

Philosophy of Science: A Subject with a Great Past

1. While it should be possible, in a free society, to introduce, to expound, to make propaganda for any subject, however absurd and however immoral, to publish books and articles, to give lectures on any topic, it must also be possible to *examine* what is being expounded by reference, not to the *internal* standards of the subject (which may be but the method according to which a particular madness is being pursued), but to standards which have the advantage of being simple, commonsensical, and accepted by all. Using such standards as a basis of judgment we must confess that much of contemporary philosophy of science and especially those ideas which have now replaced the older *epistemologies* are castles in the air, unreal dreams which have but the name in common with the activity they try to represent, that they have been erected in a spirit of *conformism* rather than with the intention of influencing the development of science, and that they have lost any chance of making a contribution to our knowledge of the world. (The medieval problem of the number of angels at the point of a pin had some rather interesting ramifications in optics and in psychology. The problem of "grue" has ramifications only in the theses of those unfortunate students who happen to have an engruesiast for a teacher.) This is my opinion. Let me now give some reasons for it.

2. The scientific revolution of the sixteenth and seventeenth centuries is characterized, among other things, by a close collaboration between science and philosophy. This is a direct consequence of the way in which science was debated both in antiquity and in the Middle Ages. The reaction against "medieval science," which in many cases was but a reaction against certain petrified aspects of it, leads to the development of new philosophical principles. It does not lead to a split between science and philosophy. The new philosophy that is being gradually developed is of course used to expose and to remove the hardened dogmas of the schools.

However, it has also a quite decisive role in building the new science and in defending new theories against their well-entrenched predecessors. For example, this philosophy plays a most important part in the arguments about the Copernican system, in the development of optics, and in the construction of a new and non-Aristotelian dynamics. Almost every work of Galileo is a mixture of philosophical, mathematical, and physical principles which collaborate intimately without giving the impression of incoherence. This is the *heroic time* of the scientific philosophy. The new philosophy is not content just to *mirror* a science that develops independently of it; nor is it so distant as to deal just with alternative *philosophies*. It plays an essential role in building up the new science that was to replace the earlier doctrines.¹

3. Now it is interesting to see how this active and critical philosophy is gradually replaced by a more conservative creed, how the new creed generates technical problems of its own which are in no way related to specific scientific problems (Hume), and how there arises a special subject that codifies science without acting back on it (Kant). One can say, without too much simplification, that the change is essentially due to Newton. Newton invents new theories, he proposes a radically empiricist methodology, and he claims that he has obtained the former with the help of the latter. He supports his claim by a manner of presentation that seems indeed to suggest, at least at first sight, that his optics and his celestial mechanics are the perfect results of a perfect method perfectly applied. Having convinced most of his contemporaries, he creates additional support both for his science (it has been obtained in a methodologically sound way and must therefore be free from major mistakes) and for his methodology (it has led to perfect scientific results and must therefore be the correct method).² Of course, his presentation is quite misleading, it is full of holes, fallacies, contradictions, and he himself violates every single rule he proposes. Yet it was influential enough to have blinded scientists, historians (including some very recent students of the history of optics, such as Westfall), and philosophers alike.³ 'Experience,' from now on, means

¹ For details concerning Galileo and his difference from Descartes and Bacon, see my essay "Bemerkungen zur Geschichte und Systematik des Empirismus," in Paul Weingartner, ed., *Grundfragen der Wissenschaften und ihre Wurzeln in der Metaphysik* (Salzburg: Pustet, 1967).

² The development in the quantum theory from 1927 to about 1955 was of exactly the same kind.

³ For optics see Goethe, *Theory of Colours*, which contains a very perceptive account of the ideological development just mentioned; V. Ronchi, *Histoire de la lumière* (Paris: Colin, 1956); A. I. Sabra, *Theories of Light from Descartes to Newton* (London:

either the results of Newton's experiments as described by him (optics), or the premises of his deductions (celestial mechanics), but it also means, by virtue of Newton's connecting maneuver, the safe, irrevocable, and gradually expanding basis of scientific reasoning. Small wonder that thinkers who seemed to sense a flaw but who lacked either the patience or the talent to combine their critical intuition with spectacular scientific discoveries were not heard, and were increasingly isolated.⁴ To survive, they changed their target from science to philosophy and thus created (or, rather, continued—for there was always a tradition that developed philosophy out of its own problems and with only the most tenuous relation to science) a self-sufficient subject, content with discussing its own problems. Science, on the other hand, being separated from philosophy, had to rely on intuitions of a different and much more narrow kind. The possibility of a fundamental criticism became more and more remote. In this respect, too, the situation was surprisingly similar to the situation that exists in certain parts of science today.⁵ However, there is one difference. The nineteenth century produced one philosopher who was not prepared to accept the status quo, who was not content to criticize science from the safe distance of a special subject either, but who proceeded to suggest concrete means for its change. The nineteenth century produced *Ernst Mach*.

4. Ernst Mach's "philosophy"⁶ contains a general criticism of the science of his time, including the house philosophy of the contemporary Newtonians, and a philosophy of science that completely abandons the idea of a foundation of knowledge. The criticism and the positive views are illustrated by his work in the history of science where factual and epistemological considerations are once more merged in perfect harmony, and they are given strength by the exhibition of shortcomings right in the center of the most advanced theories of the nineteenth century. The criticism and

Oldbourne, 1967); as well as my discussion of Newton in "Classical Empiricism," in *The Methodological Heritage of Newton*, ed. R. E. Butts and J. W. Davis (Toronto: University of Toronto Press, 1969).

⁴ An exception was Faraday, but his background philosophy remained almost completely unknown.

⁵ The similarity becomes even greater in view of the fact that the famous chocolate layer-cake model of scientific knowledge that has been developed by Nagel, Hempel, and others is nothing but a more sophisticated (and less clear) repetition of Newton's views, down to the last mistake. Cf. "Classical Empiricism," footnote 8, as well as my review of Nagel in the *British Journal for the Philosophy of Science*, 17 (1966), pp. 237–249.

⁶ "Above all, there is no Machian philosophy; there is, at the most, a methodology of science and a psychology of knowledge and like all scientific theories these two things must be regarded as preliminary and incomplete attempts." *Erkenntnis und Irrtum* (Leipzig: J. A. Barth, 1905), p. vii, footnote (against Hoenigswald).

Mach's own positive suggestions have been extremely fruitful, both in the sciences and in philosophy. In science Mach's suggestions have contributed to the development of the general theory of relativity and they play an essential role in more recent discussions, now that interest in general relativity has been revived. They had also a decisive influence, not always beneficial, on the founders of the quantum theory (even Schrödinger was often heard to say, quite emphatically: "Aber wir koennen doch nicht hinter Mach zurueckgehen!"). In philosophy it was a different story, as will be seen below.

In order to understand Mach we must distinguish most carefully (and more carefully than Mach himself did on various occasions) between his *general methodology* (s.v.v.) and the more *specific hypotheses* he used as starting points of research.

A general methodology is independent of any particular assertion about the world, however trivial, and however obvious. It is supposed to provide a point of view from which *all* such assertions can be judged and examined. It will not assume a dichotomy between an objective world and a perceiving subject who explores the world (using his mind and his senses) and gradually increases his knowledge of it. Such a dichotomy is presupposed by almost all science, it is the instinctual basis of everyday behavior (at least in Western societies), and it has been professed with almost religious fervor by thinkers who otherwise pride themselves on having made criticism a principle of science and of philosophy. Yet—is it not possible that this view is mistaken? Is it not possible that it neglects or misrepresents phenomena of an intermediate nature which show that the boundary is rather ill-defined, and perhaps altogether nonexistent? And if we *admit* this possibility, must we not ask ourselves how such a failure of realism can possibly be detected without relying on realism in the process? At this point recourse is usually made to *sensations*, and this is quite appropriate if the existence of sensations, bundles of sensations, lawful connections between sensations, is regarded as an *alternative hypothesis* rather than as an eternal measuring stick of any subject that is not explicitly about sensations. For just as the existence of the real world is a topic for critical discussion, in the very same manner the existence of sensations is a topic for critical discussion. General methodology, therefore, must refer neither to the one nor to the other (although it may provide rules for playing them off against each other) and, indeed, *no such reference is made by Mach*. According to Mach the task of science is to find simple and regular con-

nections between elements. Let us analyze the various parts of this assertion.

5. All regularity, says Mach, is imposed, or constructed, yet it never fits all relevant cases. This assertion is not entirely without content. It is assumed that there exists a domain where regularities are produced and another domain which has idiosyncrasies of its own and which can never be fully comprehended, or tamed, by the imposed laws. Every rule, every law, even the most precise formulation dealing with events carefully prepared is bound to have exceptions, and even a perception which at first sight seems perfectly symmetrical loses this symmetry on closer inspection. This idea of the two domains comes forth very clearly in passages such as "What is constant, the rule, a point of departure does not exist except in our thinking," which have a decidedly Kantian flavor.⁷ "It is we who glue things together, not nature."⁸ This seems to push us irrevocably toward a theory of sensations *but it does not*, for the elements which are related in this more or less regular manner are carefully distinguished from sensations. Speaking of sensations, Mach says quite explicitly, *already entails acceptance of a one-sided theory*.⁹ Elements are not sensations. They are not perceptions either, for perceptions are rather complex entities, containing memories and attitudes as well as "natural habits"¹⁰ of the human species. They are certainly not material objects. *They are open places, to be filled and refilled by the results of research*. All that is asserted is therefore that complexes consisting of elements which may in turn be complexes (though perhaps complexes of a simpler kind, at least at a particular stage of knowledge) are assembled into higher units whose stability is always in question and may be upset either by new theoretical procedures, or by a change of our ordering habits, or by the realization that essential things have been left out, or by a change in the elements, and so on. Now if we want to remove as many particular assumptions as possible, if we want to arrive at a truly general methodology, then we must abandon this opposition between an ordering mind and an ordered material also and must restrict ourselves to stating a *development* of *prima facie* simple elements which arrange and rearrange themselves, dissolve and recombine in

⁷ Notebook III, February 1882, p. 82. Quoted from Hugo Dingler, *Die Grundgedanken der Machschen Philosophie* (Leipzig: J. A. Barth, 1924).

⁸ Notebook I, May 1880, p. 58: "Nur wir kleben zusammen, die Natur nicht."

⁹ "Da aber in diesem Namen ('sensations') schon eine einseitige Theorie liegt, so ziehen wir vor, kurzweg von Elementen zu sprechen . . ." *Analyse der Empfindungen* (Jena, 1900), p. 15, italics in the original.

¹⁰ *Ibid.*, p. 137.

different patterns, a view that has great similarity with what is explained in Hegel's *Logik* (except that it does not contain the more specific hypothesis of modified preservation).¹¹

Now research proceeds by first filling this general scheme with more specific content and then developing consequences, always remaining critical of the particular hypotheses used. The hypothesis to which Mach appeals rather frequently identifies the elements with sensations. This hypothesis plays an important part in Mach's criticism of contemporary physics and philosophy. *But it is never regarded as being above criticism*. It functions rather like, let us say, the principle of Lorentz invariance which is constantly used for the criticism of theories without being itself exempt from criticism.

That sensations cannot be an absolute basis for Mach becomes clear from the very title of his book *The Analysis of Sensations*. Sensations are to be *analyzed*. The complexity that hides behind the simple appearance is to be discovered and reduced to other and perhaps as yet unknown elements. These new elements are again in need of analysis, "they must be further examined by physiological research,"¹² and so on. The hypothetical character of sensations also becomes clear from Mach's attempt¹³ to put them in their proper place and to free science from their dominance. In debates with Hugo Dingler whom he praised in the Foreword to the seventh edition of his *Mechanik* he admits to being a "non-empiricist" or a "not-only-empiricist,"¹⁴ and he follows with interest Dingler's earlier attempts to eliminate experience from applied geometry. This partial anti-empiricism of Mach (which in truth is nothing but his universal criticism applied to the empiricist ideology) is a fascinating topic for research.

Now having adopted a *particular cosmological hypothesis* which seems plausible to him (elements are sensations) he *applies* it, and introduces the principle that science should contain only such concepts which can be connected with sensations. (It is useful to again compare this principle with the principle that scientific theories should be Lorentz-invariant.) But it is interesting to see that he never relies on this "empirical" criticism alone. Since the use of sensations is based on a hypothesis it is necessary to

¹¹ For details see section 3 of my essay "Against Method," in vol. IV of the *Minnesota Studies in the Philosophy of Science*, ed. M. Radner and S. Winokur (Minneapolis: University of Minnesota Press, 1970).

¹² *Analyse der Empfindungen*, p. 20.

¹³ Notebooks, vol. II, p. 16 from back.

¹⁴ H. Dingler, *Die Grundgedanken der Machschen Philosophie*, p. 61, footnote.

check it at every point by independent arguments. The objections to absolute space and to atomism are a case in point.

Absolute space is nonempirical, it cannot in any way be connected with sensations. Are there perhaps reasons which nevertheless force us to adopt it? There is Newton's bucket argument. This argument is invalid not because it makes appeal to things which are not related to sensations, but because it rests on a false assumption. Centrifugal forces, it is said, arise even if there is no material around with respect to which rotation can be asserted. This is untrue, for there are the fixed stars. If one now asserts that the fixed stars have no influence one asserts what has to be shown, viz., that centrifugal forces are not due to a particular relation to the rest of the universe. In this form the argument is circular *in addition to* yielding a metaphysical conclusion. Is it perhaps possible to decide the question by experiment? In order to advance in this direction Mach develops an alternative theory of his own in which inertial forces depend on the presence of matter and can be changed by the movement of large neighboring masses (Friedländer's experiments were entirely in Mach's spirit). This alternative theory still survives and is now in the center of discussion.

Mach's criticism of the atomic theory is another example of the way in which he combines his sense-data hypothesis with other, and more concrete, arguments. Again the metaphysical criticism is supported by difficulties of the existing atomic theories (reversibility objection; recurrence objection; stability of the atom). There is no reason to discredit Mach for his unwavering opposition to atoms.¹⁵ For the mechanical atom of the nineteenth century has indeed disappeared from the scene.

6. To sum up: Mach develops the outlines of a knowledge without foundations. He introduces cosmological hypotheses as temporary measuring sticks of criticism. The cosmological hypothesis he appeals to most frequently assumes that all our knowledge is related to sensations. Using this hypothesis he criticizes physical theories such as the atomic theory and

¹⁵ There has arisen the belief that Mach changed his mind in the last few years of his life and that he finally accepted atomism. This is based on a story told by Ernst Mayer, the head of the Radium Institute in Vienna at the time, who showed Mach a scintillation screen and reports him as saying, "Jetzt glaube ich an die Atome." There is no reason to doubt Mayer's story (Mayer was an honest man). But there is much reason to doubt the *interpretation* that is usually put on it (this has been pointed out to me by Dr. Heinz Post of Chelsea). In itself the story is rather implausible. Why should Mach be convinced by a peripheral phenomenon such as the scintillation screen? There is also the fact that the optics which was published after the incident shows him as hostile to atoms as ever. Dr. Post assumes that Mach's reaction was simply the reaction of a kind man to an enthusiastic (and rather deaf) demonstrator.

Newtonian mechanics. The cosmological criticism is strengthened by a more specific examination and by the suggestion of alternatives. In this way Mach achieves unity between science and philosophy that had been lost as the result of the dominance of Newton's physics and of his philosophy.

7. The unity is achieved *and is at once lost again* by the empiricist successors of Mach. No longer is there a combined effort to criticize and to improve physics, nor is the critical attitude retained that was such an essential part of Mach's research. It is quite impossible to narrate and to explain all the developments which led from Mach through the Vienna Circle (plus the Berlin group, plus the Scandinavian groups, and so on) to the contemporary situation. Here is a fascinating field of study for the historian of ideas. All I can do in the present short note is to make a few scattered remarks and to formulate some questions.¹⁶

The first and most noticeable change is the transition from a critical *philosophy* to a *sense-data dogmatism*: elements are replaced by sensations not just temporarily, and as a matter of hypothesis, but once and for all. Sensations are regarded as the solid foundation of all knowledge. It would be interesting to know how this replacement came about and how it happened to be connected with the name of Mach. Dr. Laudan has a simple suggestion and I am inclined to follow him (in the history of ideas the profound ideas are not always the best ones): Mach was either not read at all or read with little care. As a result the important distinction between basic philosophy and special hypotheses was never discovered, let alone understood. Besides, the end of the nineteenth century teems with sensationalistic philosophies. Of course, there were also more tolerant philosophies (Neurath; Carnap), but they never got beyond the abstract statement that the selection of an observation language (sense-data language; physical-thing language) was a matter of choice. There was never any analysis of the concrete steps and of the concrete arguments which might favor one choice rather than another.¹⁷ *In practice* one assumed that a certain language was given and proceeded to discuss, reform, evaluate *theories* on its basis. And it did not last long, and the abstract principle of tolerance

¹⁶ Dr. Laudan is now studying the history of the early Vienna Circle and he has already arrived at some very interesting results. The following brief sketch is influenced by some of these results, though it is written much more carelessly than Dr. Laudan would dare.

¹⁷ I have discussed such arguments in a particular case in sections 5–10 of "Against Method."

was abandoned also and was replaced by a more restrictive philosophy.¹⁸

Secondly, criticism of science is replaced by *logical reconstruction* which, in plain English, is nothing but a highly sophisticated brand of conformism. This development is partly connected with the first: if sensations are the foundation of knowledge, then the chains which connect, say, Maxwell's equations with the basis should be clearly exhibited and should be formulated as precisely as possible. Having found a reformulation of the chains one hoped to be able to rewrite the equations themselves, using the same precise language. Now there *did* exist a language which seemed to provide ready translations for mathematical formulas, viz., the language of *Principia Mathematica*. The logical reconstruction of science therefore amounted to the attempt to rewrite science in PM and to clearly exhibit all the links with the "basis."

Now an attempt of this kind may be understood in at least two different ways; *critically*: what cannot be reconstructed in this fashion must be eliminated; *conformistically*: what cannot be reconstructed in this way shows that the methods of reconstruction are faulty and must be revised. The critical version survived for some time—for had not Mach changed science by using a vague form of the verifiability principle and was not the theory of relativity the glorious result of this newly found philosophical method? But being isolated, and without help from more specific arguments (no one thought of inventing more concrete criticisms in addition to the arguments flowing from the reconstruction program), the verifiability criterion lacked strength to survive a showdown with science. In his famous debate with Planck Mach refused to revise his view of science just because it differed so much from the actual thing. Science has become a church, he said, and I have no intention to be a member of a church, scientific or otherwise. I therefore gladly renounce the title of a scientist: "Die Gedankenfreiheit ist mir lieber."¹⁹ He argued from a strong position having both a plausible cosmological hypothesis and specific difficulties of the existing science on his side to prove his point. Not a single positivist was bold enough to wield the verifiability criterion in the same spirit. Driesch was criticized, yes, but this was a rather trivial case. There was no attempt to cut physics down to size in a similar manner. As a result the idea of a logical reconstruction became conformistic. The task was now to *correctly*

¹⁸ For a sketch of this development see my essay "Explanation, Reduction, and Empiricism" in vol. III of the *Minnesota Studies in the Philosophy of Science*, ed. H. Feigl and G. Maxwell (Minneapolis: University of Minnesota Press, 1962).

¹⁹ *Zwei Aufsätze* (Leipzig, 1919).

present rather than to *change* science (it did not take long and this attitude was extended to the language of common sense also).

8. This task in turn was soon transformed into problems of a different kind, some of which were no longer related to science at all. Trying to imitate the scientific procedure (or what one thought was the scientific procedure), one started with the discussion of *simple* cases. Now a simple case, in this context, was not a case that looked simple when viewed from inside science. It was a case that looked simple when formulated in the language of *Principia Mathematica*. In addition one concentrated on the relation to the evidence, omitting all those problems and aids which arise from the fact that every single statement of science is embedded in a rich theoretical net and chosen to fit this net in one way or another (today a "simple" physical theory is a theory that is relativistically invariant). Now I do not wish to say that the properties of nets are not being discussed, for there is a large literature on precisely that point. All I wish to assert is that there exists an enterprise²⁰ which is taken seriously by everyone in the business where simplicity, confirmation, empirical content are discussed by considering statements of the form $(x)(Ax \rightarrow Bx)$ and their relation to statements of the form $Aa, Ab, Aa \& Ba$, and so on and *this* enterprise, I assert, has nothing whatever to do with what goes on in the sciences. There is not a single discovery in this field (assuming there have been discoveries) that would enable us to attack important scientific problems in a new way or to better understand the manner in which progress was made in the past. Besides, the enterprise soon got entangled with itself (paradox of confirmation; counterfactuals; grue) so that the main issue is now its own survival and not the structure of science. That this struggle for survival is interesting to watch I am the last one to deny. What I do deny is that physics, or biology, or psychology can profit from participating in it.

9. It is much more likely that they will be *retarded*.

This can be shown both theoretically, by an analysis of some rather general features of the present state of the program of reconstruction, and practically, by exhibiting the sorry shape of the subjects (sociology; political science) which have made a vulgarized version of the program their chief methodological guide. I shall restrict myself to a brief discussion of two theoretical difficulties.²¹

²⁰ Prominent textbook: I. Scheffler's *Anatomy of Inquiry* (New York: Knopf, 1963).

²¹ For more detailed analyses involving historical material the reader is referred to my essays "Problems of Empiricism" in *Beyond the Edge of Certainty*, ed. R. G. Colodny (Englewood Cliffs, N.J.: Prentice-Hall, 1965); "Problems of Empiricism, II," in *Aim*

The empirical model assumes a language, the "observation language," that can be specified independently of all theories and that provides content and a testing basis for every theory. It is never explained how this language can be identified nor are there any indications how it can be improved. Carnap's rule that the language should be used by a language community as a means of communication and that it should also be observational in the vague sense of containing quickly decidable atomic sentences is clearly unsatisfactory. In the sixteenth and seventeenth centuries a language of this kind would have included devils, angels, incubi, succubi, absolute motions, essences, and so on. Now this fact, taken by itself, is no objection against the scheme provided the scheme permits us to overcome devils in a rational manner. This is not the case. An observation language is a final standard of appeal. There are no rules which would permit us to choose between different observation languages and there is no method that would show us how an observation language can be improved.²² We may of course try to extract the devils from their observational surroundings and to formulate a demonic theory that can be interpreted and tested on the basis of a different observational idiom. This procedure is *arbitrary* (why should we not invert our procedure and test Dirac's theory of the electron on the basis of an Aristotelian observation language? And it is excluded by the very same principles which make the model a judge of the empirical content of a notion (removing the observational embroideries of a concept changes the concept). The model is therefore incomplete at a very decisive point. And in order to remove the impression that it needs the devil to exhibit this incompleteness we should perhaps add that the transition from the physics of Aristotle to the physics of Galileo and Newton creates exactly the same problems. Here we see very clearly how the transition is achieved²³ and how complex are the arguments that bring it about (Galileo, for example, introduces a new observation language *in order to accommodate the Copernican theory*). Compared with this reality the double language model looks infantile indeed.

and *Structure of Scientific Theory*, ed. R. G. Colodny (Pittsburgh: University of Pittsburgh Press, 1969); "Against Method"; and my criticism of Nagel in the *British Journal for the Philosophy of Science*, 17 (1966), 237-249.

²² In his essay "Empiricism, Semantics and Ontology," reprinted in Leonard Linsky, ed., *Semantics and the Philosophy of Language* (Urbana: University of Illinois Press, 1952), pp. 207-228, Carnap has discussed this feature with great insight. However, apart from distinguishing between external and internal problems he does not give any indication of how one should proceed.

²³ The arguments are presented in the references in "Against Method."

The second difficulty is that not a single theory ever agrees with all the known facts in its domain.²⁴ This being the case, what shall we make of the methodological demand that a theory must be judged by experience and must be rejected if it contradicts accepted basic statements? What attitude shall we adopt toward the various theories of confirmation and corroboration which all rest upon the assumption that theories can be made to completely agree with the known facts and which use the amount of agreement reached as a principle of evaluation? This demand, these theories, are now all quite useless. They are as useless as a medicine that heals a patient only if he is completely bacteria free. *In practice* they are never obeyed by anyone. Methodologists may point to the importance of falsifications—but they blithely use falsified theories; they may sermonize how important it is to consider *all* the relevant evidence, and never mention those big and drastic facts which show that the theories which they admire and accept, the theory of relativity, the quantum theory, are at least as badly off as are the older subjects they reject. In practice methodologists slavishly repeat the most recent pronouncements of the top dogs in physics, though in doing so they must violate some very basic rules of their trade.²⁵ Is it possible to proceed in a more reasonable manner?

10. The answer is of course—yes. But the remedy needed is quite radical. What we must do is to replace the beautiful but useless formal castles in the air by a detailed study of primary sources in the history of science. *This* is the material to be analyzed, and *this* is the material from which philosophical problems should arise. And such problems should not at once be blown up into formalistic tumors which grow incessantly by feeding on their own juices but they should be kept in close contact with the process of science even if this means lots of uncertainty and a low level of precision. There are already thinkers who proceed in this way. Examples are Kuhn, Ronchi, the late Norwood Russell Hanson, and especially Imre Lakatos who has almost turned the study of concrete cases into an artistic enterprise and whose philosophical suggestions can once more be used to transform the process of science itself. It is to be hoped that such a concrete study will return to the subject the excitement and usefulness it once possessed.

²⁴ For details see section 4 of "Against Method."

²⁵ In this, of course, they merely repeat Newton. The only difference is that the theories which Newton accepted were invented by himself.