Philosophica 15, 1975 (1), pp. 5-20.

THE PRESENT STATE OF THE PHILOSOPHY OF SCIENCE*

Joseph Agassi

The reason I have chosen to present this survey is that I always find it difficult to present my own views on any specific topic, and I wish to explain this difficulty somewhat. My views are those of an apostate and one from the school of Sir Karl Popper, who was for long little known and less understood yet whose popularity is now rising somewhat. I find it hard to assume my audience to be sufficiently familiar with Popper's views to enable me to proceed to criticize him without much explanation and offer modifications to his ideas as I would wish to do. I shall soon present some gross misconceptions of Popper's views which, I feel, I have to clear beforehand. But there is a more serious obstacle than mere misconceptions. Those who are familiar with Popper's views are often defenders of the majority views; this is to be expected of course, yet it causes for me no small difficulty. I am always wary of being taken as a critic of Popper from the majority's viewpoint (from the right, as it were) whereas, in fact, if I at all enter the debate between Popper and the majority I am so much on Popper's side that my disagreement with him becomes negligible. This is, of course, no enviable position either. I confess I have myself found it amusing that, say, members of different religious camps unite against the threat of agnosticism; yet, this amusement is only just when aimed at some petty politics of a disagreement; otherwise it is perfectly reasonable that Catholic and Protestant should view differences between their views as secondary compared with what they share in opposing the agnostic. This is merely a matter of a sense of proportion; and so it would hold equally for the manner two agnostics may view any differences between themselves as compared with the differences both have with any true believer; quite naturally they will oppose him first before they start opposing each other.

I do not know how much all this will be taken for granted, how much it will be opposed. History offers us contrary examples, from religious controversies, and from political campaigns, and even from scientific and philosophical schools. There is the idea, shared by many left-wing leaders, and by Freud, which sounds perfectly practical and commonsense and reasonable. It is this. We cannot change the whole world at once. And so, at the very least we must, first and foremost, be exactly clear about what we want. Not because we are going to do things right now on a grand scale and so we must do them absolutely right, but (on the contrary) because we know our limited powers and so we must at the very least try our best to control what we can, namely our own selves, our immediate environment. And so we should keep the doctrine pure. It follows from this that the person whose opinions are closest to yours is your most dangerous opponent. In particular, the dissenter in your own midst is the bitterest enemy in the eye of the leadership of your own camp; and, of course, to the dissenter the leadership looks equally treacherous and more. I shall soon offer an example of this from the history of physics.

Those who vigilantly guard the doctrine against all impurity come to a very dead end : they cannot but repeat themselves; they incessantly teach the pure milk of the true doctrine--with or without small variations, usually not in the positive teaching but in the venomous criticism on their greatest and nearest opponents, the dissenter or the establishment as the case may be. At times they apply the old doctrine to a new case, and thereby achieve minor innovation. But on the whole they are crushing bores.

Most of us need food for thought; we want new ideas now and then. But some or most of those open to new ideas are not as open as they can be : let us consider, in particular, a doctrine so revolutionary and so important that one cannot even notice it without thereby committing a major act of heresy. I have in mind such diverse innovations as Faraday's field theory or Popper's theory of science. The historical case of Faraday is tragicomic : even recognizing Faraday's ideas as interesting was a heresy, since these ideas contradicted the well-established canons of Newtonian science. A man like Faraday could do nothing, then, except lecture and write almost exclusively about his own ideas. Let us consider the young people who went to his lectures, who were as profundly impressed by him as one might expect, who had practically no physics in high school to counter-balance his ideas. These young people, quite naturally, had the most lop-sided view of physics. They grew up ignorant of the state of the science in other countries and in their own country a generation earlier. Faraday, almost single-handed, made such a profound change, that the great revolution in nineteenth century physics--the introduction of electromagnetic field theories--went unnoticed. This is well illustrated, I think, by the following two cases. My first case is James Clerk Maxwell, the inventor of the famous electromagnetic field equations which bear his name. He came to Cambridge, England, as a young man well versed with continental studies on electricity but utterly unaware of even the existence of ideas by Faraday. His somewhat elder contemporary, William Thomson, later known as Lord Kelvin, told him to read Faraday. Thomson himself had heard about Faraday from a cousin of Faraday who was a substitute professor of natural philosophy in Edinburgh. My second case, and barely a generation later, we find Silvanus P. Thompson, the founder of the Institute of Electrical Engineering in London. He gasped at the fact that he had rediscovered some ideas of Faraday which he lectured on in the Institute and no one recognized them as Faraday's.

I cannot blame Faraday for his having taught his own ideas to children so efficiently, because he was so ostracized by his peers. But I do think it is a bad situation when people lose signt of even their immediate ancestry, when they can no longer survey their own specialty and notice the trend they swim in. Just as the unexamined life is not worth living, so the unexamined current is not worth drifting in.

Sir Karl Popper used to teach his own doctrine with as much force and fervour and, I suppose, due to as much isolation. Possibly his ideas bear fruit and possibly his ideas are becoming public property without proper introduction--as Faraday's ideas were before. And I confess I find quite unpleasant this possibility, however viable or remote it may be.

I

There is one idea which I have learned from my schooling by Popper, or perhaps it is an observation he made which has impressed me so. The school which tends to keep vigilantly the purity of the doctrine, Popper observes, keeps changing its doctrines all the same. But it does so either by producing heretics and apostates, or by surreptitious changes, namely in an underhand manner--either by changes too small to be noticed, or by reinterpretation of its terminology, or by some other subtle means. Now surreptitious changes are merely lies of a sophisticated kind, and heretics are in fact those who break from the tradition. Various schools of thought undergo these days surreptitious changes, and I think it is better if we are on our guard, because a change effected underhand is not the best planned change and it is surely one which did not enjoy the full benefit of critical examination.

Let me offer a couple of concrete historical examples. But in all fairness I owe it to my examples to warn my reader that any presentation is biased and oversimplified. It cannot but be so, since my thesis is that the examples are unnecessarily complicated and subtle--unnecessary, that is, except for the purpose of surreptitious change. Of course, I present the examples since I think they are true, but I know that people whose ideas are allegedly represented in the next few paragraphs will claim that I distort their views — and honestly so, no doubt : I do not wish to call them liars but merely muddle-heads.

My first and brief example is the influence of Freud. I do not know whether orthodox Freudians exist and how orthodox, to what degree orthodox a Freudian, say Erik Erikson considers himself to be. There is, usually, little occasion for a mature Freudian, in practice or even in research, to compare his own views with those expressed in the standard works of the Master and see how well the two agree with each other. It requires a fresh reading of a complete work or two of Freud, and there is usually not much occasion for such an excercise. The excercise naturally presented itself in recent years with the publication of a new work of the Master — the book on Woodrow Wilson by Freud and Bullit. It was a rather embarassing experience to judge by various responses. But Professor Erikson came to the rescue : he showed the work to constitute of two separate compositions : a masterpiece by the late master and something else by the late diplomat. And, need one say, when the reading became less disturbing the question of the purity of the doctrine was laid to rest too.

My second example requires an elaborate introduction--of what is known as a whole philosophical school, the Vienna Circle. Along with its derivatives in England, the United States, and elsewhere it represents a singularly barren group of people who worked between the two World Wars, produced then almost nothing, yet nevertheless managed to impress the world of learning for quite a while. The background to their philosophy was all the credentials they had : logic, empirical science, clear thinking. They repeatedly said they sided with these. There are people who still admire that group because of their devotion to the idea that clarity equals rationality equals science. That was all : naive but clear. Traditonally, rationality was identified with science. Traditionally, science was identified with empirical examination -- though another tradition of science also existed. It was, therefore, quite natural to identify the rational with the scientific with the empirically verified. Clarity came into the picture with the quaint idea that verification is what gives a proposition its meaning. Hence, the unvoiced corollary goes, either a proposition is perfectly clear or not at all. The meaning of a proposition, that is, may be crystal clear if it is verifiable, and non-existent otherwise. So far so good; but the story goes further. The pinnacle of the Vienna doctrine was put in one slogan : the meaning of a proposition, said the pundits of the Vienna Circle, is its method of verification.

I have the habit of confessing at once my inability to comprehend what I say when I say something I do not comprehend. So allow me this interruption. I repeat the slogan of the pundits of the Vienna Circle : the meaning of a proposition is its method of verification. But I do not claim to comprehend it and indeed it is my considered opinion that this is a meaningless string of words, a pseudo-proposition so-called, or, in plainer English, stuff-and-nonsense. (This, indeed, was the verdict Fredrick Waismann, one of the leading members of that group throughout its period of existence, gave years after its demise.) Of course, no one quite knows if the slogan, 'the meaning of a proposition is its method of verification', is meaningful or meaningless because we do not as yet have a sufficiently elaborate theory of meaning. We do have a theory or theories of meaning of names and of descriptive phrases. We do have rudiments of a theory of the meaning of propositions--but not enough. The Vienna Circle were quick to judge any idea meaningless when not backed by a fully fledged theory, and so perhaps by their own precepts their slogan is meaningless. But these precepts are erroneous and hence, quite possibly, their slogan, the meaning of a proposition is his method of verification, is meaningful, I do not know.

Popper thinks the Vienna Circle's slogan is meaningful, that is to say, it is either true or false; he thinks it is false. He says, first, we cannot devise a method of testing a proposition before we fully comprehend it. Moreover, says Popper, whatever proposition we can test is scientific, but some promositions are not testable yet quite meaningful. Having meaning, he goes on, is not the same as being scientific. Furthermore, he argues, empirical tests only refute scientific generalisations, they never verify them.

Somehow, before the War his Viennese colleagues staunchly ignored his idea that science is not the totality of meaningful propositions. Rather they were impressed with his idea that science is not verifiable but refutable. The result of this was a compromise, the bastard idea that though being scientific is the same as having meaning, this is not the same as being empirically verifiable but the same as empirically refutable. This bastard idea fell between two stools; it was rejected by both the Vienna Circle and by Popper. It was erroneously attributed to Popper by various authors at one time or another: The locus classicus of this attribution is Carnap's well-known *Testability and Meaning* of 1936-7.

This bastard idea is easy to refute and it was satisfactorily refuted by various authors. The refutation goes like this. Whatever makes a proposition meaningful, one thing we are sure about the meaning of a proposition : the negation of a meaningful proposition has the opposite truth-value and hence has a truth-value and is hence meaningful. Likewise, the negation of a meaningless string of words (or pseudo-proposition) is itself meaningless. Now consider a universal proposition, say (A) 'all men are black'; consider a basic statement, namely a possible candidate for an observatonal report, such as, say (B) 'in Philadelphia in January 1970, there was a non-black man'; consider also the negation of the proposition (U) 'all mean are black' i.e. (E) 'there exists a non-black man' Clearly, (B) refutes (U) and (B) verifies (E). Clearly (E) is irrefutable since the universe is possibly infinite and so we cannot possibly scan it to disprove a purely existential statement. But (U) is refutable since it contralicts (B). So (U) is meaningful and (E) is meaningless by the bastard version of Popper's theory. Since (E) is the negation of (U) it is meaningful and since it is irrefutable it is meaningless--which is absurd. And we have logically refuted the view which I have called bastard and which the Vienna Circle have erroneously ascribed to Popper. When he said scientific character equals refutability they heard him say meaning equals refutability, and they refuted what they heard.

It is a strange fact, but I recommend that those interested check it to their own satisfaction. In the fifties, series of papers appeared in England about natural theology, in *Analysis*, in a volume of essays edited by Anthony Flew on linguistic analysis and natural theology. Both believers and unbelievers among the contributors shared the refuted bastard criterion of meaning and their work, naturally, is worse than nothing. The same criterion was also employed in examining such doctrines as Marxism and other verions of the doctrine of historical inevitability, and Freudianism and offshoots of it or variants of psychoanalysis. The bastard version is sometimes used more as a criterion for a theory being scientific rather than meaningful, that is to say, there are variants of the bastard version of varying degrees of bastardness, created as if especially to illustrate my view of the folly of surreptitious change in general and of the surreptitious and unhealthy increase of Popper's influence in particular : it is doubtless the least valuable part of Popper's theory which thus far has been gaining currency, and in a distorted form to boot !

If my extended example is not too much of a nuisance, I would like to continue it a little. Of recent there is a further surreptitious change to the story, and it is even a little improvement in a way —but not a big one in any way. It is this : what has happened is simply that certain philosophers have rejected Popper's characterization of science as is clashes with their intuition that both a statement and its negation must be together scientific or together unscientific. It is a bit sad that this idea is presented seriously as a reasonable one, without any further inquiry about the force of such intuitions and in the face of the fact that from the beginning of philosophy till the rise of The Vienna Circle it was taken for granted that the negation of a scientific (i.e. proven) proposition is unscientific. It is merely the idea that meaning equals scientific character which has changed these people's intuition : take away this idea and what is left is a dangling intuition.

Again we see, I hope, how important it may be to know what is the current we are drifting in if we wish to know ourselves. And so, I am coming back to my reasons for offering a survey here. So now let me begin my survey. Let me end this long preamble by reminding my reader that all surveys are biassed. The only way to remedy this is not to search for an unbiassed survey, since all humans are suspect of bias, but to create diverse biasses in the hope that biasses may cancel each other to some extent. Well, then; here we go.

II

The label philosophy of science was invented by members of the Vienna Circle between the two World Wars. It comes to replace two entirely different ones, epistemology and scientific philosophy. Epistemology, or its modern variants, theory of knowledge, or erkenntnisslehre, deals with epistēmē, which is the opposite of doxa, namely with scientia, or knowledge, as opposed to opinion. The word epistēmē is a technical term presumably invented by Parmenides to mean knowledge in the strict sense i.e. in the sense of fully demonstrable knowledge, i.e. knowledge that is here to stay, for all times, unalterable and unshakeable. (Parmenides spoke of logon piston, i.e. of theorem.) Little reflection will show that in the ordinary use of the word knowledge may be alterable; indeed even the words proof and demonstration, and their equivalents, do not necessarily mean in ordinary discourse, once and for all times. This is a point noticed by Gomperz and by hoards of commentators since. Ordinary language philosophers who sanctify ordinary usage, go even so far as to condemn Parmenides and his followers; for my part I see not fault in Parmenides' introduction of a new use, since he — or is it Plato ? — defined it impeccably. It is not the question, how we used the work "knowledge", which matters; it is the question, how alterable do we think our views are, which matters. To take an example Sir John Herschel, Dr. William Whewell, and other spokesmen for science and for scientific philosophy in the nineteenth century, they all declared sharply that Newtonian mechanics, being scientific proper or demonstrated, is not amenable to any shaking. Mechanics will never suffer the slightest modification, they said most clearly. To them, the idea that Newtonian mechanics may be an approximation to another theory was a shocking thought. One may say, for instance, that the force of gravity does not quite act at a distance, though it does indeed travel so fast that it may be so viewed when calculating mechanical predictions. This indeed happens to be the current twentieth century view; but to nineteenth century physicists this was so unthinkable, that when Faraday said it they simply plugged their ears. Herschel and Whewell thought of Faraday as of a dear friend and as a highly esteemed colleague; yet they refused to entertain his views even tentatively. Indeed, because he was so close his views were deemed ever more dangerous, too dangerous even to muse about.

All this holds not only for physics but for any theory claiming scientific status or demonstrability, even a metaphysical theory. Thus, when Solomon Maimon accepted Kant's philosophy as true but declared its status to be that of a hypothesis, he incurred Kant's displeasure. Kant had declared — and he always remained of the staunch view — that his philosophy was scientific and so it will forever remain unchanged. He later declared that Maimon was only an intellectual parasite, as Jews so often are. I must say, anyone who could get an antisemtic remark out of the paragon of Enlightenment that Kant was, must indeed have got under his skin. Perhaps Kant was not that sure after all. But he felt he had to be sure, since he wanted to be an author of a scientific philosophy.

The word scientific philosophy is synonym for rational philosophy, and was chosen for two reasons. First, to intimate the doctrine identifying rationality with scientific character, and second, because the word rational or rationalist had, traditionally, two distinct meanings. One meaning is that which is exhibited in the contrast between rationalism and irrationalism, namely rationalism as the view that man can and ought to use his reason or intellect to determine his beliefs, guide his actions, etc. The other meaning is that which is exhibited in the contrast within the rationalist school between rationalist and empiricist sub-schools, namely rationalism as the view that the grounds of reason are in the intellect itself rather than in the senses. Immanuel Kant suggested that we substitute the word intellectualism for rationalism in this narrow sense of a sub-school. But his idea did not take. And so, when we want to speak of rationalist philosophy in the broad sense encompassing both Descartes and Locke, to the exclusion of the narrow sense encompassing Descartes but not Locke, it may be preferable to use the word scientific philosophy.

Now the title "philosophy of science" comes to designate not only the faith in verification or in science; it also designates faith in the rationality of philosophy. Yet, strictly speaking, obviously an unscientific philosophy or an irrationalist philosophy may indeed include a view of science, which may be called a philosophy of science. It is most regrettable that we regularly forget the philosophy of science of people like Croce and Gentile, like Sartre and Heidegger. These thinkers do have views about science which they expound in their books which are on the reading lists in courses on phenomenology and such. Yet their views about science are hardly complimentary. And so, courses in the philosophy of science only refer to scientific philosophies of science. And so, very regrettably, they have their share in the increased gulf of non-communication and lack of understanding between the neo-Hegelian, phenomenologists, and existentialists on the one hand and the positivists, pragmatists, etc. on the other.

The philosophy of most of the writers on the one hand is almost uniformly an instrumentalist or a pragmatist philosophy. This, of course, should immediately puzzle you, as I put the pragmatists as on the other hand and spoke of the gulf between the writers on the one hand and the pragmatists. Here, again, we see the strange results of having no easily available survey of the field.

The pragmatist philosophy of the phenomenologists and the others on the one hand says, we need not study science since it is deprived of all truth, of all intellectual value: it is purely of pragmatic value. Its truths are merely pragmatic truths, or its truths are only true when judged by some transient standards of truth, such as what is useful here and now. But not by standards of enternal truth, since these are reserved for metaphysics alone. Metaphysics alone, then, is verifiable (by the authority of our intuition, incidentally) and hence only metaphysics is a true science or truly scientific.

What will a pragmatist say to all this, for example James and Pierce ? They cannot disagree with the part of the doctrine just expounded as far as science is concerned; but they can disagree on metaphysics, and in two ways. They can either declare the same pragmatic standard of truth to apply to metaphysics; or they can declare that there is no such thing as metaphysics. As far as I understand James and Pierce, James accepted the first alternative, Pierce the second. For James even religious or theological truths are useful; for Pierce they are meaningless or else a part of our survival mechanism and hence a part of our science. In both cases of pragmatism no eternal truths are allowed.

The philosophers on the one hand, whom I consider irrationalists, are contemptuous of science. Their reason is not so much that it is merely an instrument of survival--they do like survival. But they say that they can also offer eternal truths, that their metaphysical doctrines greatly outshine science. The pragmatists offer nothing of the kind, and thus avoid any irrational practices. They avoid all claim for finality--in science or elsewhere--and thus all dogmatism.

This raises again the question, what has happened to verification in science so firmly upheld in the nineteenth century ? The answer, in brief, is, it was killed by Einstein in 1919, when he was acknowledged publicly as the one who has successfully modified Newton's views. True, the positivists either had not heard of Einstein or had not understood him; on a rare point they made a valiant last ditch effort. The great locus classicus for the twentieth century verification principle is Ludwig Wittgenstein's *Tractatus Logico-Philosophicus* of 1922. The word "verification principle", I must hasten to explain, belongs to Waismann--the one who brought Wittgenstein to the Vienna Circle; furthermore, the formulation I quoted above belongs to Schlick--the man who created and headed the Circle until he was murdered. Still, I do think the verification principle is Wittgenstein's; he used Newtonian mechanics as an example of a verified theory. And this in 1922 ! His disciples of the Vienna Circle went on talking of verification well into the thirties. Ignoring a few other writers who failed to be influenced by Einstein, let me mention P.W. Bridgman, the Nobel laureate physicist, who in 1927, made quite a pathetic declaration. The very fact that the Einstein revolution could ever occur, he said, shows that not all had been in order in the house of science. He called for more vigilance in keeping the purity of the language of science by the device of using in science only those terms which can be operationally defined. This, said Bridgman, will assure that from now on all will be in order in science. One must define length as a certain operation of measurement and time as a different operation of measurement (of behavior of clocks), and heat as a still different operation, etc. As is well-known, the concept of simultaneity was allegedly defined by Einstein in 1905, but not by any of his predecessors. Now his definition is not entirely operational as it refers to inertial systems; moreover, even the narrow constraint on Einstein's terminology as he accepted in 1905 impeded his development of his general theory of relativity so that by 1916 he gave it up. Later on Bridgman too gave up operationism--unfortunately by a surreptitious change--by adding to the operations of measurements those of pencil and paper, i.e. of thinking. Surely this will not guarantee that no revolutions may occur in science in future.

The classical theory of empirical verification crumbled — all evidence from Vienna to the contrary not withstanding. Two schools of thought are now extent. The one sticks to the empirical and gives up verification in theoretical science and the other sticks to verification and gives up the empirical nature of theoretical science. These are inductivism and conventionalism respectively. It so happens that in those fields of science which lean more towards theory than experience — e.g. theoretical physics — people are inclined towards conventionalism and those which lean more towards experiment — e.g. zoology and botany — show preference for inductivism; molecular biologists, for example, vacillate.

I find both schools extremely narrow and I only marvel at the fact that when the verification principle crumbled, its chief victim, rational metaphysics which merely claims the status of hypothesis, was not revived. So my battle cry is, back to Solomon Maimon. But I must not be carried away; I should proceed with a survey of what is, not of what I wish it to be.

III

For those who have the preconceived notion that surveys start when the field is divided to schools and subschools which are then properly characterized--for them, then, the survey begins right now. The field of the philosophy of science is divided to the dwindling minority who study empirical meaning and whom I shall ignore from now on and the majority who study the nature or empirical science. Of these, a majority studies the nature of empirical confirmation and belongs to the inductivist school; the rest are conventionalists who try to formalize systems or to discuss the division of scientific theory to empirical content and mathematical frameworks.

If I were to recommend a reading list for those who wish to become students of the field, let me say out right, I would recommend classical works -- inductivist, Francis Bacon, William Whewell -- or conventionalist, Poincaré, Duhem -- I will not trouble you with contemporary works in either field. If I were pressed for more modern stuff I would recommend Meyerson, Polanyi, and Popper. Let me elaborate on this paragraph and draw this paper to its conclusion.

The main fact to observe about both contemporary conventionalists and contemporary inductivists--particularly the inductivists--is that they are repeatedly prone to fall into the pitfall of old verificationism. They sometimes express an explicit verificationist doctrine, sometimes they only imply it. The reason is complex. First, consistency is generally difficult to maintain. Second, old pitfalls are generally hard to avoid; for example former Pagans are unable to avoid importing some of their Paganism to Christianity after they adopt it. The third and more significant reason is that contemporary views -- both conventionalist and inductivist -- are surrogate verificationism : They come to answer the same questions which the old verificationism came to answer, and when they fail to answer these questions their advocates tend to return for a while to the good old theory.

Conventionalism employs two standards of truth, absolute and pragmatist. Conventionalists recognize two facts which look identical but are not. First, that certainty of theory cannot rest on empirical fact; and second, that science never has the last word. They ascribe to scientific theory the status of certainty--and thus of immutability--but only as part and parcel of mathematics. Physics is then viewed only as a branch of applied mathematics; physics is then a system of pigeon holes to classify facts more or less neatly. Now the neatness with which theory stores facts is not a matter of mathematics and is not unalterable, since the stock of empirically know facts is alterable with the growth of science. So the neatness is judged by the alterable standard of pragmatic truth.

All this is highly sophisticated but quite unnecessarily so. Moreover, it is unsatisfactory as it takes no account of the fact that a scientific theory is not only a pigeon-hole system--it should also be confirmed by the facts. This criticism, incidentally, is not one which I endorse but which inductivist philosophers of science launch against conventionalism. More precisely, inductivists prefer, when they can, to avoid discussing conventionalism--partly because so many theoretical scientists are conventionalists.

The inductivists operate with one concept of truth — the absolute. And they characterize the latest in science not as demonstrably true but as probably true. (The strange exception is Hans Reichenbach who suggested once that probability may be viewed as a truth-value in an infinitely-many-valued logic; it did not work.) The replacement of verification by probability is neither new nor very interesting. The idea that the latest ideas accepted in science are probable, however, is a step in the right direction, to be sure; since the latest in science is possibly true.

There are two different aspects to the theory of confirmation--qualitative and quantitative--and both can be treated loosely or precisely. Qualitatively, we relate prediction to explanation, for example. Indeed, the theory of explanation has the lion's share of the literature in the field--almost always with relation to confirmation. And quantitatively, confirmation may be a probability measure, i.e. follow the axioms of the theory of games of chance, or it may not. All this is subject to much discussion. Also, the theory of chance tells us what is the probability of one event given another, but if the other is not quite given we do not know what to do about it. Thus, if hypotheses are probable, given some evidence, the witness must be reliable and honest for sure. No witness is. And so the whole discussion is vague--the more precise it seems the more it is a waste of time to examine it carefully. The most important works in the field of confirmation are qualitatively of Hempel and quantitatively of Carnap--both members of the Vienna Circle before its demise. Hempel showed how paradoxical the qualitative theory is and Carnap did the same with the quantitative theory, yet whereas they had relinquished verificationism, they stuck to their newer views, to non-verificationist inductivism. Carnap promised a second volume to his magnum opus on probability but gave it up in the preface to his second edition. Nevertheless he went on working to his dying day with hopes to solve the problem of induction by a strong and mathematical theory of confirmation of one sort or another, where confirmation is no longer assumed to follow the rules of the theory of games of chance yet relate to them in some way or another.

The various conventionalists and inductivists labour to one end--they wish to justify science, to answer the skeptic. The skeptic is not necessarily one who denies that, say, Einstein is correct : the skeptic is willing to endorse Einstein's theory but he insists that the theory has the status of a hypothesis. Just as Solomon Maimon did with respect to Newton and to Kant. But skepticism is still the target of the majority schools. Michael Polanyi shares with these schools their concern, but views their efforts futile. He thinks the status of science is definitely higher than that of a hypothesis : once one has endorsed science one has thereby endorsed the present body of scientific opinion. And though scientific opinion is alterable, its acceptance protem is imperative. On what does Polanyi base his claim that scientific opinion is obligatory ? And who does he think decides what is obligatory ? This is very interesting, and I shall have to pass it by very quickly. The elders of science, says Polanyi, decide what is current scientific opinion. They feel it in their bones or in their fingertips. He illustrates all this, but he cannot defend it, since his view is that you cannot defend science by any given rationale.

And so, to use our terminology, Polanyi has a philosophy of science--indeed he writes chiefly about science--but it is not a scientific philosophy; it rests on an intuition of the leadership and on his declaration that the leadership has authority. Thomas S. Kuhn has much enlarged on this idea of Polanyi, coupled with a Duhemian theory of pigeon-holing, etc. I must leave all this now.

The classical views of science, verificationism or any other, upheld the authority of science, but under the heading of rationality. Reason, they said, lends authority to science. When the great 20th century developments in science took place, the old views were gone. Almost all those active in the field still have the same aspirations; they still hope to reestablish the authority of science, to justify science afresh. Polanyi upholds the authority of science, without believing that reason can justify it.

Popper claims to be the exact opposite; he claims to stick to the idea that science is rational; but he has no desire to uphold any authority of science, to justify science in any manner whatsoever. And so, to return to my opening paragraph, his agnosticism and mine make us practically close allies when it comes to debates with any believer in the authority of science. But otherwise I have little to agree with him. Let me conclude by telling you what I consider his great point which is so important that it makes any philosopher who overlooks it quite antedeluvian. I do not mean that one cannot disagree with Popper on this point, but that one cannot ignore him, that he has altered our way of thinking about, or looking at, one question quite beyond reversal. The question is, how do we learn from experience ? It is the one traditionally known as the problem of induction.

The greatness of Popper's view does not depend on whether one endorses it or not. He changed the problem of induction irreversibly. When classical thinkers from Hume onward asked, how do we learn from one hypothesis rather than from another? Popper said, learning from experience is learning to reject a given hypothesis. This puts into question the implicit opinion that learning is choice. If learning is not choice, it may be the increase of the field of choice, or the decrease of it. The decrease may be done by empirical refutation; perhaps in a sense the increase too.

Whether true or false, this idea is so intriguing that one cannot overlook it except at the risk of being left behind. When the majority takes such a risk, it only means that the majority may be left behind. We have historical examples, such as the electricians who were Faraday's contemporaries yet chose to ignore his ideas. I submit that there in too much parallel between nineteenth century electromagnetism and twentieth century philosophy of science to leave one complascent.

This is not to express agreement with Popper. Having opened on a note of disagreement, I feel I should say something about it now, especially since my disagreement stems from the ideas of Emile Meyerson, the modern philosopher of science on my "must" list. Meyerson considered science as guided by metaphysical ideas of a kind akin to Kant's regulative ideas. At times he almost suggested that science is a handmaid to metaphysics, an elaboration on it. Meyerson's ideas were taken up by historians of science, particularly A. Koyré and I.B. Cohen, perhaps also E.A. Burtt. I find Popper's theory in part able to accommodate for Meyerson's view, in part opposed to it. For example, Popper views the refutability of a hypothesis as a necessary and sufficient reason for our being interested in it, also for our taking it as a realistic — true or false — view of things. Contrary to this, we do not take the continuum theory seriously as physics, for example, merely because it opposes out atomistic metaphysics. Many testable hypotheses are of mere technological interest and are approached instrumentalistically, not realistically. Other hypotheses, though for long barely testable, are taken very seriously and attempts to render them a little more empirical are made. It is no accident that most of the luminaries in the Popper galaxy are now concerned with research programs and their metaphysical background. The problem is, how damaging is all this for Popper's original philosophy ? In my own opinion this is an interesting question which is now on the agenda.

Boston University
Tel Aviv University

NOTE

*Paper delivered at the Philosophy Department, Haverford College, Haverford, Pennsylvania, in April 1970, and at the Philosophy Department, The Hebrew University of Jerusalem, Israel, in April 1971.