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Is There a Well Defined Scientific Method?

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The following four papers are amplifications of a panel discussion on the subject that was held at the History and Philosophy of Science Section of the 33rd Annual Meeting of the Minnesota Academy of Science on May 8, 1965. The authors are respectively, philosopher, philosopher, physicist, and psychologist. Dr. McMullin, Chairman of the Department of Philosophy at the University of Notre Dame, was at the Philosophy Department of the University of Minnesota at the time the Meeting was held. He acted as chairman of the section meeting.

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Does science follow some sort of standard procedure, something that can be specified and communicated? Three centuries ago, Francis Bacon[1] prophesied confidently that such a procedure could be devised so that the whole business of science could be done "as though by machinery." In the years between, scientific research has grown from an obscure and unrecognized undertaking of a handful of virtuosos to a massive and concerted endeavor on the part of hundreds of thousands of persons. What has made such a fantastic expansion possible in such a short time? Is it that people have been taught the steps by which science is carried on, so that they can go off and carry research further on their own? This is the impression given by many elementary textbooks of science; indeed, this "Baconian" view of science is found on occasion among those who are themselves distinguished for their scientific work.[2]

1. The Answer From Early Greek Science

Before trying to answer the question posed in our title, it might be of some interest to seek the answer from those in whose minds science first took shape: the natural philosophers of ancient Greece. It was a question that they quite often thought of (one of Aristotle's most influential works, *Posterior Analytics*, was largely concerned with it). Their attempts to answer the question were dominated by two special assumptions. First, they assumed that *epistémé*, true knowledge, ought to have some warrant by which men might come to recognize it. In one of his best remembered myths, Plato hinted that such recognition might be extremely difficult to accomplish; he spoke of the difficulty of seeing by sunlight after living for years in a cave. In his mind, there was no guaranteed way of bringing someone to see a truth as "science"; an approach by indirection, in which someone already in command of the truth would gradually lead an enquirer by skillful questioning to see the truth for himself, seemed to him (at least in his earlier works) to be the only feasible way. In such a view, the discovery of an entirely new truth appeared beyond the power of man, and Plato did, in fact, suggest that all apparent discovery is a "recollection" of some sort. Aristotle, on the other hand, was more optimistic about the possibility of specifying procedures—in principle open to everyone—that would mark off "science" from other forms of knowledge. Chief among these procedures was the logical frame of the syllogism: clear-cut, easily grasped, definitive in its deductive certitude.

This brings us to the second major influence on Greek thinking about science: axiomatic geometry. It was by far the most successful of the theoretical knowledges available at the time. It was certain, stable, and gave an exact knowledge about the spatial structures of the world. It possessed a method, the deductive axiomatic, that could be learned even by the young. It was natural, in consequence, to think of it as the model of what "science" should be, something with a specifiable method of procedure (possibly even a deductive one) that would serve to justify any thesis that could lay proper claim to the title "science."

Yet the model had one major problem. Theorems were easy to verify and relatively easy to discover. But what about the axioms, the "principles" or starting-points? Was there any method for deriving and justifying them? Aristotle spoke of an "induction" (*epagógé*), which led to the discovery of first principles, but he was never very definite about how it ought to work. Sometimes, if the analogy of geometry were stressed, it seemed to be an intellectual inspection of statements that were seen to be

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[1] *Novum Organum* (1620) Preface.

[2] E. g., J. R. Platt's emphatic defense of the view in "Strong inference: the new Baconians," *Science*, 146, 1964, 347-353. He argued that the rapid advances in recent biochemistry are a result of consistent application of a definite easily stated method, and he suggested that the relatively slower development of other areas can be attributed to a refusal (often on the score of prejudice) to adopt the Baconian method.

necessarily true, once their terms were fully understood. At other times, when Aristotle the patient experimental biologist was speaking, it sounded more like the collecting of relevant empirical data with a view to careful generalization. But in the latter case, how could the resultant "axiom" be stated with anything more than a high degree of probability? The tension between these two ideals, one deriving from geometry and the other from actual practice in biology and medicine, dominated much of the discussion of scientific method right up to the seventeenth century.[3] But since it was assumed without question that the proper method for science ought to be inferred from a general theory of knowledge of reality, rather than derived from below in a makeshift manner from observational techniques and practices of discovery, the axiomatic notion of "science" was the one generally accepted.

2. The Seventeenth-Century Answer

In the seventeenth century, natural science was gradually transformed under the impact of the ideas of men like Galileo, Boyle, Descartes, and Newton. It is striking to note that these pioneers assumed, as their predecessors had, that science *does* have a perfectly definite method, and that it can, in fact, be marked off from other forms of knowledge by the possession of such a method. They thought, however, that their own methods were importantly different from all that had gone before; some, like Bacon, stressed this "renovation" more than others, but all of them did in fact suppose that their "new science" was "new" principally because of its procedures. It was characteristic that they had much to say about method, though few of them went so far as to devote a special treatise to it, as Descartes did. But, when one looks at what they had to say, a surprise is in store; the difference between their methodology and that of their predecessors was not nearly as great as they claimed.[4] And this is true whether one examines their explicit methodology (i.e., their formal discussions of method), or their implicit methodology (inferred from their actual scientific practice). There were differences, of course: a greater stress on the empirical side of science, the planning of controlled experimental tests, the extensive use of mathematics at all levels, the increasing reliance on technological insights. . . . And these were, of course, quite decisive differences.

But one thing had not changed much: the axiomatic ideal of a science whose "principles" would be seen to have a conceptually necessary character, an ideal that seemed to many to be completely achieved in the mechanics of Newton. It was still plausible to think of experiment as an *occasion* of discovery rather than as an ultimate *warrant*. Galileo frequently spoke of the perceptual realm in the accents of Plato; it seems as though he thought of his experiments, in mechanics at least, more as a means of discovery or of persuasion; they could be discarded once one had come to "see" the truths. Bacon, it was true, took a far more empirical line, but it had very little echo in the actual practice of mechanics. In areas like chemistry, biology, and magnetism, research followed a much less deductivist approach, but it was natural to assume that this was only because of the complexity and relatively undeveloped character of these researches, by comparison with the sophistication of mechanics.

Thus, by the eighteenth century the answer to the question, "Is there a well-defined scientific method?" would have been an emphatic "yes" from most of the scientists of the day. If asked what that method was, they would have differed; and the differences would have been especially marked between the Cartesian physicists of France and the Newtonian physicists of England. The former would have stressed the role of deduction and conceptually necessary first principles much more than the Newtonians did; the latter would have talked about exact observations and the use of hypothesis (in less developed areas like optics, at least). But the Cartesians made observations, of course, and the Newtonians distrusted hypothesis and always hoped for something like Newton's axiomatic *Principia* in every area of science. Among scientists, therefore, there were really no *pure* rationalists or *pure* empiricists.

It was only with the nineteenth century that a tenable middle road between the two extremes began to reveal itself. But it meant giving up the older Greek ideal of an axiomatic empirical science that had been built on intuitively warranted first principles. And this was not easy to do in this century of Newtonian triumph. Methodologists like Whewell and Peirce were analyzing the actual procedures of the scientist with a precise attention to detail, and with much less assurance than their predecessors that methodology ought to be derivable from a general epistemology. If anything, they took just the opposite point of view. Instead of regarding hypothesis as a sort of crutch at the level of discovery—to be discarded when the level of justification was reached—they recognized the inescapably hypothetical character of any empirical knowledge that lays claim to generality and precision. They noted that hypotheses are warranted by the continued verification of predictions made by their means. Our basic reason for accepting a scientific theory is not that its terms are seen to be conceptually related in a way seen intuitively to be "right," but, rather, that it provides predictions that are borne out, besides being fruitful in suggesting conceptual interconnections with other theories as well as possible expansions into new areas. The method of science was thus said to be "hypothetico-deductive."

Over against this view, Mill was arguing the older Baconian thesis of an inductive and relatively automatic way of ascending from individual observations to higher and higher generalizations, by the careful planning of experimental comparisons and checks. He devised his famous "methods" of sameness, difference, and concomi-

[3] For a fuller treatment, see McMullin, E. (1964) "The nature of scientific enquiry: what makes it science?" in *Technology and Culture*, G. McLean (Ed.). Washington: Catholic University Press. Pp. 28-54.

[4] See E. Grant, "Late medieval thought, Copernicus, and the scientific revolution," *Journ. Hist. Ideas*, 30, 1962, 197-220.

tant variation, which he thought to be a summary of the best experimental practice of his day. If one followed his methods—and they were not difficult to grasp—the implication was that science could then proceed in a steady and decisive manner. The teaching of the young research scientist ought thus to center around the accurate grasp of the "methods." The trouble was that many of the century's major advances in science seemed quite remote from anything resembling a persistent application of the "methods": Maxwell had certainly not gone through any such procedures in formulating his electromagnetic theory of radiation, nor had thermodynamics depended much on their employment either.

This brings us to our own century. It began with the replacement of Newtonian mechanics by the more general and conceptually quite different system of Einstein. The best support of the rationalist theory of method was thus destroyed: One could no longer point to mechanics as a prime example of an empirical science built on intrinsically cogent principles and requiring no extrinsic hypothetico-deductive support. But it was still not clear what "the" method of science ought to be; the assumption was that such a method was gradually being revealed in the practice of science itself, with its lengthening pragmatic record of successes and failures. Now that science was becoming a dominant force for shaping the world to man's desires, the problem of how to train good scientists was coming to the fore. And science was beginning to play a much larger part in education generally than had ever been the case before. In the circumstances, it was inevitable that textbooks tended to speak of "the scientific method" as something definite and teachable, something already achieved. Yet doubts about this optimistic appraisal have continued to increase. In the remainder of this introductory essay, some elementary distinctions will be very briefly outlined in order to facilitate discussion of the present state of the question.

3. The Major Procedures of Scientific Enquiry

It is customary to distinguish between three logically different types of procedure in science, as follows:

(1) *Deduction.* One infers from premiss (evidence) to conclusion, in a way that is altogether rule-bound. The rules of deduction are in principle completely specifiable, and it is possible to test to see whether they have been correctly used. If they have, our result is validated, and no further evidence need be sought. This is the paradigm of the "well defined method," and it is found in its pure form in certain parts of the formal sciences—logic and mathematics. If a formal system is fully specified, the theorems of the system will be governed in a decidable way by an explicit set of methodological criteria. The presumption is that a formal system is already something given; since the theorems are already latent in it, discovery here reduces to a drawing out of what is already fully there. Deduction is thus very limited as a tool of *discovery*, though it is the most powerful of all tools of *verification*.

(2) *Induction.* One makes a generalization on the basis of some singular instances satisfying the generalization. For example, one might observe a number of cases in which potash is used as fertilizer and make the generalization, "potash under certain (specified) circumstances promotes the growth of certain (specified) plants." Or one might construct a continuous curve from a finite number of laboratory results; from half a dozen co-ordinate measurements of the relevant experimental parameters, one might leap to a generalization such as $pv = kT$. The evidence for the generalization would be the half-dozen original results; each would be a particular instantiation for some set of values of the parameters. Such generalizations are descriptive rather than explanatory, since they do not involve new concepts, only those in which the original observations were expressed. The "leap" here is not a matter of discovering new concepts, then, but rather of going from part to whole.

Such inference is, of course, always hazardous, always open to later disproof when other parts of the "whole" become known or when the original "part" comes to be more accurately known. Can it even be described properly as an "inference," i.e., a rationally explicable rule-guided procedure? It might seem more like guess work. Yet there are at least two domains in which such part-to-whole "leaps" are constantly and successfully made. The first is that in which part and whole can be finitely expressed and thus subjected to the techniques of mathematical probability theory. Suppose we have a large bag of marbles and draw 10 marbles, one after another, each of which turns out to be red. What are the odds that *all* the marbles in the bag are red? Given certain assumptions about how the contents of the bag were originally chosen, these odds can easily be calculated in a purely formal way. On the other hand, suppose we have a shipload of bananas and want to estimate what sort of condition they are in. It would take much too long to go through all the bunches, so we "sample" and extrapolate. Sampling is a highly skilled job that involves a lot of experience with the type of situation being tested. The sampling techniques for a cargo of bananas will differ greatly from those used for a storehouse of canned goods or for a city water supply. The techniques will implicitly involve not only probability theory, but, in addition, empirical knowledge about, for example, the spoilage causes of bananas, the layout of the particular ship, etc. Though this knowledge may be explicitly formulatable, more often than not the sampler's skill is of an "intuitive" sort. He tends to rely on clues and generalizations that he cannot make explicit.

Part-whole inference in experimental science has something of both of these: it relies on probability theory as well as on the developed skill of the "sampler." Thus, its procedures are partly formal and deductive, and partly informal and dependent upon a considerable experience with the type of situation being investigated. In practice, the commonest form of scientific induction is that involved in "curve fitting," i.e., in going from a finite number of observations to a functional correlation. In a quantified science, all experimental evidence must be expressed in terms of such "laws"; until such reproducibility is reached, there can be no assurance that an adequate statement of the relevant causal factors has been given. The "inferring" of a smooth curve from a discrete number of small fuzzy points (each involving some degree of possible experimental error) involves assumptions about the most likely form of curve, assumptions that are part aesthetic and part empirical (derived from previous acquaintance with similar types of experimental situation).[5] They can be formalized, up to a point, especially if some criterion of mathematical "simplicity" is agreed upon. But the result is, of course, never more than probable. And there is no way of providing a reliable mathematical estimate of *this* probability. That is, when we formulate a complex empirical law, there is no way of estimating the likelihood of relevant factors having been omitted in making the "leap" from part to whole.

(3) *Retroduction.* Instead of trying to generalize, we may wish to "explain," that is, to find an hypothesis in terms of which the given data will become more intelligible to us. Without dwelling on the complexity of the notion of "intelligibility" involved here, it is possible to specify that the data must be *deducible* from the hypothesis (or at least from the hypothesis plus other accepted hypotheses), that the hypothesis should not be *ad hoc* but have a certain generality, a capacity for unifying previously discrete domains of evidence, and that it introduce new conceptual elements (e.g., a model) not directly contained in the original statement of the evidence. Does the formation of hypotheses follow any "rule," any specifiable guidelines of the sort we found in deduction and induction? There has been a great deal of writing about creativity of late;[6] it seems to be fairly generally agreed that to be creative is precisely to operate *outside* the pattern of accepted rule, to juxtapose matrices of thought not hitherto related (as Koestler puts it).[7] To hit upon a hypothesis is, in general, not something rule-bound, and this is the more true the more basic the new hypothesis is.

Yet, though this is so, there will usually be at least *some* guide-lines, some restrictions on the direction one might most profitably follow in seeking an explanation. These guidelines will be of a "material" sort, that is, they will be entirely dependent upon the sort of "material" we are working with. If we are trying to find a hypothesis to account for certain patterns of scattering that occur when an electron beam hits metal foil, experience suggests that some internal structure in the atoms of the foil may "account for" it (i.e., serve as a hypothesis from which it may be deduced). What is required here is an intimate knowledge of the materials involved, of the historical development of theories concerning them, of conceivable interconnections with other domains, etc. None of this yields anything like general rules of hypothesis formation; it will entirely depend on the circumstances. But it must be emphasized that when a scientist *does* form an hypothesis, it is usually by no means a mere guess; it is informed by a definite personal skill in interpreting his domain and he could, if necessary, specify (up to a point) the sort of "reasoning" that led him to this particular hypothesis.

We can thus think of deduction as purely *formal*, i.e., entirely independent of the context of application; retroduction, on the other hand, is wholly *material*, i.e., context-dependent, involving an insight into, and prior theoretical knowledge of, the particular material context under investigation. While induction is a mixture of formal and material; insofar as it employs mathematical theories of generalization drawn from probability theory, it is formal, whereas to the extent that it demands specific knowledge of the type of situation being generalized, it is material.

Retroduction is equivalent above to hypothesis-formation. If one looks at the classic discussion of it by Peirce, or the more recent treatments by Hanson and Bocheński, one finds a recurrent ambiguity.[8] Peirce contrasted it with deduction by saying that deduction follows the pattern, p implies q, p is the case, $\therefore$ q is the case; whereas retroduction (he also called it "abduction" or "reduction") is of the form, q is the case, $p \rightarrow q$, $\therefore$ probably p. Now if retroduction be defined in this way, it is equivalent to the process by which we go from q (evidence) and $p \rightarrow q$ (a particular hypothesis implies this evidence) to the assertion that the hypothesis is worthy of qualified support. But this presupposes that the hypothesis has already been hit upon. In other words, what we spoke of as "retroduction" above was the movement of thought from evidence (q) back to hypothesis (p). The retroduction is already *contained* in Peirce's premiss $p \rightarrow q$, though the movement of thought would suggest that we alter the direction of the arrow.

Much of the recent controversy over whether or not there is a "logic of discovery" might have been avoided if this simple ambiguity had been noted. The ambiguity originally arose from the attempt to involve hypothesis in some sort of scheme of types of "inference," which would allow a neat three-fold division with deduction and induction as two of the types. But "inference" itself here can be taken in two rather different senses, depending on whether *discovery* or *justification* be the aspect stressed. To infer a conclusion from two premises in a syllogism can be regarded either as *discovery* of the conclusion (taking it to be previously unknown) or as *justification* of the conclusion (taking the premises as evidence). In deduction, to discover is to justify, and vice

[5] In *The Methods of contemporary thought* (Dordrecht: Reidel, 1965), Bocheński listed four assumptions that are basic to induction. (He took "induction" in a considerably wider sense than we have done here, however.)

[6] See the references given in McMullin, E. (1965) "Creativity and scientific discovery." In *Freedom and man*, J. C. Murray. (Ed.) New York. Pp. 105-130.

[7] In his recent exhaustive and challenging work, *The act of creation*, New York, 1964.

[8] Peirce. (1931-5), *Collected papers*. P. Weiss and C. Hartshorne. (Eds.) Harvard U.P., I, 71-4; II, 372-88; V, 189; VI, 477, 522-8. N. R. Hanson. (1963) "Retroductive inference." In *Philosophy of science*, B. Baumrin. (Ed.) New York. Pp. 21-37. I. Bocheński, *op. cit*.

versa. The same is true in induction: The procedure that leads us to the generalization is also the procedure that justifies our making this particular generalization.[9] In the domains of deduction and induction, therefore, the methodologies of discovery and of justification need not be sharply distinguished.

This has had far-reaching consequences in those methodologies in which deduction and induction were the only modes of inference recognized. Those would include not only that of Aristotle's *Posterior Analytics*, but also most modern accounts, until within the past decade, at least. In looking at the typical procedures of scientific enquiry, it did not seem important to ask whether one was concerned with discovery or with justification (proof, validation), because the same procedures seemed to function in either case. It is only when hypothesis is taken seriously as an indispensable element of enquiry that one must be very careful to separate discovery from justification. The procedures involved in hitting upon an hypothesis are not as a rule those in whose terms the hypothesis could ultimately be validated.

(4) *Reduction*: Thus, a distinction must be drawn between hypothesis-formation and hypothesis-justification. The former we have called retroduction, because it involves moving "backwards" logically speaking, i.e., groping for an approximate antecedent from which the datum can be derived. But how is an hypothesis to be justified? At the first level, it is "justified" if the original datum can be derived from it. If this were all that were to be said, then retroduction would be in the same position as the other two forms, because the procedure of discovery would also serve as the procedure of justification. What is crucial about hypothesis, however, is that it is *not* simply justified in this simple "q, p implies q, $\therefore$ probably p" way. In practice, what is done is to derive a great many *new* predictions from p, as much different in kind from q as possible. These are then tested; to the extent that they are verified, the hypothesis is progressively validated. It will be noted that the essence of this validation is not expressed by the "q, p implies q, $\therefore$ probably p" formula (which gives only a precondition for p to qualify *as* an hypothesis). Rather it is: "p implies r, s, t . . ., and r, s, t . . ., $\therefore$ probably p," where r, s, t are as different from the original datum, q, and from one another as possible, and where they have been carefully tested.

This "hypothetico-deductive (HD) method" (as we called it in Section 2) is clearly a method of validation, not a method of discovery. It is a proper *method*, that is, we are told what to do when we have an hypothesis to justify. Though it is the commonest method of validating hypotheses, it is not, however, the only one. Sometimes, an appeal will be made to some intrinsic quality of the theory, or to the way it unifies the domain without any *ad hoc* assumptions. Copernicus' theory could not predict any better than Ptolemy's could (not originally, at least), but it "explained" something the earlier theory had had to leave as an extraordinary coincidence, namely the fact that *each* of the planets had as one of its two periods of epicyclic rotation a period of exactly one year. Validation in this most famous case of scientific revolution was thus not of a HD type. But much more often, the validation of an hypothesis *will* follow the HD model exactly.

[9] This is not altogether exact, but it is adequate as a first approximation.

Thus if we are giving a scheme of types of inference, we will have three types: deduction, induction, retroduction (if inference he thought of as *discovery* of a conclusion), or deduction, induction, HD validation (if inference be considered as *justification* of a conclusion). To have a convenient name that harmonizes with the others, let us call HD validation "reduction." To "reduce" an hypothesis is thus to validate it, using HD procedures.

4. Is There a Well-Defined Scientific Method?

Now we can return to the question posed in the title, armed with the distinctions that will allow us to answer it. Scientific enquiry contains a number of different well defined methods. Scientists make extensive use of deduction and formal mathematical procedures, which are quite automatic in their operations. They also use induction in the formulation of empirical correlations, and this, as we have seen, is a relatively well defined procedure, though not entirely formal, as deduction is. They use reduction in validating hypotheses; again, this is a reasonably well defined operation, though of course it will take the insight of the trained mind to see which deductions and which unifications are likely to carry the most weight as validation.

It is only when we leave aside the question of validation and ask about discovery, and specifically about the discovery of hypotheses, that the answer becomes negative. As we have seen, retroduction is by no means a blind leap, and yet it cannot be said to be guided by formal rules or described by formal schemas, in even the widest sense of the term, "formal." There is no "logic" of hypothesis-discovery, not at least if the discovery is one of any significance. The claim that there is such a logic has usually rested on a confusion between retroduction (which has no "logic") and reduction (which *does* have a "logic" of sorts).

One final word is in order. The scientist makes use of a whole array of other procedures, differing from one science to another, most of them connected with experimental technique. Take, for example, Mill's famous "canons of method," mentioned in section 2. They have not been included in the discussion above, and for a very good reason, since they are not primarily "inferential," in the sense in which we have used that term. They were directed to the *selection* of significant parameters prior to the formation of an experimental generalization or of an hypothesis. Though they are often described as "canons of inductive method," it might be more accurate to regard them as pre-inductive (or less often, preretroductive). The experimental situation is a complex one, involving many different parameters. The problem always is to pick out the ones that are significant for one's purposes, and prevent variations in the others from spoiling one's analysis. Before we can make an induction or a retroduction, we must first isolate the parameters that are likely to yield significant results. It is this feature of enquiry that Mill wished to emphasize, but he did it in a somewhat maladroit way by describing the selection as "inference" and by suggesting that it was quasi-formal in character. The use of Mill's canons would, of course, involve induction or retroduction insofar as the formulation of a law or a theory resulted. But their emphasis was *not* on the way in which one got from evidence to law or theory; rather it was on the sorts of clue that could lead one to the significant parameters in the first place.

Likewise, one could point out various other procedures that are integral to scientific research: construction of apparatus, measurement, classification. . . . Some are better defined than others, but none can be completely formalized. There is always the stubborn complexity and resistance of the material order, summarized in "Murphy's Law" of experiment: if anything can go wrong, it will. But when people ask "Is there a well defined scientific method?" they are not thinking of these contingencies. They are thinking of *inference*. And our answer to them, in short, is: *justification*-inference follows relatively well defined patterns in empirical science, though it is never *completely* well defined (i.e., completely formal). Whereas discovery-inference is guided by well defined rules only where empirical correlations are concerned, and even there, the guidance is not a coercive one. In the far more significant area of hypothesis and theory, "methods" of discovery are tentative and extremely limited in scope. This is where genius is needed, where the incommunicable creativity of the talented individual sparks the gap. For genius is precisely the ability to stray from the well defined pathways and to find something that no amount of methodic path following would ever have revealed.

Is There a Well Defined Scientific Method?

A Philosopher's Answer

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ABSTRACT — The question "Is There A Well Defined Scientific Method?" can not be answered without taking into account the varying aims of scientific inquiry as conceived historically as well as within the framework of various sciences. The term "method" is also subject to ambiguity. The answer would seem to be negative, if we mean that there is a fixed set of well-established rules which if followed will lead to fruitful scientific results. It is positive, if we mean that science has developed fairly reliable patterns and criteria for acceptable explanatory laws and theories, experimental design, and observational confirmation, that are part of the program if not the practice of scientists at the present time.

The question prompts another question that I am, in part, inclined to suppress, namely: By what method should one try to answer the question?

Philosophers who write about science and scientific method disagree on whether or not the answer can be found by reading histories of science or historical documents, or by watching scientists at work or questioning them, or by a "rational reconstruction" of the logic of the written works of scientists — or perhaps by all of these plus an ingredient of the philosopher's own intuition. Even when one or the other of these approaches is explicitly made, a philosopher reading the finished work will find disagreement with the correct analysis of scientific method.

Scientists in their practice of science and in their published scientific reports do not generally state their rules of procedure, except as technical recipes. A philosopher also does not always make clear whether he is concerned with methods that scientists here and now do use, or agree upon, or whether he is concerned with an ideal logical "model" of the essential criteria of the methods, or for the right to have confidence in such methods. My own view is that it is actual scientific practice, within the framework of an historical period, as far as this can be isolated, with which a philosopher ought to be concerned. Otherwise, it is logic, or an ideal program of what an ideally valid science should be.

If science is to be defined, or partially defined, by its methods, it is perhaps possible at least to agree that a method is a set of rules and procedures, either stated or implicitly used, that can be deliberately followed and its value tested in terms of its results. The results will be a function of the aims. Perhaps, indeed, several methods will reach the same results equally adequately. So much

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