CRITIQUE AND COMMENT

Operationism, the doctrine that a concept can best be defined in terms of a set of operations, has been the subject of much controversy since Bridgman first formulated it in 1927. The mixed reactions concerning the value of operationism, the variety of viewpoints that have gradually evolved, and the problems involved in this approach are examined in this paper.

OPERATIONISM AS METHODOLOGY

by Robert A. Plutchik

Hofstra University[1]

THE doctrine of "operationism" formulated by Bridgman (1927) has caught the imagination of psychologists and has gradually come to dominate most psychological discussions of philosophy of science and the meaning of concepts. It has been claimed that operationism will insure against "hazy, ambiguous and contradictory notions," and will cure the "notable instability of psychology" (Stevens, 1935, p. 323). In the 1945 Symposium on Operationism, five of the six participants were clearly in favor of it while only one was opposed.

Since that time, however, there have been many reactions to this doctrine, some of which have been quite harsh in their judgment of its value. A sociologist writes that "the operational definition, as here discussed, is an obstacle to scientific advance because of its exclusion of criticism" (Adler, 1947, p. 442). It is also claimed that operationism does not answer the basic question of when an operation is pertinent to a problem, or whether operations can be selected in completely arbitrary ways (Ginsberg, 1955). Pratt (1945, p. 268) adds that "it would be unfortunate indeed if the effect of operationism in psychology were to place a stamp of approval on certain limited fields of research in which hypotheses can be neatly formulated in the language of the older sciences, and to look askance at the wide open spaces in which concepts are fluid and vague and sometimes nonsense." And Skinner (1945, p. 294) has noted that "the confusion which seems to have arisen from a principle which is supposed to eliminate confusion is discouraging."

The reason for such varied reactions to this doctrine may be related not only to the content as formulated by Bridgman but also the various modifications it has gone through at the hands of psychological theorists. It is quite possible that the critics are reacting to things quite different from those originally intended.

That this is indeed the case can be illustrated by reference to the various (nonoperational) definitions and explications of operationism that have appeared in the literature.

THE VARIETIES OF OPERATIONISM

Bridgman's (1927) own ideas may be summarized by the following propositions:

1. The concept is synonymous with the corresponding set of operations. "The concept of length involves as much as and nothing more than the set of operations by which length is determined" (p. 5).
2. The definition of a concept should not be in terms of its properties but in terms rather of actual operations.
3. "The true meaning of a term is to be found by observing what a man does with it, not by what he says about it" (p. 7).
4. All our knowledge must be relative to the operations selected to measure our scien-

[1] Currently a Special Fellow at the National Institute of Mental Health while on leave from Hofstra University, Hempstead, N.Y. The author wishes to thank Father Joseph Dorff for critically reading the manuscript and for his comments.

tific concepts. "If we have more than one set of operations, we have more than one concept" (p. 10).

In Bridgman's later writings he added two other important notions, to wit: a concept may be defined not only in terms of physical operations used to measure it but also in terms of mental or paper-and-pencil operations (Bridgman, 1950); and, secondly, that "operational analysis is mostly restricted to questions of meaning and as such can have only partial congruence with the universe of experimental method" (Bridgman, 1945, p. 248).

Stevens' (1939) view of operationism is put in a different way: ". . . the propositions of science have empirical significance only when their truth can be demonstrated by a set of concrete operations" (p. 222). In this conception, operations are thought of, not simply as ways of measuring concepts, as by Bridgman, but rather as ways of proving or disproving propositions. This view should be clearly distinguished from Bridgman's, for the problem of verification is one on which contemporary philosophers of science are far from unanimous.

Another definition of operationism is attributable to Skinner (1945): "Operationism may be defined as the practice of talking about (1) one's observations, (2) the manipulational and calculational procedures involved in making them, (3) the logical and mathematical steps which intervene between earlier and later statements, and (4) nothing else" (p. 270). He also adds, "Operationism is not regarded as a new theory or mode of definition" (p. 270).

It should be evident that this statement, although in the general pragmatic tradition, is different from Bridgman's conception of operationism. The key issue involved in Skinner's definition concerns what is to be meant by "one's observations." Does the physicist "observe" electric fields and electrons or does he observe dials? Does the psychologist "observe" answers on a test or intelligence? The problem of how one goes from pointer readings to concepts still exists. In commenting on Skinner's definition, Benjamin (1955, p. 55) writes: ". . . to use the term in this highly general sense would immediately make all scientists operationists, since one could hardly conceive of any entity entering into scientific consideration which is not either observational or inferred from something observational."

A sociologist who has written extensively on operationism has presented his interpretation of it in the following statements: "The only way of defining anything objectively is in terms of the operations involved." He also writes that by using "overt behavior of some sort, such as pointing to an object, or going through the operations which we use the new terms to designate . . . [we] avoid becoming involved in insoluble metaphysical questions of ultimate reality" (Lundberg, 1939, p. 25).

This view is much closer to that of Bridgman although it makes explicit the idea that by proper methods of defining concepts all the age-old metaphysical problems of science will be solved or eliminated at one sweep. It thus implies a right to be called a full-fledged philosophy of science.

In contrast to this view is one developed by Feigl, the philosopher who participated in the 1945 Symposium on Operationism. He wrote: "Operationism is not a system of philosophy. It is not a technique for the formation of concepts or theories. It will not by itself produce scientific results. These are brought about by the labor and ingenuity of the researchers. Operationism is, rather, a set of regulative or critical standards" (1945, p. 258).

One final point will be made about the varieties of operationism. Israel and Goldstein (1944) gather a good deal of evidence to show that psychologists have tended to interpret Bridgman's views of meaning, not so much in terms of the operations needed to *measure* a concept but rather in terms of the operations needed to *produce* a phenomenon. Thus "drive" might be defined by the length of time an animal is kept away from food rather than in terms of the behavior of a hungry animal. Now, although this may be a valid way of defining a concept, it should be recognized that it is not the same way that Bridgman chose to define physical concepts. Still other varieties of operationism are described by Benjamin (1955) and by Newbury (1953).

These illustrations indicate that the word "operationism" does not define any one clearly specified body of doctrine. Although there is a general pragmatic flavor to most of the discussions of this topic, the specific things meant by the term vary widely and thus make any critique only partially applicable.

CRITIQUE OF THE OPERATIONIST POSITION

Since the appearance of Bridgman's book, there have been many criticisms of his viewpoint, some by philosophers, some by physicists, and some by sociologists and psychologists. The following remarks represent, essentially, a summary of the various criticisms which have been made of the operationist position. The basic point of departure will be the writings of Bridgman, and thus, it is possible that not all criticisms apply to all the varieties of operationism described in the preceding section.

Measurement presupposes a concept

Expressions such as, "intelligence is what an intelligence test measures," have often been used to illustrate an operational definition of a concept. That this is a highly inadequate way of defining can be exemplified by an illustration given by Adler (1947). He defines what he calls "The C_N Test" by the answers to a series of questions:

"1. How many hours did you sleep last night? ———
2. Estimate the length of your nose in inches and multiply by 2.
3. Do you like fried liver? (Mark 1 for Yes and —1 for No.)
4. How many feet are there in a yard? ———
5. Estimate the number of glasses of ginger ale the inventor of this test drank while inventing it.

Add the above items. The sum is your crude C_N score. Take the test daily at the same hour as long as you can. Then calculate your refined C_N rate by . . ." (p. 439).

It is obvious that such a test is pure nonsense regardless of the statistical formulas used or methodological refinements involved in the statement of items or categories of analysis. The statement that "C_N is what the test measures" is completely unsatisfactory on the two grounds that we are unable to form any meaningful concept of it, and that all criticism is excluded since "The test measures C_N and C_N is what the test measures." It is also clear that an infinite number of such "tests" can be formed in this arbitrary way. Operationism provides no basis for distinguishing between meaningful and meaningless concepts defined by such measurements.

Before we can adequately measure anything we need to know, at least in general, what we want to find out, even if there is some vagueness to our concept. Science develops its measuring tools, typically, by a series of successive approximations in which the concept gradually achieves greater precision, the ambiguities are eliminated, and the relations between the concept and other concepts are more clearly formulated. Observation and measurement presuppose things with properties as well as previous theory. We never start from operations.

The fact that measurement presupposes a concept is illustrated by Prentice (1946) in a discussion of the controversy over "continuity versus non-continuity" theories of learning. He points out that "the term 'hypothesis' is misleading when it has been defined to mean a kind of behavior. If hypotheses in rats were what they are in man, the hypothesis would necessarily *precede* the behavior which indicated its presence, and lack of that behavior could not disprove the existence of the hypothesis . . . . We are still without operations which can define 'noticing the stimulus cues' or 'having a (symbolic) hypothesis' in animals but the concepts are surely not meaningless . . ." (p. 248).

Not only does measurement presuppose a concept but the concept is usually broader than the particular method used to measure it. This is what makes it possible for "habit strength" to be measured by rate of learning, errors made, probability of response, trials to reach a criterion, etc. It occurs, not uncommonly, that two or three measures of the same concept do not correlate well. Operationism provides no basis for selecting between operations which ostensibly measure the same thing or for evaluating the relative value of measurements.

The problem of generality

Pfannenstill (1951), Peters (1951), and Benjamin (1955) all have pointed to the problem of generality of concepts raised by the operational view of definition. They ask whether operationism does not make all general concepts impossible since even the smallest variation in the procedures of measurement implies a new concept. Bridgman himself has noted that yardsticks and triangulation measure two different kinds of "length." But does this not also imply that "length" as measured by a steel ruler is different from that measured by a wooden ruler, or by a micrometer, etc.; and that in the same sense, "heat" is different when measured by a mercury thermometer, an alcohol thermometer, a thermocouple, or a thermistor?

Such variations in procedure are commonplace in science, and yet it is generally believed that these different procedures or operations are designed to measure the same concept. It is also recognized that certain sources of error may operate for one measuring procedure that do not operate for another. These sources of error are gradually discovered and eliminated and the different measurement procedures converge to produce a single consistent answer. In the measurement of temperature the differential expansion rates of different substances used as thermometers provided some sources of error which were increasingly understood and taken into account. Finally, temperature as defined by the Kelvin scale became measurable by the use of general physical laws which were independent of the specific properties relating to the expansion of substances.

Similarly, the charge on the electron and the velocity of light have both been measured in a variety of ways, with increasing convergence of results. An astronomer working in the recently developed field of radio astronomy has written: "By the use of a variety of techniques, and with everlasting emphasis on high precision in the measurement of positions, we may count on slow future progress . . ." (Bok, 1955, p. 338).

Operationism would imply that the use of many different methods to determine the properties of the synapse means that many different and unrelated concepts are being studied. This, of course, is contrary to the spirit of the whole series of observations made in connection with this problem. If there are many independent operational definitions, how is it possible to arrive at general constructs? The fact is that most scientists are interested in finding general explanations and general concepts that will account for a large number of apparently isolated or unrelated observations in terms of a small number of terms or constructs. Operationism, literally interpreted, would move science in the opposite direction, greatly increasing the number of unrelated concepts and actually providing a new concept for each new observation. Such a situation would make scientific teaching and prediction almost impossible.

This problem of generality of concept has been recognized by some of the operationists and an answer has been attempted: generalization can occur in terms of the equivalence of results of operations. But this solution leads to another problem.

The problem of equivalence of operations

Bridgman has pointed out that two operations may be said to be equivalent if they lead to the same numerical results. Thus, since tape measurement and triangulation produce the same outcome in all terrestrial applications they therefore refer to the same concept, even though we must be prepared to abandon this notion of equivalent concepts when and if the two methods of measurement do not produce the same numerical results. This may seem like a perfectly valid procedure, but it is a direct denial of the basic operational principle, since if concepts are to be defined by operations, and the operation is of primary significance, then the concept cannot *also* be defined by the equivalence of numerical results obtained by using different operations. "This argument detaches the concept of quantitative value from its operational meaning and gives it the status of an absolute property which transcends the methods by which it was determined. It is a mistake, therefore, to regard two constructs as the same because they bear the same numerical designation. . . . Equivalence among operations demands a point of reference outside of the operations themselves" (Israel, 1945, p. 260).

Thus, this attempt to fit operationism to the actual practices of scientists actually leads to a contradiction in that the basic definition of a concept in terms only of operations is superseded by something which is not an operation, to wit, a numerical equivalence of results.

There is one other problem involved in this context. If two procedures provide the same outcome in measurement of a phenomenon, then the operationist may conclude that the same concept is being measured by the different methods. If, however, the two operations do not lead to the same equivalent outcome then the temptation is strong simply to conclude that two different concepts are being measured which may have nothing to do with one another. This is in contrast to asking: *Why don't the two numerical results agree?* The exploration of discrepancies, observed in the course of experimentation, has often, in the history of science, led to important discoveries and advances, and a strict following of the operationist view may prevent potentially fruitful questions from being asked and may impede the discovery of new phenomena.

The problem of error

The fact that in actual scientific practice there are often several ways of measuring a given phenomenon poses several problems for the operationist. One has already been mentioned, i.e., the problem of equivalence of operations, but a second question which arises is the question of whether one measure is in some sense "better" than any other. To be able to judge the relative value of measurements or of operations requires criteria beyond the operations themselves. If a concept is nothing but an operation, then how can we talk about being mistaken or about making errors? If "heat" stands for certain measurement procedures, then there is no sense in talking of better ways of measuring it or of being mistaken in such measures. Similarly, from a definition such as "intelligence is what IQ tests measure," one cannot construct a new test or judge how good the old one is (Ginsberg, 1955).

One of the facts of science is a continuous tendency to modify existing methods of measurement so that certain properties can be evaluated with greater and greater precision. New instruments and new designs are continuously being developed, and yet we do not identify the concept being measured with the measurement procedure or instrument being used. If we did, then improvements and changes of method would produce new concepts, which they generally do not do. Over the past half century, skin resistance, or GSR, has been measured with dozens of different instruments, circuits, designs, and procedures, and although many of these operations have had various sources of bias associated with them, the common element which binds all this research together is the fact that the particular concept "skin resistance" is being measured. As science develops the sources of error are gradually eliminated. Operationism has never adequately taken into consideration this problem of the improvement of measuring procedures and the elimination of error. Feigl (1945) has pointed out that thermometers and IQ tests did not arise in a historical vacuum but that there were repeated redefinitions. "It makes perfectly good sense to ask whether a mercury thermometer measures temperature adequately" (p. 255). The same, of course, may be asked of psychological measurements.

The problem of infinite regress

In the 1945 Symposium on Operationism the question was raised of whether operationism leads to an infinite regress: that is, since a concept is defined by the operations used to measure it, how will the operations themselves be defined? It is clear that it would be possible to define operationally each of the words used in a particular definition, but these new operational definitions would need to be defined operationally also, ad infinitum.

The only answer given in the Symposium is that we stop operationally defining when we use terms that everyone pretty much understands. Although this is a practical kind of solution necessitated by the impossible situation, it is not logically consistent with the tenets of operationism which, when carried through consistently, lead to an infinite regress.

The problem of theoretical terms

The criticisms raised against operationism under this general heading are of three sorts: (1) very few terms have in fact been operationally defined; (2) some theoretical terms *cannot* be operationally defined; and (3) some terms can be operationally defined which are not usually thought to be included in the scientific universe of discourse.

Considering the first point, it has been noted that there is actually a dearth of illustrations in the literature of operational definitions of terms, and that the few examples usually given such as "Intelligence is what the intelligence test tests" are not fair samples of the terminology of psychologists (Peters, 1951). Very few, if any, attempts have been made to define operationally such currently used terms as "field," "synapse," "emotion," "cognitive map," "Oedipus complex," "drive," "superego," etc. Almost none of the terms in any dictionary of psychology are defined operationally. This may mean either that it is very difficult to give an operational definition of a concept or that there is not much value in doing so. In the light of the criticisms of operationism that have been presented it is more than likely that the latter suggestion is true.

That there are some meaningful concepts which are not operationally definable is also clear. Many of the terms used in science refer to ideal states such as perfect gases, point masses, frictionless engines, instantaneous velocities, etc., which represent the limiting condition of an infinite series of approximations. Mathematical concepts used in science also often refer to conditions not realizable in the actual world; this is illustrated by the concepts of the calculus which use notions relating to infinity. In addition to this, there are concepts employed in science which cannot be measured by currently available techniques, for example, the earth's core, the neurophysiological basis of memory, the fossil link between man and the higher primates, etc. These concepts are not meaningless or invalid (Ginsberg, 1955).

In relation to the third point it may be said that the way operationist thinking has developed, it is possible to "operationally define" almost all the terms of our language, even those usually called metaphysical. Operationists have extended the term to include "paper-and-pencil," "mental," and "logico-mathematical" operations, which dilutes operationism to such a degree that almost anything a scientist does to get knowledge can be included. Even a metaphysician uses mental operations, and "God" and the "soul" can be deduced from certain postulates (Ginsberg, 1955). Feigl also notes (1945) that such a broad definition of operation can be applied to the speculations of theology and metaphysics. If this is the case, operationism cannot discriminate between meaningful and meaningless concepts, as has sometimes been claimed. This was also seen in the illustration given earlier of the C_N test which is patently nonsense and yet operationally definable. It is not the case that operationism can avoid or solve metaphysical questions of ultimate reality.

The problem of reductionism

There has been a tendency on the part of some operationist writers to imply that the use of operational definitions will enable psychologists to avoid theoretical terms in their analyses or at least those theoretical terms that are not firmly anchored in observables. This opinion is not shared by other writers dealing with the philosophy of science.

Braithwaite (1955, p. 80), for example, has claimed that "the theoretical terms of a science are not explicitly definable, but instead are implicitly defined by the way in which they function in a calculus representing a scientific deductive system. . . . On this view, to say that theoretical concepts exist is to assert the truth of the theory in which they occur." In a similar vein Craig (1956, p. 52) writes, "Empirical significance attaches to an entire framework of assertions or beliefs, and to individual expressions or concepts only indirectly by means of that framework. . . .

Empirical significance seems to be a matter of degree."

This view is further supported by Quine (1953, p. 41) who writes: "The dogma of reductionism survives in the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or infirmation at all. My counter-suggestion . . . is that our statements about the external world face the tribunal of sense experience not individually but only as a corporate body. . . . The unit of empirical significance is the whole of science."

The problem of whether to reduce observations to simple descriptive summarizing terms or to theoretical terms actually has a long history (Plutchik, 1954). For example, although the famous scientists Mach and Ostwald rejected the atomic theory in physics and chemistry, yet in time the atomic theory, with the host of unobserved theoretical terms it brought, came to dominate the thinking of physical scientists.

To the extent that these views cited above are valid, then operational definitions of single theoretical terms are of relatively little value. In the mathematically developed sciences, theoretical concepts are often introduced by postulation as undefined primitive terms which get their meaning only within the total framework. This is also true in the parts of psychology which are more mathematically developed.

CONCLUSIONS

In this paper, an attempt has been made to re-examine some of the issues involved in the operationist position. The conclusion reached was that the classical version of operationism is inadequate on one or more grounds.

Is this conclusion a reason for despair? Is there any adequate alternative to operationism? The answer is simple and affirmative. Scientists introduce concepts into the scientific language by a variety of procedures. Without elaborating on these in detail (or being exhaustive), terms may be defined by: *nominal* definitions (equating symbols), *ostensive* definitions (pointing at), *real* definitions (listing properties), *conditional* definitions (stating sufficient conditions), or *postulational* definitions (as in mathematical models). The stage of development of a given area of science generally determines which of these will be most often used. Psychologists will continue to use any and all of these definitional procedures in their quest for knowledge and understanding.

Pratt (1945) has noted that "most experimental work is preceded by a period of picking and choosing among various problems, a period of vague speculations, of casting up of half-formed hypotheses, of trial and error thinking, etc. Little help can or should be expected from operationism in this period" (p. 269). In their actual practice, scientists seek *theoretically fruitful* concepts rather than ones which are operationally definable. Peters (1951) concludes: "Too much pre-occupation with method often distracts people from the necessity for fertile hypotheses" (p. 61). Psychologists could well take these admonitions to heart.

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Suppose someone had been able to offer the use of a computer to a pre-Copernican astronomer, deep in his calculations. The need for a Copernican revolution would have been removed at once; eighty-three epicycles are as nothing to a good machine, and the nautical almanac for the next thousand years could have been completed in a few hours. It would have contained mistakes, it is true, but they would not have been immediately apparent, and could have been taken care of by the addition of a few extra parameters as need arose. With a reserve of circuits in the computer it would be easier, in fact, to complicate the theory than to simplify it. One might be tempted to say that this was all right; the predictions work, everything comes out just as it should, and this is the criterion of scientific success. But this would not have satisfied Copernicus—not only, one likes to think, because of his mystical attachment to circles, but because such an answer sacrifices understanding to prediction and control. For the latter all we need is to match mechanical complexity against physical complexity, but for understanding we have to match the logical complexity of our concepts against physical complexity, and this may be much harder. We may have to settle for prediction and control apart from understanding, in view of the fact that there is an upper limit to the complexity of ideas that can be grasped by man, and an upper limit to the complexity of the phenomenological unit which can be intelligently apprehended, so that we simply may not be able to reach the optimum which would represent success of a theory from the point of view of understanding. But this is a conclusion that must not be arrived at too soon, and perhaps never finally arrived at.

PETER CAWS, *Science, Computers, and the Complexity of Nature*