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Science Reason and Reality

Issues in the Philosophy of Science

Daniel Rothbart

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序

科学哲学是一门既古老又年轻的学科。古老,是因为柏拉图、亚里士多德对知识以及对当时作为知识典范的数学的考察所引发的许多形而上学和认识论问题仍然是今天科学哲学要面对的基本问题;年轻,是因为被公认为科学哲学学科的创始学派的逻辑经验主义的建立和消亡就发生在刚刚过去的那个世纪。

科学哲学曾在20世纪的哲学革命和运动中占据过非常核心的位置,现在也仍然是哲学的一个主要分支学科。这部分地是由于科学在当代人类社会中的重要性,部分地是由于科学哲学所涉及的基本问题也是整个哲学所要涉及的一些基本问题。

科学哲学是一门试图以哲学的方式回答科学是什么这个问题的学科。科学哲学对科学的对象、形式和方法作哲学的考察,并试图回答由此引起的形而上学的、认识论的和方法论的问题。实际上,科学和哲学具有相同的起源,都起源于人类试图以理性的、批判的和概念的方式理解和把握世界(实在)的努力。科学与理性、科学与实在的关系自然就成为科学哲学的基本问题。科学是理性的,这意味着科学假说和理论的选择和辩护都要依据理性的程序和规则;而这些理性的程序和规则的目的则是为了尽可能的使科学假说和理论所表达的是关于世界或实在的真理。丹尼尔·洛斯巴特编的这本科学哲学读本以《科学、理性与实在》为书名,可以说是很好地捉住了科学哲学的主题,当然也是很好地反映了本书的内容。以理性和实在为主线,本书分为五个主题:一、实验证据的特性及其在理论评估中的作用;二、关于科学说明的对立观念;三、库恩的范式主导的科学观;四、对科学的相对主义批判;五、实在论与反实在论的争论。本书所选的这五个主题涵盖了历史上的和当代的科学哲学所涉及的基本课题,兼顾经典和当代文献,每个主题前都附有清晰浅显的导言,对于初次接触科学哲学的读者把握所选文献很有帮助。

本书适用的读者面应该是相当广泛的。首先,本书非常适合作为科学技术哲学专业研究生科学哲学课程的教材,以本书为核心组织学生的阅读和讨论;其次,本书也很适合作为哲学其他专业的人士了解科学哲学的基本读物;第三,本书对于那些对科学技术的哲学问题有兴趣,但又缺乏足够的时间或背景去啃大部头著作的广大文理科学生和教师来说,也是一本可以经常翻阅的读物;第四,本书还为一切愿意思想的人们提供了足够丰富的原料。

孙永平

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PREFACE

This anthology was motivated from the belief that students with no previous training in philosophy can actively engage in a serious discussion of the philosophical basis of science. Once students realize that all scientific knowledge rests on various philosophical assumptions, certain questions can be raised. What exactly are the conditions for acquiring scientific knowledge? How can we be reasonably confident that the scientists' pronouncements of success, failure, or indeed, any pronouncements at all are, in fact, warranted? What are the "objective" indications of scientific progress?

The articles of this anthology were selected and organized with certain goals in mind. First, an anthology in philosophy of science should focus on some of the central philosophical topics of the day, as discussed in the current body of philosophical literature. Of course, this goal must be balanced against limitations of length. To fulfill this goal, the present anthology is divided into the following five topics: the character of experimental evidence and its role in the appraisal of theories (Topic I), rival conceptions of a scientific explanation (Topic II), the Kuhnian conception of paradigm-driven science (Topic III), the relativist critiques of science (Topic IV), and the realist/antirealist debates concerning the possibility of a theory to refer to real-world processes (Topic V). In contrast to some anthologies on the market, the realism/antirealism controversy is prominent in the present work, reflecting the centrality of this issue in the current discussion in philosophy of science. Furthermore, the relativist critiques of science comprise a unique topic of the anthology, including selections from Bruno Latour, as well as Barry Barnes and David Bloor. Such critiques are particularly stimulating to students.

Second, the sequence of articles in each topic reflects a diversity of philosophical opinion. The present anthology avoids the problem of presenting an artificially narrow range of philosophical opinion. The selection of articles shows students how each position is subject to constructive criticism, and how some criticism motivates alternative positions.

Third, an anthology should highlight the work of the most prominent and influential scholars in the field. The present collection contains classic articles by Carnap, Hanson, Hempel, Hume, Lakatos, Popper and Kuhn (the selection from Kuhn's work includes three chapters from his *The Structure of Scientific Revolution* and one chapter from *The Essential Tension*). The anthology includes as well work by the participants currently engaged in the philosophical debate. I suspect that some of these articles may become classics for a later generation. At the end of each topic is a fairly extensive bibliography designed to assist those students wishing to pursue particular issues further.

Fourth, an anthology should be accessible to undergraduate students with no previous training in philosophy. The present anthology avoids the excessively technical terminology and highly rigorous arguments common to advanced discussions. Some selections are reprints of chapters from the author's own introductory text, such as work by Carnap, Hanson, Hempel, and Harré. Most of the other selections are equally comprehensible. A few articles may necessitate preparation by the instructor. But as a pedagogical aid, all selections are summarized briefly in the topic introductions.

The present anthology is intended as a primary text for an introductory course in philosophy of science. The instructor may supplement this work with extensive scientific writings from the natural sciences. From my experience, some writings from the modern period of science can be quite accessible to students. In another course this text could also function as a primary text for the theory of knowledge, or a course on the philosophical dimensions of science and religion.

Many people kindly offered extensive advice throughout the development of this project. My colleague Emmett Holman was particularly generous with his time and valuable suggestions. Considerable credit goes to the following reviewers for their detailed and constructive comments: Peter Achinstein, Johns Hopkins University; Michael Bishop, Iowa State University; Herbert Burhenn, The University of Tennessee at Chattanooga; Robert L. Causey, The University of Texas at Austin; Brian B. Clayton, Gonzaga University; Wayne Davis, Georgetown University; Malcolm R. Forster, University of Wisconsin-Madison; Richard Hassing, The Catholic University; John L. King, University of North Carolina at Greensboro; Hugh Lacey, Swarthmore College; Joseph LeFevre, Xavier University of Louisiana; Robert N. McCauley, Emory University; Alfred Nordmann, University of South Carolina; Bonnie Paller, California State University-Northridge; Thomas W. Platt, West Chester University; and Paul C. L. Tang, California State University-Long Beach. Many thanks go to the developmental editor, Diane Drexler, for her support and keen editorial judgment.

CONTENTS

THEORY AND OBSERVATION

INTRODUCTION

Imagine holding a diamond ring in one hand and a lead pencil in another. Contrary to expectation, scientists tell us that both the diamond and the "lead" from a pencil are composed of the same material—carbon atoms. The primary difference is the arrangement of these atoms.

We know that the apparent "disappearance" of a rabbit during a magic act is a form of trickery. But, according to astrophysicists, if any relatively small object, such as a rabbit, comes very close to a certain region of space, known as a black hole, the object eventually disappears from our universe without a trace. No one really knows what happens to it.

Why should we believe that the scientists are correct in these cases which seem to violate common sense? Exactly how is scientific understanding of nature ever possible, given the many prejudices, biases, and limitations that afflict human beings? Can scientists demonstrate that their ideas sometimes reveal the truths of nature? Absolutely not, according to some critics of science. The scientists' declarations that they have discovered truths about the world border on self-serving nonsense, closer to fraud than to fact. On this view the preference for astronomy over astrology, for example, merely reflects the propaganda victories of oppressive social institutions.

The debate concerning the possibility of genuine scientific knowledge of nature is the primary issue in the philosophy of science. What can scientists do to show that their beliefs are right? Of course, a major activity designed to demonstrate the truth of a certain belief is the performance of scientific experiments. Experiments give scientists "reliable" evidence upon which beliefs (or dis-beliefs) are founded. Scientists typically use the experimental data to justify (demonstrate, confirm, or corroborate) their beliefs about events that they have not yet observed, such as events in the future.

This goal of an experiment raises three important philosophical questions: First, *exactly how do scientists use evidence from an experiment about observed events to acquire information about unobserved events?* Scientists frequently declare that data from past experiments can be used to predict the future. But are they right? At stake here is nothing less than the capacity of scientists to justify their beliefs about the world. This important question is addressed in articles by David Hume, Rudolf Carnap, and Karl Popper.

Second, *what exactly is the character of observation reports arising from an experiment?* The whole purpose of an experiment is to provide a reliable test for determining whether or not a certain belief is correct. If the test is performed properly, the experimental data should be correct, or so one would hope. But why should anyone, especially nonscientists, believe that such data are correct? This issue is addressed in the article by Norwood Russell Hanson.

Third, scientists typically convey their beliefs about the universe through scientific theories. *What exactly is the character of a scientific theory?* This is addressed in articles by Hilary Putnam and Frederick Suppe.

SUMMARY OF READINGS

Let us begin with the first major question of Topic I: Exactly how can scientists use evidence from an experiment about observed events to acquire information about unobserved events? As David Hume argued in the eighteenth century (selection #1), there are no rational grounds for using evidence from observed phenomena to draw out information about events which have not been observed. No principle of logic or doctrine of reasoning of any kind allows us to infer knowledge of unobserved events from observed events. Hume recognizes that we strongly expect the pattern of events to remain the same, based on our experience that events of one type are constantly conjoined with events of another type. For example, I have acquired an expectation from past experience that the floor in my classroom will hold the weight of my body. This expectation grows with the frequency of such experience, leading to a belief in a cause-and-effect relationship between such events. Nevertheless, we are never warranted rationally in believing that the pattern of past events will be sustained in the future. The very possibility of rational science is at stake. If Hume is correct, we can never distinguish objective facts from psychological habit, or, according to some commentators, never distinguish sanity from insanity. How can Hume's skepticism be addressed in ways that save rational science?

Rudolf Carnap attempts to overcome Hume's challenge by appeal to the method of scientific experimentation (selection #2). For Carnap the scientific method is identical to the method of evaluating scientific ideas through the use of experimental tests. When certain beliefs are subjected to tests, two results are possible: The experimental evidence either violates or it supports the beliefs. When a violation occurs, the actual evidence contradicts what one would expect from these beliefs. In such a case scientists can reasonably claim that the ideas are inaccurate. This of course assumes that the experiment was properly designed and performed. For Carnap, when beliefs pass a sufficient number and variety of tests, the beliefs are confirmed. This confirmation never establishes the absolute certainty, because *we can never compile enough evidence to show that a scientific belief is correct for all the events it describes.* The most we can say is that the belief is very likely to be true on the basis of extensive evidence. Such evidence increases our confidence that the belief correctly describes future events, though we never achieve complete certainty.

For Karl Popper, however, Carnap's entire experimental methodology collapses (selection #3). If we follow Carnap's proposal to use experiments to predict future events, we are again confronted with the Humean obstacle. Carnap's method of experimentation cannot yield knowledge of the events we have not observed. In a very controversial proposal Popper flatly rejects all procedures which try to use evidence on behalf of a statement's truth, or even its probability. Popper's rejection is rather stunning: *Any attempt to use evidence positively to demonstrate the truth or probability of a universal statement about nature is doomed from the outset.* No matter how many tests such a statement passes, our level of confidence that the statement is true is always nil, from a rational standpoint. We can easily show that a general statement is false by discovering one (repeatable) event which violates the statement, but the chances that the statement is true never improve with further testing. The Popperian conception of rational science stresses the following: A theory can be easily dismissed, but it can never be confirmed, not even partially.

So, in rational science a theory should be subjected to severe experimental scrutiny in order to expose its weakest point. To this end, Popper advocates the following methodological principles:

(1) The best indication of a theory's success, prior to any experience, is not the number of possible events a theory can explain, but is rather the number of situations that are prohibited by the theory.
(2) Every genuinely scientific theory, as opposed to a theory of pseudoscience, must show how one type of event is prohibited.
(3) A legitimate experimental test must subject the theory to the severest possible scrutiny to exploit its "weakest point" in an effort to efficiently falsify the theory.
(4) No matter how many tests a theory passes, it is at best tentatively accepted and awaits future scrutiny.

Carnap's positive method and Popper's falsification method represent rival conceptions of rational science. As Imre Lakatos argues (selection #4), any viable methodology serves two vital functions. First, a methodology provides rules for the acceptance and rejection of theories, or what he calls research programs. Second, a methodology provides normative guidelines for the interpretation of the history of science. Rather than a collection of neutral facts, the historical study of science is driven by its own "rules for discovery" toward a rational reconstruction of the major episodes of scientific progress. For example, a positive methodology will direct the historian to the factual evidence associated with a scientific discovery, while a falsifying methodology centers on historical cases of bold conjectures and severe challenges through rigorous tests.

The second major question of Topic I is as follows: What exactly is the character of observation reports arising from an experiment? According to both Carnap and Popper, our ability to test a theory rests on discovering the "facts" of nature, and expressing "facts" through the use of observation reports. But can we ever remove all theoretical influences from such reports? According to Norwood Russell Hanson, such reports are inescapably dependent on theoretical beliefs (selection #5). For example, when looking through an X-ray tube, would a physicist see the same thing as a baby? Of course not, says Hanson. Seeing requires not only visual perception but also theoretical understanding. The infant simply cannot organize the visual landscape in the same way that a trained physicist can. For Hanson, there is more to seeing than meets the eyeball.

Obviously, the primary purpose of an experiment is to determine whether to accept a theory. But what exactly is a scientific theory? This is the third important question of Topic I. One attempt to answer this question is given by the "Received View" conception of a theory. According to this conception, a theory is an organized description of an infinite number of possible events. For example, Newton's theory of motion presumably gives information about any physical body moving in space or on earth. But there is no way to provide descriptions of all such bodies in motion, because the list would be literally endless. A more efficient method would require the use of laws of nature. A law of nature is a statement which conveys information about an infinite number of events, only some of which can be observed. For example, Newton's theory includes the first law of motion: Every physical body continues in a state of rest, or a state of uniform motion, unless compelled to change its state by an external force. We can easily apply this law to the following simple situation: A stationary billiard ball on a polished tile floor remains at rest unless it is subjected to some external force, such as bombardment by another ball. All the laws of Newton's theory should be organized into a logical system. Consequently, from some laws we can logically infer other laws, and from these still other laws can be inferred, and so on. In this way all of the laws in Newton's theory are organized very well.

The "Received View" conception of a theory assumes a definite distinction between a theoretical statement and an observational statement. Many laws of nature are theoretical statements, because they include "theoretical terms." For example, Newton's first law of motion (above) includes the theoretical term *force*. But when we apply a law to a specific situation that we observe, we must come up with observation statements. Newton's first law presumably describes in observational terms how the billiard ball will move under specific circumstances. Such descriptions provide an interpretation of the law in a specific context of application.

However, Hilary Putnam rejects the "Received View" conception (selection #6). He argues that the "Received View" rests on a bogus distinction between theoretical statements and observation statements. The primary culprit is the notion of an observation term. If "observation term" is defined as a term that always refers to observable things, then these terms simply do not arise in the scientific literature. Putnam concludes that the distinction between observational term and theoretical term cannot be drawn in a way that salvages the "Received View."

What is a viable alternative to the "Received View"? According to Frederick Suppe, the "Received View" fails to capture an important function of scientific theories (selection #7). A theory does not tell us what actually occurred or what will occur in the world, but how phenomena *would behave* under certain idealized conditions. A theory of gravitation, for example, tells us how a material object would fall, assuming no wind resistance, atmospheric interference, and so on. Of course, such conditions cannot be realized outside of an experimental test. Again, a theory should inform us what would happen under ideal circumstances. Suppe argues that such information is best provided by abstract "pictures" of phenomena in idealized settings. Consequently, such "pictures" comprise the central content of every scientific theory.

DAVID HUME

SCEPTICAL DOUBTS CONCERNING THE OPERATIONS OF THE UNDERSTANDING

PART I

20 All the objects of human reason or enquiry may naturally be divided into two kinds, to wit, *Relations of Ideas,* and *Matters of Fact.* Of the first kind are the sciences of Geometry, Algebra, and Arithmetic; and in short, every affirmation which is either intuitively or demonstratively certain. *That the square of the hypothenuse is equal to the square of the two sides,* is a proposition which expresses a relation between these figures. *That three times five is equal to the half of thirty,* expresses a relation between these numbers. Propositions of this kind are discoverable by the mere operation of thought, without dependence on what is anywhere existent in the universe. Though there never were a circle or triangle in nature, the truths demonstrated by Euclid would for ever retain their certainty and evidence.

21 Matters of fact, which are the second objects of human reason, are not ascertained in the same manner; nor is our evidence of their truth, however great, of a like nature with the foregoing. The contrary of every matter of fact is still possible; because it can never imply a contradiction, and is conceived by the mind with the same facility and distinctness, as if ever so comformable to reality. *That the sun will not rise to-morrow* is no less intelligible a proposition, and implies no more contradiction, than the affirmation, *that it will rise.* We should in vain, therefore, attempt to demonstrate its falsehood. Were it demonstratively false, it would imply a contradiction, and could never be distinctly conceived by the mind.

It may, therefore, be a subject worthy of curiosity, to enquire what is the nature of that evidence which assures us of any real existence and matter of fact, beyond the present testimony of our senses, or the records of our memory. This part of philosophy, it is observable, has been little cultivated, either by the ancients or moderns; and therefore our doubts and errors, in the prosecution of so important an enquiry, may be the more excusable; while we march through such difficult paths without any guide or direction. They may even prove useful, by exciting curiosity, and destroying that implicit faith and security, which is the bane of all reasoning and free enquiry. The discovery of defects in the common philosophy, if any such there be, will not, I presume, be a discouragement, but rather an incitement, as is usual, to attempt something more full and satisfactory than has yet been proposed to the public.

22 All reasonings concerning matter of fact seem to be founded on the relation of *Cause and Effect.* By means of that relation alone we can go beyond the evidence of our memory and senses. If you were to ask a man, why he believes any matter of fact, which is absent; for instance, that his friend is in the country, or in France; he would give you a reason; and this reason would be some other fact; as a letter received from him, or the knowledge of his former resolutions and promises. A man finding a watch or any other machine in a desert island, would conclude that there had once been men in that island. All our reasonings concerning fact are of the