

Logic for the Clinician

Theodore R. Sarbin, Ronald Taft, and Daniel E. Bailey

Clinical Inference and Cognitive Theory. New York: Holt, Rinehart and Winston, 1960. Pp. x + 293, \$5.50.

Reviewed by PAUL E. MEEHL

There are three authors. Sarbin is Professor of Psychology at the University of California in Berkeley. He published a methodological analysis of clinical prediction in 1944 and made the first systematic empirical research comparing clinical and actuarial methods in 1941. Taft is a Reader at the University of Western Australia. He has been concerned with the ability to judge others. He and Sarbin together published An Essay on Inference in the Psychological Sciences (Garden Library Press, 1952). Bailey is Research Associate at the University of California in Berkeley. With Block he has developed an actuarially-based set of Q-sort descriptions appropriate for MMPI interpretation. The reviewer, Paul Meehl, now President of the American Psychological Association, is known to everyone. He is Professor of Psychology at the University of Minnesota and Professor of Clinical Psychology at its Medical School. He calls himself a "hybrid clinician and rat-psychologist," but of course he is well known for his contributions in the logic of science and on psychological models. He is a Diplomate in Clinical Psychology and has served on the American Board of Examiners in Professional Psychology, the first psychologist to serve on that Board who was not a 'Grandfather' Diplomate.

This book formulates all clinical inference in terms of a general theory of cognition, based mainly upon the ideas of Brunswik and Tolman and bringing together research from clinical, social, perceptual, and learning psychology, but its ambitious aim to provide an adequate reconstruction has not been achieved because the authors' analysis suffers from four fundamental and pervasive errors.

First is their confusion of psychological and logical questions. Although aware of Reichenbach's distinction between the contexts of discovery and justification (pp. 77, 80, 84), they rarely make it, leaving the reader unclear as to when he is being offered causal analysis and when logical reconstruction. Such metalinguistic terms as *postulate*, *inference*, *syllogism*, and *premise* are applied to nonlinguistic events, a departure in "usage from that of the classical logician" (p. 46) which is deliberate. "Inference is the cognitive transformation of one set of events through another set.... This does not imply that the process ... is accessible to self-report" (p. 45). "Some of the propositions ... cannot [!] be expressed in sentences" (p. 46). The use of another discipline's technical terms in a way expressly forbidden within that discipline requires a very strong defense, which is not provided. By such odd semantics they dispose of clinical intuition and creative hypothesis-formation, incorrectly attributing to Allport and Meehl the preposterous notion that such events "emerge from the void without knowable antecedents" (p. 82) or are "without a history or evolution" (p. 187)—views specifically rejected in the very monograph they are discussing (See P. E. Meehl, *Clinical versus Statistical Prediction*, Univ. Minn. Press, 1954, pp. 51, 53, 58f., 82).

Secondly, all valid inference is reduced to the categorical syllogism, a restriction harmonious with the authors' preference for "taxonomic" clinical examples. "All fulminating schizophrenics are dangerous; this man is a fulminating schizophrenic; therefore, this man is dangerous," p. 52). This is not the kind of 'hard case' to examine when analyzing

clinical inference! Structural-dynamic hypotheses involving construct variables rather than behavioral predicates are avoided. "The equivalence of other syllogistic forms to the categorical can be demonstrated" (p. 52) is true in a sense far too abstract to justify their taxonomic approach.

Thirdly, empirical knowledge is grossly oversimplified by identifying all non-deductive inference with simple enumerative "induction" [= frequency-counts]. This is the book's core mistake, which rationalizes its simplistic treatment of the clinician as knower. For example, clinicians are said to validate a hypothesized father-identification by determining whether the "length of the list of similarities exceeds some arbitrary value" (p. 233). I assume that other psychotherapists will find this 'frequency' model of corroboration as ludicrous as I do. All nonsyllogistic inference except enumerative "induction" is labeled "non-inductive" and considered fallacious. I am not here raising a 'philosophical' issue to criticize a substantive 'psychological' theory; the book is consciously epistemological and its cognitive model is defended by philosophical arguments, mostly unsound.

A large group of clinical inferences (including most of the 'hard cases' for a purely actuarial view) are called "fallacious" because they "affirm the consequent" (pp. 233, 234, 235, 266). Are the authors really unaware that *all* empirical inference is in the third figure of the hypothetical syllogism and hence (formally) "invalid"? This is precisely what differentiates formal and empirical sciences, producing the statisticians' controversies over Fisher and the logicians' puzzles about confirmation. The form: p [nomothetic theory or idiographic hypothesis] entails q [experimental or naturalistic observation]; q [observation made]; 'therefore' p [hypothesis corroborated], is the standard model of empirical inference in science, history, the law courts, and common life. This 'therefore' is obviously not intended as the *ergo* of deduction. If the authors' syllogistic criterion were generally thus misapplied, all human knowledge (except pure

logic and mathematics) would be discarded as “fallacious.” Whatever one’s views on inductive logic, *no* reconstruction can afford to classify the third figure as illegitimate in the empirical domain. Even the logician Karl Popper, who disbelieves in any special inductive logic (e.g., Catnap’s ‘degree of confirmation’), readily allows the third figure as corroborative.

Fourthly, the authors assume that all theoretical constructs are susceptible of a purely abstractive analysis, other types being condemned as “fictions” (pp. 232, 233, 236). To denigrate clinicians as “not always rational” (p. 72), their constructions as “arbitrary” or “whimsical” (p. 233), and third-figure corroborations as merely “affective” or producing a “glow experience” (p. 235), on the ground that such hypothesizing does not fit an *epistemological* model which is itself considered unsound by most logicians, is pretty high-handed!

In clinical settings, it is not uncommon for a behavior analyst to instantiate an occurrence and collocate the resulting minor premise with a major premise containing a fictional entity. For example, the occurrence about Jones, “talks excessively,” provides the minor premise: “Jones is a member of a species characterized by excessive talk.” If this is collocated with the major: “all persons who talk excessively are orally fixated,” then the inference follows that Jones is orally fixated. But oral fixation is a dispositional construct, a fiction. How can the behavior analyst determine if the statement has truth-value? If the fictional entity *appears* to be congruent with expectations in the general postulate-system of the inferrer, then the inferrer may regard his inference as having validity. In this instance, if the analyst believes that all humans may be characterized by placement on a dimension of psychosexual maturity, and if orality is measured by any oral manifestation that is intense or frequent, then the conclusion is regarded as possessing intrinsic validity.

A moment’s reflection reveals the fallacy in this chain of reasoning. The occurrence which was specified as the operational measurement of the construct has no *necessary* relationship to the construct. “Excessive talking” may be related to an infinite number of fictive antecedents. To borrow from a medieval theory, the major premise might be, “A person who talks excessively is possessed by demons.” Therefore, Jones is possessed by demons. (P. 232.)

This tendentious passage is typical of the book’s anti-psychoanalytic bias (how is it that a treatise on clinical inference indexes Meehl’s name 19 times, Cronbach’s 8, Estes’ 5, but Freud’s only twice?) The inference from “talking excessively” to “orally fixated” is *not* deductive, nor would any competent psychoanalyst assert the “major premise.” Furthermore, if “fiction” is defined epistemologically (i.e., as a construction by human minds trying to make causal sense of observations), then fictions are not necessarily ‘bad,’ although a particular fiction may

be rejected on scientific grounds. Caloric no longer appears in the nomological network of physics, having been replaced by the fiction of molecular motion. If, on the other hand, the term *fiction* is meant scientifically (e.g., Don Quixote is ‘fictional,’ because no such person existed), then the construct “oral fixation” must be examined on its merits. The authors easily settle a difficult empirical question (Is there an entity = oral fixation?) by invoking an irrelevant epistemological truism (Construct-terms are not in the observation-language).

The possibility of differential empirical tests is rejected:

In order to eliminate such alternate constructs, the clinician may seek confirmation of his conclusion by noting other instances which are presumably related to the construct. To pursue the same illustration: Jones has been observed picking his teeth. “Picking one’s teeth” is regarded as another exemplar of oral fixation. The conclusion then “affirms” the first conclusion: Jones is orally fixated.

The persistence of the employment of congruent validity is no doubt influenced by the fact that clinicians create and use one set of conjectures and not another. Suppose the same minor premise had been collocated with a major premise containing a different construct: persons characterized by cleanliness are anally fixated; Jones picks his teeth (a form of cleanliness—dental hygiene); therefore Jones is anally fixated.

In short, defining an occurrence term as an instance of a dispositional construct is arbitrary and may even be whimsical. It is simply not possible to attach an empirical truth value to clinical inferences in which the predicate is fictive. (Pp. 232f.)

Thus the mere existence of multiple hypotheses capable of explaining the same facts is taken to demonstrate the illegitimacy of hypothesis-construction, as if science and inductive logic lacked methods of differential corroboration leading to rational choice.

The nondeducibility of explanatory hypotheses from their corroborating explananda is *not* a matter on which tastes may differ or ‘philosophies of science’ conflict but is a matter of formal logic and just about as controversial as the binomial theorem. When we say that explanatory hypotheses are only “suggested” by the facts, we are not only making a psychological comment in the context of discovery; we express also a *logical* relation in the context of justification, namely, that the entailment is only one-way.

Restricting their clinical examples to the taxonomic and dispositional becomes difficult for these authors when the three “strategies of search for input” (scanning, scrutinizing, and probing) are analyzed (pp. 154–159). In this otherwise insightful discussion, theoretical constructs are spoken of as if somehow peculiarly observable. “In probing, the perceptual apparatus is directed toward occurrences that are not on the surface but must be uncovered in

order to make an inference" (p. 156). The clinical "inferer ... removes emitted but irrelevant occurrences [a neat trick!] from the ecological surface which may conceal relevant events on the sub-surface" (p. 157). The ecological surface is "more finely inspected" and "broken" (p. 175) by suitable interview tactics. Such locutions, which convey an impression that interviewers peel off outer layers of a personality to 'see' what's underneath, are necessitated by the poverty-stricken logical apparatus. Adopting this picture would retard development of mathematical and structural models suitable for formulating and testing psychodynamic theory. One does not "inspect" unconscious behavior determinants by removing some sort of obfuscating shell; he *hypothesizes* them.

We must insist upon this methodological point regardless of our views on current psychodynamic theory, because the latter can be neither corroborated *nor* disconfirmed without the invention of models which, while more rigorous and explicit than present verbal formulations, possess a structural richness adequate to represent the essential substance.

The authors also avoid historical reconstructions and hypotheses involving complex *content* in exemplifying the model. A case can be made that personology is more akin to history than to either botany or physics, a view with which the book never really comes to grips, thereby neglecting a rich source of counter-actuarial support. Personologists like Allport, Holt, McArthur, and Murray cannot be expected to accept even the few 'historical' examples analyzed, since those chosen do not reflect the distinctively historical content and method of clinical reconstructions. "Severe child-rearing practices produce neurosis ... early toilet-training is one of those practices ... a late toilet-trained person will be free from neurosis" (p. 123). Perhaps the clinician's process of historical reconstruction can be analyzed into components of this kind but such a claim must be tested upon the complicated instances clinicians adduce.

The authors' attitude toward historical and documentary method is revealed in the following passage:

The idiographic method—the truth-value of which depends upon internal consistency—is the method of history, biography, and literature. In these enterprises inferences need have no future reference. Logical coherence (congruent validity) is the test of the truth-value of a proposition. But when inferences have a future referent—when decisions are likely to follow from inferences—then prediction becomes the pragmatic test of truth. The novelist or biographer is justified in using internal consistency as a criterion; we require something more from the behavior scientist. (P. 256.)

Here again, a difficult methodological issue is settled by fiat. Is the *time* relation between a hypothesis' invention and observing one of its

implications relevant to the degree of corroboration? Logicians disagree. Simon thinks it is, Carnap thinks not, Feigl is undecided. How can the authors make such a debatable point a test of who is a "behavior scientist"?

The stimulating chapter on "modules" as the basic cognitive unit which has a stochastic isomorphism with the "ecology" might provide tools sufficient for analyzing structural-dynamic hypotheses, but instead the purely taxonomic and dispositional orientation prevails, so nothing interesting happens. The reductionistic bias of the authors appears even in the actuarial context, where they quickly dismiss configural scoring (p. 242) without understanding it. The essential feature of configural prediction systems as originally defined by Meehl and rigorously generalized by Horst, Lubin, and others is the nonvanishing of second-order mixed partial derivatives (corresponding to interaction effects in the discontinuous case). Allegedly, such systems can be handled by linear regression through population subdivision by a cut on the configured variables' difference distribution. That is, a configural function $F(x,y) = ax + bxy + cy$ is represented by two linear functions $f_k(x,y) = k_x x + k_y y$ and $f_m(x,y) = m_x x + m_y y$, the decision to apply f_k or f_m being determined by an inequality $(x - y) > d$. This implies that two sets of N_k and N_m simultaneous linear equations in two unknowns always have solutions, which is of course false.

All in all the book is very disappointing. The authors have surveyed and summarized the relevant research, but they integrate it by means of an inadequate logical model. Whatever plausibility the resulting conceptualization has is achieved by ignoring the really interesting cases of personological inference. As a move in the history of ideas about clinical cognition, we must regretfully rate the volume as essentially retrogressive.

pdf by lly March 2003