between competing scientific theories and between competing metaphysical theories or frameworks.

The first case is that of crucial experiments. The assertion that there are no crucial experiment is o bviously false. Moreover, the claim that there is no refutation rests on the incommensurability thesis and on the fact that overthrow occurs after a crucial experiment—because allegedly crucial experiments do not exist! The claim that crucial experiments refute incommensurability thesis was made in W. V. Quine, "In Praise of Observation Sentences", J. P., 90, 1993, 107-16.
As to the second case, of conflicting frameworks, it was discussed by Sylvain Bromberger, who stands out among leading philosophers of science in his critique of Hempel's theory of explanation and his related discussion of the logic of questions and its relevance to research, thus bridging between the philosophy and the history of science.
[53] For more details see my "The Structure of the Quantum Revolution", Philosophy of the Social Sciences, 13, 1983 , 367-81, reprinted in my Radiation Theory and the Quantum Revolution, Birkhuser, Basel, 1993.
[54] See my "The Riddle of Bacon", Studies in Early Modern Philosophy, 2, 1988, 103-136.
[55] The inability to control one's beliefs was noted by Robert Boyle in his Occasional Reflections. Yet the ability to suspend one's belief is certainly an asset, as it means the ability to think and act on the supposition that one's beliefs are false, the ability to be self-critical. In addition to the proposal to suspend one's beliefs there is the proposal to suspend one's disbelief, as discussed by Gerald Holton in his The Scientific Imagination, Cambridge University Press, London, 1978, pp. 71-2, and The Advancement of Science, op. cit., p. 9. This is dramatically exhibited in Michael Ventriss' decipherment of Linear B. See John Chadwick, The Decipherment of Linear B , Cambridge University