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The Myth of Operationism

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Psychologists' belief in operational definition has remained unshaken since its inception in the 1930's despite numerous other changes in psychological systems. This paper argues that continued faith in operationism is unjustified. First, operational definition was quickly abandoned by its proponents, the Logical Positivists, as an impossibly rigid requirement to impose on any scientific theory. Second, the definitions given by psychologists as operational are valid and useful, but are often not operational. Third, operational definitions are really low-level laws, not definitions. It is concluded that continued advocacy and pretended use of operational definition is merely liturgical, and obscures important aspects of actual scientific practice.

The concept of operational definition has a powerful hold on contemporary psychologists. Students quickly learn to think it a methodological touchstone and a necessary guarantor of scientific rigor for any theory or experiment, and this attitude persists into professional adulthood. Behaviorists and Cognitivists may agree on little else, but both believe in careful 'operationalization' of theoretical terms. It is widely believed that a term that cannot be operationally defined must be excluded from the circle of science. What I will argue here is that faith in operationism is faith in a myth, and that in practice we are all heretics anyway. Strict operational definition is an impossible requirement for any science; furthermore, the definitions we call "operational" are not operational in the correct sense. "Operational definition" has become an empty liturgical phrase that obscures significant aspects of scientific practice and serves only to protect psychologists from the ever present fear that psychology is not a science.

Historical Background

Operationism and Logical Positivism

The term "operational definition" was introduced by the physicist Percy Bridgman in his *The Logic of Modern Physics* (1927). Bridgman made only narrow use of it, however, and later dissociated himself from the movement of Operationism (e.g., Bridgman, 1945). The same basic idea, however, was also developed by the Vienna Circle Logical Positivists, as Operationists themselves later acknowledged (e.g., Stevens, 1939/1976). They were motivated by the same concerns as Bridgman, but placed

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operational definition in a sophisticated general theory of science and language that had widespread influence. For all these reasons I will examine the development of operational definition within Logical Positivism, reserving until later the dissolution of the Positivist program.

It had been the goal of Positivists since the time of Comte (Comte, ?/1975) to eliminate theological and metaphysical "nonsense" from language, or at least from serious philosophical discourse. Positivism is a radical form of empiricism, which believes observation to be both the psychological origin and justifying ground of all knowledge. In this spirit Comte argued that science should aim at accurate description of unquestionable empirical facts, rather than at explanation based on hypothetical entities or processes. The physicist-philosopher Ernst Mach eschewed Comte's political excesses, but developed a careful radical positivism that reduced all knowledge statements to sensations (Mach, 1886/1959). In debates over the atomic hypothesis in the late nineteenth century, Mach refused to accept atoms as legitimate concepts, asking "Have you ever seen one?" (Blackmore, 1972). It has been argued (Stevens, 1935a) that Mach's radical sensationism provided the basis for Einstein's attack on Newton's hypothetical concepts of absolute space and time (but for a contrary view see Zahar, 1977). In psychology, Radical Behaviorism (e.g., Skinner, 1953) continues to uphold Mach's extreme positivism.

Nevertheless, despite Mach, chemists accepted atoms, and as twentieth century science developed, it became clear that theoretical concepts could not be given up for pure description and correlation. But the readmission of theoretical terms seemed to open the door to the crime of metaphysics. Since theoretical terms refer to unseen entities just as metaphysical terms do, the problem arose of legitimating meaningful scientific theory while continuing to exclude metaphysical nonsense. How can we allow the scientist to say "atom" without letting the Hegelian philosopher say "Absolute Spirit"?

The Logical Positivists sought to resolve this dilemma. They divided the terms of language into two mutually exclusive categories, *observation terms* that refer to what is directly observable (e.g., "yellow"; "53 centimeters"), and *theoretical terms* that refer to proposed hypothetical entities whether scientific (e.g., "length"; "proton") or metaphysical (e.g., "Absolute Spirit"). Then they proposed that a term was to be considered meaningful only if it were either an observation term (and hence reliable knowledge) or could be defined in terms of observations (and hence tied to reliable knowledge). Terms which failed to meet this criterion of meaning were dismissed as gibberish. Thus, a proton may be defined as a particular kind of observable track in a cloud chamber; Absolute Spirit cannot be similarly defined.[1]

In the positivist view, then, Science is a two tiered affair. On one level

[1]The Logical Positivist's theory of meaning is quite similar to the pragmatic conception of truth advocated by Peirce (1905/1970) and James (1907/1970) although only the latter's ac-

there is a theory, a collection of theoretical sentences in which theoretical terms are connected by using logical connectives (e.g., "and"; "equals"). The only constraint on theoretical axioms was the syntax of formal logic, and axioms were considered to be only analytically true or false. In Newtonian physics, for example, Force = Mass x Acceleration (F = M x A) is a theoretical sentence; so is Hull's ${}_{S}E_{R} = {}_{S}H_{R} \times D$. The other level, providing the root and ground for theory, consists of observation sentences describing observable states of affairs, such as "This ball weighs 2 kg" or "This rat has not eaten in 24 hours." Observation sentences are synthetically true or false, since their verification depends on empirical fact. A theoretical term is to be defined by establishing a correspondence rule linking it to some observation; such linkage guarantees that the term is not empirically empty metaphysics. So, "restmass" would be defined as "stationary weight in grams at sea level," or "drive" would be defined as "so many hours of food deprivation." Correspondence rules are also called "mixed sentences" because they contain one theoretical term, being defined, and one observational term, doing the defining. As definitions, mixed sentences are analytically true or false.

As summarized by Suppe (1974, p. 17), "Correspondence rules serve three functions in the Received View (Logical Positivism): first, they define theoretical terms; second, they guarantee the cognitive significance of theoretical terms; third, they specify the admissable experimental procedures for applying a theory to phenomena." Psychologists tend to focus only on the first function, subsuming the other two under it. However, each posed separate problems for the Positivists as they strove to formulate acceptable correspondence rules. Wrestling with these problems caused most of the changes in the Received View after its founding by the Vienna Circle.

The first formulation given to correspondence rules was explicit definition which was much the same as Bridgman's operational definition. For any theoretical term *T* there was to be provided an observable operation and result that completely defined it. So "length" might be completely defined by the operation of applying a meter stick to an object and reading off the length. Or "drive" would be completely defined as the operation of depriving a rat of food for *x* hours. In the logical formula required by the Received View, an operational definition must have the form

$$(x)(Tx \equiv Ox)$$

where O is an observation or operation. So, for hunger we would have

$$(x)\ (Hx \equiv Dx)e$$

or, an animal *x* is hungry if and only if he is deprived of food. According to Logical Positivism, such explicit definitions carry the entire meaning of a concept; "surplus meaning," meaning not opperationally tied to direct observation and therefore fraught with metaphysical dangers, is excised. As Bridgman put it: "We mean by a concept nothing more than a set of

count includes subjective factors in experience. It is interesting to note that Peirce was originally a physicist, and that James admired Mach.

operations" (quoted by Suppe, 1974, p. 19).

However, as Carnap pointed out in 1936 (see Hempel, 1950/1965) explicic definition fails as a correspondence rule for dispositional terms. The classic example was "fragile." An explicit operational definition of fragile must be

$$Fx \equiv (t)(Sxt \supset Bxt)$$

or, "An object x is fragile if and only if, if at time t the object is struck, then object x breaks." But, by the logic of the conditional ($\supset$) an if-then statement is true whenever its antecedent (Sxt) is false. Therefore, any unstruck object (Sxt is false; so that Sxt $\supset$ Bxt is true) must be considered fragile (Fx is true).

Before turning to Carnap's proposed solution to the problem, we should note certain special difficulties for Bridgman's formulation of operational definitions. First, they share the problem of defining dispositional terms, since they take the same form as explicit definitions (e.g., "A person x is anxious if and only if, at time t he takes the Taylor Manifest Anxiety Test, he scores in the 90th percentile." All operational definitions of personality traits will share this problem.). Second, science often has several operations for the same concept length may be measured by a meter stick or triangulation; intelligence by the Stanford-Binet or the WISC. But if an operation completely defines a concept, there will be as many concepts as operations: two distinct concepts of length or intelligence will be created by the two different operationalizations of each. Concepts will necessarily proliferate as new measurement techniques are devised. Although Bridgman accepted this consequence, it is an implausible description of science, for scientists act as if there is only one concept measured in interchangeable ways. Operationists might propose that various specific operational definitions be subsumed under a more general definition. So there might be a super-concept "length" subsuming all measurement operationalizations. But such an approach is unworkable. For if the superconcept is operationally definable, the others become unnecessary; if it is not, then it is not meaningful. Moreover, in many cases there is no way to show two operations to be equivalent. No one will ever be able to measure the distance to the sun with a meter stick, and IQ scores on different tests differ enough to cause uncertainty that all "intelligence" tests are measuring the same thing.

Carnap proposed to replace explicit definitions with reduction sentences. Reduction sentences are *not* definitions because they "specify the meaning of a term only conditionally or partially" (Hempel, 1950/1965, p. 109). "Fragile" would have as a reduction sentence

$$(x)(t)\ [Sxt \supset (Fx \equiv Bxt)]$$

or "For any object x, if at time t it is struck, then x is fragile if and only if it breaks." Similarly, "anxiety" might be reduced

$$(x)(t)\ [Txt \supset (Ax \equiv Pxt)]$$

or "for any person x, if at time t he or she takes the Taylor Manifest Anxiety Test, then x is anxious if and only if he or she scores over a critical

percentile."

Reduction sentences avoid the difficulty of untested objects or persons by leaving the theoretical term undecided when the test has not been applied. Furthermore, a single concept may be given many reduction sentences, one for each relevant operation (e.g., striking, dropping; Taylor test, GSR). Thus each reduction sentence only *partially* defines a theoretical term. As long as new operations may be invented, a term is incompletely defined. Moreover, because operations only partially define a term, the door is open to surplus meaning, that is, meaning not tied to empirical facts. Theoretical meaning is allowable under partial interpretation (Suppe, 1971); so-called "surplus" meaning is rehabilitated.

Psychologists have only rarely followed the Received View so far, and further changes in positivist criteria of cognitive significance have been virtually ignored. As positivism continued to develop, reduction sentences were rejected in their turn. First, practicing physicists usually introduce new terms by fitting them into an overall theoretical system, not by defining them piecemeal *via* reduction sentences. Second, certain terms accepted in physics, such as the ψ function in quantum physics, cannot be reduced to observation (Suppe, 1974). Third, on the basis of a highly technical argument, Hempel (1950/1965) showed that metric terms cannot be given reduction sentence formulation.

So by 1954 Hempel was arguing that it is impossible to require term-by-term reduction of theoretical concepts to observable tests. Instead, only larger "interpretive systems," or whole theories, could be required to be cognitively significant, that is, to be confirmable. But in that case "the originally intended sharp distinction between cognitively meaningful and cognitively meaningless hypotheses (or systems of such) has to be abandoned; and it appears doubtful whether the distinction between analytic [theoretical and mixed] and synthetic [observational] sentences can be effectively maintained in reference to the language of empirical sciences" (Hempel, 1954/1965, p. 133).

Logical Positivism had failed in its original aim — to sharply excise meaningless metaphysics from philosophy and science. Other philosophers attacked operationism in revealing ways, and we shall return to them after surveying operationism in psychology.

Operationism in Psychology

Bridgman's original formulation of operational definition was introduced to psychology by S.S. Stevens in 1935(a). It was immediately applied to psychological concepts by Boring (1935), McGeoch (1935), and Stevens himself (1935b). Stevens began by noting how unstable and prone to revolution psychology had proved to be, and he offered operationism as "the revolution that will put an end to the possibility of revolution" (1935a, p. 323). Stevens stayed close to Bridgman's approach, maintaining that "what is essential to the notion of *meaning* is wholly a matter of operations" (p. 326), and embracing the proliferation of concepts entailed

by strict operationism. Like the positivists, Stevens distinguished "empirical" concepts, which are scientific and acceptable, from "hypothetical" concepts which are "mere fictions."

In Stevens' view, "It is the sole business of psychology to test and measure the discriminatory capacities of the organism" (p. 325), because discrimination is the fundamental operation of science. Thus, psychology plays an important role in regulating all scientific practice because psychology can "achieve more and more exact definitions of terms through a more precise formulation of the operations which constitute the criteria of a term's applicability," through "discriminatory analysis" of observers (p. 326). By 1939 Stevens asked "does it not appear that the Science of Science must go directly to psychology for an answer to many of its problems?" Stevens' suggestion would restore psychology to its place as the fundamental science given it by David Hume (1739/1817). More specifically, Stevens asked is not "behavioristic psychology . . . the only one that can be of much help in this enterprise?" (Stevens, 1939/1963, p. 73), establishing behaviorism as not only the only correct scientific psychology, but the only correct account of scientific practice.

At the same time, Clark Hull (e.g., 1935) was introducing to psychology another aspect of the Received View, its requirement for axiomatized theory. Although he did not refer to operationism, the essential similarity of the two approaches was quickly recognized (Stevens, 1939/1963; Bergmann & Spence, 1941; Koch, 1941; Bergmann, 1951). Hence, after 1940, psychological discussions of operationism, although retaining the term, drew heavily on Logical Positivism. The move away from Bridgman's operationism was hastened by his extension of valid operational procedures to include mental operations (Bridgman, 1936) which Behaviorists could not accept. Bridgman (1945) dissociated himself from the movement of Operationism, saying it went far beyond his original modest proposal.

Although psychologists turned to the Positivists for guidance in building valid psychological theories, only rarely did they attend to changes in criteria of cognitive significance or grasp the importance of these changes. Sigmund Koch's master's thesis of 1939 (published 1941) discussed Carnap's replacement of explicit definition by reduction sentences. MacCorquodale's and Meehl's famous 1948 paper distinguishing intervening variables and hypothetical constructs followed Carnap's new usage for intervening variables, and said that hypothetical constructs, but not intervening variables, have "surplus meaning." Partial definition, however, can never exclude non-empirical meaning; thus their distinction fails. In any event, psychologists were disposed to transform hypothetical constructs "into operationally valid intervening variables, which are the only kinds of constructs ultimately admissible into sound scientific theory" (Marx, 1951, p. 246). Psychologists wanted to stay as close to pure operationism as possible. They refused to see the implications of Carnap's reduction sentences, still speaking of "operational definitions" in the old

way. And they persisted (and persist still) in attempting to "define" each theoretical term empirically even after the positivists had given it up for cognitively significant interpretive systems (Hempel, 1950/1965).

What is most remarkable in the historical record, however, is how quickly and with how little dissent operationism caught hold in psychology. In his 1939 thesis Koch wrote: "Almost every psychology sophomore knows it is bad form if reference to 'definition' is not qualified by the adjective 'operational' " (Koch, 1941, p. 15). Koch also makes clear the appeal of operationism: "Hitch the constructs appearing in your postulates to a field of empirical fact, and only then do you get a scientific theory" (p. 17). To scientifically insecure psychologists Koch's proposal meant not just that operationism was a prerequisite to science, but that it was the royal road to science.

Operationism quickly became the order of the day after Stevens' 1935 paper. Arguments about operationism usually concerned its proper use, not its validity. McGeoch (1937) noted its faults but said it was "the best available procedure for the construction of the concepts of psychology." McGeoch is clearly saying operationism gives us concepts in addition to testing them. Pennington and Finan (1940) accepted operationism but wanted to broaden it. Weber (1942) thought operationists dodged ontological questions, but offered no basic criticism or alternative. Skinner (1945) thought that operationism simply hadn't been used rigorously enough. Boring organized a symposium on operationism that occuped an entire issue (No. 5) of the 1945 *Psychological Review*. Of the eminent participants (Boring, Bridgman, Feigl, Israel, Pratt, and Skinner) only one (Israel) had serious reservations about the technique of operational definition, although there were differences over the details of its application.

Some deeper criticisms were made. Israel and Goldstein (1944), in a paper that occasioned the 1945 symposium, noted that "operationism had attained much greater prominence in psychology than in any of the other sciences" and that psychological operationists had overinflated operational definition into a "complete method" supposed to eliminate all dissent (p. 177). They asserted that psychologists used operational definition incorrectly. Bridgman had meant it to specify measurement operations (e.g., length), but psychologists use it to manipulate organisms to produce, not measure, phenomena (e.g., depriving an animal of food to produce hunger). At the 1945 symposium, Israel further argued that a term such as "purpose" could not be operationally defined.

In a comment on Boring's symposium, Prentice (1946) offered a brief attack on operationism itself. He argued that operationism stifles the development of new and useful theories by quashing all non-operational concepts. "So long as psychologists feel forced to supply operations where none exist, they cannot escape the dilemma: give up your concept (at least do not publish it) or supply inadequate definitions which confuse scientific thinking despite a spurious appearance of precision" (pp. 248-249). Prentice says that without operationism, psychologists could use meaningful

but non-operationally definable concepts and abandon them should they prove unfruitful. But "the operationist kibitzer here seemed to say 'You cannot even talk about such things in science unless you define them in operational terms. And I mean now' " (p. 249). Prentice concluded that the psychology of learning, in particular the Krechevsky-Spence debate over rat hypotheses, "has been snowed under by meaningless experiments and more meaningless argument" (p. 249).

Edward Newbury (1953) argued that operationism is not the theoretically neutral procedure its advocates pretended it to be. It makes assumptions about the nature of reality; it especially assumes the truth of physicalism. Given this assumption, psychological Behaviorism follows, for mind as a non-physical entity, cannot exist. Rather than merely choosing between alternative viewpoints as its adherents maintain, Operationism supports only one viewpoint, Behaviorism.

These criticisms did nothing to dislodge operationism from its place in psychology. In the 1950's operational definition was a regular part of scientific practice and analysis. Adequate operational definition became the most important, and sometimes the only, methodological touchstone of good theory. Theories that eluded operational definition, such as psychoanalysis, became suspect.

The last lengthy discussion of operationism by a psychologist was Underwood (1957, ch. 3). In his widely used experimental textbook, Underwood said that operationism needed a thorough restatement, observing that "surrounding the idea of operational definitions there is a general aura of righteousness" but that psychologists forced to defend them would be reduced to "mouthing a few cliches" (p. 51). Underwood's tone is dogmatic and forceful:

> Do I accept the practice of operationally defining concepts? It is not a mere matter of accepting or rejecting operationism, for to ask such a question is to ask whether one accepts science as a technique for understanding the laws of nature. Indeed, I would assert that a criterion of and whether or not a so-called empirical concept is a scientific concept is whether or not it has been operationally defined . . . Operational definitions . . . are the necessary base for science. (p. 52)

Underwood goes on to discuss six types of "operational definition" in a confused presentation. At no point does he refer to any of the philosophical roots of operational definition nor to any of the forms presented by Bridgman, Carnap, or Hempel. He says that each term must be operationally defined — clearly unaware of Hempel's argument for interpretive systems and against piecemeal operational definition. Moreover, he is unclear about whether concepts are to be defined by explicit definitions or reduction sentences. In discussing tests he fears that since each test could define a new concept, as an explicit definition, concepts would proliferate. He wants to guard against this by intercorrelating tests and calling those that correlate highly definitions of the same concept. As we have seen, in considering explicit definition, such intercorrelations will not work. Furthermore, he does not address the problem

of defining (presumably operationally) how high a "high" correlation is in this instance. Another problem he fails to consider is that things may be closely correlated but only incidentally, not causally, connected: When there is snow on the ground, there are no leaves on my oak tree.

A further difficulty arises when we consider an example of an approved operational definition, such as *closure*. Underwood offers Bobbitt's (1942) "operational definition" of closure: "He presented triangles with varying amounts of the perimeters missing and asked subjects to report when they did and did not see a triangle. He then defined closure as the amount of perimeter which must be present before 50 percent of the subjects reported seeing a triangle" (pp. 66-67).

Let us try to canonically formulate this approved operational definition. Since "closure" is clearly a dispositional term, we must use a reduction sentence, in which case, of course, the experiment no longer defines closure because reduction sentences are not full definitions. Operationally defined concepts must be entities (e.g., "proton") or attributes of entities (e.g., "fragile"). Closure is in the latter category. Because the operational manipulation was carried out on a series of triangles, "triangle" must be the object that has the attribute. Thus, the following reduction sentence might be proposed

$$(x) \equiv (Px \supset Tx)$$

or, "An object has 'closure' if and only if, if it is presented to several subjects, then at least 50% of them call it a triangle."

One trouble is immediately obvious. The object x is unspecified, so that anything, even a Mack truck, is a candidate for "closure" if it is called a "triangle" by 50% of subjects. Nor can x be specified as a triangle to begin with, because (a) the objects Bobbitt used were not really triangles and (b) it would be absurd to say a triangle is seen as closed if and only if, etc. Furthermore, squares, ovals, rectangles, circles, pictures of faces, also may show closure, but this definition does not touch them. Additional definitions would have to be specified for objects being seen as "circles," "squares," and so on by 50 percent of subjects, and we would get no nearer to defining the general property of closure. Presumably, Bobbitt's subjects, if asked is that *really* a triangle, would often reply, "No, but it's close enough," thus introducing further ambiguity to a supposedly rigorous, water-tight, scientific definition.

Clearly something is seriously wrong here; somehow "closure" is not being defined. Nor is repression, given Underwood's "operational definition": Repression is "defined" if "retention of unpleasant words [in a list] is poorer than the pleasant." But the reduction-sentence list here could clearly expand to infinity because people also repress traumatic experiences, dreams, Oedipal desires, unpleasant details of faculty meetings, and so on.

These difficulties support the criticisms of Prentice (1946) and Newbury (1953). In each of these cases operationism is ascribing to objects a psychological process meant to explain some behavior. As Prentice argued, to

define "hypothesis" as trial-and-error behavior is nonsense: To do so robs the concept of "hypothesis" of its point, which is to *explain* trial and error behavior. Similarly, closure (or more precisely, good continuation) is a psychological process that *explains* why we tend to close certain figures in perception; it is not the *same thing* as calling a figure a triangle 50 percent of the time. Likewise, repression is a process which *explains* why we forget certain unpleasant events; it is not the *same thing* as forgetting unpleasant word-lists. "Hypothesis," "closure," and "repression" are all theoretical terms that can be given intelligible, if non-operational definitions. Moreover, these concepts have empirical consequences in behavior related to learning, perceiving, and remembering. But to "define" these terms solely with reference to specific test behaviors is pointless and misleading.

These definitional practices betray the physicalistic and behaviorist bias of operationism pointed out by Newbury. The physicalist and behaviorist consider reference to mental processes illegitimate unless they are operationally defined. To identify "hypothesis" with trial and error behavior, "closure" with 50 percent of subjects calling x a triangle, and "repression" with relatively poor recall of unpleasant words, is to rob these terms of their explanatory raison d'etre. For if the term *is* the behavior, the term does not explain the behavior, in which case we may as well dispense with the terms altogether and refer only to the behaviors themselves. Thus, operationism supports physicalism and behaviorism. Naturally, this line of argument is used by Skinner (1953) for eliminating theory and mentalism from psychology. Nevertheless, it is not consistent with what physicists or, increasingly, with what psychologists actually do.

The problem of physicalistic bias was present in operationism from the beginning. Stevens (1935a) argues that the fundamental operation underlying all others is discrimination or, more philosophically, the act of denoting. He then claimed that "a term is vague for which there are many possible denotations, many different things to which one can point," for example, "pleasant" (p. 326). Stevens' attempt to refer the meaning of a concept entirely to the objects classed together, and then to measure "vagueness" operationally as a measurable aspect of that class, i.e., its size. According to Stevens' criterion, "proton" is a vaguer concept than "peasant" or "middle-class" because there are so many protons. But on the contrary, what makes a concept vague is not its size, but our uncertainty about its boundaries, and this uncertainty is not a physical feature of objects but a psychological feature of human beings.

Nevertheless, Underwood's discussion and Stevens' paper are classic references on operationism to which students are directed by more recent textbooks. Operational definition is a familiar and comfortable piece of furniture in the house of psychology, one that started to sink into the background as early as 1939 (Koch, 1941). It is discussed less openly today than it used to be, and students are referred to Underwood or Stevens for full discussions. But it still has its overt proponents. In the new edition of the widely used *Theories in Contemporary Psychology* (Marx & Goodson,

1976), Marx himself says that operationism is "logically unassailable," "fundamentally secure," and "an *ultimate* requirement" for scientific theory. Oddly enough, this is written by the editor of a book containing numerous articles assailing the fundamental tenets of positivism that supported operationism in the first place!

Whither Operationism?

Extra-Positivistic Criticism of Operationism

Non-positivist philosophers have offered serious criticisms of two related distinctions central to the Received View and operationism, the analytic-synthetic distinction and the theoretical-observational distinction. The former is central to positivism, for it asserts that theoretical statements and operational definitions are analytical statements, true by linguistic convention, as "A bachelor is an unmarried man" is true by convention. Synthetic statements are about observations, and their truth depends on empirical confirmation, e.g., "All bachelors are at least 5 ft. tall." This distinction underlies and helps support the theoretical-observational distinction which tries to guarantee the cognitive significance of theoretical concept by reducing their meaning to observable operations. Should either or both distinctions be impossible to draw, the need for operational links between supposedly separate theoretical and observational terms will vanish, and the supposedly analytical character of operational definitions will be undermined.

Beginning with Quine (1953/1961) both distinctions were scrutinized, although Quine concentrated on the analytic-synthetic distinction. Most of Quine's objections are philosophically technical but can be illustrated by examining operational practice in psychology.

An analytical statement is one whose truth can be defended by appealing only to the meaning of the terms found in the statement, and without regard to empirical fact, e.g., "A vixen is a female" (Achinstein, 1968). Underwood (1958) seems to accept that operational definitions are analytic statements, asserting that an "operational definition of mechanical aptitude, defined as performance on a test in which the questions are about Roman literature, is perfectly sound. That is, you can easily defend [it] . . . on grounds of strict operationism" (p. 60). Which is to say, the person proposing the definition could defend it on operational grounds regardless of empirical fact.

But can either example hold up? As Quine pointed out, appeal to linguistic convention, i.e., to a dictionary, rests on the lexicographers' empirical research into how people actually use a given word such as "vixen," or "female"; the lexicographer could be mistaken about current popular usage. The difficulty is even clearer in scientific operational definition. Belief that operational definition is purely definition, i.e., is analytic, has led to such uses as — "intelligence may be defined as the score made on the characteristic measured by a specific test" (Underwood, 1958, p. 61;

see also Spiker & McCandless, 1954). According to operationism, empirical evidence is irrelevant to constructing definitions. Hence, the belief that "Intelligence is what this test tests" can be defended against contrary evidence as purely a matter of definition, not research.

But can it? Of course a test maker could always insist on his definition despite its rejection by the scientific community. Indeed, Underwood suggests that the mechanical aptitude test based on Roman literature could not be justified on "social scientific" grounds. The test maker, by clinging to his idiosyncratic definition, would be cast out of the scientific community, and his definition would not become part of the linguistic conventions of science. The test maker must persuade the psychological community that his "definition" of mechanical aptitude is a good one. He or she does this by demonstrating its scientific or practical usefulness; e.g., people who do well on the mechanical aptitude test make good engineers. But this involves an appeal to empirical fact, and the "definition" will stand or fall on research outcomes. Thus, the ultimate defense of an "operational definition" is not an appeal to linguistic convention or logical analysis, but to scientific usefulness, which is a matter of empirical results. We find that operational definitions are not analytic truths, but are subject to empirical confirmation. This suggests that they are not "definitions" at all.

The erosion of the analytic/synthetic distinction undermined the theoretical-observational distinction and the associated positivist doctrine of cognitive significance, as we have seem Hempel (1954/1965) already acknowledge. Theoretical statements and correspondence rules, particularly operational definitions, were meant to be analytic, and the terms of the former were to be linked by the latter to synthetic observation statements. But if the analytic-synthetic distinction is blurry, so is the theoretical-observational one. So-called "theoretical" or "metaphysical" statements cannot be sharply demarcated from, and therefore defined only in terms of, observables.

Achinstein (1968) has effectively criticized the theoretical-observational distinction. His critique resembles Quine's in so far as he agrees that the distinction cannot be drawn in any absolute sense because what counts as observable or non-observable depends on a larger context not specifiable in advance. Therefore, in reporting the results of a cloud chamber experiment, a physicist might report, "I observed an electron," "I observed a path of ionized particles," or "I observed long thin lines." What one reports depends upon one's own background, that of one's hearer, the point of the experiment, and a theory of how the cloud chamber works. "Electron" may be either a theoretical or an observational term, depending on context.

Even what is directly observable may be questioned. I look into a telescope and see a moon of Jupiter not visible to the naked eye. Have I observed it? The answer to this depends on just how indirect one considers observation with a telescope. Certainly the Cardinals who rejected

Galileo's "observations" of Jovian moons thought them too indirect. If an optical telescope is "direct," what about a radio telescope? Or an optical vs. an electron microscope? What is "observable" and what is "theoretical" depend on situational constraints not philosophically specifiable; thus no absolute observational-theoretical distinction can be drawn.

This distinction has also been undermined by philosophers such as Feyerabend (1970), Hanson (1969), and Kuhn (1970), who, interestingly, appeal to psychological concepts and research for support. They argue that what one observes depends on one's pre-existing intellectual background and commitments, so that what is "observed" is shaped or even created by theoretical context. The idea that perception is shaped by culture, beliefs, values, needs, and motivational states should hardly be a novel claim to psychologists, going back as it does at least to the Gestalt psychologists and continuing through the New Look in perception and the work of Bransford and his associates (e.g., Bransford & Johnson, 1973). Yet, such findings, familiar to every introductory psychology student, undermine the observational-theoretical distinction and the positivist doctrine of cognitive significance that made the invention of correpondence rules necessary. Theory-free observations were to provide meaning to wholly abstract theoretical terms, not the other way around. Yet a certain cloud-chamber track becomes meaningful and cognitively significant only to someone with certain theoretical knowledge. And the same track may mean different things to different theoreticians. We must conclude that operational definitions, and correspondence rules more generally, simply do not exist. We cannot formulate important terms operationally, we cannot justify a definition without recourse to empirical evidence, and we cannot keep separate theory and observation. But psychological followers of Logical Positivism continue to "operationalize."

What "Operational Definnitions" Are

Operational definition is a myth, a remnant of an obsolete philosophy of science, but a myth that commands the allegiance of most psychologists. Yet this myth, like many myths, has some hidden substance. Testing for mental abilities; relating experimental findings to theory; depriving animals of food to get them to learn are all legitimate and important scientific practices, but they are not definitions.

Consider the classic operational definition, "Drive is 24 hours of food deprivation." Reflection shows it is simply *not* a definition in any sense of the term. One organism, such as a hummingbird, may be dead after 24 hours without food, while another, such as a boa constrictor, will still be satiated. The statement is fundamentally an empirical one covering only certain species, one whose truth must be discovered by experiment with different animals. The so-called "definition" expresses a reliable empirical generalization and is, thus, a low-level scientific law, not a definition. This law is assumed and used as a tool in learning research, and it might be the

direct object of investigation in motivational research. It is never a definition.

Some "operational definitions" are statements of an explanatory relationship. "Hypothesis" is meant to explain trial and error behavior, not to be defined by it. "Intelligence" or "anxiety" is meant to explain certain behaviors, including test behaviors, that go together. The behaviors do not define the explaining term.

Operationism's goal was to ensure that psychology was scientific by accepting the positivist analysis of science. The positivist recipe boiled down to attempting to methodologically guarantee that science, including psychology, be closely tied to, and tested against, empirical results. It is not necessary, however, to continue "operationalizing" (a barbarous usage anyway) to guarantee psychology's empirical character. All we need do is hold that any theory, considered as a whole, must help us understand, explain, and predict our experience, and that the best efforts of scientists must be devoted to improving the abilities of their theories to do just that. Theories and concepts will compete, and the inadequate theories will be discarded. No single methodological prescription can dictate in advance what form theories may take or how terms are to be defined without putting science in a straightjacket and inventing false distinctions.

Conclusion

In their pioneering attempt to understand the nature of science, the Logical Positivists made a fatal error. They tried to set *formal* criteria for what was science, and provide *formal* recipes for scientific progress. As their critics have amply shown, however, science can never be understood as a logical matter of constructing axioms and performing crucial experiments. A science is a complex human enterprise involving elements both conscious and unconscious, and this enterprise develops and proves itself over long stretches of time, not in isolated crucial tests. The formal distinctions of analytic vs. synthetic statements and observation vs. theory, the distinctions that necessitated inventing correspondence rules, simply do not help us understand what science is, how it works, how it progresses.

The positivists provided a blueprint for the establishment and practice of science. Psychologists, being perpetually insecure about their status as scientists, grasped at the first version of the positivist blueprint and have clung to it ever since. Operational definition was a central part of the blueprint and had a natural appeal to psychologists. The mind, psychology's initial subject matter, seemed an especially tricky thing to study, locked up inside each person's head. Behaviorism proposed that you could not erect a science on the slippery slopes of mind, that mind was metaphysical. Operational definition confirmed this. Science had to deal with what is observable, what could be counted, measured and weighed. Operationism helped exorcise the specter of the soul from psychology and put it on a supposedly firmer physicalist footing.

But in accepting the operationism as a central feature of science, psy-

chologists accepted the fatal error of Logical Positivism. They thought, contrary to their own research results, that observation is unassailably valid, unaffected by theoretical biases. Contrary to actual scientific practice, even in psychology, they thought that operationally defining something was a mere exercise in semantics. They accepted, even when they were not aware of it, the formalist program of Logical Positivism. The tombstone to psychological formalism is Hull's gargantuan axiomatic-deductive theory.

Psychologists, with the anti-historical bias of all scientists, forgot where operationism came from, the problems it was meant to solve. Similarly cut off from philosophy, they were ignorant of the changes in Logical Positivism and its own emtombment as the Received View. Psychologists felt that the impulse behind operationism was sound — that science should be empirical — and so they kept it as a sign that psychology is a science.

But removed from its historical context and stripped of its philosophical justification, operationism became a talisman, and "operational definition" a liturgical phrase. Continued use of the operational liturgy blinds psychologists to the nature of science as a pragmatic struggle of human minds against the facts of experience. Operationism correctly fixes our eyes on the data, but at the cost of drawing them from what we do as scientists. Clutching the talisman of operationism, we rarely reflect on the nature of our enterprise as scientists, and miss opportunities to improve it. Those who make reflective noises are rarely as rewarded as the narrow empirical researcher (Wachtel, 1980).

There is an old story about a man who visits a psychiatrist. The psychiatrist notices that the man claps his hands frequently and asks why. The client replies, "To keep the elephants away." To which the psychiatrist rejoins, "There are no elephants in New York." And the man says, "See, it works."

Undoubtedly the client's empirically supported faith in his hand-clapping made him feel secure and in control of his environment. It apparently succeeded in warding off a fearsome creature. I suggest that psychologists have always been haunted by the fear that psychology is not a science, and that they originally latched onto operational definition because it seemed to guarantee that good, i.e., operational, psychology would be scientific. As Koch wrote in 1941: "Hitch your constructs . . . to a field of empirical 'fact,' and only then do you get a scientific theory" (p. 17). But we continue to "operationalize" because we still fear that psychology is not a science.

The client's hand-clapping was a myth that seemed to work, that provided security, but which deluded him about the nature of reality. Similarly, operationism is a myth that seems to work, that offers security, but which deludes psychologists about the nature of science. The client must surrender his obsessive ritual to grow as a person; I hope psychology can surrender its operationist ritual to grow as a science.

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