Review: Broken Bootstraps

Reviewed Work(s): Theory and Evidence by Clark Glymour

Review by: John Worrall

Source: *Erkenntnis (1975-)*, Jul., 1982, Vol. 18, No. 1 (Jul., 1982), pp. 105-130

Published by: Springer

Stable URL: https://www.jstor.org/stable/20010797

REVIEW ARTICLE

BROKEN BOOTSTRAPS*

A review of Clark Glymour, *Theory and Evidence*, Princeton UP, 1980, pp xi + 383, pbk \$9.45, hbk. \$25.

In his recent book, Clark Glymour aims to provide an account of how scientific theories are confirmed by evidence which is in accord with the particular judgments of confirmation actually made by scientists. The feature of these judgments which Glymour especially stresses is their selective nature:

> No working scientist acts as though the entire sweep of scientific theory faces the tribunal of experience as a single undifferentiated whole... On the contrary, much of the scientist's business is to construct arguments that aim to show that a particular piece of experiment or observation bears on a particular piece of theory... (p. 3)[1]

Glymour propounds a most challenging, negative thesis – that the predominant traditions in the theory of confirmation are based on a fundamental mistake. The mistake is "hypothetico-deductivism" – the idea, very roughly, that confirmation is a question of what can be deduced *about* the evidence *from* the theory concerned. The tell-tale sign of the mistakenness of this idea is precisely that it cannot account for the selective nature of scientists' particular judgments of confirmation. Instead any "hypothetico-deductivist" account inevitably spreads confirmation out over whole systems of theories and, indeed, in the end over the 'whole of our knowledge'. What is needed, then, is an entirely different approach to confirmation and Glymour's positive proposal (based on hints found in Carnap, Reichenbach and Weyl) is his "bootstrap" theory. This stands "hypothetico-deductivism" on its head: the entailments which are important for confirmation are not ones going 'down' from theories to evidence but rather 'up' from evidence to (instances of) theories.

Glymour's book has many virtues, some of which may already be clear. It wrestles with important, interesting and sharply posed problems, and proposes its own challenging and sharply posed solutions. Moreover it combines precise logical analysis of the general notions of confirmation and evidential relevance with detailed historical analysis of particular judg-

Erkenntnis **18** (1982) 105–130. 0165–0106/82/0181–0105 \$02.60

ments of confirmation and particular methods of arguing for theories actually used by scientists. (Various episodes in the history of science, or in one case, perhaps, of pseudoscience, are investigated in some depth – Ptolemy v. Copernicus, Newton's development of his theory of gravitation, the rise of the atomic theory in the 19th Century, Freud's Rat Man case, and the General Theory of Relativity.) Also, earlier approaches to confirmation are examined, both for defects and for hints of the author's own approach;[2] examples are not restricted to physics and chemistry – particular emphasis being placed on the social ('un-natural') sciences; attempts are made to use the new confirmation theory to tackle other philosophical and methodological problems (notably those of the under-determination of theory by data and of which curve fits the data best); Glymour shows a refreshing willingness to admit shortcomings in his own views and writes in a style which, in view of the wealth of logical and historical detail contained in the book, is surprisingly lively and engaging. It is, then, a book which I strongly recommend to all philosophers of science. Moreover many of the consequences which Glymour claims for his theory of confirmation and which might sound strange to some ears seem to me exactly the right sort of consequences to have: for example, that two theories may be empirically entirely equivalent and yet one be more highly confirmed by the evidence than another, or that a theory may be better tested and better confirmed than is the set of its observational consequences.

Nonetheless I remain unconvinced by the central thesis of the book. There seem to me strong arguments to suggest that, in fundamentals, the "bootstrap" account differs only in style from earlier "hypothetico-deductive" accounts.[3] I shall first try to indicate why Glymour thinks the "hypothetico-deductivist" is in trouble (with the negative part of the book I largely agree). Next I shall try to argue that there is no fundamental difference between the "bootstrapping" and "hypothetico-deductive" approaches and that the apparent differences highlighted by Glymour are generated by incorporating *ad hoc* extra conditions on his confirmation relation – conditions for which the hypothetico-deductivist could, if he wished (and I shall indicate that he mightn't always so wish) readily provide counterparts. Glymour in fact explicitly faces up to the charge that bootstrapping 'is really just the old hypothetico-deductive view fitted out in new and less becoming garb'. (p. 167) Obviously I feel that his response to this charge is unsatisfactory and, in the final part of my review, I will briefly explain why. (I am afraid that this concentration on the main thesis inevitably means leaving out of account many fascinating details on which I should have liked to comment).

The following is a brief (and, in some places, heavily reconstructed) account of Glymour's argument against "hypothetico-deductivism". Consider first the view that a necessary *and sufficient* condition for confirmation of a theory T by some evidence e is that some statement be deducible from T which compares favourably with e.[4] This view is rather obviously unsatisfactory. It ends up in "radical holism" (which I agree with Glymour is entirely unacceptable). Two simple arguments highlight many of the essential difficulties. The first is the famous "tacking paradox". If an hypothesis h_1 entails e then so of course does any hypothesis $h'_1 \equiv h_1 \wedge t$ where t is any statement whatsoever. Hence on this "necessary *and* sufficient" view, h'_1 is confirmed if h_1 is. This is clearly at odds with the judgments of working scientists. The second argument stems from a second way in which a theory can be trivially (and unacceptably) 'inflated' whilst retaining all its old empirical consequences. Take any hypothesis h_2 and replace *every* occurrence of the theoretical predicate $P(x)$ by any combination of *new*, *theoretical* predicates, say $Q(x)$ & $R(x)$ & $S(x)$ or $Q(x) \vee R(x) \vee S(x)$, where the assumption that Q, R and S are 'new' carries the implication that they do not occur in any other accepted theory and the assumption that P, Q, R and S are all 'theoretical' implies that none occur (non-trivially) in the statement of evidence. The result is what Glymour calls a 'de-occamised' theory h'_2. Clearly if h_2 entails e then so will h'_2. Again on the 'necessary and sufficient' view, h'_2 is therefore confirmed if h_2 is and again this is, he says, contrary to working scientists' judgments.

The "hypothetico-deductivist" has two (non-exclusive) options. He could say (as some Bayesians would) that, assuming they have been correctly reported by Glymour, the "working scientists" are confusing two separate notions: 'confirmed' and 'confirmed enough to warrant rational belief'. Surely we do have *extra* reason to believe h'_1 once the evidence e is in, no matter how horribly 'cobbled up' h'_1 is, and even if this extra reason still gives us very little reason to believe h'_2 in total. (I shall return to this option shortly.)

The second option for the "hypothetico-deductivist" in the face of the above two ways of inflating theories is one that he would surely want to insist that he had really taken all along. It involves imposing extra requirements on acceptable theories beyond that of simply yielding the correct observational results. These extra requirements invariably involve notions such as 'naturalness', 'simplicity' or 'organic unity'. The confirmation theorist taking this option can *either* say that only certain theories (the 'simple' or 'unified' ones) are *candidates* for confirmation (deducibility of the evidence continuing to be a necessary and sufficient condition for confirmation but only amongst suitable theories) *or* he can say that deducibility of the evidence, although necessary, is *not* a sufficient condition for confirmation (thus writing the 'naturalness' requirement into the confirmation relation). Clearly these two formulations *are* only different formulations of the same position. Einstein used the first formulation, speaking of two requirements on a good scientific theory: an 'internal' requirement of 'inner perfection' and a separate requirement of 'external confirmation'.[5] Glymour tends to use the second formulation – putting all the weight on the confirmation relation.

He has two arguments against this qualified sort of hypothetico-deductivism. (I am reconstructing slightly). The first is that philosophers pursuing this line, although able to give clear *particular examples* of theories which are 'natural' or 'organically unified' and particular examples of theories which are not have been quite unable to give a *general characterisation* of the notion. (This is, of course, fully admitted by those who have pursued this line.[6]) The second argument (which Glymour is perhaps the first to present in such detail) is that, even if such a general characterisation could be given, it would not fully solve the problem of explaining how confirmation is localised in the way that it is by scientists. Even if e follows from some perfectly 'unified' theoretical system T, scientists will not always see the whole of T as confirmed by e – usually it will be some *sub-theory* of T which is regarded as particularly supported by e. In other words, the tacking problem arises, not only when the t 'tacked on' to some h (which already entails e) is entirely unconnected to h, but also when h & t form a 'natural' 'unified' unit. For example, Newton's and Maxwell's theory formed a 'natural' 'unified' unit: classical physics, dealing with the motions of charged and uncharged bodies.[7] But the observation of the time it takes for an uncharged, freely falling body to fall to the ground from a height h should surely not be taken as a possible confirmation of Maxwell's theory, nor even of the whole of classical physics. Rather such an observation is a test, and hence a possible confirmation of the Newtonian sub-part of classical physics. (As we shall see, Glymour's own favourite example concerning Kepler's 3rd law and observations of single planetary orbits needs some slight qualification.)

The underlying problem, for Glymour, is, then, that of specifying when a theory has 'redundant' parts relative to some evidence: if some sub-theory of T already entails e then the further parts of T are 'redundant' at least so far as e is concerned. If 'redundancy' could be clearly characterised then an acceptable "hypothetico-deductivist" account of confirmation might be given by specifying that only a theory T which contains no redundant parts relative to e is confirmable by e.[8] We certainly have a strong intuition that both the 'tacked-on' h_1' and the 'de-occamised' h_2' *above* contain redundancies – redundant sub-theories in the case of the former and redundant predicates in the case of the latter. Glymour argues, effectively, that any attempt to make this intuition precise will fail. The argument (one of the sharpest and best features of the book) is that any such attempt will be blocked by obstacles precisely analogous to those which foiled the logical positivists' pursuit of an acceptable criterion of empirical significance.

In Glymour's nutshell history, the positivists, having found that the criterion of complete verifiability was much too stringent, next proposed to regard any statement as significant if it is as least empirically testable. It was soon pointed out, however, that this requirement is too *lax*: it re-admits all the old metaphysical rubbish, at least as *parts* of empirically significant theories. If, for example, T: 'All freely falling bodies close to the earth's surface fall with constant acceleration' is significant because testable, so also is T': 'All freely falling bodies close to the earth's surface fall with constant acceleration *and* the real is rational'. The natural response to this problem is, of course, to point out that adding 'the real is rational' has not increased the empirical content of the theory – that the 'real is rational' is *isolated* in T'; and this in turn suggests the amended criterion that only theories which are testable but have no isolated parts are empirically significant. This sounds fine if we start with T and then consider the (alleged) extension to T'. But what if we started with S: 'If the real is rational then all freely falling bodies close to the earth's surface fall with constant acceleration'? This presumably has no empirical consequences as it stands, but add 'the real is rational' to it to create S', then S' does have empirical consequences. It seems that now 'the real is rational' is *not* isolated. Indeed since S' and T' are clearly equivalent we can see that which sentences (or sub-theories) are isolated within a given theory is dependent upon how the theory is axiomatised. If we try to avoid this by considering the theory as given by its deductive closure and saying that any sub-theory is isolated if it can be removed without weakening the empirical content of the theory then clearly *only* observational consequences will be non-isolated: *all* sentences with any theoretical import will be isolated in this sense.

As with 'isolation' for the positivist, so (according to Glymour) with 'redundancy' for the "hypothetico-deductivist". His argument here is crucial for what follows and so I shall follow through its basic steps in some detail. Glymour's own favourite example concerns Kepler's third law (K_3). He states that 'no observations of a single planet would constitute evidence for or against [K_3]' (p. 84).[6] The basic justification for this is that K_3 *relates* features of *any two* planetary orbits (saying of course that the ratio of the cube of the mean distance and the square of the period is the same for both orbits) and hence evidence on just one orbit is intuitively irrelevant. This is not perhaps as straightforward as Glymour assumes. It is difficult not to regard K_3 as carrying the implication (admittedly already entailed by the first two laws) that the period and mean distance are *constants* of any planetary orbit. But in that case (as Alan Musgrave pointed out to me) K_3 *is* testable (and hence confirmable) by observations on a single planet: what if astronomers found, through extended observation, evidence that either the planet's period or its mean distance from the sun was changing over time? Still, such evidence would also refute K_1 & K_2. In order to test, as it were, the 'distinctive part' of K_3 we should indeed need data from two planets. So regarding K_3 as *simply* asserting that the ratio of the cube of the mean distance and the square of the period is the same for all planets, Glymour's example does provide a good, simple illustration of his point.

K_3, as thus understood, is testable, and hence confirmable, only by observations on at least two planetary orbits. However, there clearly are theoretical systems (even unified systems such as that consisting of all three of Kepler's laws (K)) which include K_3 and which entail results about single planetary orbits. To fix ideas, let O be an experimental report about a number of successive positions of, say, Mars which constituted a genuine and successful test of the first two of Kepler's laws (it will be important for what follows that the observational consequence 'verified' by O is a consequence only of the conjunction K_1 & K_2 and not of either K_1 or K_2 taken separately). Glymour challenges the "hypothetico-deductivist" to produce an account of confirmation which allows that either of Kepler's first two laws is confirmable by such an O but does *not* allow that K_3 is thus confirmable *either* when taken in isolation *or* when taken in conjunction with (or 'against the background of') other theoretical assumptions.[9]

Many confirmation theorists (including the Bayesians) will reject Glymour's challenge as based on an error. Adopting the line indicated *above* p. 107), they will argue that there are many theoretical systems which include K_3 which *are* confirmable by results about single planetary orbits. Surely even if the predictions verified by O already followed from K_1 & K_2, we still have more reason to believe the bigger conjunction K_1 & K_2 & K_3 once the evidence O is in than we had reason to believe K_1 & K_2 & K_3 before O was in. Hence the Bayesian result that K_1 & K_2 & K_3 is confirmed by O since $P(K_1 \ \& \ K_2 \ \& \ K_3 | O) > P(K_1 \ \& \ K_2 \ \& \ K_3)$ is, *contra* Glymour, perfectly in accord with intuition. And so a confirmation theory which met Glymour's challenge would simply be wrong. This seems to me an entirely reasonable position, but it does not solve Glymour's main problem. Instead it simply shifts it outside the area of confirmation theory. Even if we go along with the Bayesian and allow that O does confirm K_1 & K_2 & K_3, we would still presumably want to say that O is *irrelevant* to the K_3 part of this theory. Glymour wants to solve this problem by providing a confirmation theory which does not have O confirming K_1 & K_2 & K_3 (or any other theory involving K_3 as a component), while the Bayesian – with a rather different understanding of confirmation – sees relevance as a separate problem.

From here on in I shall go along with Glymour's terminology and assume we are looking for an account of confirmation which stops O confirming K_3 either alone or as part of some stronger theory. Those who find this an odd and confusing way of talking should perhaps replace every subsequent occurrence of 'confirms T' by 'confirms *and* is relevant to all of T'.[10]

Suppose, then, that the "hypothetico-deductivist" takes up Glymour's challenge. And suppose that he first argues that only that part of an overall theory is confirmed which is *necessary* for the derivation of the observational consequence concerned. In our example, since in deriving O from the overall system K, we do not need K_3, K_3 is not confirmed by O. Glymour can readily dispose of this ploy: which sentences are needed in the derivation of given consequences of a theory is *dependent on how the theory is axiomatised.* Admittedly, if K is axiomatised in the usual way as $\{K_1, K_2, K_3\}$, we should need to invoke only K_1 and K_2, and not K_3, in deriving O. But how we axiomatise K is a conventional matter – what if we instead used the axiom-set $\{K_3, K_3 \rightarrow K_1, K_3 \rightarrow K_2\}$? In that case we *should* need to invoke K_3 in order to derive O.

Perhaps, then, we should say that K_3 is not confirmed by O if *there is* an axiomatisation of the overall theoretical system K, in deriving O from which, K_3 need not be invoked. This certainly means that K_3 is not confirmed by O, but unfortunately it also means that K_1 & K_2 is not confirmed either. Indeed, on this suggestion, no theoretical assumption is ever going to be confirmed by any observational consequence – since we could always make the observational consequence itself an axiom of the system.[11]

Suppose, then, that the "hypothetico-deductivist" invokes the logical independence of K_3 and O. Glymour does not, perhaps, pay sufficient attention to this line which seems a much more promising one, since no amount of re-axiomatisation can, of course, affect this independence. So our deductivist now says that K_3 is not confirmed by O because K_3 and O are logically independent and hence, *had* O turned out to be false, then merely changing K_3 could not have restored consistency with observation.[12] The problem is (assuming that O follows only from the conjunction K_1 & K_2 and not from either K_1 or K_2 taken alone) that K_1, say, is also logically independent of O and therefore also not confirmed by it on this latest criterion. Can this problem be solved by the Bayesian? Again he might say that, since K_3 and O are logically independent, $P(K_3 \mid O) = P(K_3)$ and hence K_3 is not confirmed by O.[13] But K_1 too, considered on its own, is logically independent of O (we are assuming that O follows only from K_1 & K_2) and so, unless special extra assumptions are made,[14] $P(K_1 \mid O) = P(K_1)$, and hence, by parity of reasoning, K_1 is not confirmed by O. If it is said in response that K_1 is confirmed by O because there are other assumptions (K_2) together with which K_1 entails O, then we are back with the old problem – K_3 should then be confirmed by O because there are other assumptions (K_1 & K_2 and, still worse, $(K_3 \rightarrow K_2)$ & $(K_3 \rightarrow K_1)$)[15] together with which K_3 entails O.

If (as seems to me sensible) we speak, not of individual theories being confirmed, relative to (or 'against the background of') other assumptions, but rather of *groups* of theories being confirmed, then we might seem nearer to an answer. $\{K_1, K_2\}$ is confirmable by O since K_1 & K_2 entails O, $\{K_3\}$ is not confirmable by O since K_3 and O are independent. But how about $\{K_1, K_2, K_3\}$ or $\{K_1, K_2, K_3$, 'The real is rational'$\}$? We seem to be forced to say that *only the logically weakest sub-theory of a theory T which entails O is confirmable by O*. But this is again obviously axiomatisation-dependent. It does yield the "intuitively correct" answers ($\{K_1, K_2\}$ confirmed by O, $\{K_1, K_2, K_3\}$ *not* confirmed by O), but only so long as K is axiomatised as $\{K_1, K_2, K_3\}$ and we allow sub-theories to be created only by selecting axioms out of this set. But suppose, for example, that O consists of observations of the orbit of just the planet Mars. We can replace the sub-theory $\{K_1, K_2\}$ by the equivalent set $\{K'_1, K'_2, M_1, M_2\}$ where

K'_1:	All planets, except Mars, move in ellipses with the sun at one focus.
K'_2:	All planets, except Mars, move in such a way that the radius vector joining their centre to the sun's sweeps out equal areas in equal times.
M_1:	Mars moves in an ellipse with the sun at one focus.
M_2:	Mars moves in such a way that the radius vector joining its centre to the sun's sweeps out equal areas in equal times.[16]

On this latest stipulation $\{K_1, K_2\}$ is now *not* confirmed by O since the logically weaker $\{M_1, M_2\}$ already entails O.

There is still a final resort for the "hypothetico-deductivist". He can claim that some axiomatisations are 'more natural' that others – $\{K_1, K_2, K_3\}$ is a natural axiomatisation of K, while both $\{K_3, K_3 \rightarrow K_1, K_3 \rightarrow K_2\}$ and $\{K'_1, K'_2, M_1, M_2, K_3\}$ are 'unnatural'. This final version of deductivism would then presumably say that a theory T (which may itself be a sub-theory of some wider theory) is confirmed by O if (i) it entails O,[17] (ii) it is itself naturally axiomatised and (iii) no logically weaker but also naturally axiomatised sub-theory entails O. This seems to yield all the answers Glymour would like: O is not relevant to and hence does not confirm the whole of K since there is a naturally axiomatised sub-theory of K, $\{K_1, K_2\}$, which already entails O; moreover O does confirm $\{K_1, K_2\}$ since no weaker, naturally axiomatised theory entails O.

Glymour cannot reject this final view, then, on the grounds that it yields particular judgments at odds with scientific practice. He claims instead that it is hopelessly vague:

A satisfactory explanation [of why, for instance, K_3 is not confirmed by O] might be given if one could say that the hypotheses tested are those necessary for the deduction of the evidence statement from certain "natural" axiom systems, but the positivists had no account of what, if anything makes one system of axioms more "natural" than another ... and today we are no better off in this regard. (p. 139)[18]

This charge can scarely be denied: "hypothetico-deductivism" if it is to yield the 'correct' judgments about confirmation must finally resort to vague notions and liberal helpings of 'scare quotes'. If Glymour could provide an account of confirmation which avoids resort to such notions then we should have every reason to be grateful to him. Of course, it is not enough simply to avoid mentioning the notions: if, instead of a candid admission that we must invoke 'naturalness' 'unity' and the like, we are given an account full of *ad hoc* conditions whose only justification, if probed, is that they disallow various 'unnatural' practices, then we shall justly feel disappointed. I turn now, then, to Glymour's positive contribution: to his "bootstrap" theory of confirmation. But we are, I am afraid, in for a disappointment of exactly this kind.

The 'central idea' behind "bootstrapping" sounds simple enough:

hypotheses are confirmed with respect to a theory by a piece of evidence provided that, using the theory, we can deduce from the evidence an instance of the hypothesis, and the deduction is such that it does not guarantee that we would have gotten an instance of the hypothesis regardless of what the evidence might have been. (p. 127)

I cannot of course give a full account of the details with which Glymour clothes this basic idea, but, to give at least a flavour of the full account, here is how he treats a particular example of a hypothesis consisting of a set of mathematical equations. The equations making up the theory are

1. $A_1 = E_1;$
2. $B_1 = G_1 + G_2 + E_2;$
3. $A_2 = E_1 + E_2;$
4. $B_2 = G_1 + G_2;$
5. $A_3 = G_1 + E_1;$
6. $B_3 = G_2 + E_2.$

The As and Bs are experimentally measurable quantities, while the Es and Gs are not.[19] Let O consist of experimental values of A_1, A_2, B_1 and B_2.

Then O constitutes a genuine 'bootstrap' test of, for example, equation 1. This is because these experimental values, together with the set of equations allow us to express all the 'theoretical' quantities occurring in equation 1 (in fact, the single quantity E_1) as *two independent* functions of these observable quantities. Not only does 1 tell us that $E_1 = A_1$, we can also calculate from 2, 3 and 4 that $E_1 = A_2 - (B_1 - B_2)$. Hence O may provide a non-trivial 'instantiation' of equation 1, and hence may confirm equation 1 (relative to the whole theory). Confirmation will in fact occur if the two calculated values of E_1 turned out to be same, or in other words, if the values cited in O satisfy the clearly non-identical equation $A_1 = A_2 - (B_1 - B_2$. Hence equation 1 is 'bootstrap-confirmable' with respect to the rest of the theory (and so also, it turns out, is equation 3). Equation 6 is *not* confirmable in this way and nor, more importantly, is either equation 2 or equation 4. This is because both 2 and 4 involve the quantities G_1 and G_2, and although two independent expressions for the sum of these two quantities in terms of the 'observables' A_1, A_2, B_1, B_2 can be derived, there are no such expressions for G_1 and G_2 separately. Hence Glymour's account certainly allows for the 'localisation' of experimental confirmation within a complex theory. This is also true of the extension of the account from equations to *sentences* couched in first order logic.

I shall argue *below* that the way in which bootstrapping localises confirmation does not yield the intuitively correct results in all cases (indeed equations 1–6 constitute such a case). But the general ability to localise confirmation certainly seems to be a feather in the bootstrapper's cap. How exactly did it get there?

In order to answer this question I must backtrack a little and return to the general idea behind "bootstrapping". Glymour is surely right to reject the idea that the only important deductions in science go 'downwards' from theory to evidence. Sometimes specific features of theories are deduced *from* the evidence. An important class of examples is provided by 'parameter-fixing': a theory is proposed in which some parameter is initially free and the value of this parameter is then "read off" some particular experimental results; this creates a more precise theory which may then be tested against further experiments. For example, the classical wave theory of light provided no independent theoretical reason for assigning a particular wavelength (or range of wavelengths) to a particular part of the solar spectrum. Instead, wavelengths were "read-off" the results of experiments: the theory entailed that the (measurable) distance between light fringes in a particular experimental arrangement is some precise (and one-to-one) function of the wavelength involved; assuming that the theory is correct the (theoretical) wavelengths can therefore be deduced from the (observational) fringe-spacings.[20]

'Parameter-fixing' seems to be a specific case of a more general occurrence – the 'deduction of theories from the phenomena'.[21] Here, briefly and roughly, a very general theory is proposed (this proposal will generally have the status of a conjecture, not itself being based on any deduction); the general theory is then made more specific by "reading off" some of its precise features from given observational laws. The paradigm case (Glymour gives it particular weight) is Newton's argument for his inverse square law starting from Kepler's observational laws. Newton himself claimed to have 'deduced' his theory from Kepler's 'phenomena', which (if deduction is understood as we now understand it) is impossible.[22] Still, Newton *did* perform some important deductions here – ones which are repeated in nearly every textbook of classical mechanics (and ones which account for the reluctance of most working scientists to accept Duhem's straightforward demonstration of the logical inconsistency of Newton's theory and Kepler's law). For example, Newton showed that if it is assumed that the sun is stationary or moving inertially and if a planet moves around it in strict accordance with Kepler's laws and if his own second law of motion is correct then the planet is acted on by a net force directed towards the sun and inversely proportional to the square of its distance from the sun.[23]

Such deductions as Newton's and as those involved in parameter-fixing have been of undeniable heuristic importance. Glymour sees in them, however, a general model for confirmation. Kepler's laws confirm Newton's theory of gravitation precisely because *instances* of that theory can be deduced from the laws (together with other theoretical principles and, in this case, certain simplifying assumptions) and because this deduction was not guaranteed in advance (that is, whatever the data might have been) to yield such an instance. Or to take a much simpler and clearer example – one of parameter-fixing – the ideal gas law, $PV = kT$ with (k initially regarded as free) can be tested, and hence possibly confirmed, in "bootstrap" fashion, as follows. Assume all quantities, except k, are directly measurable:

Then the hypothesis may be tested by obtaining two sets of values for P, V, and T, using the first set of values with the hypothesis to be tested to determine a value for k,

$$k = PV/T$$

and using the value of k thus obtained together with the second set of values for P, V and T either to instantiate or to contradict the hypothesis. (p. 111)

But the question which immediately springs to mind is how this differs, except in style, from the deductivist account – that the gas law is tested here by checking its directly testable consequence $P_1V_1/T_1 = P_2V_2/T_2$, where P_1, V_1, T_1 and P_2, V_2, T_2 are the two sets of values? Similarly in the Newton case, leaving aside any heuristic considerations and concentrating solely on experimental confirmation, we can surely just as well represent Newton's 'deduction' of an 'instance' of his gravitational principle from Kepler's laws as a deduction of those laws from that principle (using approximating and auxiliary assumptions). *Of course*, scientists sometimes use Newton's second law together with the observed acceleration (a_1) of a body of known mass (m_1) to calculate 'upwards' to the value of the force (F) acting on the body and then proceed to predict that the acceleration (a_2) of a second body of mass m_2 subject to the same force will be F/m_2. But this test could surely equally well be described as a direct 'downwards' test of the second law's deductive consequence $m_1a_1 = m_2a_2$. The claim that the general idea of bootstrapping has great advantages over the general idea of hypothetico-deductivism as a basis for confirmation theory seems to have rather the same status as a claim that French has great advantages over English as a language for science.

Of course the general ideas behind the two approaches can be filled out differently by adding different specific assumptions. For example on Hempel's particular and well known version of the instantiation or satisfaction approach,[24] certain results follow which differentiate it at least from standard hypothetico-deductive approaches. Not all of these differences redound to the credit of the instantiationist approach. On Hempel's criterion, for example, any existentially quantified hypothesis is automatically *disconfirmed* by the observation of non-instances. 'Some swans are black', for instance, is disconfirmed by the (surely intuitively *neutral*) observation of two red herrings. On the other hand 'a is a raven and either black or dark brown' (the light may not have been perfect) is on

Hempel's criterion, neutral *vis à vis* 'All ravens are black' when surely it ought to confirm it, at least a little. Glymour, although he formally adopts (and elaborates on) Hempel's satisfaction criterion, is eager to stress that it is the basic skeleton of the bootstrap strategy which he thinks important rather than the particular details which flesh it out (and which could always be modified piecemeal to accommodate difficulties). However, at the level of general strategy, differences between the bootstrap and hypothetico-deductivist approaches are surely differences only of style.

Consider one of Glymour's own simplified examples (simplified a little further to avoid details which are irrelevant for present purposes). In this example the hypothesis H is '$\forall x(Nx \rightarrow Px)$', and, the 'background theory' T is '$\forall x(O_1x \rightarrow Nx)$ & $\forall x(O_2x \leftrightarrow Px)$', where O_1, O_2 are observational, N, P theoretical, predicates. H is bootstrap-confirmable relative to T because there are possible observational data (*e.g.* O_1a & O_2a) which together with T entail an instance (Na & Pa) of H, and because there are other possible observational data (O_1a & $\neg O_2a$) which together with T entail an instance (Na & $\neg Pa$) of the negation of H. Hence if O_1a and O_2a is what is actually observed then we have a non-trivial confirmation of H, relative to T. But again the "bootstrapper" here has no advantage over the hypothetico-deductivist who would simply arrive at this same judgment, remarking that H together with T and an initial condition statement like O_1a entails observationally testable consequences like O_2a.

Glymour would no doubt agree that, in a very simple case like this one, "bootstrapping" has indeed no clear cut advantage. But the new strategy is meant to come into its own when the hypothesis concerned is more complex and when our intuitive judgment is that some particular experimental result is only really a confirmation of a *part* of the hypothesis. The chief virtue which Glymour claims for bootstrapping is, as I remarked before, its ability to localise confirmation and hence avoid 'holism'. This brings us back, then, to the question of how precisely this localising power is achieved.

In the case of sentences the required result must be that we may be able to deduce from an observation O and background theory T an instance of some hypothesis H, but be unable similarly to deduce an instance of the stronger hypothesis H & H'. This result certainly does not follow just from the basic bootstrap idea: on the orthodox (Hempelian) account of instantiation the observation of an individual a which turns out to be a white swan instantiates the theory 'All swans are white and all herrings are white', since that evidence entails the "development" of that hypothesis for the set $\{a\}$.[25]

Merely switching to the general bootstrap approach helps not all; in order to secure his account's ability to localise confirmation, Glymour must impose some extra, particular condition. (Glymour repeatedly insists that it is the general 'upwards' view of confirmation which is important: the details in which this view is clothed are less important since they can be tinkered around with as necessary. The above shows that this is quite wrong – *only* the details matter.) The condition which Glymour in fact imposes is that no hypothesis H is bootstrap-confirmed (by O relative to T) if it is equivalent to a conjunction H' & H^* (neither conjunct being logically true) where either H^* or H' is itself *not* confirmed (by O relative to T).[26]

But this is surely a case of 'theft' rather than 'honest toil'. If he is allowed to introduce stipulations like this one, the "hypothetico-deductivist" can just as easily solve the problem with which Glymour constantly challenges him. He might say, perhaps, that no hypothesis H is confirmed by a result O which it entails, if H is equivalent to a conjunction H' & H^* (neither logically true), where H' or H^* already entails O. Hence K_1 & K_2 & K_3 is not confirmed by the observation O of some single planetary orbit, since although K_1 & K_2 & K_3 certainly entails O, so already does K_1 & K_2.

But wait a moment: weren't there difficulties with this proposal – difficulties stemming from the possibility of reformulating Kepler's laws? There were indeed, but these hit Glymour's own proposal, just as well as the hypothetico-deductivist's. Glymour's proposal, as it stands, has the happy consequence that no result O about a single planetary orbit can confirm K_1 & K_2 & K_3, but it also has the unhappy result that no such O can confirm K_1 & K_2. Let K_1 & K_2 = H. H can easily be represented as a conjunction H' & H^* where only H^* is confirmed on Glymour's criteria. For example, if O is about the orbit of Mars, then we can use the statements M_1, M_2, K'_1, K'_2 defined *above* and let H^* be M_1 & M_2, while H' is K'_1 & K'_2.

Glymour is, of course, aware of this problem and he solves it by adding a rider to his stipulation. This rider says that, although H is indeed not confirmed it it can be split into H' and H^* only one of which is confirmed, we are not allowed, in creating H' and H^*, to import extra nonlogical vocabulary which does not already appear in H. Otherwise

> if additional vocabulary were permitted, we would always decompose any hypothesis into two conjuncts, jointly equivalent to it such that one of the conjuncts would fail to [be confirmed on the rest of the conditions]. (p. 132)

But if the hypothetico-deductivist is given equal freedom to invoke extra conditions like this one, won't he easily be able to localise confirmation within complex theories as well? He might claim, returning again to our old example, that K_1 & K_2 is confirmable by O since K_1 & K_2 is the minimum unit which entails O, except for theories which can only be formulated with the aid of 'additional vocabularly'. While, on the other hand, K_1 & K_2 & K_3 is not confirmable by O since it can be split without using 'additional vocabulary' into two parts one of which (K_1 & K_2) already entails O.

So, even if this rider were acceptable, Glymour still does not seem to have revealed any substantial way in which bootstrapping differs from hypothetico-deductivism. But *is* the rider acceptable? Its force clearly depends on what is taken to constitute 'additional vocabulary' or, equivalently, on what vocabulary is taken already to 'appear' in some hypothesis H. The most natural suggestion is surely that all predicate and individual constants in *the language of H* 'appear', explicitly or *implicitly*, in H. After all, H will have consequences involving all such constants. Let H again be the conjunction of Kepler's first two laws. We can regard H as couched in many different, more or less expressive, formal languages. But if any language is to do full justice to H it must obviously be able to express the observational results O judged clearly relevant to H. If O is again some statement about several positions of Mars, then O must name Mars (*via*, we may suppose, an individual constant, say a_1). Equality is presumably also present in the language as a logical term (Glymour in fact explicitly allows this). But we need no further expressive power in order to split H into an H' and H^* only one of which is confirmed by O – implying, contrary to intuition, that O cannot confirm H on Glymour's account. In fact, H' could be '$\forall x((x$ is a planet & $x \neq a_1) \rightarrow (x$ moves in an ellipse with the sun at one focus and the line joining x's centre to the sun's sweeps out equal areas in equal times))', whilst H^* is '$\forall x((x$ is a planet and $x = a_1) \rightarrow$ (x moves in an ellipse with the sun...))'. This shows that no 'additional vocabulary', in the sense of "fancy predicates" imported from outside H's field need be invoked in order to split a theory in a way which defeats Glymour's modified criterion.

The only alternative seems to be to construe any predicate or individual constant as constituting 'additional vocabulary' unless it *happens* explicitly to appear in the *particular formulation* of H which we happen to be considering. But this alternative construal is surely absurd. If it were acceptable then it would mean, of course, that the "hypothetico-deductivist" could dismiss, just as readily as the "bootstrapper", the problems posed by equivalent reformulations of theories. He could say, for example, that he is considering Kepler's laws in the formulation $\{K_1, K_2, K_3\}$ and if his criteria give the right answers for that formulation then no one should bother him with any "fancy" reformulations that don't fit the criteria. But as this stress on formulation indicates, Glymour's account, on this construal, violates the equivalence condition. Whilst K_1 & K_2 might be confirmed by O, the logically equivalent theory K_1' & K_2' & M_1 & M_2 will *not* be, since it can be split, using only vocabulary explicitly used, into two non-trivial parts one of which already entails O. If we happen to start with the 'natural' formulation $\{K_1, K_2\}$ then Glymour's rider yields the intuitively correct results, but what if some perverse scientist insists on formulating Kepler's first two laws as $\{K_1', K_2', M_1, M_2\}$ or what if a (rather less perverse, though perhaps rather pedantic) scientist insisted on spelling out these laws as 'All planets, namely Mars, Mercury, Venus... move in ellipses...'. In either case no confirmation by O can result on Glymour's account. The only way out for the "bootstrapper" that I can see is to say that Kepler's first two laws are confirmed by O if there is a 'natural formulation' of those laws which is not as a conjunction only one of which is confirmed on the other conditions. But this appeal to 'naturalness' is precisely what we were looking to the "bootstrapper" to help us avoid.

So far then as the 'evidential relevance' (or 'tacking') problem is concerned Glymour seems to have failed to reveal any essential advantage of his approach over older approaches. What of the other unacceptably holistic tendency that Glymour sees in "hypothetico-deductivism" – its inability to ban "de-Occamisation"? Does the "bootstrapper" do any better on this score; if so, why?

At the level of basic approach, the bootstrap stategy surely again has no advantage over its rival. Consider again our simplified example in which H is '$\forall x(Nx \rightarrow Px)$' and 'background theory' T is '$\forall x(O_1x \rightarrow Nx)$ & $\forall x(O_2x$

$\leftrightarrow Px)$'. Next "de-occamise" H, to say, H': '$\forall x((N'x \,\&\, N''x) \rightarrow (P'x \vee P''x))$'. Here N and P are theoretical predicates, O_1, O_2 observational and N', N'', P', P'' are all 'new' theoretical predicates. Assume (for the moment) along with Glymour that the correct intuitive judgment is that the result O: $O_1a \,\&\, O_2a$ confirms H but does not confirm H'. The deductivist explains why O confirms H (relative to T) by pointing out that $H \,\&\, T$ together with the initial condition O_1a entails the observationally testable statement O_2a. He then runs into the problem that, if T is similarly "de-occamised" to T', then $H' \,\&\, T'$ stands in exactly the same relation to O. But does anything change if we switch our attention to 'instances' of H and H'? We clearly can, using T', infer from the possible data $O_1a \,\&\, O_2a$ an instance of H' (*i.e.* $N'a \,\&\, N''a \,\&\, (P'a \vee P''a)$) and we can infer from the possible data $O_1a \,\&\, \neg\, O_2a$ a counter instance of H' (*i.e.* $N'a \,\&\, N''a \,\&\, \neg\, P'a \,\&\, \neg\, P''a$). Indeed nothing *can* have been gained by this switch to 'instances', because both the bootstrapper and the deductivist are interested in whether a deduction is valid (slightly different deductions in the two cases) and it is a general feature of valid deductions that validity is preserved if a new predicate (or truthfunctional combination of predicates) is systematically substituted for a given predicate. So exactly similar problems must arise on the two accounts. Each account, if it seeks to bar 'de-occamisation', must impose extra requirements.

The key extra requirement in Glymour's account is smuggled in rather quietly in the following way. He begins with hypotheses consisting of mathematical equations, like the one reported *above*, p. 114. An equation is confirmed if 'values' of the theoretical quantities occurring in it can be deduced from the data (and background theory) – values which non-trivially 'instantiate' the equation. In extending the treatment to cover predicate-logic sentences, Glymour defines a 'quantity' to be, in that case, an *atomic* formula. ('Values' of these 'quantities' are obtained by substituting variable-free terms or definite descriptions for the free-variables.) This requirement of atomicity 'solves' the problem. In our example, neither '$N'a \,\&\, N''a$' nor '$P'a \vee P''a$' is a value of a 'quantity' and hence, despite appearances, H' is not bootstrap-confirmed by '$O_1a \,\&\, O_2a$'.

But surely the hypothetico-deductivist could dream up some equally *ad hoc* analogue to this requirement which solved the problem equally well (or equally badly) within his framework. He could perhaps say that no theory is confirmed by O if there is another theory involving fewer truthfunctional connectives which already entails O. Of course this depends on a prior account of what counts as a theory (otherwise since O entails itself we would again be threatened with no confirmations of theories). The suggestion is also highly language-dependent – but then so is Glymour's (switch to a new language in which there are atomic predicates N''' and P''' which happen to hold of any individual precisely in the cases in which it was both N' and N'' and either P' or P'', respectively, and we now again have bootstrap-confirmation).

Although I believe that the hypothetico-deductivist could thus mimic Glymour's requirement, I don't recommend that he do so, since the requirement in fact leads to unacceptable results. Indeed this particular feature of his account would, I conjecture, be rejected on the very criterion he himself so often stresses: agreement with the intuitive judgments of 'working scientists'. Consider again the theory given by equations 1 to 6 (p. 114 *above*) and an observation report O consisting of measured values of the quantities A_1, A_2, B_1 and B_2. We can in fact deduce from equations 1–4 (the deduction requires all four equations) the consequence

$$7. \qquad A_2 = A_1 + B_1 - B_2$$

which is directly tested by O. Assume that the values recorded in O do satisfy equation 7. The hypothetico-deductivist would surely say that this confirms equations 1–4, or, if our attention happens to be focussed on one of these equations, that *any one* of these equations is confirmed by 7 relative to (or given) the other three. Moreover this is a judgment with which the average working scientist in the street would surely concur. But on Glymour's account neither 2 nor 3 is thus confirmed despite being *necessary* (given, of course, these particular axioms) for the deduction of 7. The reason is that individual values of the 'theoretical quantities' G_1 and G_2 cannot be calculated from the rest of the theory plus the values in O. Only values of the quantity (or rather, for Glymour, non-quantity) $G_1 + G_2$ can be so calculated. (Hence the theory, as it stands, is 'de-occamised'.)

I hesitate to speak for 'working scientists' but surely they would find it hard to differentiate in this case between, say, equations 1 and 2 on grounds of empirical confirmation. Of course they would no doubt agree that there is something peculiar about 2 in that it involves quantities G_1 and G_2 which, *as things stand*, are doing no separate work and hence could just as well be replaced by a single joint quantity G_3. No doubt the reason why the theory was formulated in this way is that scientists working on it have in mind an eventual extension of the theory which will make G_1 and G_2 do separate work. But do we really want to say that equations 2 and 3 are, in the meanwhile, incapable of empirical confirmation (despite being necessary for the deduction of directly empirically testable results)?[27]

This particular case illustrates a general difficulty. Certainly Glymour is right that it is pointless and unscientific to take an already well-confirmed theory and inflate it in this arbitrary way by adding new and undeterminable quantities. But a theory may be perfectly scientific and yet contain a 'quantity' which, so far as the evidence at some particular time is concerned, is indeed entirely redundant. The reason this quantity is present is that scientists already have at least some vague ideas about how to elaborate the theory so that the quantity 'does some work'. It is only once several elaborations of this kind have been tried and failed that scientists will start to regard the quantity as an undesirable feature – *but they will surely not say that, during this whole process, the various versions of the theory were incapable of empirical confirmation.* Glymour's proposal cannot distinguish between the case of artificial inflation and this sort of scientifically interesting case and so it makes many worthwhile scientific theories unconfirmable whilst missing the rationale for banning artificial inflation (if we really know nothing else about these inflating predicates we are simply creating a linguistically more complicated but factually equivalent theory).

Glymour's book contains, though admittedly rather in the form of brief asides, the material for at least two possible rejoinders to the above argument[28] – possible rejoinders which I ought to mention briefly. First, he himself faces up to the charge that the bootstrapper is really only the hypothetico-deductivist 'in new, and less becoming garb', and he develops briefly a defence against it. This defence, however, rests largely on a comparison with a clearly unacceptable version of hypothetico-deductivism, namely the 'simple' view that H is confirmed relative to T by O if H & T does, but T alone does not, entail O. But the version which Glymour's own arguments show to be strongest is that which invokes 'naturalness'. It is this latter version which bootstrapping fails to improve on. This version of deductivism says, more explicitly, that hypothesis H is confirmed, relative to 'background theory' T, by observational evidence O if (i) H & T deductively entails an observational consequence of which O is a direct, and successful test, (ii) there is no natural reformulation of H & T of which H is not an explicit conjunct and (iii) there is no logically weaker, naturally expressed theory which already has the observational consequence concerned. If it is argued that Glymour's account at least improves on this in that it localises confirmation within complex theories without explicitly mentioning 'naturalness' and the like, then my response is that, in so far as condition (iii) is concerned, in fact Glymour's account remains entirely unsatisfactory, and that the hypothetico-deductivist can I think easily mimic Glymour's 'success' concerning condition (ii) by re-expressing it as '(ii)′ there is no reformulation of H & T which involves fewer logical connectives and of which H is not an explicit conjunct'.

This all leads straight to the second possible rejoinder. Glymour might agree that this latter version of hypothetico-deductivism works, superficially at least, quite as well as bootstrapping (indeed he says as much more than once), but argue that it manages this only because it is in fact 'parasitic upon the bootstrap strategy' (p. 171). The vital notion of 'naturalness' has to be imported from quite outside the hypothetico-deductivist framework, but the notion can itself be characterised quite naturally within the bootstrap framework. This is a very interesting argument which, if it succeeded, would indeed overturn much of the preceeding criticism.

Glymour's characterisation of a 'natural' axiom system is as follows:

> I shall say that an axiom system is *natural* if every axiom of that system which is necessary in order to deduce an observational consequence of the theory is [bootstrap-] tested with respect to that theory by values of the measured variables occurring (non vacuously) in the observational consequence, and if, conversely, every axiom tested by values of a set of measured variables occurring in an observational consequence of the theory is necessary for the deduction of that observational consequence. (p. 313)[29]

Glymour throws out this suggestion rather as an aside, and certainly it needs further elaboration if it is really to establish the superiority of the bootstrap approach. As it stands it seems to me subject to several objections. First it is surely no pure bootstrap notion but rather a hybrid inheriting at least as much from hypothetico-deductivism. Second it pronounces 'unnatural' any theory involving quantities which are, as the theory stands, 'redundant'. But surely there is nothing necessarily 'unnatural' about the theory consisting of equations 1–6 *above*. Finally, as it stands, Glymour's suggestion *seems* to pronounce the intuitively 'unnatural' $\{K_1', K_2', M_1, M_2\}$ natural, since observational consequences about the motion of Mars will be deducible from M_1 and M_2 alone (and will bootstrap-test those statements) and observational consequences about the motion of any other planet will stand in the same relation to K_1' and K_2'.

What conclusions are, then, to be drawn about the general problems raised by Glymour? First, he is surely right that any genuine holism is absurd. Fresnel's bi-prism experiment confirmed the wave theory of light, *not* that theory plus Harvey's theory of circulation of the blood, still less the 'whole of science'. Secondly, he is surely right that this is not simply a question of excluding the 'tacking on' of intuitively totally unrelated sub-hypotheses. Sometimes only certain sub-theories of *unified* theories are given the credit for getting a particular experimental result right. Confirmation is not spread out indiscriminately. At the same time it is spread out to *some extent*: scientists quite generally regard theories whose logical content greatly transcends the empirical data as confirmed by that data. And they accept (tentatively) such theories (and hence regard it as somehow 'rational' to use them for technological application) at least partly on the strength of such confirmation: theory-acceptance is a content-increasing process. The problem is to produce an account of experimental confirmation which allows it to flow upwards just enough, *but not too far*. Returning to the Kepler case and our findings O about Mars: although O could, of course, have been deduced from a very restricted sub-theory of K, it seems right to allow O to give some confirmation to the temporally general theory M_1 & M_2 and even to the still more general theory K_1 & K_2 – even though, formally speaking, the latter is obtained from the former by 'tacking on' the strictly unnecessary K_1' & K_2'. Scientists do it seems have at least a little extra confidence in the predictions of Kepler's laws about, say, Venus, given that similar predictions have been confirmed in the case of Mars. However this does not, of course, hold for just *any* strengthening of M_1 & M_2. If we tacked some statement about tachyons on to M_1 & M_2 the resulting theory, whilst still of course entailing O, is not confirmed by it – at least not in the sense that scientists will have anymore confidence in its extra predictions – about tachyons – because of its success with O. This even applies to extensions of M_1 & M_2 which cannot be condemned as obviously unnatural. Scientists will need some extra evidence beyond O if the *whole* of K_1 & K_2 & K_3 is to be regarded as confirmed (even by a little).

This problem of allowing just so much theory, and no more, to be confirmed is, of course, *the* problem of philosophy of science – the problem of induction – in new guise. We seem no nearer to solving it now than when Hume first sharply delineated the problem in its old guise. Glymour, in his attack on older views of confirmation, has highlighted the problem very strikingly; but the problem is not one which is engendered only by these older views, it affects his own view of confirmation equally (if less explicitly). He has, I am afraid, taken us no nearer to a solution than ever. Except in fairy tales, all that happens if someone pulls hard at his own bootstraps is that they break.

London School of Economics JOHN WORRALL

NOTES

* I gratefully acknowledge the helpful comments on an earlier draft of this review from Gregory Currie, Clark Glymour, Colin Howson, Noretta Koertge, Alan Musgrave, Peter Urbach, John Watkins, and Elie Zahar.

[1] Page numbers in brackets without further details refer to the book under review.

[2] While leaning over backwards to find things to admire in the work of his positivist predecessors, Glymour is perhaps over-ready to heap calumny on more recent philosophers – especially those he brands practitioners of the 'new fuzziness'. No doubt some of these philosophers in trying to tackle important, general philosophical problems have not always attended sufficiently to questions of formal rigour; but this was surely an understandable reaction to earlier philosophers' obsession with small logical details to the detriment of the central issues. Some of Glymour's own formal-sounding definitions are perhaps more precise than the subject allows (examples of the 'new fussiness'?)

[3] This thesis has already been argued (in response to an earlier article of Glymour's) by Paul Horwich in his 'An Appraisal of Glymour's Confirmation Theory', *Journal of Philosophy* **75** (1978), 98–113. However my own arguments are rather different from Horwich's. (Glymour replies to Horwich at various places in his book).

[4] Standardly, of course, e will not actually be deducible from T. Rather T will typically imply conditional statements which state that if certain 'initial conditions' hold then the result of the observation or experiment will be such-and-such; while e says (if favourable) that the initial conditions did hold and the result was indeed such-and-such. (In this sense even the standard 'hypothetico-deductive' view is instantiationist.) It has, however, become usual to speak of evidence being deducible from a theory. Glymour often adopts this manner of speaking and so shall I. However, whenever below I say 'e is deducible from T' this is to be taken as shorthand for the above more complicated statement.

[5] See his 'Autobiographical Notes' in Schilpp (*ed.*): *Albert Einstein, Philosopher-Scientist*. Compare Popper's requirement that a good theory should 'proceed from some simple, new and powerful, unifying idea' (*Conjectures and Refutations*, p. 241).

[6] Einstein admits that 'an exact formulation [of the notion of 'naturalness' or of 'logical simplicity of premises'] meets with great difficulties' since it involves 'a kind of reciprocal weighing of incommensurable quantities'. (Einstein, *op. cit.* p. 23). Similarly Popper admits that the 'requirements of simplicity is a bit vague, and it seems difficult to formulate very clearly'. This is because it is 'intimately connected with the idea that our theories should describe the structural properties of the world – an idea which it is hard to think out fully without becoming involved in an infinite regress'. (Popper, *ibid.*).

[7] 'Unified' in a sense that, say, the conjunction of quantum mechanics and the neo-classical economic theory of oligopoly would not be. I do not, of course deny that there were specific, but important problems involved in achieving a deep unification of Newton's and Maxwell's theories – problems which eventually led to the Einsteinian revolution and hence the overthrow of classical physics.

[8] This seems to be Glymour's position (see his own account of naturalness discussed *below*, p. 19). However it seems very doubtful to me that naturalness is wholly a question of irredundancy. Indeed anything beyond e itself is, strictly speaking, 'redundant' for the derivation of e. (See my discussion *below*, pp. 112–113).

[9] Glymour always assumes that the way to respond to Duhem's famous point about no scientific theory being testable in isolation is to regard single theories as testable (and hence confirmable) against the background of other assumptions. A more direct response would of course be that only (finite!) groups of theories are confirmable (at least directly), but I shall usually follow Glymour's usage.

[10] Horwich, in the note referred to *above* (fn. 3), seems to claim that the Bayesian can solve this problem of relevance too (see p. 106) by pointing out that $P(O|\ K_1\ \&\ K_2) = P(O|\ K_1\ \&\ K_2\ \&\ K_3)$. But if this is to make K_3 irrelevant, we ought also to say that almost all of $K_1\ \&\ K_2$ is irrelevant to O as well. Indeed if we take O to be an actual observational consequence of K (rather than a stronger statement which verifies such a consequence) then, on this criterion, the only part of K which is *not* irrelevant to O is O itself. See also *below*, pp. 112–113.

[11] Or we could make a straightforward generalisation of the result into an axiom.

[12] In this sense, K_3 was never *directly* at risk in the test whose outcome is recorded in O. Of course it is possible that *had* O turned out differently K_3 might have been abandoned and replaced as an indirect result. But K_3's rejection would not have been *necessitated* by this adverse result: A simple failure to distinguish between these two sorts of ways (direct and indirect) in which our knowledge is 'at risk' from empirical evidence is, I believe, at the root of much support for 'holism'.

[13] This suggestion is made by Richard Swinburne in a review of Glymour's book in *The British Journal for the Philosophy of Science* **32** (1981), p. 317.

[14] These would amount to 'simplicity' and 'connectedness' assumptions of various kinds and hence be subject to Glymour's second attack (below, p. 114). (The Bayesian can generally do a good deal better than Glymour is inclined to allow – but not well enough to solve all the problems.)

[15] "Still worse" because '$K_3 \rightarrow K_2\ \&\ K_3 \rightarrow K_1$', does not (we may suppose) on its own entail O. Hence we have a 'background theory' which is certainly not refuted, which on its own does not, but which together with K_3 does, entail O. Why then should K_3 not be regarded as confirmed by O?

[16] The logical purist should read M_1 and M_2 as each beginning with the phrase 'for any x, if x is a planet and x = Mars then...'. Otherwise $\{K_1', K_2', M_1, M_2\}$ is not equivalent to, but slightly stronger than $\{K_1, K_2, K_3\}$ (since the former tells us that Mars is a planet).

[17] See *above* Note 4.

[18] As we shall see Glymour subsequently rather goes against this and tries to provide a

characterisation of 'naturalness' himself. His charge against "hypothetico-deductivism" then changes to the accusation that it can only operate on the back of 'bootstrapping'.

[19] See pp. 118–20.

[20] This makes the essential point, but is certainly reconstructed history. More accurately, Thomas Young re-interpreted Newton's *corpuscular-theoretic* explanation of the 'Newton's rings' phenomena, the wavelengths of the light of various colours being a straightforward function of Newton's 'interval of fits'. Newton had read off values of these 'fit intervals' from experiment, and so Young had just to convert these into wavelengths.

[21] Jon Dorling has made a particular study of the historical role of such deductions (see, e.g. his 'Demonstrative Induction: Its Significant Role in the History of Physics', *Philosophy of Science* 40 (1973), 360–72 and 'Henry Cavendish's Deduction of the Electrostatic Inverse Square Law from the Result of a Single Experiment', *Studies in the History and Philosophy of Science* **4** (1974), 327–48.

[22] Newton's theory is *intuitively* logically stronger than Kepler's containing essential reference to concepts (such as force) not contained in the latter. But deductive logic is not content-increasing. Still more decisively, Newton's theory and Kepler's laws are *formally* strictly inconsistent with one another – as was emphasised by Duhem and again by Popper.

[23] For a *precise* account of the logical and heuristic relationships involved here see my colleague Elie Zahar's forthcoming paper 'Logic of Discovery or Psychology of Invention?' *British Journal for the Philosophy of Science.*

[24] See his 'Studies in the Logic of Confirmation' in *Aspects of Scientific Explanation*, Free Press, 1965. Glymour incorporates Hempel's criterion in his formal account but, as I remark *below*, he several times declares himself committed only to the general idea of bootstrapping and views Hempel's as only one way of filling out this general idea.

[25] This same observation result also instantiates the theory 'All swans are white and all herrings are red' on Hempel's account provided we take it as "given" that no swan is a herring (*i.e.*, if we take the evidence that *a* is a white swan to imply that *a* is no herring).

[26] See p. 131.

[27] Hence it seems to me that the only hope that Glymour's approach holds out for solving the Duhem problem disappears. Let me elaborate a little on this important point. Suppose, for example, that in order to deduce some consequence at the level of telescopic observations from Newton's theory of gravity we need to invoke several other assumptions – optical assumptions (*e.g.* about atmospheric refraction), instrumental assumptions and so on. The Duhem problem, as usually understood, is that of explaining how we should apportion blame or credit (depending on whether the observational prediction comes out wrong or right) amongst these various theories and assumptions. Now, so far as I can see, if all these assumptions are necessary for the deduction of the observational result concerned then each of them will be confirmed (or disconfirmed) relative to the rest on Glymour's approach, *unless* one of them contains 'redundant quantities'. But this is the wrong result.

(Otherwise Glymour's approach promises only to solve the extra problem – not really faced by Duhem – of why a *still broader* group of assumptions should not be regarded as confirmed in such a situation (assuming the observations come out right). The "deductivist" can only solve this problem by invoking considerations of natural axiomatisation and the like, but, as we are seeeing, the "bootstrapper" is no better off.)

[28] Glymour *also* claims (p. 169) that there are cases in which scientists have regarded e as confirming a fully deterministic theory T, in which e is not deducible from T and yet in which e bootstrap-confirms T. This, if true, would indeed mark an irreconcileable difference between the two approaches since deducibility is certainly *necessary* on the hypothetico-deductive view. Unfortunately, Glymour's claim seems to rest on a mistake. The case he has in mind is

again that of Newton's theory and Kepler's laws. He claims (correctly of course) that the laws cannot be deduced from the theory. What he seems really to be claiming is that not even a modified version of Kepler's laws can be deduced from Newton, since an extra premise (saying that gravity is the *only* operative force) is required. However, 'there is a consequence of the gravitational law... that is [bootstrap-]confirmed by Kepler's laws with respect to the laws of motion. The consequence I have in mind is that every planet is subject to a force directed to the center of the sun, and the forces are in inverse proportion to the distances [*sic*] of the planets from the sun. That consequence claims the *existence* of a force; it does not claim anything about what the total force is in any situation'. *But doesn't the assumption that gravity is the only operative force equally well get smuggled into this bootstrap-reasoning*? Of course it does.

Glymour's argument surely rests on a play on the word 'force' which we sometimes use as meaning 'one of the forces operating' and sometimes as 'net force'. The calculation from Newton's second law plus Kepler's laws (plus simplifying assumptions) of the *net force* acting on a planet can only supply an 'instance' or (special case) of the gravitational principle if we take it that the net force here *is* gravity.

[29] Glymour goes on to characterise 'very natural' systems.