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BLACKWELL, Richard J. (1969) *Discovery in the Physical Sciences*. Notre Dame, Ind.: University of Notre Dame Press. £4.05. Pp. xii+240.

Just two good things can be said about this book. It is clearly written. It deals with an interesting and rather neglected topic. Apart from that it is a sadly bungled affair.

The book sets out to clear the ground for, and in part develop, a theory of scientific discovery. The discussion begins promisingly enough with an outline of the traditional distinction between a method of discovery and a method of verification. Professor Blackwell points out that there is a long standing and rather confused debate about the question of whether it is possible to develop a methodology of scientific discovery, in addition to a methodology of verification. It is of course Blackwell's view—and here I am in full sympathy with him—that a methodology of discovery is a possibility.

After this initial promise, the book deteriorates rapidly. Chapter two draws a number of dubious and unhelpful distinctions, and succeeds only in muddying up what little clarity has been achieved so far. To take two examples. Blackwell spends much time on distinguishing factual and explanatory theories, without considering that this distinction may simply be one of degree, and of one's point of view. It is interesting to note that Blackwell cites Copernicus' theory as an obvious example of an explanatory theory, and then, without a tremor, gives us Kepler's laws as constituting an obvious example of a factual theory.

Again, Blackwell is at pains to contrast an approach to science through *discovery* with an approach through the Hempel hypothetico-deductive account of *explanation*. This somewhat puzzling distinction then takes the place of the earlier and much more helpful distinction between discovery and *verification*.

Chapter three distinguishes four different approaches to the problem of developing a theory of scientific discovery, namely the approaches of logic, psychology, history and epistemology. Under the first three headings we get little more than the barest outline of work done by Hanson, Koestler and Kuhn respectively. About the fourth, the epistemological approach, we are told that it 'examines discovery from the point of view of meaning-content of the theories involved'. I have no idea what this means.

The rest of the book is devoted to developing a theory of discovery from the epistemological standpoint. According to Blackwell, a fundamental problem here is how we are able to make discoveries at all, why nature and mind should prove to have so much in common. Neither materialism, idealism nor Cartesian dualism, we are told, can provide a satisfactory solution to this problem. A solution only becomes possible if we hold '(1) mind and nature are not identical but exhibit distinctively different properties, and (2) mind and nature are intrinsically relational entities which possess reality and meaning only within the overall matrix of these relations'. This means, it turns out, that scientific progress leads not only to the adaptation of mind to nature, but also to the adaptation of *nature* to *mind*. This amazing theory is Blackwell's adaptation theory of scientific discovery.

Under the guise of developing this extraordinary theory, Blackwell develops simply a number of technical terms. Thus we are told that a 'structure' is what is asserted of nature by mind. 'Concepts and facts', we are told 'do not exist in isolated worlds. They are two aspects of a structure.' Discovery then takes two forms. There is the 'elaboration' of structure which amounts to the development of 'factual' theories. Secondly there is the 'transformation, of structures which is the development of 'explanatory' theories. One important aspect of elaboration is the 'creative postulation of theoretical entities'. The last chapter of the book discusses the problem of the ontological status of theoretical entities from the standpoint of the 'adaptation theory of discovery'.

Here are my no doubt somewhat redundant comments.

1. Most of the book is simply irrelevant to the important problem posed at the beginning of the book: Is it possible to develop a methodology of discovery in addition to a methodology of verification? The issue here is simply: can we develop any general rules which, if followed, give us the best, the most rational, hope of developing important new scientific theories? Nowhere in the book is there a formulation of the problem even half as clear as this not particularly dazzling statement of the problem. Seven pages only are devoted to this issue—the fundamental problem of the book—and even here all that we get is a brief consideration of Hanson's work on this topic.

Throughout the book Blackwell makes the elementary mistake of supposing that a discussion of *what* is discovered in science is a discussion of *how* discoveries are made, i.e. is a discussion of discovery itself. Thus throughout the book Blackwell confidently imagines he is discussing discovery when really he is doing nothing of the kind. At the end of chapter six Blackwell asserts that he has been able to show that 'there are repeated epistemological patterns present in the act of discovery'. These 'repeated patterns' however all relate to *what* is discovered, not to *how* the discovery is made. It is as if one argued that there must be a method for the discovery of gold since every discovery in this field exhibits the common pattern of leading to the acquisition of—gold.

2. The so-called 'adaptation theory of discovery', quite apart from being irrelevant to the topic under consideration, is in itself bizarre to the point of absurdity. Taken literally, it asserts that Nature kindly adjusts herself to our latest theories about her, the universe presumably becoming successively Aristotelian, Newtonian, Einsteinian, Quantum Mechanical. What happens, one wonders, when a number of rival theories contend for acceptance, as in modern cosmology?

A generous interpretation of this adaptation theory is that it is simply a para-

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doxical statement of the conventionalist position about the theoretical entities of physics. But in this case why does Blackwell discuss the problem of the ontological status of theoretical entities as if it were an additional issue?

In proposing the adaptation theory as a solution to the problem of how we are able to make discoveries, Blackwell scarcely mentions, let alone discusses, other possible solutions to the problem. He does not, for example, consider the possibility that we are able to make discoveries in physics because the world really does have certain relatively simple structural properties.

3. The book lacks even the first glimmerings of philosophical sophistication. Whenever Blackwell sets out to clarify an issue or thesis he invariably succeeds in drowning whatever initial clarity there is in an ocean of ambiguity and confusion. His discussion of the question 'Do the theoretical entities of physics exist?' is a case in point. Beginning with this question, we learn that the real issue is whether physical entities exist in nature, which in turn brings us to the question 'What is nature?', and to the discovery that there are many different 'natures', but that 'nature as immediately perceived' has a special status in that it is what we begin with, so that we must conclude that a physical entity is real to the extent that it expresses a dictate of nature as immediately perceived.

All in all, I do not feel that I can recommend this book.

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COHEN, Robert S. and WARTOFSKY, Marx W., Eds. (1967) *Boston Studies in the Philosophy of Science Volume 3*. (Proceedings of the Boston Colloquium for the Philosophy of Science 1964/6.) Dordrecht: D. Reidel. Pp. xlix+489.

This volume is dedicated to the memory of Norwood Russell Hanson. It starts with a portrait and thirty-eight tributes, and ends with an article by Hanson, 'What I Don't Believe'. This is a fine vigorous piece of popular philosophical antitheology, which, among other things, argues forcefully that agnosticism is not a coherent option; but it is not philosophy of science.

It would be rude, but not quite wrong, to say that the same is true of much of the rest of the volume. There are fifteen articles, with comments on seven of them, and a symposium on innate ideas. The order, I suppose, is that in which they were read, since it has no other detectable rationale. I shall group them differently, taking first some that seem more concerned with science than with philosophy.

M. Sachs sketches a proposed general theory which is intended to incorporate both quantum theory and relativity. I am not competent to comment on its technical details, but the central thesis, that 'one must necessarily consider the element of measurement, "observer-signal-observed" as the fundamental building block *of nature*' (p. 67, my italics) seems philosophically indefensible, and, incidentally, inconsistent with Sachs's own ocean-allegory (pp. 77–8); what is there cannot fundamentally incorporate an observer.

C. Lanczos's 'Rationalism and the Physical World' likewise hints at enterprising speculations going beyond those of Einstein's later years. The central

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