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Do We Need Critical Relativism?

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Comments

Do We Need Critical Relativism? Comments on "On Method in Consumer Research"

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The opening quote from Paul Feyerabend in Anderson's (1986) presentation of the critical relativists' perspective leaves the impression that it is only *fear* that keeps us from embracing a relativistic view in a post-positivistic scientific world. Yet we are told that critical relativism rejects the basic premise ". . . that there is a single knowable reality" and asserts that it is wrong to assume ". . . that 'truth' plays a role in the ontology of critical relativism" (p. 157). It is this casual attitude toward *truth* and *reality* that leads many scientists to reject critical relativism.

In the place of "truth" and "reality" the critical relativists rely on the conventional agreements among scientists. Without denying the importance that the scientific culture has in what is published and accepted in a research domain, one can still seek something firmer in science. To do otherwise would be to confuse the sociology and psychology of scientists with the philosophy of science.

At its core, scientific endeavor rests on having a viable epistemology. It is not my purpose to debate the critical relativist's epistemological claims. But an understanding of the epistemological events that led to the demise of logical positivism can show us what needs fixing and what does not. This is the topic of the next section. The last section proposes a simple solution that corresponds to what many scientists have been doing to advance knowledge in a post-positivistic era.

THE DEMISE OF LOGICAL POSITIVISM

The logical positivist tradition suffered three blows whose cumulative impact took an inexorable toll.[1] Whitehead and Russell's (1910–1913) *Principia Mathematica* attempted to reduce all mathematics to logic. The idea was to establish mathematical logic as the language that explained all scientific theories. If successful, this would provide an algebraically elegant and apparently certain basis for theory. The first blow was delivered by Gödel (1931). His theorem demonstrated that there were formally undecidable propositions in *Principia Mathematica.* This means that theory requires something other than algebra. The representation theorems of mathematical psychology and the algebraic speculations of mathematical economics can be informative, but without other sources of evidence, they are not science. But this was not a fatal blow in itself since logical positivists recognized observational terms (entities, processes, or states we observe or measure) as well as hypothetical terms (concepts or constructs). Using operational definitions to relate observational terms (such as a Likert scale rating) to hypothetical terms (such as behavioral intentions or attitudes) seemed to provide the positivist tradition with the kind of certainty it desired.

The second blow came from Carnap (1936, 1937). He demonstrated that operational definitions did not work for dispositional terms. Consider the dispositional phrase "consumer acceptance." The operational definition would be that *a brand is acceptable* if and only if it satisfies the following conditions: if the brand is consumed, the consumer reports the brand *acceptable.* The truth of the phrase "a brand is acceptable" rests on the truth of the conditional phrase "if the brand is consumed, the consumer reports the brand *acceptable.*" This conditional phrase is true whenever the antecedent (if the brand is consumed) is true *and* the consequent (the brand is reported *acceptable*) is true. But the phrase

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[1]The discussion in this section is based on Suppe 1977 and is presented more fully in Cooper and Levine 1985.

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is also considered to be logically true whenever the antecedent is false (cf. Quine 1953). Thus a brand is considered acceptable if it is never consumed. This ridiculous state of affairs (called the *contrafactual positive*) is true for *all* operational definitions.

The operational definition was replaced by the reduction sentence, which provides a partial, open definition of scientific terms. The corresponding reduction sentence for *consumer acceptance* would be: a brand is acceptable if the brand is consumed *and* is rated *acceptable.* One would come up with other reduction sentences to deal with situations in which the consumer is not the direct judge of acceptance (e.g., infant products, pet products). The style of definition is open and partial, since we can always do more research and write more reduction sentences. The more reduction sentences, the more completely defined the term would be.

But even this progressive revision could not save logical positivism from the final blows. The reduction sentence still relied on the distinction between observational terms and hypothetical terms. The final blows were administered by Putnam (1962) and Achinstein (1965, 1968). They demonstrated that no tenable distinction has been drawn between theoretical terms and observational terms. Observation is preconditioned by theoretical cognition and intention. This *theory ladenness* of observation removed much of the interest of philosophers of science in logical positivism because one could no longer evaluate the *truth* of scientific propositions.

BUILDING SCIENTIFIC KNOWLEDGE

So in the final analysis the problem reduces to the indefensible distinction between hypothetical terms and observational terms. We have at least two alternatives. First we could attempt to develop some tenable distinction between these kind of terms. This would amount to attempting a rescue of logical positivism. I am uncertain that this can be done, or that it is worth doing.

Second, we can simply recognize that there is only one type of term, not two. The meaning of all terms is *relational,* not *stipulative.* A scientific term is not synonymous with the set of operations used to measure it (a stipulative form of definition), for the measures themselves are scientific terms and must gain meaning through psychometric study and use in other contexts. So the meaning of measures and the constructs to which they *relate* is open, and we specify their meaning more fully as we *relate* them to other constructs or networks of constructs and measures.

Justifying the meaning of our measures and constructs is what we do all the time in introductory or literature review sections of our journal articles. We then proceed with the report of findings in a particular research context. What we report we use as evidence. Evidence flows osmotically from any area of higher concentration to any area of lower concentration—increasing or decreasing our belief in our assertions.

Our methods of science may be rigorous, but within a single research context we only build scientific belief, not the degree of certainty we would wish to assert the "truth" (i.e., justified true belief) of a proposition. Scientific belief can come from statistical inference, reliable measurement, experimentation, or prior theory or research in a *domain* (Shapere 1977) of inquiry. The truth of an assertion can be evaluated against the collective evidence in the domain. Practitioners routinely judge when sufficient certainty exists to provide a basis for managerial decision making. And scientists can judge the obstacles to increasing our certainty. We know quite well the research required to make our findings more general or certain. Perhaps it is the unwillingness of our journals to publish replications or minor variations that keeps us from pursuing issues more tenaciously. But there is an extremely important difference between the practical impediments to seeking truth and the assertion of the critical relativists that there is no truth worth seeking.

We, as scientists, do not need to embrace a critical relativists' perspective.

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