

## The Role of the Philosopher among the Scientists: Nuisance or Necessity?

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### 1. Where is the trouble?

Let us take it for granted that a person can be interested in researches that go on in different fields, for example, in physics and in psychology. Undoubtedly, this will raise problems not shared by a person whose research is confined to one field only. There may be difficulty in deciding which of the two is that person's primary field of interest; members of his secondary field of interest may be flattered or feel slighted or even threatened by his intrusion into their field. Let us fully ignore all these problems and pretend that they do not exist. Nevertheless, there may be different kinds of problems, and such that are hard to overlook. For example, a researcher may study the behavior of another researcher as a secondary field of research or even, worse, as a primary one. A professor of medical education who has studied the lecturing techniques of professors in his own medical school and found them very poor, meets with an understandable hostility in his own medical school. A (structuralist) sociologist may discover that his colleagues persistently speak of their teaching chores as serious undertakings and as sheer nuisance; he will be considered an enemy of education. A social psychologist may find a certain group of mathematicians or physicists in his own school or country over-ambitious and so rather unfriendly to competitors; they will then not like that social psychologist. A professor of education who thinks that biology professors shamelessly and immorally exploit their graduate students by forcing them to play cogs in their own research machinery, will find it difficult to get a job in a university with a strong biology department. And so it goes on. Let us agree that we can live with this difficulty too and even defend the researcher who meets this kind of hostility. Now the worst case is still different: i.e. when the outsider is also interested in the same research as the insider, and criticizes it and offers better ways of doing it. This is barely tolerable.

Let me mention two historical examples. Take professors of biology who use Aristotle or Galen as their standard text and who consider the peak of significant research any attempt to clarify an obscure passage in the given text, or to reconcile two seemingly conflicting passages in it. And suppose a philosopher who lives around the corner says he is likewise interested in biology but he does not care for either Aristotle or Galen, that he prescribes that we all learn not from old books but fresh from the Book of Nature. It is

too naive to expect the leading biologists of the day to feel friendly towards such a person, and no one will be surprised to hear that he was, in historical fact, barely tolerated by them.

The opposite historical example is one that may be idealized, more a piece of fiction than a real individual. I mention him because when I was a science student with an interest in philosophy I met his image in the classroom as the model philosopher. I do not know why my professors of physics spoke of him. They may have mentioned him only in passing, so that my citing them on him is a piece of enormous exaggeration, or they may have mentioned him because they knew that I was there and philosophically inclined, since I belonged to a small group of science students working for a master's degree. Anyway, they told me that philosophers are interested, among other things, in the nature of things, and that they study it in a manner quite different from the manner employed and commended by scientists, and that they, the philosophers, usually come up with results which are both insufficient and outdated. The paradigm, which was even to be found in some of my physics textbooks, was the very important philosopher Georg Wilhelm Friedrich Hegel, who proved in his doctoral dissertation that there are exactly seven planets, without taking recourse to the empirical methods of science. When Hegel was told of the discovery of a new planet which, obviously, upsets his conclusion, he said, too bad for the facts `Umso Schlimmer für die Tatsache'. Truth to tell, he said it jokingly and many times later expressed regrets at having cracked that joke. So perhaps I am unfair. But we have a joke from the other side, a joke made by Galileo Galilei, no less, against an opponent who refused to look down the telescope and see for himself the moons of Jupiter. That fellow died one day, and Galileo expressed the hope that on his way to heaven that fellow could see the moons of Jupiter without the use of a telescope.

Today philosophers are a bit more wary. Philosophers nor friendly to the cause of science are careful not to pronounce any verdict on questions that may be handled by science. They will ignore the question, how many planets the solar system has, and what are black holes, and even the question, was the universe created? This, briefly, is sheer cowardice, because creation belongs by any honest criterion to philosophy and not to science. Hence, if anyone is here transgressing it is the scientist; he is welcome to it, I think, but he cannot oust the philosopher: we can share problems and activities, and we can have a division of labor, but ousting is a bit excessive. Nevertheless, let us now observe that most philosophers are deferential enough and avoid competing with scientists. Let us also observe that unlike social psychologists and sociologists, they

never call scientists over-ambitious and unlike educationists, they do not notice that at times they devour their research students. Can there, then, be peace between scientists and philosophers? The answer is no - not yet, at least.

Among the details of the clash between science and philosophy that I encountered in my sensitive days as a science student concerned with philosophy, the most unpleasant was a remark I read, originally by the great Ernest Rutherford. If only philosophers would get off our backs, he said, we would do all right. This, of course, echoes the two examples I mentioned already, of one philosopher telling a scientist to study nature not in a speculative manner but in the empirical manner, and of another philosopher telling a scientist not to study nature empirically, but in a speculative manner. I am deeply opposed, not to say hostile, to both the speculative and the empirical methods. I gave an example from each, by the way, in order not to appear in favor of one of the two over the other, though, truth to tell, I would prefer the speculative to the empirical one, were I forced to choose between them. This, really, is not the point. Rutherford's hostility seems to have been directed not against any philosopher in particular, not even to philosophy in particular. Rather, it is the anger of a worker against the idle spectator who not only looks at the performance of the job at hand with amused detachment, but also has the gall to offer critical comments and unwanted advice. Let me call the idle spectator with the unwanted critical comments and advice the backseat driver.

## 2. The trouble with backseat drivers

Rutherford's rank hostility to the backseat driver aroused my curiosity when my hurt at his hostility subsided. Who would have dared play backseat driver to Rutherford I wondered. I could not imagine that any of his contemporaries in Cambridge who qualified as students of scientific method would do that. Bertrand Russell and his younger contemporaries, Wittgenstein, Ramsay, Keynes and Braithwaite were there. They were all in awe of what he was doing. If we remember C. P. Snow's *The Masters* where he describes a scientist of the following generation, we can see that no one in the college was in a position to offer any comment on a research physicist's work. Rumor has it that the scientist in that novel, Luke by name, is a portrait of a leading disciple of Rutherford, Blackett by name. If so, then it might be curious to note that Blackett, like Rutherford, pronounced some views on scientific method, and both said some of the stupidest things on record on the matter. Yet philosophers seldom complain when wonderful scientists say foolish things in philosophy; and they accept the censure of scientists who declare

that philosophers as philosophers say foolish things about Mother Nature and can say nothing worthwhile about her.

Yet, not all of Rutherford's disciples were philosophically crude. At least one, Lancelot Law Whyte, was a most charming individual, who moved with grace both among philosophers and among scientists. Though by training a scientist, he was by inclination and occupation a philosopher, even though not by profession despite his having achieved a modest but solid reputation as a philosopher - I think he had no profession and may count as one of the last of the free lancers. I had the fortune and the pleasure of his company, and I once gently tried to find out from him what Rutherford's attitude to philosophy was. He reported that Rutherford was more hostile to philosophy than my own teachers, and, I suppose, hinted that Rutherford considered him, Whyte, too philosophically minded to deserve a position in a physics department anywhere. I asked for more personal details, and Whyte drew my attention to the fact that as a youth Rutherford was interested in philosophy, and arrived at physics via his own philosophical interests.

This, then, sounds convincing: quite possibly, the backseat driver whom Rutherford wanted off his own back was none other than Rutherford himself. We do not need Freud to hold such a hypothesis; Dostoevsky will do amply well. The evidence for this hypothesis need not be from Rutherford's own inner life. It is a matter of public knowledge. Let us assume that scientific research is best conducted by opportunists. To that effect, we have the testimony of such diverse people as Albert Einstein and James Watson - Watson of the double helix, of course. Assume that opportunism is unsavory. We may remember here that the president of Harvard University where Watson is engaged prevented the publication by Harvard University Press of Watson's memoir about the discovery of the double helix. Another opportunist, Thor Heyerdahl, wrote a lovely confession of his sense of guilt about his practicing opportunism despite his adherence to the strictest of empirical views. He even had a lovely aperçu about it: opportunists take risks, and they may, just may, get along well, if despite their opportunism their final products accord with the strict empiricist code. Let me elaborate, it is like the opportunist in the business world who is willing to cut corners but finally ends up on the right side of the law. Incidentally, the passage I have in mind is the final and lovely chapter of Heyerdahl's Aku Aku.

I have now done something that, no doubt, might anger some scientists. The view I offer here is that researchers regularly act as opportunists, are able and willing to violate the

empiricist code in order to have a worthwhile result, yet a result to be worthwhile must, finally, accord with the strictest empiricist code. This view is quite unflattering. The father of modern empiricism, the great philosopher Sir Francis Bacon, said a scientific researcher may commit two grand crimes in research. First, he may report as observations what he thinks he might have seen had he taken the care and trouble to observe. Now no one will deny that to say I saw this and that not having seen it is a lie, an immoral act both within science and out. The second crime, said Bacon, is to present a speculation legitimized by facts after the fact as if it grew naturally out of the facts that legitimize it. The difference between shooting to a given target and shooting and then drawing a target around the place where one has hit is clear to all. Bacon said, this can always be done: no matter how erroneous one's speculation is, one can find facts which would legitimize it to one's own satisfaction and to the satisfaction of one's adherents and followers. The strict code, said Bacon, is not to take any opportunity, and not to chase novelties but to let the facts speak for themselves.

Let me offer an empirical observation: many scientists - as well as many philosophers of science - resent anyone who reports to them the following empirical observation. Many scientists hold Bacon's view, yet when cornered permit the opportunistic mode of venturing a guess on condition that the end product is right. Incidentally, Bacon himself already did so, but this matters little: he also reported many observations that we know he never made. Yet, the permission to take a risk at times was also given by other leading anti-venturist strict empiricists in the history of science, such as Laplace, Sir John Herschel and Ernst Mach. The fact that most scientists follow Bacon and oppose opportunism yet recommend it in a pinch is not one which scientists like to be told about. I think Rutherford kept telling it to Rutherford, and so I am not surprised that Rutherford was very uncomfortable in the company of Rutherford - or of any other philosopher, for that matter. Philosophers do sit on scientists' backs, Rutherford reported as an observation of fact, and though I am no empiricist, I share with the empiricists the respect for the observation of facts, and I take quite seriously his observation of being bothered by philosophers.

I have, at last, arrived at my problem. I have already observed that Lancelot Law Whyte was a lovable gentle soul - indeed, in my memory he stands out as one of the gentlest people I ever met. I mentioned to you that he was too gentle to say that Rutherford maltreated him, namely that he managed to upset Rutherford, his own gentle disposition notwithstanding. This is a story containing a profound moral. It shows how difficult the question is that I have finally come up with.

Can philosophers move freely among scientists without annoying them? If so, how? My answer is yes. But for this two conditions have to be fulfilled. First, all the empiricist canons of science should be officially repudiated, except the respect for empirical observations of the facts of nature. Second, the world of science should endorse canons that enable the backseat drivers to have a proper and recognizable job that they can perform without thereby offering drivers any cause for taking offense. I will explain this in some detail, but say at once that if scientific research is admitted to be opportunistic and adventurous, then perhaps this in itself will make it useful to have backseat drivers now and then.

### 3. The manners o/ backseat drivers

Let us consider backseat drivers for a moment. I mean literally people who do things with words -who participate in a physical activity like driving by the use of words. The most conspicuous backseat drivers are the subjects of situational comedies. They are persons who are inactive and insignificant, who are pained by the fact that they are inactive and insignificant, and who wish to have a piece of the action in order to be noticed favorably. Due to general ineptitude, they are noticed unfavorably. We all feel that such backseat drivers, let us call them nuisances, are better ignored than noticed. Nuisances often find ways of behavior which make it hard to ignore them. In such circumstances, they need friends who can give them what is known as 'a good talking to'. That is to say, nuisances must be convinced that though they wish to be noticed favorably, they are noticed quite unfavorably, to the point that they are better off not being noticed at all; or, if they wish to do better, then they must find different ways to make positive contributions.

It is no doubt that scholars of all sorts, academic ones included, are blessed with all sorts of nuisances. I would say off hand, with no examination and no evidence, that most nuisances are easily dismissed. They are at times dismissed by scientists with the greatest rudeness and get away with it, as I can personally testify. For example, in a public symposium at which I was invited to comment critically on a lecture by a Nobel laureate, he, the lecturer, stopped me in the midst of my comments and the chair did not intervene. I do not know how typical this event is, but I can report that I, for one, did not find it surprising or unusual. Hence, I conclude that nuisances can usually easily be ignored and even silenced.

It is quite a different question whether it is wise to silence a nuisance. In the more sophisticated situation comedies, we find at times cases in which the nuisance saves the day. Consider an excellent driver with a real nuisance for a backseat driver, and consider

the case that once, and only once, the excellent driver was going to do a foolish thing like crossing a red light and was prevented by the backseat driver. If thus lives are saved, then perhaps the nuisance was worth it. Things are not that simple, however: the nuisance may cause serious trouble as well, and even be a considerable factor contributing to highway accidents and so on, especially since it is hard to ignore a backseat driver who sits in a small vehicle with the driver and who has the gift of forcing his company on others.

To simplify matters, we may want the occasional rare benefit of the nuisance while reducing considerably his nuisance value. To do this we should find out why is he such a nuisance, whom the nuisance particularly annoys, and how.

There are two traditional answers to this question, both true, one psychological one intellectual. The psychological answer is that the nuisance annoys most those who are unsure of themselves, who tend to be dangerously hesitant and to whose hesitance the backseat driver contributes when he offers advice contrary to their inclination, and who tend to be themselves in need of approval. Those who are advised to do what they are doing anyway or who are offered criticisms of their past actions which they already know, have two possible responses, one is of signaling agreement with the nuisance, thereby offering approval, and one of signaling annoyance, thereby taking the nuisance's signal as one of disapproval. Those who have a strong character or position will choose the first response, those who feel weak will choose the second. This is an empirical fact. It tallies with Eric Berne's famous transactional psychology in its view of the nuisance and his victim as strongly interacting. Berne is also right in proposing that they interact in an erroneous sense that they may improve their personalities through such interactions, so that they may be cured by being offered better ways to improve, with the resultant subsiding of the unpleasant interaction as a mere byproduct.

Yet, Berne's theory always refers to some psychological weaknesses, whereas at times we suffer from intellectual ones. If the nuisance pushes a philosophy which his victim half-fears is true and if the victim tries to cope by suppressing the suspicion while suspecting that suppression is the wrong technique, then the victim invites a nuisance to pester him. I have already hinted that this may very well be the relationship between a scientist and a philosopher. In such a case the cure may be analogous to the one Eric Berne proposes except that what needs clarification is less a personal matter and more an intellectual matter. I propose that the major cause of the nuisance value of philosophers to scientists is intellectual: they both have some idea, perhaps even a mere

feel, concerning the peculiarity of scientific research; both feel that the rules of scientific research are morally obligatory, both know that they do not really know what these rules are, and they rub each other up the wrong way out of the intellectual and moral confusion of the situation.

It is an acknowledged fact that one who does not know the rules of a game yet plays a high stakes is anxious, that one's anxiety grows by a lack of readiness to admit ignorance of the rules, and, most specifically, when one does not admit this even to one's own self. Let me show that this is the case, and then discuss the importance of the facts. That philosophers do not know their role vis-a-vis scientists is clear: it regularly troubled philosophers that scientists consider them nuisances and they often argue that they are not, that they are not even backseat drivers. What, then, are they?

The question should be empirically decidable, but it is not. There are many books on scientific method written by philosophers. Are these books instruction manuals? Are they proposals to reform science? Are they empirical, sociological observations? To take the case concretely: philosophers of science wish to justify science and thereby act as defenders of the accused. Is science the accused, then? Or are specific scientists accused? At times, yes. There is not the slightest doubt that as an empirical fact both Einstein and Planck were accused, though perhaps not in the same way child molesters and bank robbers are accused. They were first accused for having rocked the boat of classical physics. They wanted no advocates, least of all among the philosophers. They had the mainstream of physics join them and the accusation dropped. They rocked the boat again, or at least tried to. They were accused and stayed isolated and condemned to the last. Worse, still, they were so condemned on account of their determinism, which means that they chose the wrong philosophy. That is to say, mainstream physics chose the right philosophy. Did philosophers help them in this choice? No; they acted as backseat drivers and most of them approved of the choice made by scientists.

This made philosophers of science worse than nuisances, worse than any sort of backseat drivers. It made them sheer ballast, sheer 'yes-men', despised even by their bosses. There is no way out. Backseat drivers who do not pester drivers are nothing but yes-men; backseat drivers who pester drivers are nuisances; backseat drivers who are invited to make comments and whose comments are quite appreciated are very scarce indeed. Query: is there legitimate and natural room for this role? Will philosophers ever occupy such a role? If not, can we get rid of the philosophy of science once and for all and stop the miserable bickering?

#### 4. Who is the real driver?

It is time to observe that most books and articles about science are rationalistic. They refer to such abstract entities as logic, mathematics, scientific and unscientific ideas, and factual information; that literature also refers, from time to time, to the conduct of scientists, namely to observation and measurement, to the production, sifting, rectifying and modifying of ideas, and to deduction and calculations and, rarely, even to such acts as the survey of the literature, the pooling of information, and more; finally, that literature also refers to some thinking subject, the first-person singular so prominent in Descartes' philosophical writings, and to that subject's rational beliefs. Some writers in philosophy prefer to ignore the thinking subject and all matters of beliefs and to stay in the abstract realm of ideas, information, and the act of fitting them which includes calculations: they say science is not a matter of rational beliefs but of rational action. These are the essentialists and the instrumentalists, respectively.

Of the remaining books and articles about science, of the irrationalists, most of them speak of the scientist and of his leaders and his community, they describe his conduct and his obligations to his community. The discussion on these items may be somewhat abstract, yet it is remarkably concrete in comparison with the discussions conducted by the majority. Clearly, these philosophers come close enough to be doing sociology of science, which is a recognized specialty within the science of sociology. One might, therefore, expect them to clash with each other, either by expressing competing opinions or by expressing the same opinions and so competing for the same task. In fact, however, they seem to be on good terms, of either cooperating and favorably acknowledging each other's contribution, or at least tolerating each other. How come?

There is little doubt that philosophers and scientists share much in the way of studying the nature of things. Whatever science does, the claim that it seeks to offer causal explanations, which is a standard topic of philosophical disputes, already is an encroachment on science, just as determinism - the view that every event has a cause - is claimed to conflict with modern physical science. The assertions that humans possess souls, as well as its negation, which are philosophical par excellence, encroach on biology, especially on neurophysiology, and on psychology, especially on psychopathology. There is, indeed, no concern of philosophy that does not somehow encroach on some science or another. Even the theory of rational belief, which concerns so many philosophers of science, may clash with the sociology of science and of knowledge and with the psychology of belief and with decision theory and more. A group

of philosophers that includes John Austin and his followers, toyed with the idea of making philosophy scientific by studying words carefully and scientifically and by classifying in a proper manner all the words of natural language, before daring to come up with any generalization. This idea, at least, was ascribed to Austin explicitly in Geoffrey Warnock's famous obituary on Austin. It appears very straightforward: to win respectability philosophy must carefully follow the strict rules of scientific method. Yet, this program is quite hopeless, not because it espouses an antiquated theory of scientific method, but because studying language scientifically renders a student a scientific linguist and not a philosopher.

Perhaps this is the crux: perhaps, really, there is the proper scientific method which makes every student who uses it a scientist, and anyone who does not employ it is in the wrong, regardless of whether the field of study is philosophy or religion or anything else. Perhaps Austin and Warnock were in error, both about method and about the proper subject matter of philosophy; but the proper, method makes the study any subject-matter science proper and no other method will do.

What, then, is the method of the notorious backseat driver? What makes him, in particular, such a nuisance? Assume for a minute that philosophy can be scientifically investigated. The philosopher, then, will be a scientist proper, not a backseat driver at all. The fact that a philosopher studies scientifically the conduct of a physicist or a biologist while doing research, will then be as legitimate as the psychologist's and the sociologist's study of the very same people, and there will be no ground for annoyance.

Therefore, the only question remains is, 'what is the method of science, and how is it useful for philosophy?' This question is asked, to repeat, by the leading school of the philosophy of science which attempts to provide rules of rational belief or of rational conduct - the essentialists and the instrumentalists - and the minority school which studies the conditions of scientists within the scientific community. Why is the minority more popular among scientists? Why was Rutherford annoyed with the philosophers of science of his day, all of whom belonged to the first group?

We have here important questions, the answers to which may help settle the troublesome difficulty at hand. For, we have agreed that were philosophy scientific, its authority will not be contested by scientists and we want to know how philosophy can become scientific, and we have at least two, but in fact three, views of the matter: the first, the abstract school, of philosophers of science as rational belief (the essentialists)

and of philosophers of science as sets of practical tools (the instrumentalists); and the second, the sociological school. How can we go about matters here?

Science goes about matters, we were once told, by the use of empirical means: anyone who doubts Galileo's results is encouraged both to doubt them and to learn how to use the telescope. This way Copernicanism won and only philosophers and theologians and other enemies of science could afford the luxury of expostulating about the nature of heaven and earth without taking recourse to the telescope.

Suppose that this were the case. We have a serious debate in philosophy and we wish to settle it empirically. How? Shall we go and ask scientists what methods they employ? This is known in social anthropology as the method of using informants. Social anthropologists found it very misleading, and so not scientific enough. They found it preferable to live among the natives, to use the method of so-called participant observation. Can we learn about science by participant observation? Will a person who is both a scientist and a philosopher who writes about philosophy count as an informant, who is thus not very reliable, or as a participant observer, who is thus very reliable? Either way, this will not resolve the difficulty, since we have philosopher scientists in each philosophical camp.

##### 5. Between the descriptive and the prescriptive

We must examine the situation slightly more carefully. What do we engage in when we study the method of science? Do we describe how individuals behave who deserve the title of scientists? It is no doubt the case, as a matter of quite uncontested fact, that we ascribe the honorary title of scientists to individuals who never claimed it, such as Archimedes and Robert Boyle, who hardly thought of themselves as scientists. Not only is the very concept of scientist less than a century and a half old; we know that research is characterizable by its method, yet the modern views of method were not known in antiquity or in some alien cultures, yet we will not hesitate to label some of their members scientists. Furthermore, we agree without any question that even the best centers of science on occasion house individuals who wrongly claim the title of scientist for themselves.

Let us agree. Due to these empirical facts, then, we retreat and say, we study the method of science in the sense of our approval of them, not in the sense of what goes on in the real world. We can then be experts on science with no need to look around. For, what can we see looking around? Either what we approve of or what we do not approve

of. If everybody uses the method we disapprove of, we may still disapprove of it. Why, then, do we need look around?

Holding this position, we invite the practicing research scientists to disapprove of our methods. And though this need not lead to the hostility we saw between the driver and the nuisance that the backseat driver is, this very position surely does condemn the philosopher to the position of backseat driver.

Yet this is not peculiar to philosophy; it is common to any study in which a distinction is made between the norm and the average. Most of these studies, however, do not distinguish between the two to the extent of considering them unrelated. Let us take grammar. Grammar may be ideal, the grammar of an ideal language. This, at least, is how Bertrand Russell considered his system of logic. He also thought that the natural languages are imperfect reflections of the ideal language, but this is not enough to help us investigate empirically the structure of ordinary languages. Noam Chomsky disagrees with Russell about the ideal: he thinks ideally every sentence has but one subject and Russell thinks some sentences ideally must have more than one subject (like the sentence of the form X and Y are brothers (of each other)): we have no empirical means to decide between them. Grammar may be empirical: we can say, school teachers say one should not use the word `aint' instead of `is not' or `are not' and this word is regularly used by this or that population of by such-and-such a percentage of this and that population or so-and-so often by such-and-such a percentage of this-and-that population. Neither utterly ideal nor utterly empirical grammar is grammar proper, as Dr Johnson and Dr Priestley had already noted in the eighteenth century. How, otherwise, can grammar be studied? Dr Johnson said, by recording the better usage, thus both observing and exercising judgment. Of course, this opens a wide gate for bias, but this is unavoidable, and if two grammarians disagree and yet can employ some empirical study that may adjudicate between them, then perhaps their work, though biased, may still count as scientific.

Grammar, to repeat, stands here for any kind of study that mixes the empirical and the evaluative. Public morality is no less amenable to scientific investigation, then, than grammar. Scientific research may also fall into the same category. The question is, how can one examine empirically theories that are half descriptive and half prescriptive? It is clear that this is a tricky situation: once we stick to the prescriptive part of our view, no description can force us to change our mind, and vice versa. Yet, clearly, in such cases neither the descriptive nor the prescriptive part of our theory really matters, but rather our

insistence that we were right from the very start. I hope I am allowed to stress this. People fear that the mixture of the descriptive and the prescriptive will allow a party criticized for it to shift to the descriptive when their prescription is criticized and vice versa. However, this is erroneous since anyone foolish enough to wish to dodge criticism can do so, as William Whewell argued in the nineteenth century and which has recently been labeled the Duhem-Quine argument. It is clear that one who welcomes criticism may offer a mixture of descriptions and prescriptions as challenging to some critics and so ass all to the good, and the only one that is not in need of challenge and does not invite criticism is one who knows that one is always in the right. Indeed, one who is convinced of being right from the very start is better off not undertaking any examination of one's views to begin with.

The last sentence is of the kind that is sure to annoy many people, scientists no less than politicians. The reason for this is that the sentence itself is true ideally, not in real life. Ideally, we experiment when in doubt, and when we are sure we are right, then experimentation is a sheer waste of effort. Yet in real life I know I am right and I have invited you to peer through my telescope so that you see how erroneous you are. And when an experiment goes wrong, as anything can, it may be easy for you to gloat, but still know I am right. Moreover, here comes my backseat driver, that nuisance of a philosopher, and shows the temerity of declaring his willingness to assess the fit between my theory and the facts so as to measure the credibility of my theory.

Why do scientists feel obliged to convince others? Because they depend on the goodwill of the society in which they live, since they live on high salaries while performing expensive experiments and observation. This is why they cannot easily shake off their backseat drivers even if they themselves have no hang-ups, psychological or intellectual or any other. Now, in order to cope with their dependence on society at large, scientists will do well to investigate this matter scientifically. Instead, they prefer the ideas of the philosophers of the minority group- i.e. those who offer ideas helpful for them in practice, and, as philosophical ideas, not in need of empirical examination.

In other words, scientists can cheat too, and express annoyance at philosophers when the philosophers threaten their prestige and high social position and high expenses. And they can honestly say, while not being quite honest about the situation, that anti-scientific philosophers oppose all scientific budgets, that pro-science philosophers should generally support the cause of science. They present the case correctly this way, but vaguely enough to omit from the picture the fact that some experiments are better than

others and that not all highly prestigious highly paid scientists are really good scientists. This is not to blame scientists. We all cheat a bit here and there, and if a scientist is not very open and scientific about the public relations of science because science is conducted in the real world, etc., then we can easily understand and sympathize. Yet, we may conduct public relations more efficiently after having studied them scientifically. This allows the sociologists of science and the irrationalist philosophers of science like Thomas Kuhn incentives to tell scientists that their public relations system is perfect so that we cannot improve upon it. Alas we need very little scientific research to find out that it does not keep to exactly the same high standard as science does.

Try to tell a scientist that. He will, I report, be angry at you. Try to calm him down by the claim that he cannot possibly declare science one-hundred percent efficient. He will say, it is very efficient, he will say, scientists by themselves do weed out the wrong persons and need no help, and he will say that only scientists can spot the wrong person, that others cannot read research papers to decide whether they are genuine. This is an empirical fact, and it proves that scientists can be on the defensive, and that they have then good reason for not wanting backseat drivers. The question, then, still is, if philosophers cannot comment on science - because of ignorance or of anything else -and if philosophers cannot comment on any topic sub-judice under scientific investigation, and if scarcely any topic is not given to scientific investigation, what then is a philosopher to do? A philosopher called Norman Campbell, a contemporary and colleague of the physicist Ernest Rutherford, incidentally, wrote a book called What is Science? early in the century, in which he declared any attempt to describe anything at all as being amenable to scientific investigation. 'What then remains for philosophy?' he must have asked himself. For, he answered the question, and in rather a cavalier way (the book is very slim): philosophy is prescriptive and science is descriptive, and the twain shall never meet. However, we have seen that most of the significant studies of human affairs from grammar to scientific practice (the grammar of science, Karl Pearson called it), are intrinsically both descriptive and prescriptive. Philosophy is simply pushed off the edge. All that remains for philosophers to do is to investigate scientifically certain problems that traditionally belong to philosophy.

Will this not anger scientists? Perhaps. I began by saying that scientists may not like an education researcher who investigate scientifically their lecturing techniques and finds them poor. We understand their resentment, we do not resent it, and make rules to protect the scientific research into teaching methods. Scientific research may show that research funds are poorly allocated, and this may annoy scientists too, yet we do not see

here a serious problem. The study of the efficiency of lectures or of the allocation of research funds is not exactly a philosophical study; so what is?

## 6. The Philosopher as a co-drivers

Since I have said so many unpleasant things about scientists, I should confess that my motive is sheer envy. It is easy to find fault with honest crowds with high aspirations, especially since in their honesty they always show awareness of their shortcomings by being annoyed at them. It is much harder to find fault at a tight-Tipped serene being who only looks at you with faint amusement knowing that you cannot see through him because he is opaque. Scientists try to explain their deepest ideas, their most difficult findings, their most mathematical theories. Philosophers may smile when you complain that they are incomprehensible, that you do not know what they are at, that they puzzle you by their pronouncement. And it is easy to ascribe all this to the irrationalist philosophers. The trouble is that it holds for rationalist philosophers as well. It is so hard to provoke the most rationalistic philosopher of science to say why he is concerned with rational belief, even when he is asked very politely to do so. I almost never found it possible to elicit from philosophers statements about their agenda, much less explanations of them. This is very regrettable, because their agenda is all wrong and wrong-headed, and dishonorable.

That the agenda may be erroneous is sufficient reason for recommending that it be discussed. In a democratic system the body in charge of the agenda - usually known as the steering committee - is a very important body: running the committee undemocratically destroys the democratic system as a whole; if a ruler can prevent important items from appearing on the agenda, then the democratic process will be wasted on minor details; This is no democracy. In a definite sense, then, the commonwealth of learning is a perfect democracy: every member of it can set their own agenda, there is utter freedom of research, and utter freedom of information. The only experiment that a scientist is not allowed to perform are those forbidden by the law of the land. It seems, therefore, that science needs no agenda, and, by the same token, neither does philosophy.

Yet, what was said above is ideal. In fact, scientists follow a set agenda. One major reason for their going to conferences and for their talking shop-talk is that they want to know of every change in the public agenda of their profession as soon as they can. Why? Because they want to be appreciated, because they want to publish their results, because they need research movies, and lots of them.

What exactly is the agenda of a given science at a given time? Who sets it? I do not know. I once studied the works of Michael Faraday, the father of electro magnetic -field theory; I wrote a book about him. I found it amazing that he knew all that. He set an agenda for physics: he explained it; he advocated it; he invited others to utilize his ideas. They refused. His proposals were ignored. In his lifetime and later the agenda was presented by leading continental physicists: Ampere, Weber, Gauss, Helmholtz, Duhem, Ritz. Their ideas are now almost entirely forgotten, and those of Faraday and his followers, Kelvin, Maxwell, Hertz and Einstein, are now presented as if they were always on the agenda. Only historians of science at times speak of the slow spread of these ideas, but even they tend to ignore the fact that the official agenda blocked the Faraday one.

Who fixes the agenda? The sociological philosophers of science, the irrationalists Michael Polanyi and Thomas S. Kuhn say, the scientific leadership. Who belong to the scientific leadership? Those who fix the agenda. How can we identify them? By asking scientists, what is the agenda and who has originated it, or by searching the originators in the leading centers of learning which are famous as leading centers so that anyone can tell you where they are.

Is this not a matter of fact? It certainly is. Should it, therefore, not be investigated scientifically? Certainly, it should. Are there not a group of scientific specialists whose job it is to explore this? Yes, there is: the sociologists of science, who are a recognized sector of the national and international sociological associations, and who have a few international societies and journals all to themselves. Have they investigated this? Not at all. Why? It is not on their agenda. Why?

Let us look at the philosophers. The situation in their quarters is abysmal. The majority of philosophers have had a fixed agenda for centuries. Some alterations of the agenda were made early in the twentieth century and up to World War 11; they proved marginal, and more a matter of method than of issues. The agenda of philosophy is more or less fixed, especially in the philosophy of science.

The chief agenda of most of the philosophy of science is the justification of science as rational belief or as rational practice, we remember. The new item, of the sociological school, is the justification of science as an autonomous subculture with powerful leaderships that determine the scientific agenda.

Does science need justification? Who is prosecuting science and in which court, that science should need justification?

As long as philosophers attack or defend science, they can hardly expect to do anything useful. If, instead, they look at science as a part of our culture, and study its rules of conduct, its agenda, its contributions in diverse directions, then the critical assessment of the performance of the various sciences in the various aspects of public life by itself may be a great incentive for cooperation between the sciences and the philosophy of science. The philosopher may then scientifically examine some of the most common items of information about science, such as the idea that science has no leadership, or that the leadership of science decides the agenda of science, or that science has no bias, or that a new and important scientific idea which emerges from a small place takes a decade or two to arrive on the agenda of the profession at large.

There are few matters on which science has to adjust to the new social setting. Science has won the battle against the anti-scientific philosophers and theologians, yet many scientists are still defensive (like Rutherford and the Nobel laureate mentioned above who felt he had to stop my critical comments on his lecture). Science often settles disputes by dividing territories and calling competing theories by names which makes them appear to be non-competitors. No doubt, these things belong to the rules of the game - to the grammar of science - yet the rules are not always the best there are. Philosophers can do much to improve things - at least by making proposals for improvements that may be checked. The very checking of these proposals will constitute improvements, regardless of the outcomes of the checking. Once this is accepted practice, some philosophers and some scientists will be able to view themselves as co-drivers.