GLYMOUR ON EVIDENTIAL RELEVANCE*

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Glymour's "bootstrap" account of confirmation is designed to provide an analysis of evidential relevance, which has been a serious problem for hypothetico-deductivism. As set out in *Theory and Evidence*, however, the "bootstrap" condition allows confirmation in clear cases of evidential irrelevance. The difficulties with Glymour's account seem to be due to a basic feature which it shares with hypothetico-deductive accounts, and which may explain why neither can give a satisfactory analysis of evidential relevance.

Introduction. Clark Glymour, in *Theory and Evidence*, proposes an analysis of confirmation which is based on a notion of instance derived from Hempel's notion, and Glymour's own "bootstrap condition". The bootstrap condition is designed to allow a sentence including only "observational" vocabulary to confirm hypotheses framed in a richer "theoretical" vocabulary. The aim of the "bootstrap strategy" is to allow the derivation of an instance of the tested hypothesis from the evidence, by using parts of a general theory *which typically includes the tested hypothesis*. Unlike other attempts along these lines, Glymour's doesn't rely on any special class of "meaning postulates" or "analytic truths". It treats a theory as simply a consistent, deductively closed collection of sentences, whose members are all on a par with one another.

The most significant merit of the bootstrap condition, according to Glymour, is that it provides an account of how a bit of evidence can confirm (or disconfirm) different parts of a theory unequally. The most attractive alternative account of purely qualitative confirmation, the hypothetico-deductive (*H-D*) account, seems to be unable to explain this feature of scientific reasoning; and this is the main (and by far the most convincing) criticism Glymour offers of hypothetico-deductivism. Also, his positive account of other aspects of scientific practice relies heavily on his account of evidential relevance. The bootstrap strategy figures in his explanation of our preference for "varied" evidence and our distaste for theories containing "unobservable quantities", and provides Glymour with a way of

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rejecting Quinean holism about confirmation without relying on an analytic/synthetic distinction.

I will argue that the bootstrap strategy does not provide the account of evidential relevance that Glymour wants it to, and while I have no proof that nothing of the kind will work, I will suggest reasons for pessimism on that score.[1]

I

Glymour's statement of the bootstrap condition, and the technical terms involved in it, are complicated by various conditions designed to remedy difficulties not relevant here. For that reason, I'll give only a rough statement of the condition. The central notion used is that of a *computation* of a *value* of a *quantity*. A *quantity* is simply an open atomic formula, and a *value* of a quantity is an atomic sentence, or its negation (or a sentence logically equivalent to one that would be atomic, if one or more definite descriptions were replaced by individual constants). Thus '$P(a)$', '$\sim P(b)$', or '$P(ixG(x))$' would all be values of the quantity '$P(x)$'.

A *computation* for a quantity will be represented by a graph, such as this one:

$$\begin{array}{ccc} & \underline{P(x)} & \\ \nearrow & & \nwarrow \\ R(x) & & S(x) \end{array} \quad \leftrightarrow \forall x[(R(x) \wedge S(x)) \supset P(x)]$$

Given certain values for '$R(x)$' and '$S(x)$', the sentence displayed to the side (the hypothesis "used" in this computation) will yield a value for '$P(x)$'. Computations may have many branches, and more than two levels. When they have more than two levels, only the topmost and the bottommost need be restricted to quantities; other levels may contain nonatomic open formulae.

Glymour's bootstrap condition gives his account of when an evidence statement E confirms an hypothesis H with respect to a theory T. It roughly requires that (i) $E \wedge H$ be consistent with T, (ii) that E, together with a set C of computations that use only hypotheses from T, determines values for a set of quantities which include occurrences of all predicates in H, (iii) that this set of values (together with E) confirms H according to an

[1] In "Bootstraps and Probabilities", Glymour uses a variant of his bootstrap condition in conjunction with probabilities rather than Hempelian instances, to give an alternate account of confirmation. My criticism will not apply to this account. But it should be noted that probabilities come with a prima facie account of evidential relevance built in: positive relevance. Since Glymour does not reject the use of positive relevance to help determine evidential relevance (his account, in fact, depends on it), the motivation for using a bootstrap condition in addition is not as clear. For more on this, see Horwich (1980).

extended version of Hempel's satisfaction criterion, and (iv) there is a sentence E' containing only vocabulary occurring in E, such that the computations in C would yield from E' a set of values that would confirm $\sim H$ according to the extended satisfaction criterion.

The last condition is designed to prevent cases where the computations used guarantee Hempelian confirmation of H, irrespective of E. E' is required to be compatible with the conjunction of all hypotheses used in C (except, possibly, for H itself), and to contain no impossible values of its quantities. (The values need not be simultaneously possible, given natural law; in fact they often won't be. But they must be individually possible, in a fairly common-sense sense of possibility, taking into account the instruments and procedures used in obtaining E.)

The requirement of evidential relevance is supposed to be provided essentially by the condition that all of the predicates in H occur in the quantities computable from E. If one is favorably disposed towards instantial theories of confirmation, one would think that

$$E: R(a) \wedge B(a)$$

should confirm

$$H_1: \forall x[R(x) \supset B(x)].$$

But if one's theory T is equivalent to the conjunction of H_1 and

$$H_2: \forall x G(x),$$

one would not want E to confirm H_2. (Take H_1 to be the famous "Raven hypothesis", and H_2 to be the pantheistic hypothesis.) It seems plausible that the computability requirement would give this result.

On Glymour's account, E does indeed confirm H_1 relative to T. But it also confirms H_2. Consider the following computation:

$$\begin{array}{ccc} & \underline{G(x)} & \\ \nearrow & & \nwarrow \\ R(x) & & B(x) \end{array} \quad \leftrightarrow \forall x[(R(x) \supset B(x)) \equiv G(x)]$$

E will yield '$G(a)$' from this computation, which is an instance of H_2. The hypothesis used in the computation is not H_1 or H_2, but it is a consequence of them, and hence is in T. Finally,

$$E': R(a) \wedge \sim B(a)$$

is consistent with the used hypothesis, and would yield '$\sim G(a)$', which is an instance of $\sim H_2$.

It is worth noting that the example doesn't depend on the particular simplicity of the added hypothesis. Take T to be equivalent to '$\forall x[R(x) \supset B(x)] \wedge \forall x[S(x) \supset C(x)]$'. Calling the conjuncts H_1 and H_2 respectively, consider the following set of computations:

$$\underline{S(x)} \qquad \begin{array}{ccc} & \underline{C(x)} & \\ \nearrow & \uparrow & \nwarrow \\ R(x) & B(x) & S(x) \end{array} \qquad \leftrightarrow \forall x[(R(x) \supset B(x)) \equiv (S(x) \supset C(x))]$$

E = '$R(a) \wedge B(a) \wedge S(a)$' will confirm H_2, E' = '$R(a) \wedge \sim B(a) \wedge S(a)$' will confirm $\sim H_2$, and both E and E' are consistent with the used hypothesis. Yet surely E should confirm only H_1, on Glymour's intuitions.

Glymour's own main example of evidential relevance involves Kepler's laws. The first two of these laws describe how any planetary body must move around the sun. The Keplerian orbit of a planet may be determined by three well-chosen observations of it; a fourth observation will then test the first two laws.

The third law, however, says that there will be a certain fixed ratio between certain parameters of the orbit of any planet, but does not say what the ratio is. Thus while one may calculate the ratio for any planet by making observations of it, it would seem that one should have to do this with at least two planets to test the third law. Glymour writes:

> To test Kepler's third law we need estimates of the periods and mean distances from the sun of at least two planets, but from the observations of one planet alone we cannot compute, using Kepler's laws and their consequences, the parameters of the orbit of any other planet. We can, of course, compute under those circumstances the *ratio* of the square of the period to the cube of the mean distance from the sun for any planet whatsoever, but only by using Kepler's third law itself. So, even if we count such a ratio as one quantity, the representative of Kepler's third law for the requisite computations will be a trivial identity, and hence the third law will not be tested (Glymour 1980a, pp. 134–135).[2]

Without working out the details here, I would like to indicate informally how the type of problem pointed out above may infect the real examples of evidential relevance that Glymour is interested in explaining. The problems will be most clear if we treat the ratio as one quantity, '$k(x)$'. The third law will then simply state that for any two planets x and y, $k(x) = k(y)$. A confirming set of computations must be capable of

[2]To say that the representative of the third law will be a trivial identity is roughly equivalent to claiming that the computations would guarantee confirmation of the third law irrespective of E. This would violate the requirement in clause iv of the bootstrap condition that there be some E' which would disconfirm the third law by the same set of computations.

delivering both positive and negative instances, as Glymour points out. But the instances need not be computed by means of the third law itself. It will be a consequence of the theory, for example, that

($C1$) if planet a obeys Kepler's first two laws (in a given set of observations), then the k-value for planet b will be the same as that of planet a.

It will also be a consequence of the theory that

($C2$) if planet a doesn't obey the first two laws, then the k-value of planet b will be twice that of planet a.

($C2$ will be a consequence of the theory simply because the negation of its antecedent is.)

Now the k-value of planet a may be obtained by observations of it alone. And observations of planet a alone can also determine truth-values for the antecedents of $C1$ and $C2$. The conjunction of $C1$ and $C2$ can thus be used in a set of computations that will give us positive or negative instances of the third law, depending only on whether the observed planet is well-behaved with respect to the first two laws. We will have tested the third law by observations of a single planet.

It might be insisted at this point that we not treat '$k(x)$' as a single quantity; after all, it might be considered as merely an abbreviation for '$T^2(x)/d^3(x)$'. Perhaps it should be required that a test determine values for the parameters T and d for the two planets.[3] This cannot be done as simply as before, for even if the observed planet is well-behaved and we know its T and d-values, the theory won't tell us the T or d-value of any unobserved planet—it will only tell us their ratio. Thus we can't use $C1$ to get our positive instances as before.

There are still, I think, computations that will confirm the third law from evidence that is intuitively insufficient. Suppose one has observed planet a sufficiently to obtain its values for both d and T. Also suppose that one has observed planet b only enough to obtain one of these values for it. Since one can compute k for planet b as above, one can then use the one value one has for planet b to compute the value remaining. But it is obvious that one does not know enough about planet b to confirm the third law; this can be brought out by the realization that the confir-

[3]A. Edidin (1981) suggests the advisability of adopting a requirement of this type in response to a criticism of Glymour's account made by P. Horwich (1978). And Glymour himself says that a distinction between definitions and hypotheses must be made in using the bootstrap strategy (see Glymour 1980a, p. 320).

mation or disconfirmation is entirely independent of the observed value that one has for it.[4]

II

The problem with the bootstrap strategy's account of evidential relevance is strikingly similar to the problem Glymour points to in trying to give a hypothetico-deductive account of evidential relevance. The *H-D* account most naturally gives a simple two-place confirmation relation between an evidence statement E and an hypothesis or theory H: E confirms H just in case H entails E (or E entails H). It can naturally be extended to give a three-place relation between evidence, hypothesis, and a body of background information assumed to be true: E confirms H relative to T iff $H \wedge T$ entails E, but T alone doesn't (or if $E \wedge T$, but not T, entails H).

But this is not the relation that Glymour has in mind, for on this account the body of assumed background knowledge cannot include (or entail) the hypothesis to be confirmed. In a sense, the background knowledge performs the same function that analytic sentences were supposed to in positivistic accounts: it can provide a bridge from evidence to hypothesis, without itself being tested. Glymour wants to reject picking out any special class of sentences to use in this way; but if that is not done, then how is one to choose which parts of a theory may be used to confirm which other parts? If no restrictions are imposed, then almost any bit of evidence entailed by the theory will be counted as relevant to almost any hypothesis in the theory. So unless some way of choosing among subtheories is added to the *H-D* account, it will be unable to satisfactorily explain evidential relevance.

The phenomenon Glymour wants to account for is exemplified by the fact that observations of one planet can (intuitively) confirm Kepler's first two laws, but not the third, *for a scientist whose theory includes all three.* It is interesting to look at his argument against the likelihood of finding an acceptable *H-D* account of this phenomenon. (Here, $K_1 - K_3$ are Kepler's laws, C is the relevant part of Copernican theory, the astronomical theory T is equivalent to $C \wedge K_1 \wedge K_2 \wedge K_3$, and O' is roughly an observation statement to the effect that some planet obeys $K_1 \wedge K_2$):

[4]The above discussion is all based on the bootstrap account presented in chapter 5 of *Theory and Evidence*. A somewhat different bootstrap account, presented in chapter 7, deals with theories as systems of equations. Although it doesn't allow sentences of conditional form to be used in computations, similar counterintuitive examples can be formed for that account. (For example, the right-hand computation in the footnote on p. 117 of *Theory and Evidence* can be collapsed so that only the equation $A = D$ is used.) Glymour has pointed out to me in correspondence that the chapter 7 account might be fixable even if the chapter 5 account isn't. I haven't, however, discovered a way to do this.

> [It will not] do to propose that the difference between K_1 and K_2, on the one hand, and K_3, on the other hand, is that C & K_1 & K_2 entails O', but C & K_3 does not. For what could be the point of that remark? Not, surely, just that there is a subtheory of T containing K_1 and K_2, but not K_3, and entailing O'; for there are subtheories of T containing K_3, but not the first two of Kepler's laws, and entailing O', and there are even subtheories of T that entail O' but that don't contain any of Kepler's laws (O' itself is such a subtheory). The difficulty with K_3 cannot be that K_3 is unnecessary in order to deduce O' from the axioms of T, for there are other axioms for T, logically equivalent to those given, such that from these alternative axioms K_3 *is* necessary to deduce O' but K_1 and K_2 are not. A satisfactory explanation 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, in any sense imaginably relevant to confirmation, and today we are no better off in this regard (Glymour 1980a, pp. 38–39).

One example of an "unnatural" axiom set for T that would require K_3, but not K_1 or K_2, to deduce O', would replace $\{C, K_1, K_2, K_3\}$ by $\{(K_3 \supset C), (K_3 \supset K_1), (K_3 \supset K_2), K_3\}$. It is even easy to construct an equivalent set which would yield O' from a subset of axioms not strong enough to yield K_1 or K_2, but which would require K_3 to yield O': $\{(K_3 \supset O'), K_3, (O' \supset C \wedge K_1 \wedge K_2)\}$.

What is intuitively unnatural about such axiom sets? One answer is that while they contain only sentences entailed by the "natural" set, some of the sentences they contain as axioms are entailed by the natural set only "accidentally": they do not express any intuitive regularities of nature that would occur to us as explaining the data. Put in a somewhat different way: the only reason we believe, for instance, $K_3 \supset K_2$ is that we believe K_2 already.

The same derivativeness and lack of intuitive explanatory appeal is present in the hypotheses used in the problematic computations above. This undoubtedly accounts for Glymour's not realizing that such computations were possible, just as it accounts for the initial plausibility of saying that K_1, but not K_3, is necessary for deriving O' from T.

Glymour suggests that the *H-D* account's problem might be solvable if an account could be given of what would constitute a "natural" axiomatization of a theory (though he is pessimistic about the possibility of the hypothetico-deductivist finding such an account.) Confirmation would then presumably be determined by which of the "natural" axioms were required to entail the evidence. Similarly, perhaps by placing some restrictions on the hypotheses used in Glymour's computations, we could block the ones that cause problems in his account.

Many of the initially attractive conditions of this type do not work. One might try requiring that the hypotheses used in the computations not entail the equivalence of the tested hypothesis H with any other consequence of the theory, or perhaps with any consequence of the theory that is logically independent of the tested hypothesis. One might also try restrictions on the possible counter-evidence E': that it be consistent with every consequence of the theory that is logically independent of H, or that it be consistent with $H \supset T$. These, and the other conditions I've tried have all been too weak (allowing the unintuitive confirmations to occur) or too strong (preventing legitimate confirmations); and often both.

This does not, of course, show that no such condition will work. I cannot cite a long history of failures to produce a satisfactory Glymourian account of evidential relevance, as one might in the case of the *H-D* account. But the apparently similar failures in the two accounts suggest the possibility of explaining both failures in terms of features common to the accounts.

The most obvious such feature, and one which is clearly related to the difficulties, is that each account attempts to define the confirmation relations within a theory by looking only at the set of sentences entailed by the theory. One possible explanation of the difficulty in terms of this feature is just that the relations of evidential relevance within a theory are not determined by the collection of sentences the theory entails.[5] If something like this is true, Glymour's account is faced with the same obstacle that prevents the *H-D* account from providing a satisfactory explanation of evidential relevance.

The following example seems to me to suggest that something like this may well be true. Consider theory T_1, which consists of the following two hypotheses:

$$T_1: \quad H_1: \forall x[R(x) \supset B(x)]$$

$$H_2: \forall x[R(x) \supset F(x)]$$

Take H_1 to be the famous raven hypothesis, and H_2 to be the hypothesis that all ravens have a certain type of feather. Now it seems intuitively that

$$E: \quad R(a) \wedge F(a)$$

should not count toward confirming H_1, even though it should (at least on the instantial view) confirm H_2.

[5]Here, and subsequently, I'll be using 'theory' loosely, without commitment to a theory's being (or not being) a deductively closed set of sentences.

We can work out the usual type of problematic computation in Glymour's theory as follows:

$$\begin{array}{ccc} & \underline{B(x)} & \\ \nearrow & & \nwarrow \\ R(x) & & F(x) \end{array} \quad \leftrightarrow \forall x[R(x) \supset (F(x) \equiv B(x))]$$

Not surprisingly, the used hypothesis (call it H_3) is an unnatural-looking consequence of H_1 and H_2. It seems like exactly the type of hypothesis we'd want to exclude in fixing Glymour's account.

But now consider theory T_2, which consists of the hypothesis that all ravens are black along with the hypothesis that ravens are black just in case they have a certain kind of feather (in other words H_1 and H_3).

$$T_2\colon \quad H_1\colon \forall x[R(x) \supset B(x)]$$

$$H_3\colon \forall x[R(x) \supset (B(x) \equiv F(x))]$$

Consider again

$$E\colon \quad R(a) \wedge F(a).$$

From the perspective of T_2, it does not seem odd at all that E would confirm H_1, using H_3. In fact, if 'B' is imagined to stand for a "theoretical" predicate and 'F' for an "observational" one, this would seem like exactly the type of confirmation Glymour's account is designed to allow for.

The point of all this is not, of course, just that E sometimes can confirm H_1, using H_3. It's that from the perspective that treats theories as deductively closed sets of sentences, T_1 and T_2 are identical. If one's theory consists of H_1 and H_2, then H_3 will be a consequence of one's theory (though it might intuitively be an 'accidental' consequence, which wouldn't express a regularity of nature that would occur to one in explaining the data). On the other hand, if one starts out believing H_1 and H_3, then H_2 will be a consequence. But in this second case, where believing H_3 is one's reason for believing H_2 rather than vice-versa, it seems entirely reasonable to use it to allow '$R(a) \wedge F(a)$' to confirm H_1.

If my intuitions about relevance in these cases are reasonable, and if T_1 and T_2 are plausible types of theory, it would seem that different sets of evidential-relevance relations may be associated with theories having the same set of deductive consequences. This does not seem so surprising if one keeps in mind the severity of the limitation of looking only at the set of sentences a theory entails.

A scientist postulates that certain regularities exist in nature, and these may be thought of as "natural" axioms for his theory. He thinks of his theory in terms of its "natural" axiomatization, not in terms of a deductively closed set of sentences. That is why we do not find the suggestion surprising that a natural axiomatization of a theory can give us a handle on the evidential relevance relations within the theory. But there is nothing in the syntax of a deductively closed set of sentences which tells us which of its sentences are intended to correspond to intuitive regularities in the world. So it should not be surprising that we haven't found a way to recover the "natural" axiomatization of a set of sentences, or that two theories with different sets of natural axioms, corresponding to different sets of intuitive regularities, can entail the same set of sentences. Scientists holding these theories may well respect different sets of evidential-relevance relations.

The two raven theories in the above example seem to illustrate just this point. In T_1, H_3 was included in the theory just because H_1 and H_2 were. If a scientist holding T_1 were to give up H_1 or H_2, he'd have no reason for holding onto H_3. In T_2, by contrast, H_3 was intended to represent an intuitive regularity; its inclusion in the theory did not depend on H_1 or H_2. A scientist holding T_2 might well give up H_1 (and consequently give up H_2) without having any reason to give up H_3. This is the sort of scientific practice Glymour is trying to account for, but it seems to depend on something beyond the data Glymour allows himself to use.

I've been using 'theory' as if T_1 and T_2 were different theories. But the suggestion I'm making here is not that it is wrong to individuate theories by their consequences. Perhaps T_1 and T_2 are best seen as two ways of holding the same theory. Whether we use 'theory' to refer to a deductively closed set of sentences or to something more isn't at issue. The suggestion is just that if we take theories to be simply deductively closed sets of sentences, then a theory won't come with a complete set of confirmation-relations built in.

What needs to be added to a set of sentences if we are to be able to determine confirmation relations? Answering this question satisfactorily would require, I think, giving a satisfactory theory of confirmation. But some possibilities suggest themselves immediately: for instance, simply specifying a subset of the sentences as the "natural axioms". If one did that, then perhaps a satisfactory *H-D* account would be possible. Another possibility is to specify a probability-distribution over the sentences, and use probabilistic positive relevance. Of course, the more one has to specify, the less work is left for the confirmation theory to do. But the example suggests the possibility that no confirmation theory can satisfactorily account for our intuitions by looking only at the sets of sentences entailed by our theories.

The force of the example, of course, depends on taking T_1 and T_2 as plausible examples of theories. They are extremely simple, and one might object that anyone who held them would need also to hold some additional hypotheses, which might be different in the two cases. Thus it might be argued that my intuitions on confirmation in these two cases depend on some such implicit assumptions, and that if the additional hypotheses were added, it would allow the theories to be differentiated in the right way by some modified Glymourian account.

Perhaps some specific argument along these lines can be made. But until one is offered, I see little reason for supposing that one exists. For I see no reason for discounting the possibility that two theories with the same set of consequences can have very different evidential-relevance relations. If that turns out to be the case, Glymour's account and the *H-D* account are faced with the same impossible task.

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