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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science and scientific method disagree on whether or not the answer can be found by reading histories of science or historical documents, or by watching scientists at work or questioning them, or by a “rational reconstruction” of the logic of the written works of scientists—or perhaps by all of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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for a definition of "method." Scientific method, then, is the method or methods used to achieve the particular aims regarded as scientific. The success of the method would be defined in terms of its achievement of the aims.

What do philosophers of science see as the method of science? Philosophers who analyze science are often gazing at a Rorschach inkblot and projecting into it their own private intellectual wishes. One sees in it a game, another an instrument, or a building erected above a swamp; others see in it half-closed zippers, nets, or maps, or an evolutionary tree. From these metaphors, they may move on to law-like sentences, quasi-deductive systems, hypothetico-deductive-inductive systems, pure and impure deductive systems, model-languages, models, basic statements, observation statements, and even sense-data. By this time a metala-language has been constructed in such a way that it is difficult often to see its bearing upon the less esoteric and abstract features of actual science.

Hempel and Oppenheimer (The Logic of Scientific Explanation) say that "a view will be rejected because it does not seem to accord with the meaning customarily assigned to the concept in science and methodological inquiry." Yet, we are still stuck with philosopher's "views": Nagel's views, Carnap's views, Ramsey's views, Braithwaite's, Popper's, et al. Perhaps the best we can hope for is some kind of statistical or evidential convergence so that it will become more probable that a "view" is correct if it is agreed upon by the largest number of competent analysts, or observers.

I shall illustrate by an example from Popper (The Logic of Scientific Discovery). "Science does not rest on rock-bottom. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or given base; and when we cease our attempts to drive our piles into a deeper layer it is not because we have reached firm ground. We simply stop when we are satisfied that they are firm enough to carry the structure for the time being." But Braithwaite tells us, "The peaks of science may appear to be floating in the clouds but their foundations are in the hard facts of experience." Popper tells us, "Just as chess might be defined by the rules proper to it, so empirical science might be defined by means of methodological rules. First a supreme rule is laid down which serves as a kind of norm for deciding on the remaining rules; . . . methodological rules are regarded as conventions. They might be described as the rules of the game of empirical science. An example of a rule is as follows: The game of science is in principle without end."

Science has been defined historically not so much in terms of rules of methodology, as in terms of the aims of science. Methods of science follow upon the aims. The most general aim of science has always been to "understand nature." But what counts as "understanding" has differed, with respect to emphasis on what needs to be understood, as well as the best, or most rational procedure for understanding it. Stephen Toulmin (Foresight and Understanding), has pointed out that the pre-Socratics were concerned with theoretical possibilities of giving a rational account of the multiplicity of nature. Their methods were attuned to this goal and therefore did not involve testable empirical hypotheses, or attempts at prediction, or even attempts to formulate laws. Rather, they were exploring the logic of scientific explanation and understanding. To explain change scientifically, for example, would involve relating the changing to something that did not change, thus setting up a "model" for the search for invariants in nature. Forecasting, alone, was not regarded as definitive in giving an "intelligible account of" nature. It was only much later that the idea of giving an account of the success of forecasting techniques began to enter into science as one of its additional aims. Early philosopher-scientists, then, were concerned with exploring patterns of understanding and of explaining nature. Let me take an example: The annual flooding of the Nile was a problem that aroused interest, practical and theoretical. Herodotus (The Persian Wars), in his attempt at explanation, certainly combined appeal to existing knowledge, as well as to the rejection of Thales's theory on the grounds that the Nile flooded in the absence of winds as well as in their presence, and to the observation that other rivers should behave similarly but did not. Observation, as well as the search for a theory that was logically consistent with other information already known, guided his account. A theory to be scientific must be capable of disproof, or falsifiable; it cannot be "obscure," according to Herodotus. Forecasting, however, had already been mastered. There was no question of predicting when the Nile would rise. The criteria of the scientific adequacy of his explanation, while not explicitly stated, were implied in his rejection of previous theories: They contradicted the evidence from observation.

Lucretius (The Nature of the Universe) stated explicitly in explaining the attraction of iron to the magnet, that "In matters of this sort it is necessary to establish a number of facts before you can offer an explanation of them" (on the action of the magnet). The "facts" Lucretius offered consisted of his theory of the particulate nature of matter, empty space, and some gross observations of the properties of metals.

Within physics, the analysis of the motion of bodies and its explanations has provided one of the longest traditions in the history of science. Galileo (On Falling Bodies) exemplifies the method of discrediting and rejecting one method of explanation, and establishing and defending another. Conceptual analysis, experimentation — actual or imagined—and the use of mathematics are prominent in Naturally Accelerated and Projectile Motion. Such a model became the exemplar for many subsequent scientific investigations. It is not erroneous to say that Newton (The Rules of Reasoning in Philosophy*) provided the culmination of this model in a mathematical deductive model, taking principles and laws as basic pos-

1. The rule of parsimony or simplicity.
2. The rule of same cause, same effect.
3. The whole-part rule.
4. The rule of confidence in induction until future experience renders the induction questionable or establishes it more accurately.
tulates. Newton’s *New Theory About Light and Colors* exemplifies experimental method; and Huygen’s *Treatise on Light* shows the influence of the introduction of hypothetical entities (“waves” of light).

If the question were directed to the Newtonian model, and Newtonian aims, the answer, based upon the *Principia* would have to be yes for that aspect and phase of physics. The method as Newton saw it was well defined. The aim was to give an axiomatic deductive systematic account of the motions of bodies.

We now find the following statement made with respect to theoretical physics: “The purpose of theoretical physical science is to postulate a conceptual model of nature from which the observed behavior may be predicted quantitatively.” (Scientific Monthly, Vol. 81, No. 1, July 1955.) “The method is (i) postulate a model based on existing experimental measurements; (ii) check the predictions of this model against further measurements; and (iii) adjust or replace the model as required by new measurements. – No claim is made about the “reality” of the model; the sole criterion is successful prediction from the simplest or most convenient or most satisfying model.” (Marshall J. Walker, “An Orientation Toward Modern Physical Theory”). It is Walker’s view that this method is the result of a continuous, cumulative and corrected, scientific process. The creative work of scientists is in the postulation of the new model and in the ingenuity needed to test it. “Understanding a model” is defined as regarding it as a special case of a more general model.

To summarize: Predictive success, while counting as one of the aims of scientific inquiry, exists alongside other aims, among these to give an explanation of the predictive success of a model or theory. There is no universal recipe or set of methods that is valid for all science at all times because the aims of science vary, and because science is in part creative and inventive as well as developmental. Speculative imagination, logical techniques, computational skill and invention, controlled observation, an intuitive “sense” of evidence, and new conceptual ideas and ideals, all figure in the methods of scientific inquiry. Predictive success is only one of the aims of model, or theory construction; it is only one of the criteria of “rational understanding.” What is taken to be “rational” and “natural” may change, as well as the aims of science. New aims may generate new methods both of discovery as well as of explanation. New techniques—logical, computational, experimental—may generate new demands upon acceptance and rejection of scientific aims.

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**Is There a Well Defined Scientific Method?**

*A Physicist’s Answer*

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**ABSTRACT** — The traditional view of the scientific method is an oversimplification that ignores the vagaries of the creative process. Several examples that indicate the method is not infallible are explored. The reasons why the misconception is so widespread are discussed.

From the viewpoint of a physicist, the answer to the question “Is there a well defined scientific method?” would be “No.” Before justifying this answer, however, we must define what we are saying “no” to. Many elementary science texts carry on at great length about the “scientific method” as an objective, foolproof procedure for proceeding from ignorance to knowledge. The “Method” allows the scientist to proceed from observation to hypothesis to prediction to another observation, in a never-ending spiral of progress.

Certainly, in the broadest sense, this is the way by which scientists make discoveries. In fact, for a simple problem (such as diagnosing the trouble in a faulty piece of laboratory equipment) it may work quite well. But a scientist trying to make a significant discovery finds that the real world is just too complicated, and that progress is neither as objective nor as guaranteed as the “method” would supposedly make it.

One can easily think of six reasons why the “method” is an oversimplification.

1. The key observation may result from an accident. This breaks the link between prediction and observation. Any practicing physicist knows that an experiment is often performed, not to verify some prediction, but because it seems like an easy one to do or because the apparatus is available. In other cases, the interesting result may be the unexpected byproduct of some other work.

   A famous example is the discovery of electron diffraction by Davisson and Germer (1927). To quote their paper:

   The investigation reported in this paper was begun as the result of an accident which occurred in this laboratory...