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Discussion:

Let's dump Hypothetico-Deductivism for the right reasons

Glymour's (1980) thesis that hypothetico-deductivism is hopeless is one with which I cannot agree more strongly (see Rozeboom, 1970, pp. 93-96, 1972, pp. 100-103). But the specifics of his argument are disconcerting. Either something strange has come over the concept of "confirmation" or older obscurities in this notion have begun to fester. In any case, Glymour's intuitions about what 'confirm' means are importantly at odds with my own. If one of us is not confused here, there must be a plurality of confirmation concepts so disparate in their force that further debate on confirmation principles will remain pointless until these notions are disambiguated.

Whatever divides us in this matter goes far deeper than the contrast previously observed by Carnap (1962, pp. *xv*ff., 462ff.) between confirmation viewed as suitably high conditional credibility ("firmness") and confirmation more properly understood as the enhancement of conditional credibility by new evidence. It takes little effort to dismiss firmness as a tolerable explication of what confirming evidence provides, and it seems fair to claim, without argument, that Glymour finds the firmness construal of confirmation as unacceptable as I do. But neither, it would appear, is he satisfied by mere credibility enhancement.

The locution under which Glymour engages hypothetico-deductivism is 'sentence h is confirmed by sentence e with respect to theory T'. I shall abbreviate this as '$C^+(h,\, e\,|\,T)$' with the reservation that I take its arguments to be triples of propositions rather than of sentences. Presumably, $C^+(h, e\,|\,T)$ and its qualitative alternatives, h's being disconfirmed *wrt* T (i.e., $C^+(\sim h, e\,|\,T)$ and h's being indifferent to e *wrt* T (i.e. neither $C^+(h, e\,|\,T)$ nor $C^+(\sim h, e\,|\,T)$) comprise a partition of the range of some more finely graded measure of confirmation/disconfirmation. Glymour does not say what it is for confirmation to be "relative to theory T" (his introduction of this phrase in Glymour, 1975, p. 413, does not clarify it either); but I take $C^+(h, e\,|\,T)$ to be a subjunctive conditional that warrants a heuristic reading something like "For someone whose knowledge were to comprise (alternatively, to include) T, learning e *would* rationally confirm h." Be that as it may, Glymour's note points out the technical degeneracy of a proposal by Merrill (1979) whose gist he summarizes as

> h will only be confirmed by e with respect to T if h cannot be divided into two strictly weaker sentences ... at least one of which is confirmed by e with respect to T. *That is not only a natural idea, it is so natural* that if it fails to

> work [as a constraint on the excesses of standard hypothetico-deductivism], one is hard-pressed to believe that anything plausible will (Glymour, 1980, p. 323, italics added).

But unlike Glymour and Merrill, I find it startling that this suggestion could seem to have any initial plausibility whatever. Never mind the merits of Merrill's technical proposal; the motivation behind it is what really needs to be fathomed.

The prospect of hypothetico-deductive confirmation of h by e *wrt* T arises when h entails e given T, i.e. when $\vdash T \supset (h \supset e)$ or, equivalently, $h \cdot T \vdash e$. But we need to exclude degenerate entailments, so Glymour takes the basic precondition of hypothetico-deductive inference to be

Definition. A triple $\langle h, e, T\rangle$ of propositions is an *HD-test* (of h, by e, given T) iff (a) $h \cdot T$ is consistent, (b) $h \cdot T$ entails e, and (c) T alone does not entail e. (Glymour also adds that e is true; however, if confirmation is subjunctive, de facto truth is no more needed for e than for T.)

A variant of this precondition more appropriate for confirmation models based on credibility change replaces logical necessity by doxic certainty (maximal credibility), i.e.

Definition. A triple $\langle h, e, T\rangle$ of propositions is a *CR(edibilistically deductive)* test (of h, by e, given T) iff (a) $\sim h$ is not certain given T, (b) e is certain given $h \cdot T$, and (c) e is not certain given merely T. (Here and hereafter we take "certainty" to be rational certainty governed by synchronic coherence and whatever principles may characterize rational belief change.)

However, we can generally ignore the difference between HD-tests and CR-tests insomuch as these become co-extensive under the simplifying assumption that p is certain given q only if q entails p. (That presumption can fail in special cases, notably when p comprises all but one of the alternatives in an infinite partition of possibility-space, but it is not a salient issue here.) Let us also say

Definition. Propositions p and q are *separable* given T iff neither entails the other given T.

Then Merrill's gist that Glymour finds so intuitively compelling is

Proposal 1 (*Merrill's Intuition*). If $\langle g \cdot h,\, e,\, T\rangle$ is an HD-test in which g and h are separable given T, then $C^{+}(g \cdot h,\, e\,|T)$ only if not $C^{+}(h,\, e\,|T)$.

But P-1 is surely outrageous: if, for example, e confirms both g and h separately, why would we not expect it generally to confirm their conjunction as well? Indeed,

P-l is incompatible with the strongly appealing

Proposal 2. Given T, evidence e confirms every deductive consequence of $e \cdot T$ that is uncertain given just T.

For under P-2, with appropriately standard conditions on the propositions involved, $g \cdot h$ taken for e confirms every one of g, h, and $g \cdot h$ given T.

Yet might there be some sense of confirmation under which P-2 fails? Or at least may not P-l merely carry to an untenable extreme a solid intuition that will sustain a softened version of P-l? That is the promise of three objections to hypothetico-deductivism with which Glymour commences his note. First, he expresses distaste for any model of confirmation that forbids $C^+(h, e\,|\,T)$ whenever T entails h. Since $T \vdash h$ precludes $\langle h, e, T\rangle$ being an HD-test in the first place, it is not clear why Glymour scores this against hypothetico-deductivism; even so, his desire that T's deductive consequences remain further confirmable *wrt* T seems to detach confirmation from credibility enhancement. Secondly and more saliently, what Glymour takes to be the "typical modern version" of hypothetico-deductivism, namely

Proposal 3. Whenever $\langle h, e, T\rangle$ is an HD-test, $C^+(h, e\,|\,T)$,

is made intolerable for him by its corollary equivalent

Proposal 3*a*. If $\langle h, e, T\rangle$ is an HD-test, then $C^+(g \cdot h, e\,|\,T)$ for every proposition g that is consistent with $h \cdot T$, even when g and h are separable given T. (For then, $\langle g \cdot h, e, T\rangle$ is also an HD-test. P-3a subsumes P-3 by taking g to be h, and is equivalent to saying that any e not entailed by T confirms-given-T the conjunction of e with any other proposition with which e is compatible given T.)

And neither can Glymour countenance

Proposal 4. $C^+(h, e\,|\,h \supset e)$ whenever $\langle h, e, h \supset e\rangle$ is an HD-test (i.e. whenever $h \cdot e$ and $\sim h \cdot \sim e$ are both consistent).

Since $h \supset e$ entails $h \equiv e \cdot (e \supset h)$, P-4 amounts to the special case of P-3a in which T is $h \supset e$, h is e, g is $e \supset h$, and e is separable from $e \supset h$ given $h \supset e$ unless $e \vdash h$. Since instances of P-4 in which e entails h are surely inoffensive to Glymour, whatever outrage is to be found in P-4 is already manifest more generically in P-3a.

And how does Merrill's Intuition bear on this? Well, every $\langle g \cdot h,\ e,\ T\rangle$ that satisfies P-3a is in violation of P-1 if g and h are separable given T. So Glymour consistently senses conflict between $C^+(g \cdot h, e\,|\,T)$ and $C^+(h, e\,|\,T)$ when $\langle g \cdot h, e, T\rangle$

is an HD-test in which g and h are separable given T. More precisely, Glymour sees the task of salvaging hypothetico-deductivism as finding some reasonable constraint on P-3 that blocks the inference to P-3a, with P-l seeming to point out what is needed.

Is there some useful sense of evidential support that justifies Glymour's aversion to P-3a? I think that indeed there may be, but only if Glymour's notion of this is sharply distinguished from ordinary confirmation, and even then not without some attenuation of P-l. As *I* understand it—and I have never encountered any applied usage to the contrary—"confirmation" is simply credibility enhancement. And in this ordinary sense, one can generally confirm the whole of a conjunctive proposition simply by increasing the credibility of one component, just as a proposition can be refuted (maximally disconfirmed) by disproving any one of its consequences. It does not follow that e confirms *every* uncertain hypothesis h of which it confirms a part, for complications can arise when e is not entailed by h. (See cases where $C^+(g, e\,|\,T)$ but $h = g \cdot {\sim} e$.) But whenever h does entail an uncertain e to which it is not equivalent, h is equivalent to $e \cdot r$ for some residual r of h separable from e (take $r = (e \vee g) \supset h$ for arbitrary g), and the joint uncertainty of $e \cdot r$ can be construed as the uncertainty of r given e exacerbated by the uncertainty of e. Then the credibility of h, i.e. of $e \cdot r$, given e is greater than the prior credibility of h simply because learning e eliminates the e-part of h's uncertainty while leaving its r-given-e remainder unaffected. (This is an elementary theorem of Bayesian confirmation, and might well be treated as a condition of adequacy on any alternative to the Bayesian model.) So in the credibility-enhancement sense of "confirmation", the only thing at all amiss in P-3 and its consequences P-3a, 4 is reading 'HD-test' where the proper precondition is 'CR-test'. Henceforth I shall designate this emendation of P-3 as "P_c-3".

But P_c-3 is not hard-core hypothetico-deductivism, although it serves as Potemkin Village for the latter. Rather, what makes the hypothetico-deductive outlook on theory appraisal an epistemological disaster is its slide from the triviality of $P_c - 3$ to the fallacy of

> *Proposal* 5 (*The Hypothetico-deductive Confirmational Pervasiveness Presumption*). Whenever $\langle g \cdot h,\, e,\, T\rangle$ is a CR-test of $g \cdot h$, not only $C^+(g \cdot h,\, e\,|\,T)$ but also $C^+(g,\, e\,|\,T)$ so long as g is uncertain given T.

But P-5 is incompatible with P_c-3. For, let r be any proposition that is *not* confirmed by e given T even though ${\sim} r$ is uncertain given $e \cdot T$ and e is uncertain given T. Then $C^+(e \cdot r,\, e\,|\,T)$ by P_c-3 and hence, by P-5, also $C^+(r,\, e\,|\,T)$ contrary to assumption. That is, under P-5, any evidence can confirm essentially any proposition r with which e is compatible through the mediation of hypothesis $e \cdot r$. Success of P_c-3 and failure of P-5 are two sides of the same coin: if a hypothesis h

can be confirmed merely by verifying one of its uncertain consequences, then verifying one of h's uncertain consequences cannot suffice to confirm other uncertain parts of h separable from the first. Even so, that is exactly the praxis of scientific inference—the *only* praxis—that hypothetico-deductivism urges upon us.

Yet hold on. Isn't the whole *point* of creating and appraising hypotheses in science to pass from the credence conferred upon the ones our observations support to increased confidence that the rest of what these tell us may also be true? The Popperian outlook on theory testing, which appears to be the guiding force behind Glymour (1975) and Merrill (1979), demands that such tests be tough and thorough. A *proper* test of hypothesis h by evidence e (given T) should be one that penetrates throughout h, that confronts *all* parts of h, not just some minor fragment thereof. That is why Merrill sought to free h of irrelevancies for e before allowing e to confirm h, why Glymour finds P-3a so absurd to his way of thinking, and perhaps why, at the very outset of philosophical confirmation theory, Hempel (1945) intuited that any model of confirmation should satisfy the Special Consequence Condition that confirming a hypothesis h also confirms each consequence of h.

In short, if we continue to understand "confirmation" in the weak (though, I insist, basic) sense of holistic credibility enhancement for which P_c-3 is truistic, Glymour and others evidently yearn for a much stronger kind of epistemic support whose ideal would be something like

Definition. Evidence e *pervasively confirms* (more briefly, *p-confirms*) hypothesis h given theory/background T iff h is uncertain given T and $C^+(g,\, e\,|\,T)$ for every consequence g of $h \cdot T$ that is uncertain given T.

But p-confirmation, so defined, is much *too* ideal. For with e uncertain given T, e p-confirms h given T only if h is certain given $e \cdot T$. Otherwise, if g is h or any other consequence of $h \cdot T$ that is uncertain given $e \cdot T$, $e \supset g$, i.e. $\sim(e\cdot \sim g)$, is an uncertain consequence of $h \cdot T$ which is disconfirmed by e given T. For if g is uncertain given $e \cdot T$, $\sim(e\cdot \sim g)$ too is uncertain given $e \cdot T$; hence with uncertainty of e given T also stipulated, $\langle e \cdot \sim g,\, e,\, T\rangle$ is a CR-test of $e\cdot \sim g$ by which, under P_c-3, e confirms $e\cdot \sim g$ given T and thus disconfirms $e \cdot g$ given T. That is, given T, any evidence that confirms a hypothesis h without conclusively verifying it necessarily disconfirms some consequence of h.

Similarly, if e and g are consequences of $h \cdot T$ with g and $\sim g$ both uncertain given T and e uncertain given $g \cdot T$, $\langle \sim g,\, g \supset e,\, T\rangle$ is a CR-test by which, under P_c-3, $g \supset e$ is a consequence of $e \cdot T$ and hence of $h \cdot T$ that confirms $\sim g$ given T and hence disconfirms g given T. This says that given T, any evidence e not entailed by (more precisely that is uncertain conditional on) an unsettled conjecture g has a consequence that disconfirms g—whence in particular, every proper part of an unsettled hypothesis h is disconfirmed by some other consequence of h. Unlike disconfirmation of $e \supset g$ by e, this latter construction need not jeopardize thorough confirmation of h by any consequence of h that might genuinely be the evidence under consideration. But it does motivate attempting to distinguish evidence that is "natural" from artifices that arise in practice only as irrelevant spin-off (i.e. any $g \supset e$ entailed by e) from the data e we actually interpret.

Despite the confirmational perversity of material conditionals, it is still possible to retain the spirit of p-confirmation while evading its extremistic degeneracy. Insomuch as the recalcitrant consequences of $h \cdot T$ are unnatural contrivances whose disconfirmation by natural evidence e seems irrelevant to whether e confirms everything of epistemic significance in h given T, we simply try to ignore the former when adjudicating the latter. Any implementation of this proposal will instantiate the following generic concept of "τ-support", in which 'τ' is heuristic for "thorough" or "total" while demarking an open parameter:

Definition, Let τ be some fixed function on 3-tuples of propositions whose value $\tau(h, e \,|\, T)$ for each argument $\langle h, e, T\rangle$ is some subset of the consequences of $h \cdot T$ such that no proposition in $\tau(h, e \,|\, T)$ is equivalent given T to $e \supset g$ for any consequence g of $h \cdot T$. Then e τ-*supports* h *given* T iff (a) $C^{+}(h, e \,|\, T)$ and (b) $C^{+}(g, e \,|\, T)$ for every g in $\tau(h, e \,|\, T)$ that is uncertain given T.

That is, $\tau(h, e \,|\, T)$ picks out all consequences of $h \cdot T$ of a certain sort that we deem relevant to e's confirmational bearing on h given T; and for e to τ-support h given T we require e to confirm-given-T not merely h but everything in $\tau(h, e \,|\, T)$ that T has not already settled. The generic constraint on T in this definition yields that e disconfirms-given-T no g in $\tau(h, e \,|\, T)$ by virtue of $\sim g$ entailing e given T. For if $\sim g \cdot T \vdash e$, g is equivalent to $e \supset g$ given T and is hence excluded from $\tau(h, e \,|\, T)$. So when $\langle h, e, T\rangle$ is an HD-test or CR-test, P-3 or P_c-3 allows that e can indeed τ-support h given T even when h is uncertain given $e \cdot T$.

When appraising how thoroughly evidence e confirms the assorted constituents of hypothesis h given theory/background T, the reason for absolving e of any obligation to support any $e \supset g$ entailed by $h \cdot T$ is clear. (When g is certain given $e \cdot T$, e does not disconfirm $e \supset g$ given T but cannot confirm it either, insomuch as $e \supset g$ is then certain given T). But there may well be additional entailments of $h \cdot T$ (e.g. some or all of $\{r \supset g\}$ where $h \cdot T \vdash g$ and $C^{+}(r, e \,|\, T)$ even though not $r \cdot T \vdash e$) that we also wish to disregard when assessing the quality of e's support for h given T. Precisely what those exclusions should be is far from evident, but τ-support is parametrically open to negotiations in that regard.

Indeed, the generic definition of τ-support is rather too open, insomuch as τ accepts instantiations under which τ-support is identical with ordinary confirmation, namely, when for all $\langle h, e, T\rangle$, $\tau(h, e \,|\, T)$ includes at most h. If $\tau(h, e \,|\, T)$ is to itemize everything in $h \cdot T$ that merits e's confirmational scrutiny given T, we want to require further that each subset $\tau(h, e \,|\, T)$ of h's consequences under T entails h given T but is not limited to h. And we will also see reason to desire that each $\tau(h, e|T)$ be nonredundant in that no proposition in $\tau(h, e|T)$ is entailed given T by the remainder of $\tau(h, e \,|\, T)$. Therefore, say that selector function T is *standard* iff it satisfies these two additional constraints over whatever portion of τ's domain is salient for the confirmation-theoretic purpose at hand. Beyond that, the intuition that $\tau(h, e \,|\, T)$ should pick out an *epistemic basis* for h given T whenever e consists of natural evidence further delimits the acceptable interpretations of T.

Were I to pursue this matter more deeply here, I would expand upon my arguments elsewhere (Rozeboom 1968, 1971) that rational inference is ineluctably grounded upon commitments to a *becausal* (explanatory) ordering of propositions that includes first of all analytic (de dicto) dependencies, and beyond that causal determinations conditional upon factual premises of the nomic sort developed by scientific theories presumably paradigmatic of the T to which Glymourian confirmation is relative. If so, some consequences of h given T are fully explained by others (e.g. conjunctions and disjunctions by their components); and one preferred specification of τ might be that whenever e is natural, $\tau(h, e \,|\, T)$ is to comprise the largest subtuple of consequences of $h \cdot T$ such that no proposition in $\tau(h, e \,|\, T)$ is fully explained given T by some subtuple of the remainder of $\tau(h, e \,|\, T)$. Whether this is the only good way to cash out intuitions about h's "epistemic basis" given T, or what special conditions may need to obtain before all desiderata on $\tau(h, e \,|\, T)$ can be simultaneously realized, are issues for some other occasion.

If I am correct in having argued that Glymour, Merrill, and perhaps others take hypothesis testing to be a discriminatingly detailed appraisal that yields full-blooded confirmation only when it strengthens our confidence throughout the foundations of the hypothesis at issue, then surely some version of τ-support is what Glymour has in mind when he speaks of "confirmation". For unless he seeks to sever confirmation/disconfirmation from credibility change altogether, his intuitive requirements for e to confirm h given T must place some restrictions on what parts of $h \cdot T$ the confirming evidence e is allowed to discredit or be irrelevant to. Those constraints then define a specification τ_G of τ that needs only Glymour's assent to its sufficiency as well as necessity for conclusion that his confirmation is τ_G-support.

If so, three points remain for closing comment. First, Glymour's proclaimed objections to hypothetico-deductive orthodoxy: his rejection of P-3, $3a$, 4 is entirely reasonable if confirmation therein is converted to τ-support. But it remains unclear why he should begrudge that e cannot τ-support h under T if h is already certain given T. And P-l is scarcely more appetizing for τ-support than for ordinary confirmation. Perhaps Glymour, and before him Merrill, has twisted the intuition that

> Given T, e's τ-supporting h does not suffice for e also to τ-support $g \cdot h$ when g and h are separable, not even when $\langle g \cdot h,\ e,\ T \rangle$ is a CR-test,

which is sound enough for τ-support in contrast to ordinary confirmation, into the non-starter that

> Given T, e's τ-supporting h precludes e's also τ-supporting $g \cdot h$ if g and h are separable.

Can anything else be said on P-l's behalf?

Secondly, when Glymour and I both decry hypothetico-deductivism, are we protesting the same thing? Prima facie it would not seem so; for Glymour directs his quarrel at P-3 whereas I hold P-3 (or rather P_c-3) to be unobjectionable and claim instead that hypothetico-deductivism's failure lies in its tacit urging of P-5. But if, to suppress terminological differences, our positions are respectively recast as

	The Hypothetico-deductive Fallacy
Rozeboom:	Confounding ordinary confirmation with p-confirmation;
Glymour:	Taking verification of any uncertain consequence to suffice for τ_G-support;

little variance remains between us, and I am prepared to split the difference. But perhaps that should not be for us to say. What do you folks who still believe in it take hypothetico-deductivism to be?

Finally, we must not overlook that the operational problems of theory appraisal remain untouched by debating what does or does not constitute hypothetico-deductivism and what, precisely, is wrong with it. To put this point into focus, let me suggest a new version of hypothetico-deductivism that neither Glymour nor I have yet impugned. For any standard version of τ-support that includes in $\tau(h,\ e \,|\, T)$ for natural e the important consequences of h given T, consider

Proposal 6 (τ-*based hypothetico-deductivism*). Evidence e τ-supports hypothesis h under theory/background T whenever (a) $\langle h, e, T \rangle$ is a CR-test and (b) no proper subset of $\tau(h,\ e \,|\, T)$ entails e given T.

(Standard τ is needed here first of all to ensure that $\tau(h,\ e \,|\, T)$ entails h given T, and secondly to make it possible—as it would not be were $\tau(h,\ e \,|\, T)$ redundant—that h but no proper subset of $\tau(h,\ e \,|\, T)$ entails e given T.) P-6 is essentially the model of confirmation sought by Merrill, and escapes Glymour's *absurdum* put-down of his original. In fact, for all I know, P-6 may be true for at least some nontrivial choices of standard τ. It is certainly not a theorem of Bayesian confirmation theory; but rational belief requires something more than just diachronic adjustments run by conditionalization on synchronically coherent but otherwise arbitrary prior credibilities, and perhaps that something-more provides the added constraints needed for P-6 to hold.

Actually, for reasons that are unimportant here, I very much doubt that P-6 will prove tenable for any nontrivial standard τ. But the salient objection to P-6 is simply that, even if true, it is useless. To guide and justify data interpretation in scientific practice we want plausible confirmation theories under which, for at least some serious hypotheses h considered in light of assumptions T that seem reasonable enough for us to act upon, we can conceive of an open-ended sequence $e_1, e_2, \ldots$ of possible data that do not entail h given T but for which the credibility of h given $e_1 \cdot \ldots \cdot e_n \cdot T$ $(n = 1, 2, \ldots)$ increases with n to a level of assurance approaching practical certainty. For any such $\langle h, T, e_1, e_2, \ldots \rangle$, let us say that h is operationally verifiable by $e_1, e_2, \ldots$ given T. (Note that if h is operationally verifiable by $e_1, e_2, \ldots$ given T, then so is every initially-uncertain consequence of $h \cdot T$.) And say also that an account θ of inference principles is a theory of *operational ampliation* with scope S iff S is a suitably restricted but nontrivial class of hypothesis/presupposition pairs $\langle h, T \rangle$ for which θ identifies at least one possible data sequence $e_1, e_2, \ldots$ by which h is operationally verifiable given T and θ. We need theories of operational ampliation because conclusions derived nondeductively but with high confidence from hard evidence are not merely a primary pursuit of natural science, criminal law, medicine, industrial technology, and other disciplines that professionalize epistemic engineering, but also, correctly or not, are in sociological fact often felt to be attained.

And that is why hypothetico-deductivism—even a defensible version like P-6—is a counterproductive distraction. For it is not even remotely a theory of operational ampliation. Being informed merely that, for each $n = 1, 2, \ldots, e_{(n+1)}$ confirms or even τ-supports h given $e_1 \cdot \ldots \cdot e_n \cdot T$ tells us virtually nothing about the character of the credibility increments so wrought. Qualitative confirmation as such counts for little; what really matters in applied hypothesis appraisals is for us to develop some feeling for how much confidence our evidence $e_1, \ldots, e_n$ warrants under T for each separable component of h, with special concern for the *differential* support h's varied contents thereby receive (see Rozeboom, 1970, pp. 98-102; also Glymour, 1975, p. 426).

Statistical induction is the one sector of scientific inference for which usable theories of operational ampliation have become available. But the scope of statistical ampliation is severely limited in that, roughly speaking, statistical conclusions contain no descriptive predicates that do not occur in the evidence from which they are derived. Statistical induction does not license our frequent *theoretical* abductions that explain our observations by appeal to their alleged underlying sources. Historically, hypothetico-deductivism has been a sugar teat to dull our hunger for some way to vouchsafe such explanations, and that philosophers should have clung to it, despite its lack of epistemic nourishment, so long as no alternative less stultifying than Humean skepticism has been in sight, is entirely understandable. But gratifyingly expansive alternatives *are* available. As I have shown elsewhere (Rozeboom, 1961; 1966, pp. 201ff.; 1972), determinately patterned abductions wherein data compel their own explanatory interpretations are prevalent in technical science and everyday life. To bring theoretical conclusions within the scope of operational ampliation theories no less articulate and forceful than what we have already attained for statistical reasoning, we need only to shake off the beguilement of hypothetico-deductivism and look with care at what, inferentially, practicing scientists actually *do.*

References

Carnap, R. (1962). *Logical foundations of probability.* 2nd. ed. Chicago: University of Chicago Press.

Glymour, C. (1975). Relevant evidence. *Journal of Philosophy*, *72*, 403–426.

Glymour, C. (1980). Hypothetico-deductivism is hopeless. *Philosophy of Science*, *47*, 322–325.

Hempel, C. G. (1945). Studies in the logic of confirmation. *Mind*, *54*, 97–120.

Merrill, G. (1979). Confirmation and predication? *Philosophy of Science*, *46*, 98–117.

Rozeboom, W. W. (1961). Ontological induction and the logical typology of scientific variables. *Philosophy of Science*, *28*, 337-377.

Rozeboom, W. W. (1966). Scaling theory and the nature of measurement. *Synthese*, *16*, 170–233.

Rozeboom, W. W. (1970). The art of metascience, or, What should a psychological theory be? In J. R. Royce (Ed.), *Toward unification in psychology.* Toronto: Toronto University Press.

Rozeboom, W. W. (1972). Scientific inference: The myth and the reality. In R. S. Brown & D. J. Brenner (Eds.), *Science, psychology, and communication: Essays honoring William Stephenson.* New York: Teachers College Press.