Review
Reviewed Work(s): Scientific Discovery: Case Studies by Thomas Nickles
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Source: *Philosophy of Science*, Vol. 49, No. 1 (Mar., 1982), pp. 138-140
Published by: The University of Chicago Press on behalf of the Philosophy of Science Association
Stable URL: https://www.jstor.org/stable/186886
Accessed: 08-06-2021 11:06 UTC

BOOK REVIEWS

THOMAS NICKLES (editor), ***Scientific Discovery: Case Studies.*** Dordrecht: Reidel (1980), xxv + 379 pp., cloth $36.50, paper $15.95.

After a long absence, scientific discovery is making a comeback as a subject for philosophical investigation. Rejecting the positivists' view that discovery should be consigned to the behavioral sciences, a number of philosophers concerned with the history of science are insisting that discovery is 'epistemologically interesting and important' (xiii). In their view, history and philosophy of science are not separate undertakings and the philosopher can contribute to our understanding of discovery no less than the psychologist or the historian. But combining history and philosophy, however intriguing and necessary, is an exceptionally difficult task. While the intricacies of history tend to overwhelm the philosophy, philosophical analysis tends to oversimplify the historical record.

This volume, which consists of essays from a conference held at the University of Nevada at Reno in 1978, aims to extricate discovery from the limbo to which the positivists relegated it. With the exception of a paper by Marx Wartofsky on 'Scientific Judgment' and a panel discussion on 'The Rational Explanation of Historical Discoveries', the book is devoted to discussions of particular cases of scientific research and to attempts to establish general points about scientific inquiry by reference to specific historical studies. Unsurprisingly since the contributors include historians, psychologists and philosophers, the ways in which these topics are treated vary markedly in both content and general approach. There are discussions of familiar topics such as Copernicus's and Darwin's discoveries and of more esoteric issues such as Burnet's discovery of the clonal selection theory of acquired immunity and Heitler and London's quantum mechanical explanation of the chemical bond. There is an attempt to apply a general psychological theory to a specific historical case. And there is an interesting diversion in the panel discussion concerning Robert Westman's psychoanalytic explanation of why Rheticus became a Copernican. Less clear is why there should be these alternatives to the positivist's approach, or indeed why an alternative to it is needed.

Some readers of *Scientific Discovery* will be disappointed because many of the essays deal with specific discoveries rather than with the phenomenon of discovery itself. The alleged epistemological importance of discovery is often difficult to discern, and the considerations adduced by some contributors have more to do with "justification" than with "discovery". Although the views of Reichenbach, Popper and other philosophers who insist on a sharp distinction between discovery and justification are not overlooked, for the most part they serve as an excuse for examining other issues instead of being the focus for analysis one would expect. Moreover there are lapses: for instance, one participant in the panel discussion observes that Popperian scientists could in principle work in isolation, whereas Popper himself has emphasized the importance of the social nature of science. And it is unfortunate that some of the more straightforwardly historical studies fall short of the high standards set by recent work in the history of science, a result perhaps of a misguided attempt to curry favor with philosophers concerned with wide-ranging issues.

The interest of the collection lies in its illustrating both the pitfalls and the virtues of the "historical turn" in the philosophy of science. A long paper by William Wimsatt on reductionistic biases in population biology is an important contribution to the literature on reduction and research strategies in recent biological theory. David Bantz's paper on the evolution of structural accounts of chemical bonding develops the interesting point that orthodox philosophical theories of science cannot explain why a theory such as Heitler and London's was seen not as routine but as a major contribution to science. And papers by Rachel Laudan and Hank Frankel on different aspects of the development of global plate tectonics reveal in different ways the importance of examining the methodological issues associated with sciences in the making or undergoing rapid change.

Philosophical issues concerning discovery are more central to the contributions of Lin-

dley Darden, Michael Ruse and Kenneth Schaffner. Darden points out the role of analogies and 'interfield connections' in attempts, from Darwin to Morgan, to construct a theory of heredity. She claims that discovery must be seen as an on-going process and that interfield connections (i.e., scientific hypotheses about the relationships between entities or processes in neighboring fields) are a better source of new ideas than analogies. Both points seem right, but it is questionable whether the distinction between analogies and interfield connections is as clear as Darden supposes and, more importantly, whether those who advocate 'concentrating on justification to the exclusion of discovery' (151) would deny that discovery in Darden's sense is an on-going process. Michael Ruse, in a nicely argued paper, isolates several aspects of Darwin's theory that cannot be properly understood without knowing how Darwin developed the theory. According to Ruse, Darwin's route to the theory had an important bearing on his choice of language, on the role artificial selection plays in the *Origin* and on Darwin's views concerning sexual selection. One difficulty here stems from Ruse's distinction between the private aspect of science which is affected by the way discoveries are made and the public aspect of it which is not. Ruse claims that 'when it comes to understanding a completed theory, discovery is irrelevant' (135), but this sits poorly with his view that we cannot understand the language in which Darwin couched his theory nor why Darwin endorsed sexual selection (142) without examining his route to the theory. As Schaffner sees the matter, the positivists were wrong to rule out the possibility of a logic of discovery, while those like Hanson who believed in such a logic failed to appreciate that it comprises both a logic for the generation of new hypotheses and one for their preliminary evaluation. Schaffner's argument turns on the observation that computer programs have been developed which provide what a logic of generation is supposed to provide, namely 'heretofore unasserted conclusions' (179). However, various difficulties remain: first, as Nancy Maull points out in an incisive comment on Schaffner's paper, a logic of discovery of the kind envisioned by Schaffner makes no provision for the all-important process by which problems are generated and recognized as problems; second, Schaffner's conclusion is less strong than it might appear since he does not hold that ''justification'' is anything more than ''preliminary evaluation'' done in a more organized way (190).

The studies in this volume are likely to make one more rather than less skeptical about the existence of a distinctly philosophical problem of discovery. Some of the contributors note that the positivists thought that discovery was not a fit topic for philosophical scrutiny, but none consider why. The positivists certainly did not believe that discovery was unimportant or inscrutable. Their view was that philosophers should concern themselves with the logic of scientific language and leave discovery and other aspects of science to historians, psychologists and sociologists. The suggestion that the positivists believed that discovery must be passed over in silence misrepresents their position. Despite numerous remarks to the contrary, detailed examination of the ways in which scientists develop their ideas does not run foul of positivists methodology. Such studies are essentially empirical and should be judged with respect to the standards of the appropriate sciences.

This is not to say that scientific discovery is not epistemologically significant nor that there is nothing of interest to be said about discovery as such. Nevertheless, on the evidence of this volume the prospects for a general account of what several contributors refer to as 'the rationality of discovery' seem slight. Edward MacKinnon, in an interesting short paper on the origins of quantum mechanics, expresses the hope that 'underlying structures' may emerge from a 'quasi-phenomenological' study of particular discoveries (268) and Wartofsky appeals for a theory of deliberation encompassing scientific discovery. But discoveries as ''eureka events'' still seem best treated by psychologists, and discoveries as on-going processes still seem subjects for historical rather than philosophical analysis. Once we accept that discovery typically involves deliberation and that this can be isolated, described and analyzed, the need to postulate underlying structures to account for the historical evidence becomes moot. It is indeed a major—albeit unintended—contribution of this volume to show just how much can be achieved without assuming any such structures.

Besides the papers mentioned, *Scientific Discovery* includes papers by Bruce Wrightman on Copernicus's acceptance of terrestrial mobility, Bruce Moran on the organization of collective and observational projects at the court of Wilhelm IV of Hesse-Kassel, Howard

Gruber on Darwin's early thought from a systems-theoretic point of view, and William Scott on discovery in the physics of clouds and rain. The volume is well edited by Thomas Nickles, who also contributes a useful short introduction. *Andrew Lugg, University of Ottawa.*

WESLEY SALMON (editor), ***Hans Reichenbach: Logical Empiricist.*** Dordrecht: D. Reidel Publishing Company (1979). xi + 782 pp. $39.50

That Hans Reichenbach was never the subject of a volume in *The Library of Living Philosophers* was due only to ill-luck. A joint treatment of Reichenbach's and Carnap's philosophies was planned, but Reichenbach's death resulted in a volume devoted to Carnap's work alone. *Hans Reichenbach: Logical Empiricist* is not a substitute for that planned earlier work, and indeed does not pretend to contain a definitive assessment of his views. Rather, it demonstrates how stimulating Reichenbach's ideas can still be in providing guidelines for solutions to philosophical problems. The twenty-five essays in the collection span conventionalism, probability, space-time, quantum theory, induction, logic, causality, and explanation, and the range of topics covered means that almost any philosopher of science will find something of interest here. More important than mere scope, however, is the coherence exhibited by groups of articles on what initially seem to be disparate topics, and which are, by the nature of the subjects treated, often quite technical. The volume thus successfully avoids that all too common pitfall of *Festschriften* and memorial manuals—that they frequently result in dreadfully disorganized collections of solicited articles, leaving their readers with the impression that the philosopher concerned had resigned early from the Unity of Thought movement. The coherence of Reichenbach's views is a great aid here, and the seventy page introductory survey by Wesley Salmon is recommended for anyone unfamiliar with Reichenbach's thought. Within the confines of this review, I can only pick out some themes which recur in the book, concentrating in the main on causality and probability.

In his early works on space and time, Reichenbach had stressed the importance of causality, not only for arriving at a causal theory of time, but in showing how the principle of avoiding causal anomalies can be used to refute Kantian philosophies. Later, in his posthumously published *The Direction of Time,* he introduced a probabilistic theory of causality to arrive at a macroscopic theory of temporal ordering and the asymmetry of time. The notions used there serve as a starting point for Wesley Salmon's "Why ask 'Why?'?". In this piece (a slightly modified reprint of his 1978 APA Presidential Address), Salmon abandons his influential statistical-relevance model of explanation in favor of a model using probabilistic causal influences transmitted by processes. Although this view had been sketched in his "Theoretical Explanation" paper (in *Explanation,* S. Körner (ed.) Basil Blackwell, 1975), the insistence upon probabilistic causes underpinning explanations, aided by the introduction of interactive forks and causal interactions to complement the conjunctive forks used by Reichenbach in *The Direction of Time,* should enable the causal-relevance model of explanation to avoid most of the objections lodged against the earlier statistical model and its unsatisfactory "screening off" relation. There is much that is true in this account, and despite Salmon's acknowledgment in the introductory essay that Whitehead's metaphysics were an early influence on him until displaced by Reichenbach's 1947 APA address, we need not, I think, brace ourselves for a revival of process philosophy. There is, nevertheless, a problem with this approach which may well seriously limit its scope. Probabilistic explanations ought; of course, to apply to indeterministic phenomena, as Salmon himself has insisted. But if we require explanatory models to have spatio-temporally continuous processes which are capable of being marked, and use the common cause principle to argue for realism in explanations, then Reichenbach's Principle of Causal Anomaly will pose serious difficulties for Salmon's approach. This principle asserts that "an exhaustive interpretation of quantum mechanics is possible only if causal anomalies are admitted"—in more modern jargon: that a realistic interpretation of phenomena such as the spin or photon correlations appearing in Bell-type experiments will involve non-local causation. This principle is discussed in detail with examples in Roger Jones' article