Theory and Evidence by Clark Glymour
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Philosophy of Science, Vol. 48, No. 3 (Sep., 1981), pp. 498-500
Published by: The University of Chicago Press on behalf of the Philosophy of Science Association
Stable URL: http://www.jstor.org/stable/186994
Accessed: 11/10/2012 13:32

BOOK REVIEWS

CLARK GLYMOUR. ***Theory and Evidence.*** Princeton: Princeton University Press, 1980. 382 pp.

The range of topics discussed in this book is wide. There are discussions, each more or less self-contained, of the confirmation of theories, the testing of hypotheses, various issues in early astronomy, 19th century atomism, and 20th century space-time theory. There are hints of accounts of explanation, probability, and psychoanalytic evidence. And for all that the book is a unity, held together by one idea. I think that any attentive reader will conclude that there is *something* to the idea, that it illuminates all these various topics. It may not be exactly what Glymour thinks it is, though.

Glymour's central point rests on an observation, that in old-fashioned logical empiricist accounts of confirmation two different claims are treated as one. The first is the postulation of a direct relation between particular items of evidence and particular theoretical claims, which when it holds gives people in possession of the evidence reason to believe the theories, quite independently of what else they believe or are up to. The second is the idea that there are definite formulable confirmation relations, in terms of which the connections between evidence and theory can be explained. As a matter of fact, says Glymour, the first of these claims is false, as almost all of us now know, and the second is true, as he will show.

The core of his argument is an almost purely logical point. If one has a confirmation-relation R between evidence and theory, one can always extend it to a wider relation R′ between evidence, background theory, and confirmed theory, by stipulating that evidence e_1 and background theory b bear R′ to hypothesis h whenever e_1 and b jointly 'support' some e_2 which bears R to h. Glymour is most interested in the case in which R is some austere Hempelian confirmation relation and 'supports' means simply 'deductively implies'. Then the point is that allowing the obvious facts of scientific practice to intrude to the extent of admitting that large and diffuse background theories enter into every confirmation situation, does not prevent one from retaining a strict relation of confirmation between general theoretical sentences and their instances at the heart of one's account of the support of hypotheses. For an item of observed evidence in conjunction with a piece of background theory can entail an instance of a hypothesis, which is thus confirmed. This he calls the 'bootstrap strategy'. He argues that we need it in order to explain another evident fact about scientific practice, that evidence is usually seen as relevant to (as testing) particular hypotheses rather than the whole body of our theory.

The general point is wider than this, though. One could take R to be something much looser, for example inference to the best explanation. 'Supports' could mean a variety of things, too. R′ could hold between e_1 b, and h when some e_2 is together with b the best explanation of e_1, and h is the best available explanation of e_2. (Would h then inevitably be the best available explanation of e_1? I don't understand explanation well enough to know.) Or, one could take h as confirmed when e_2 and b provide the best explanation of e_1, and e_2 confirms h according to some austere Hempelian relation. Or . . .

I fear that this way of describing it may make Glymour's project seem yet another Quixotic attempt to shape all of science around a few inductive relations. And his own exposition may give this impression. Actually, the idea, in any of the forms above, allows one to make a point about selective testing which, if it is right, is of fundamental importance for the history of science and for practical methodology. Scientists have nearly always acted as if there was such a thing as a test of a hypothesis which, if passed, can tell which of variant rival hypotheses is correct and which, if failed, can tell on which hypothesis the blame for the conflict with experience should be fixed. Glymour's point is that the possibility of such tests requires connections between evidence and theory strong enough to lead to hypotheses whose vocabulary is entirely theoretical, and yet fine enough that we can tell with some precision which of various relevant hypotheses are tested by a given bit of evidence. Just by having the concept of a test as part of their equipment, then, scientists have always put themselves at odds with both inductive and holistic methodologies. It does not actually follow from this that something like Glymour's bootstrapping has always been a part of normal methodologies, but once one sees the characteristics that bootstrapping can claim to combine and once one sees the combination which the concept of a test calls for, the conjecture becomes very attractive.

Glymour therefore devotes much of the book to arguing that one can make a suitable model of hypothesis testing out of his materials, and that a number of episodes in the history of science are illuminated by the claim that some idea of test was being implicitly appealed to. He succeeds, while leaving many doubts in his wake.

To begin with the model of testing, though the two must obviously be closely related, my doubts center on Glymour's apparent conviction that there is just one notion of test operating in past and present science and that it amounts to a special case of confirmation. His definition of confirmation is too involved for me to cite all its details here, but it amounts to the bootstrap relation I mentioned above, hedged round with various safeguards against trivialization. A test of the theory is (I gather) just a situation in which evidence confirms it, via bootstrapping depending on well-established background theories. Glymour only talks explicitly about tests in connection with the special case of systems of equations (i.e., hypotheses in which, in prenex form, the matrix is a conjunction of identities and the prefix is a string of universal quantifiers? He isn't very explicit on the point.) And in the historical chapters when alluding to tests and testability he refers back to the bootstrapping model of confirmation rather than to the discussion of tests of systems of equations. Whatever the details, bootstrapping does have some strong attractions as part of a theory of test, essentially those I described above. But it is very hard to see what the price that is paid for its attractive features is. I worry whether indiscriminate holism, the testing of a theory by all the evidence it helps to explain, is really kept at bay. The question is: do perversely meandering deductions exist, from unnaturally scattered collections of evidence and intuitively irrelevant lumps of background theory to instances of hypotheses that are not intuitively tested by that evidence? It is hard to tell, because it is always hard to tell that a deduction does *not* exist. Very likely my worries are groundless, but if they are not, then other, rather less syntactic, factors will have to be brought in to the rescue. Factors not taken into account do in any case determine a number of methodological properties of tests. Tests are for example severe or trivial, and it tells more for a hypothesis that it has passed a severe test. Does this mean just that some evidence gives strong support (by reason of its quantity?) to the hypothesis? Presumably one of Glymour's conditions on bootstrap tests, that the evidence 'could have' led to a disconfirmation of the hypothesis, is relevant here: a test would be more severe if the 'could have' had more bite. But Glymour's own purely syntactic interpretation of the 'could have' will not be of much use here.

The historical sections are in a way the most important part of the book, since Glymour's most central claims are about the patterns of reasoning that work and have worked in actual science. And there is no doubt that these sections are profitable and illuminating. I have space only to mention his discussion of historical issues in the physical sciences. The discussion of the logic of the Copernican hypothesis is a model of clear exposition. He argues there that Copernican astronomy has the advantage over Ptolemaic, that more of the values of the quantities it assumes to explain planetary motions may be determined (by bootstrap arguments, of course) from observation. The discussion of Newton makes sense of some of the convolutions of Newton's argument for universal gravitation in terms of the claim that Newton, being unsatisfied just with the fact of the explanatory force of the law of gravitation and various hypotheses about the gravitational nature of various forces as providing a reason for believing these claims, was searching about for a more direct or inductive way of arguing in which successively more daring propositions are made to rest on one another, supported by the second and third laws of motion and his 'rules of reasoning'. The discussion of 19th century atomic theory (Dalton, Dumas, Canizzaro, & Co) argues that the heart of the debate about atomic hypotheses in Chemistry was not about whether they could account for the chemical facts as much as whether the quantities they supposed, particularly atomic weights, could be determined. And the discussion of tests of general relativity is meant to show that the theory came to be accepted by physicists when it became clear that crucial quantities in the field equations did not have to be assumed for their explanatory power but could be determined (using other hypotheses from the theory). This discussion fits naturally with one later in the book, in which it is argued that various alternatives to orthodox space-time theories, which physicists and philosophers have suggested, are not serious competitors since the tests of the standard theories do not test them.

These historical sections are rich and illuminating. Historians should consider them. They do not do all that Glymour wants, though. (I mean, they don't completely convince *me*; I assume Glymour wanted to convince me.) The virtues of Copernican astronomy he mentions are real and interesting (though some of them seem actually to suggest that compared to Ptolemaic astronomy Copernican astronomy is not more testable but more easily tested). But if these qualities have the importance he claims for them, why did Copernicanism not more quickly become the received view? Some more argument is needed, to the effect that those who were converted were those who saw Glymour's point. The discussion of Newton does indeed show more than the familiar point that Newton was looking for more than an 'inference to the best explanation' to support universal gravitation. And I find myself tending to agree that some sort of roughly bootstraplike strategy is involved. But is it really the strategy Glymour specifies in the sections on confirmation and testing, with just those restrictions and just those deductive and Hempelian links? Clearly not. Might some variant idea do more justice to Newton? Perhaps.

Similar hesitations and quibbles apply to all the historical discussions of the book. No doubt those with better historical knowledge than I will have more. This does not make the historical sections less fascinating, or prevent them from supporting the theory of confirmation and testing. It does prevent them from *testing* that theory though: Glymour can plausibly claim to have fitted a powerful model to various of our intuitions and our practices, but he cannot (cannot yet) claim of the particular features of the model that they are exactly what the task requires. Perhaps some other neo-inductive heresy can do better.

There is an additional importance to the book, which I should like to end by mentioning. In recent years various philosophers have tried to construct accounts of the way theoretical terms in scientific theories refer to the objective properties of things which account for the phenomena the theories try to explain. The aim of such philosophers (I am thinking especially of Putnam, Field, and Boyd) has been to preserve the insights into scientific method found in anti-inductivist views such as those of Quine, Popper, or Kuhn, without accepting the picture of science as a disconnected non-cumulative series of successive world-views, that some have thought to be entailed by these views. Glymour's work is a part of this general reaction. And it can play an essential role in it. Suppose we take his theory not as a theory of confirmation but as a theory of testing. (Testability is one reason, among others, for taking a hypothesis seriously; tests of a hypothesis provide one kind of reason, not the only one, for believing it.) Then we can motivate the methodological importance of testability and testing, construed along bootstrap lines, as follows: a bootstrap test of a hypothesis shows that a particular quantity or predicate hypothesised by the theory is needed for that theory's explanation of the data. And as a result we know not just that the theory as a whole has explanatory power, but that exactly this postulation of exactly this quantity is needed in this theory to obtain it. Under such a semantical reformulation, Glymour's account of testing reflects not the desire of science to have exactly-confirmed theories, but the preference of scientists for knowing what they are talking about. *Adam Morton, University of Bristol.*

DAVID PAPINEAU. ***Theory and Meaning.*** Oxford: Clarendon Press, 1979. viii + 210 pp. $40.00.

Papineau's principal aim is to show that a holistic theory of meaning—where meaning depends on total theoretical context, observation is theory-dependent, distinct theories are incommensurable, etc.—is consistent with scientific rationality, objectivity, realism, and progress. Thus holism in theory of meaning does not degenerate into subjectivism con-