

## Operationism: A Recipe for Reducing Confusion and Ambiguity

Howard. H. Kendler

*University of California, Santa Barbara*

Leahey's insistence on viewing operationism within a global philosophical framework prevents him from perceiving the empirical and theoretical benefits of operational analysis. He fails to comprehend that the purpose of operationism is not to achieve what a concept *should* mean but instead what a concept, as used, *does* mean. His treatment of *intentionality* as a critical case against operationism proves, upon examination, to expose the limitations of this concept for a natural-science psychology. Leahey's hopes of clarifying the methodology of psychology by the combined use of the concept *ideology* ("false consciousness") and psychotherapeutic techniques are doomed to failure because the intrinsic ambiguity of the argument will inevitably substitute purely rational conclusions for needed empirical evidence.

My goal, in these final remarks, will be to illuminate the methodological disagreements between Leahey and myself. My rejoinder (Kendler, 1981a) to his original paper (Leahey, 1980) was intended to delineate the beneficial effects of operational analysis. Equating a concept's meaning with the operations performed by a scientist when investigating the concept can do much to identify sources of ambiguity and confusion in scientific communication. This point was illustrated by brief operational analyses of two concepts: *intelligence* and *self actualization*.

Leahey finds fault with my analyses for specific reasons that will soon be discussed. More importantly, he believes, contrary to my own view, that operational analysis cannot be detached from larger philosophical positions such as nominalism, realism, positivism, ideology, etc. This disagreement stems from the different frameworks that we employ to view operationism. My career in psychology has primarily been that of a researcher and theorist whereas Leahey's major concern has been with the philosophical underpinnings of psychology. This recognition neither implies an invidious comparison nor a necessary incompatibility between the two orientations. But in the present controversy operationism is being judged by entirely different standards which emerge from contrasting views of psychology and methodology.

### Operational Analysis

In reaction to my treatment of intelligence Leahey (1981) writes that I stay "close to operationism" and end up "in a blind alley" because my "argument

demonstrates one of [operationism's] well-known failings; that it proliferates concepts which we believe ought to be the same but which by operational definition are rendered completely distinct." Leahey should have used the pronoun "I" instead of "we" for the simple reason that for the empiricist the aim of operational analysis is not to achieve agreement but to clarify meaning. To suggest that researchers in the field of intelligence shared a common meaning is incorrect as an examination of the early history of intelligence testing reveals (Tuddenham, 1962).

Galton and Cattell were influenced by the associationistic view that complex ideas are combinations of elementary sensations. This preconception led to the belief that tests of sensory powers could reflect intellectual ability. Their tests however failed to correlate with any reasonable criterion of intelligence and hence were soon ignored.

Alfred Binet's early work in intelligence testing stemmed from the theoretical assumptions of faculty psychology which postulated that the mind is endowed with a number of independent powers (faculties). Binet's preconception encouraged him to construct individual tests for "faculties of the mind" (e.g., imagery, memory, imagination). Unlike Galton and Cattell, Binet was not willing to accept his theoretical preconceptions as automatically valid. His intuitions were that scores on a valid test of intelligence should increase with age and correlate with school performance. When his faculty-inspired test failed to correlate closely with age and school performance, Binet was forced to choose between the faculty theory and his intuitive notions. Pressures to identify children who were unable to profit from regular work in elementary schools encouraged Binet to opt in favor of the plan to construct his famous test of intelligence on the bases of his intuitions rather than on the faculty theory of intelligence.

Leahey is correct in concluding that the operational definition of Binet's test—*intelligence is what the intelligence test measures*—is tautological; but he is incorrect in suggesting that such a definition ends up "in a blind alley." The circularity cannot be avoided but it can be overcome by demonstrating the usefulness of the tautologically defined test in a variety of situations. Tests that have their origins in Binet's test have demonstrated a versatility that justifies their use; their predictive powers of academic success are not limited to the early years of elementary school but to the entire range of education, including college, professional schools, and graduate school. They also have proved effective in selecting individuals for special jobs in government, industry, and the military and in diagnosing psychological disorders.

The correlation between scores on intelligence tests and school grades are not perfect, thus supporting the hypothesis that variables other than academic talent are involved in school performance. This expectation is supported by the fact that intelligence test scores correlate better with objective educational

test scores than with subjective school grades, and also better with grades in intellectually oriented courses such as science and English than with grades in practical courses (e.g., home economics, shop work).

In sum, differences in intuitive conceptions of intelligence led to differences in operational definitions and those definitions (e.g., Binet's test) which proved empirically fruitful survived. These fruitful definitions not only served practical purposes but also contributed important notions to the theoretical nature of intelligence. It is difficult to understand the objections to an operational analysis of intelligence except if it threatens preconceived notions as to what intelligence is and what the facts should be.

Although Leahey acknowledges "the excellence and acuteness of" my treatment of Maslow's concept of self actualization (we do not disagree about everything!), he argues that it "has nothing to do with operationism." This surprising conclusion stems from Leahey's insistence that operationism must be judged independently of other techniques of epistemological analyses. The fact remains however that only by operational analyses can one see through Maslow's seductive argument that a "scientific way" of discovering "the best set of values" is possible. The "scientific way" of identifying the best set of values proves to be, by operational analysis, simply an expression of Maslow's personal ethical preferences (Kendler, 1980). Leahey's complaint that you cannot reject Maslow's conclusions on operational grounds is true. But operationism, as already noted, is designed to define concepts—clearly not to evaluate their theoretical potential. Operational analysis proves to be the first step in judging the merits of Maslow's proposal.

### Operationism and Philosophy

Leahey's obsession with global philosophical issues blinds him to other problems. For example, in the beginning of his second reply, he mistakenly transforms an innocuous remark of mine into a denial of metaphysical assumptions about psychology. I merely objected to his criticism of Underwood (Leahey, 1980) for failing to consider the operational views of "Bridgman, Carnap, or Hempel." Underwood has been an exceedingly active and productive researcher in the field of memory and like all active scholars must make decisions about how his time commitments should be distributed. Must he be condemned for discussing operationsim from his own research perspective because he fails to consider the views of philosophers on Leahey's required list of readings?

Leahey's accusation that I deny metaphysical assumptions in psychology is also surprising because he admits to having read my recent book (Kendler, 1981b, p. 371) which concludes that "Psychology is a multidisciplinary field with different segments employing irreconcilable orientations" and "A

choice of competing methodological alternatives cannot be made by purely rational means." If one acknowledges that different conceptions of the subject matter of psychology, as well as modes of interpreting psychological events, are possible, does not one recognize different metaphysical views of psychology? Although I appreciate being paired with Isaac Newton, I must reject the reasons for it.

The rigidity of Leahey's analysis is also expressed in his need to assign my methodological position to some philosophical category. I am not offended by being diagnosed a *positivist* and *nominalist* but do believe that the classifications are somewhat inappropriate. My self-image, whatever value it has, is not that of an extreme positivist; I believe that there are limits to science but nevertheless insist on what many anti-positivist psychologists refuse to acknowledge, namely, that the reliable knowledge that natural science generates cannot be duplicated by other modes of inquiry.

In preference to the designation *nominalist* I would prefer my own whimsical classification, *sophisticated naive realist*. I believe that a reality exists beyond my sense observations but I am sufficiently sophisticated to know that I cannot prove this assumption. At the same time I believe a strategic advantage accrues to the theorist whose efforts are guided by talented guesses about the nature of "reality" rather than by irrelevant analogies. An examination of the history of theoretical psychology suggests that the greatest hope of formulating a natural science theory of psychology is via a biological approach (Kendler, 1981b).

In spite of my conviction that a biological approach has strategic advantages over black-box formulations, I do not believe that a discussion of the relative merits of nominalism versus realism is particularly important for the research psychologist because it is often impossible to categorize a theory as belonging to one of these two metaphysical positions (e.g., models of genetic transmission prior to the discovery of DNA, Hullian theory). The crucial issue is not between nominalism and realism as Leahey suggests but instead the criteria one adopts for achieving understanding (explanation), a topic that Leahey, in his wide ranging criticism of operationism, chooses to ignore. By ignoring this issue, he fails to address the problems inherent in two of his favorite concepts: *intentionality* and *ideology*.

### The Noncritical Case: Intentionality

One of Leahey's major arguments against operationism is that intentionality, "a perfectly clear psychological concept . . . cannot be operationally defined" (Leahey, 1983, p. 87). He never entertains the possibility that the trouble lies with the concept of *intentionality*, as he defines it, and not with operationism. Intentionality is a phenomenological concept and suffers from

the limitations of all mentalistic constructs based *solely* on the observations of the experiencing individual. Because universal agreement about the mental status of *intentionality* fails to prevail one can question Leahey's judgment that the concept is "perfectly clear."

Titchener suggested that Brentano's conception of the mind was generated by an inappropriate methodology: "The act-and-content psychology . . . [is] a psychology not of observation but of reflection" (Titchener, 1909, p. 53). In other words, Brentano was not accurately observing conscious experience; his reports were contaminated by an inappropriate perspective. I am not arguing that Titchener is right, and Brentano is wrong. I am simply noting that the history of psychology provides evidence to suggest that consensual agreement about the nature of mental events is impossible to attain with *phenomenological evidence alone*. Because different methods of phenomenological research offer different pictures of the mind, we must conclude that we do not really have different methods of phenomenology, but actually different phenomenologies (Kendler, 1981a).

The failure to achieve consensual agreement about phenomenological descriptions does not imply that such descriptions are valueless or invalid or that conscious experience is an illusion. Although the dividing line between natural science and non-natural science is not precise, and probably can never be exact, the historical evidence strongly suggests that a psychology that depends exclusively upon phenomenological reports cannot yield reliable knowledge of the sort that physics, chemistry, and biology have generated.

If one seeks reliable knowledge while being convinced that intentionality is a core psychological concept then intentionality must be operationalized either as an *inferred* mental event or as a physiological process. Then an empirical testable model of the mind could be produced that would allow one to evaluate the usefulness of intentionality. If, however, one wants to preserve the immeasurability of intentionality then one is essentially arguing against the possibility of a natural science psychology (Kendler, in press). But it should be noted that such an argument will not dissuade those who pursue the natural science path. One would hope that those who employ the concept of intentionality can offer a constructive methodological program that will identify the kind of knowledge that is capable of being generated and the rules for judging the truth value of theoretical statements.

It should also be noted that Leahey's discussion of intentionality and its neurophysiological basis tilts toward the position of denying any relationship between the two. We agree that intentional states are "ineluctably mental"; a feeling of depression is not equivalent to the biochemical conditions of the brain to which the experience is associated. But a relationship between the two cannot reasonably be denied. Leahey's analysis approaches such a denial when he suggests that his "thinking of Washington, D.C.," could not have a

physiological basis because he could think of the city in different ways. Essentially he is denying common neural events when thinking about the same city. What purpose is served by disembodiment of thoughts to such an extent?

### **Ideology, Psychoanalysis, and Science**

I am not suggesting that producing a natural science psychology is easy. Our history does not reveal major theoretical breakthroughs of the sort that occurred in other sciences (e.g., genetics). One has the choice of pursuing this noble goal with as much ingenuity and persistence as possible or shifting away from natural science psychology in hope that some instant miracles will be forthcoming. To look to Marxism and psychoanalysis for the salvation of psychology as a natural science can only be described as seeking success through failures. Although Marxism and psychoanalysis are major intellectual, and even literary, achievements they both fall far short of offering scientifically valid interpretations of the phenomena they seek to explain. One important example is the failure of Marxist predictions about the polarization of the class struggle in industrial capitalistic societies to materialize. In spite of valiant efforts of some psychoanalytically-inspired researchers to test the truth value of Freudian formulation, that formulation still remains, after 80 years, unable to meet the standards of falsifiability; the theory is easy to "confirm," but impossible to refute.

The argument, stemming from the social philosophy of Habermas, that psychotherapeutic techniques can be employed to reveal misleading ideologies about the nature of psychology and science is unconvincing. It lacks operational criteria to differentiate the "false consciousness" of ideologies from "valid" conceptions of reality. The concepts are so ambiguous and the procedures are so elastic that it only encourages those who are attracted to these "obscure arguments" (Skinner, 1982) to find what they seek: that their own preconceptions about psychology, science, and society are correct. Purely rational arguments are substituted for supporting empirical evidence.

The alternative I offer psychologists (Kendler, 1981b) is less elusive. Psychologists have choices in regards to the subject matter of their discipline, the criterion of truth to adopt, and the social responsibility of their profession. Agreement about these methodological decisions can neither be demanded nor necessarily achieved. Operational analysis, however, can serve as a useful tool in understanding the different kinds of knowledge that contemporary psychologists are offering to their audience. To those who have never employed operational analysis, I can only suggest, "Try it, you'll like it!"

### References

- Kendler, H.H. Self fulfillment: Psychological fact or moral prescription? *Academic Psychology Bulletin*, 1980, 2, 287-295.
- Kendler, H.H. The reality of operationism: A rejoinder. *The Journal of Mind and Behavior*, 1981, 2(3), 331-334.(a)
- Kendler, H.H. *Psychology: A science in conflict*. New York: Oxford, 1981.(b)
- Kendler, H.H. Behaviorism and psychology: An uneasy alliance. In S. Koch and D.T. Leary (Eds.), *A century of psychology as science*. New York: McGraw-Hill, (in press).
- Leahey, T.H. The myth of operationism. *The Journal of Mind and Behavior*, 1980, 1(2), 127-143.
- Leahey, T.H. Operationism still isn't real: A temporary reply to Kendler. *The Journal of Mind and Behavior*, 1981, 2(3), 343-348.
- Leahey, T.H. Operationism and ideology: Reply to Kendler. *The Journal of Mind and Behavior*, 1983, 4(1), 81-90.
- Skinner, Q. Habermas's reformation. *The New York Review of Books*, 1982, 29(15), 35-38.
- Tuddenham, R.D. The nature and measurement of intelligence. In L. Postman (Ed.), *Psychology in the making: Histories of selected research problems*. New York: Knopf, 1962.