

Hypothetico-Deductivism Is a Fraud

Kimble (March 1989) stated, “The only alternative to hypothetico-deductive theorizing that I can think of is the radical empiricistic approach” (p. 498). This may well be true of Kimble's ken, but certainly not because no third alternative exists in practice. Over the years, I have repeatedly argued that although the traditional (Popperian) hypothetico-deductive model of theory development is perniciously vacuous (Rozeboom, 1970, pp. 90-102, 1972, pp. 100-105, 1982), effective real-world reasoning at all levels of sophistication embodies principles by which we *discover* (or confirm)—not fantasize—the underlying sources of observed events through the structure they impose on their effects (see especially Rozeboom, 1961, 1972, 1973; also Williams, 1986). Among philosophers of science, the epistemic fecklessness of hypothetico-deductive thinking has remained a dirty little secret, scarcely ever mentioned in public (but see Glymour, 1980; Hesse, 1970) because academic philosophers have had little notion of more operational guidelines for scientific theorizing (cf. recent philosophical views on “inference to the best explanation,” which do not have a clue as to what should count as best or even good). But experienced scientists whose expert intuitions have not been stunted by doctrinaire radical empiricism on one hand or by simplistic hypothetico-deductivism on the other no more require philosophers' models to guide their explanatory inductions than one needs training in Gestalt principles to perceive patterns.

Even so—and this is why I am taking Kimble to task for what is a very small part of his well-motivated essay—it is important for two reasons to make explanatory induction's logic explicit and give it pride of place as our premier account of scientific reasoning. First, the inferences we make with deft confidence under favorable circumstances—statistical and elliptically deductive arguments as well as explanatory inductions—become disturbingly problematic when based on inferior evidence. (Consider the ease of interpreting large-sample vs. small-sample statistics.) How well we practice explanatory induction in tough cases or contrive to set up its preconditions of reliability cannot help but profit from a thoughtfully articulate study of its methodology. (Consider science's gain from the development of statistical theory.)

Second, hypothetico-deductivism is not a harmless textbook fable; it does real damage when students are taught to appraise theories this holistically. Explanatory induction has no quarrel with imaginative hypotheses, for when properly analyzed these alert us to prospects for

provocative data patterning whose existence might otherwise escape our recognition. What explanatory induction does decry is the foolish notion that when a hypothesis H implies some unexpected empirical phenomenon that proves to be roughly as predicted, this significantly confirms every one of the distinguishable propositions explicitly or implicitly conjoined in H. Explanatory induction welcomes the phenomenon, but insists that we heed all of its interpretable details, not just fragments most congenial to H, and that our interpretation take pains to distinguish explanatory conclusions that the extant data substantially *warrant* (which may or may not be among H's central tenets and which may or may not be points of contention between H and its main rivals) from conjectures in H that these data leave no more plausible, if not less so, than before. Only in this way can explanatory speculations be purified and annealed into theories provisionally worth *believing*. I leave it for you to judge how often in psychology's modern climate of hypothetico-deductive licentiousness we have managed to achieve the latter.

This is scarcely the place for a crash course in explanatory induction's operational specifics. But before that can be installed in our graduate methodology education with the same respect (though, it is hoped, less rote indoctrination) now given to sampling statistics, our spokespersons on professional affairs must first come to appreciate its practical importance if we still aspire for psychology to be a hard science.

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