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Operationsim and the Source of Meaning in Bridging the Theory/Method Bifurcation

Joseph F. Rychlak
Purdue University

After first distinguishing between the contexts of theory and method in scientific activity, it is suggested that Leahey views theoretical generation as the ultimate source of meaning, and Kendler views the empirical method of validation as the context in which ultimate meanings take root. Operationism was Bridgman's proposal to clarify meanings which had to bridge the theory-method gap. Unlike Einstein, who performed thought experiments, Bridgman stressed empirical experimentation. For this and other reasons his instrumental form of operationism has been stressed to the virtual exclusion of paper-pencil or "symbolical" operations. Machian phenomenalism was influential early in the evolution of logical positivism. Later, thanks to Neurath, a physicalistic realism supplanted this more idealistic emphasis. Academic psychology was already committed to a realistic, reductive form of explanation when its leaders adopted operationism and logical positivism. The discussion closes with a defense and demonstration of how it is possible to theorize about behavior in a teleological manner and yet retain the rigors of operationism and experimental validation in the methodological context.

Readers of JMB have been following the contrasting positions of Professors Leahey (1980, 1981, 1983) and Kendler (1981, 1983) on the question of operationism or operational definitions in the science of psychology. Ray Russ has asked me to advance some thoughts on this topic, knowing of my interests in this general area (see Rychlak, 1980; 1981a, pp. 46-49) as well as my avowed commitment to a rigorous humanism in which I contend that we *can* put teleological theories to test (Rychlak, 1977). Professor Leahey (1983) suggests that strict adherence to operationism excludes intentionality from scientific investigation. Judging from the exclusively non-teleological theories which are currently being put to test in psychology, he would seem to be correct. I hope to show that our experimental activity need not be so restrictive, and that the only reason this state of affairs persists is because we psychologists have never appreciated the necessity of having to distinguish between our (many) theories and our (common) method of doing scientific work.

Theory Versus Method

In the Leahey/Kendler exchange we see the question put concerning whether or not it is possible to avoid metaphysics in doing the work of a

Requests for reprints should be sent to Joseph F. Rychlak, Ph.D., Department of Psychological Sciences, Purdue University, West Lafayette, Indiana 47907.

psychologist. I agree with Leahey that *any* statement we make is necessarily grounded on a discernible (through self-examination) assumption, or series of assumptions, and hence it is therefore impossible to avoid something like "meta-" analysis of our theories—which is what metaphysics is all about. But as we are not philosophers, I think it also wise that we psychologists attempt to conduct scientific research on the problems of psychology *as well as* carry out meta-theoretical analyses of our theoretical viewpoints. I think that some of the anti-metaphysical bias of empirically-oriented psychologists like Kendler has to do with what they perceive to be the strictly talky-talk orientation of those psychologists who seem always to detract from the empirical efforts of scientific psychology through metaphysical arguments. Do these psychologists really carry out their criticisms in some line of empirical research? Do they criticize and *then* do science as well? Often, they do not. They criticize a lot but show no innovation in the everyday effort of carrying psychology forward as a science. Though he is not this blunt about it, I think we see an attitude of this sort reflected in Kendler's (1983) response to Leahey.

Now, it seems to me that within this very issue of whether or not we need to self-examine, single out assumptions, and put questions as well as alternative explanatory possibilities to ourselves before returning to the data collections of the laboratory, we have already presaged an *essential bifurcation* in our activity as scientists. I have for some years insisted that psychology recognize the basic distinction between *theory* on the one hand and method on the other. These terms are arbitrary. I could just as readily refer to these two sides of the psychological coin as *thinking* and *proving*.

Reduced to its essentials, a *theory* is simply a pattern of relationship between two or more concepts, constructs, labels, images, and so on—whatever we wish to name those items that we bring into *meaningful* association in order to enrich our understanding (Rychlak, 1981a, p. 42). Meaning is a relational term, uniting two items (constructs, images, etc.) in some way, and the more such ties we have the more complex is the theoretical meaning involved. Associationistic philosophy depicts these "connections" or "bonds" (Thorndike, Watson, Hull, etc.) occurring without intentionality, but etymologically the meaning of meaning is heavily teleological. What something means is what it "points" to, the aim or intention symbolized by the concept in question. Here we have one of those metaphysical assumptions that divides psychologists. Associationists believe that meanings are shaped by external circumstance, so that the behaving organism is not an originating source of meaning-generation and meaning-extension. As a nativistic psychologist who believes that human beings are innately capable of reasoning dialectically and hence learning the very *opposite* of their external shapings (inputs, reinforcements, etc.), I think of meaning as something framed and then applied by the person in the manner of affirming a logical premise (see Rychlak, 1977, Chap. 8). Meaning is a formal- and final-cause conception, *not* an efficient-cause conception as the associationists would have us believe (*Ibid.*, pp. 5-6, 56).

The patterned (formal-cause) relationship framed by any theoretical statement (hypothesis, assumption, belief, attitude, etc.) may be done recklessly, with tongue in cheek, or seriously, with a genuine intent to describe or explain something in a comprehensive manner. Theorizing is most assuredly thinking, and scientists have no more right to propose thoughtful ideas about things than any one else. What scientists *do* have, and something for which they should be recognized in a balanced fashion, is a certain *method* of proving their ideas—a method that has both strengths and weaknesses but on sum is very important to the advance of knowledge.

By *method* is meant a means or manner of determining whether a theoretical claim (hypothesis, proposition, "idea," etc.) is true or false. We might even say that a method is what we do in order to decide whether a conception of reason that has occurred to us or to someone else should be taken seriously. The concept of truth has been interpreted in terms of two broadly conceived theories (Adler, 1927, p. 18). According to the *coherence theory*, a theoretical proposition is true which is logically or aesthetically consistent with what is already known, conforming in essence to the demands of common sense and reflecting an internal consistency that is intuitively convincing. I have argued that this is the sort of evidential support that we employ in order to proceed in our various efforts to understand experience theoretically (mentally, conceptually, intuitively, etc.); hence, we have *procedural evidence* as a form of cognitive method, putting our ideas to test in a common-sense fashion (Rychlak, 1981a, p. 75). Procedural evidence is sometimes called "theoretical proof."

According to our second, *correspondence theory* of truth, a theoretical proposition is true if its meaningful claims match an *independent* criterion that has been named beforehand as a suitable test or standard. In the rise of science, this form of evidential proof was emphasized, and we usually think of it as *validating evidence* (*Ibid.*, p. 77). An experimental design is a pre-arranging of independent circumstances that are specifically selected to test a theoretical hypothesis. Once controls have been assigned and a prediction from the theoretical hypothesis rendered, there is no longer a role for procedural evidence until such time as the positive results are in and an elaboration of the theory is called for. If the results are negative, a defense of the theory may also require arguments drawing upon the plausibilities of procedural evidence. Hence, the scientific method, resting on the logical analyses and mathematical tests of procedural evidence, attempts to go beyond such evidential supports to a reliance upon the prediction of events in an empirical, objective (i.e., clearly understood) context of proof. We psychological scientists "control and predict" not to be controlling things *per se* or to compete with actuaries, but in order to show that we have knowledge that is both plausible and practically demonstrable.

Operationism as a Bridge Concept

Now that we have distinguished between theory and method it is possible to put this distinction to work in clarifying what our terminology actually means. For example, I would suggest that a construct like *reinforcement* has little theoretical meaning today, but it retains its time-honored position in psychology thanks to the methodological experimental designs known as classical or operant conditioning. There was a time in our history when reinforcement theory was reasonably clear, but the recent findings on awareness in both classical and operant conditioning (see, e.g., Brewer, 1974) have completely dislodged this simplistic account of antecedents being hooked-up to or made more probable on the occurrence of consequents without the conscious, intentional assistance of the subject within the apparatus (the experimental design). Rather than being manipulated hither and yon by reinforcements human subjects are seen to influence the course of this shaping, and even purposively to *negate* it when the whim suits them (Page, 1972). Whatever reinforcement used to mean theoretically, it most certainly does not mean that today.

And we have the methodological context to thank for this dislodgment of traditional reinforcement theory. Despite all of the metaphysico-theoretical bias framing the traditional explanations of learning, a Kuhnian (1970) anomaly has developed in the paradigmatic assumptions of reinforcement theory thanks to the methodological context in which this theory was put to test. Though I think this anomaly is still being swept under the rug, I am very confident that the future will find psychology acknowledging its presence and in time an altered image (theory) of humanity will emerge (Rychlak, in press).

This example from psychology's history teaches us a few things. First, it should be noted that the line of meaning-extension in scientific activity is usually and I would say ideally "from" theory to method. If we take assumptions into consideration we could even say that meanings are extended from metaphysics-to-theory-to-method. I think Kendler (1983) may dislike this emphasis; he considers himself a theorist but is not anxious to reach for the larger philosophical issues which he admits "are there" for examination. I think the reason psychologists like Leahey often want to call attention to these abstract influences on both theory and method is because so many of their colleagues fail to recognize them—which means they fail to appreciate their personal contribution to the process. Of course, the ideal progression which I have suggested does not always result in a happy ending in the methodological context. As we all know, things rarely go as we expect them to, particularly if our experimental design is without precedent and the hypothesis being put to test is a risky one.

When altered findings or no findings result, the scientist may still find a regularity—a repeatable phenomenon—in the methodological context. The experimental design generates a repeatable "effect" that the scientist had not predicted. What could this mean? From this point forward change in the theory prompting the design, or, a reinterpretation of the design in light of the theory is called for. Here is the second point we should have learned from our experience with reinforcement theory: There is a reciprocal relationship between theory and method, so that in many instances the latter encourages meaningful changes in the former. The theory-method bifurcation is an arbitrary distinction that we have made in order to analyze our *complete* activity as scientists with greater understanding. It should be no shock to our sensibilities to discover that we may have a two-way street here.

I believe that debates of the sort Leahey and Kendler find themselves in are at heart disagreements over the ultimate *source* of meaning. Leahey comes at the question of operationism from the theoretical end of the street and Kendler approaches the question from the methodological end. The pattern which is basic to a meaning is likely to be said by Leahey to be construed or conceptualized intellectually and by Kendler to be seen and abstracted from a source totally independent of such mental considerations. Leahey (1983) tells us that even if scientists' perceptions of empirical data are veridical, what they will believe in depends on their other (e.g., presumptive) beliefs; and Kendler (1981) tells us that his view is more from the research perspective, where he can engage the problem of "reconstructing" knowledge. Operational analysis for Kendler (1983) can "clarify meaning" because the ultimate source of that meaning is to be found within the methodological context. Both Leahey and Kendler realize that an interaction takes place between the theoretical and methodological contexts, but I am speaking now about the ultimate (basic, initial, actual, etc.) source of such meaningful relations. We shall return to this question in the next section, when we consider the ramifications of operationism on the traditional topics of philosophy.

I would suggest that the concept of *operationism* or *operational definition* was proposed by Bridgman as a bridge conception through which it was hoped that the meanings at *either* end of the two-way street could be made explicit and properly tested. Thus, he tells us that in 1914 he was asked to teach two advanced courses in electrodynamics that included material from the restricted theory of relativity: "The underlying conceptual [*theoretical*] situation in this whole area seemed very obscure to me and caused me much intellectual distress, which I tried to alleviate as best I could. Another cause of distress was the situation in dimensional analysis, which at that time was often so expounded as to raise doubt whether experimental work [*method*] was really necessary at all" (Bridgman, 1961, p. 76).

Feeling quite at sea in his profession, Bridgman then did what he later (in 1923) termed an operational analysis of his field. His actual writing of *The Logic of Modern Physics* (Bridgman, 1927) is described as a hurried effort, one which left many questions unanswered such as "an analysis of what it is that makes an operation suitable for the formulation of a scientific concept" (*Ibid.*, p. 77). As Leahey points out, the course of operationism for Bridgman was not exactly pleasant and in time he was to draw back from colleagues who seemed to be making an ideological school out of what was for him a purely logical analysis of the total scientific effort. He preferred to think of operationism as a general attitude or point of view rather than an "-ism." He nicely reflects the theory-to-method direction of meaning-extension in the following: "I think that there need be no qualms that the operational point of view will ever place the slightest restriction on the freedom of the theoretical physicist to explore the consequences of any free mental construction that he is ingenious enough to make" (*Ibid.*, pp. 79-80).

Bridgman felt that if such a freely generated theoretical question had meaning there were certain operations that should follow, explicating how one would go about investigating such mental constructions. He discussed several operational measures, but the primary operational analyses he focused upon were either of a *paper-pencil* (symbolical) type or an *instrumental* (via experimental devices) type (Bridgman, 1961, p. 78). If neither paper-pencil nor instrumental operations could be stated, the theoretical question being put was said to be *meaningless*. In the terminology of the present paper, Bridgman was asking for either a well-defined course of *procedural evidence* (paper-pencil) or *validating evidence* (instrumentation) preliminary to taking a theoretical suggestion seriously. As is well known, many of Einstein's theoretical contributions were proposed and tested purely on the basis of paper-pencil or symbolical arguments. He performed "thought experiments" which had implications for eventual translation into instrumental operations. But strictly considered, Einstein's "operations" were non-empirical, i.e., paper-pencil or mathematical ratiocinations.

But if this is the case, then where does the meaning embraced in Einstein's theory issue from? If he could *think* his way through a problem, which might in time be operationalized not simply in his mathematical calculations (procedural evidence) but in an actual empirical demonstration (validating evidence), where does the ultimate source of meaning issue from? Einstein (1934) opines: "Experience remains, of course, the sole criterion of the physical utility of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed" (p. 18). Note the theory-to-method direction assumed by Einstein here.

It puzzled Bridgman (1961, p. 77) to see how much attention was paid to the instrumental form of operationalizing concepts at the expense of the operational analysis of paper-pencil activities. The focus has almost always been on the instrumental form of operationalizing. In psychology this has been exclusively the case so far as I can determine, and this tradition is reflected in the Leahey/Kendler debate. Bridgman soon found himself at the center of a host of conflicting interpretations of just what operationism implied concerning the traditional philosophical categories of knowledge, such as realism, idealism, empiricism, nominalism, subjectivism, relativism, solopsism, and so on. Leahey is drawn to these questions, as Kendler bemoans (1983), and I think his analysis is generally accurate. I think that Bridgman is probably at fault for the emphasis placed on strictly instrumental operations. Unlike Einstein, Bridgman was an empirical ("no thought experiments") type of experimenter. His comments on Heisenberg's indeterminacy principle suggest that he believed it may simply reflect an instrumentation or measurement problem at the sub-atomic level rather than a basic limitation in human conceptualization (see Bridgman, 1958, p. 59; see also Munitz, 1958, p. 64). Even so, he was sensitive to the changing views of reality implied by the fact that the instrument used to "view" that reality may be a determining factor in what is "seen":

> . . . Since an object never occurs naked but always in conjunction with an instrument of measurement or the means by which we acquire knowledge of it, the concept of "object," as something in and of itself, is an illegitimate one. Acceptance of this insight with all its implications is obviously going to react strongly on our idea of "reality," and many of the objections to the orthodox view arise precisely from unwillingness to accept the altered view of "reality." (Bridgman, 1958, p. 48)

If we add now the metaphysical presumptions which the experimenter brings to bear in any observation—including, for example, the assumptions made in building a certain type of "instrument of measurement"—it is ever clearer that our two-way street is a busy one, and that any one who clings to the older vision of a reality standing outside our intellects in mute isolation but capable of being mapped or tracked with indisputable certainty is in conceptual difficulty. Reality in modern science is what we either choose to make it, or, what we are capable of making it, given the restrictions of our conceptualizing capacities (Zukav, 1979). The only requirement demanded of the scientist is that his or her theoretical account of "reality" meet the demands of rigorous validating evidence.

Operationism and Logical Positivism

Though he wrote on the supposed myth of operationism, most of Leahey's (1980) initial analysis was done on logical positivism. Mach and Carnap are given central consideration in this analysis. We can get a better appreciation of the myriad philosophical issues which Bridgman found himself immersed in by considering the parallels between operationism and what the logical positivists were to call the *principle of verifiability.* Ayer (1946) defined the latter principle as follows:

> The criterion which we use to test the genuineness of apparent statements of fact is the criterion of verifiability. We say that a sentence is factually significant to any given person if, and only if, he knows how to verify the propositions which it purports to express—that is, if he knows what observations would lead him, under certain conditions, to accept the proposition as being true, or reject it as being false . . . . We enquire in every case what observations would lead us to answer the question, one way or the other; and, if none can be discovered, we must conclude that the sentence under consideration does not, as far as we are concerned, express a genuine question, however strongly its grammatical appearance may suggest that it does. (p. 35)

The paper-pencil (symbolic) and instrumental distinction is not drawn here, but I do think the implication is clear that theorizing demands verification (theory vs. method); and that unless we can extend our theoretical claims through rational analysis (procedural evidence) to a form of observational testing (validating evidence) we are not dealing with meaningful ("genuine") questions. There are many ramifications of a statement like this. Given the strictest of readings, we might expect verifiability to severely limit the topics that psychology might engage in. Yet, Carnap (1950, p. 50) discusses the use of quantification in psychology more liberally than most tough-minded psychologists, whom he feels are too prone to discard concepts that are difficult to quantify. Members of the Vienna Circle rarely have branded Freudian theory as meaningless or tautological, even though such labels have been freely applied by the positivists to philosophical doctrines like Platonism or Thomism (Hook, 1959, p. 308).

We return now to the question we were considering above, concerning the ultimate source of meaning in knowledge. I think that Leahey's presentation of logical positivism is a bit misleading. That is, when he tells us that "positivists are nominalists" (Leahey, 1983) he is giving "an" interpretation of positivism, leaving out a good deal of the historical soul-searching and philosophical rumination that took place in arriving at the collection of ideas we know today as logical positivism. The same is true of Leahey's presentation of Mach, whom he also sees as an "extreme positivist" (Leahey, 1980, p. 128), which although true in one sense does not quite capture the reasons why Mach called his approach a *phenomenological* form of physics.

Mach found himself in the same position that we noted Bridgman had faced—of having all kinds of philosophical analyses done on his writings when he specifically denied that he was a philosopher (Frank, 1950, p. 81). And even today, when we attempt to use traditional philosophical terms like nominalism, realism, or idealism to discuss Mach we are doing a sort of violence to his actual intent (*Ibid.*). But, I do not think it is entirely wrong to do so. I think we can learn from such exercises so long as we recognize their limitations. Let us carry forward the lead that Leahey (1983) has given us. He tells us that thanks to a paper sent to him by a philosopher (Amundson) he came to realize that operationism—and presumably, verifiability—denies the possibility of a psychologist taking a realistic position on the nature of data.

I would think that such an assertion would surprise most tough-minded psychologists. When the typical experimental psychologist employs an operational definition I believe that he or she is *in fact* presuming that there is some tie here between the theoretical construct being put to test and the "independent reality" which Leahey tells us cannot be assumed by an operationist. I think that he is correct in his strictly philosophical—as opposed to a psychological—assertion. If all we do is focus on the act of operationalizing a theoretical construct disregarding the psychology of the scientist carrying out this effort, then we can show how his or her approach necessarily results in a nominalism: things are merely what we name them to be. We can operationalize the same concept different ways. There are no independent criteria against which our constructs are to be judged when our operations define our terms. Indeed, the operations become the criteria, hamstrung by the limitations of their definitions to the experiments in which they have been framed.

Leahey's arguments here hinge on his assumption (see above) that it is the *person* who generates the meanings in any operationalizing of terms. But cannot Kendler's (1983) self-admitted realism and biological reductionism underwrite an alternative form of explanation—one which *begins* in a realistic assumption? What if Kendler or another psychologist with his general outlook holds that the meanings a scientist operationalizes are merely the tail-end of a series of inputs, etched upon his or her cognitive intelligence over a lifetime? Are not *all* our mental contents learned? Where else but from an independent reality would we "get" the information contained in the operational definition? Kendler might now ask Leahey if there are also immaculate conceptualizations to match the immaculate perceptions. Based on his information-processing assumptions, Kendler presumes from the outset that *all* meanings—operationalized or otherwise—*originate* in and are drawn (input) from an independent reality rather than serving to create that reality in the nominalistic sense. This is why we can identify them through clearly operationalizing our terms. Reality is the home ground for all knowledge.

I realize that I am putting words into Kendler's mouth here. But I do believe that such a realism is the predominant view among psychological experimentalists today. Incidentally, I found Kendler's (1983) deft self-characterization as a *sophisticated naive realist* amusing. Traditionally, naive realism is opposed to critical realism, with the former implying that reality is as it is found in observation and the latter suggesting that there is always a contribution to the perception of reality in the conceptualizations of the observer. Kendler's "sophistication" is aimed at heading an idealistic critic off at the pass. There is no point in pressing him on how he can be absolutely certain about the validity of the observational data he accrues. He admits he cannot. But this form of sophistication does not extend to a sophistication concerning how an observer might be assumptively influencing and/or creating that which is seen as a reflection of this independently existing reality. I find this formulation basically untenable, but so beautifully dialectical that I must applaud Kendler for his magnificant sophistry.

I did not follow Leahey's (1983) arguments regarding the cognitive map. Holding to a belief in cognitive maps does not strike me as necessarily indicating a realism, and it does not surprise me to see the biologically oriented Kendler rejecting this type of explanation. Realists tend to view cognitive maps as epiphenomena, beliefs about reality which are akin to learned "effects" rather than intended "causes" of behavior. Hence, I think Leahey is on shaky grounds trying to prove that Kendler has dismissed realism in the past because he has asked that we not reify cognitive maps.

Mach was not so ready to dismiss cognitive maps as a determining aspect of human knowledge. When he referred to himself as a phenomenological physicist he was suggesting that the human mind has a cognitive capacity to frame events in certain ways, and that we mistakenly take these characteristics as "in" reality when in fact they are "in" mentality. In other words, Mach was a genuine critical realist. Though he rejected some aspects of Kantian philosophy, Mach had this to say about inherent categories of mental conceptualization: "According to the doctrine of another great philosopher, Kant, time and space lie not so much in things, as *in us*; they are inescapable modes of perception in which we necessarily observe the external world and the processes within ourselves" (Bradley, 1971, p. 47). This willingness to see ideas as emanating from the theoretical physicist rather than from "reality" is what places Mach on the side of Leahey in our present exchange. I like to think of such a viewpoint as a *naturalism*; i.e., Mach is saying that we human beings naturally frame things in terms of time and space. Yet, this does not make reality "so"—it is just how our innate equipment makes things appear. We have to question our phenomenal awareness and thereby study ourselves even as we study empirical events.

Mach once asked William James to stand on a bridge and try to shift his grounds of perception so that, taking the water as the "fixed" point of reference, he would experience the illusion of having the *bridge* move over water that is standing still (James, 1952, p. 783). So when Mach speaks of scientific laws as convenient relations between perceptions of a positivistic nature, he is including in this the framing conceptualizations of the "thinking physicist" who happens to be doing the perceiving. And this perceptual relation—to use our term, *meaning*—would also be brought into relationship with the metaphysical assumptions of the physicist. When Mach asked his colleagues whether they had ever seen an atom, he was *not* therefore expressing a theory of knowledge which implied that "all knowledge comes from sensory input," as I have suggested that most psychologists do today. He was asking for validating evidence of a mechanistic-reductive theoretical conceptualization (i.e., the atom) that his colleagues were advancing based upon their personally generated theories as tested by their procedural evidence in cognition (Frank, 1950, p. 6). Mach well knew that "reality" was a confining conception which, once believed in, limits the range of ideas which can be put to test concerning it. He is often cited as the real father of relativism in physics, and Einstein has acknowledged a debt to Mach in this regard. Mach beautifully reflects the theory-method bifurcation and his relativism in the following:

> Different ideas can express the facts with the *same* exactness in the domain accessible to observation. The *facts* [method] must hence be carefully distinguished from the *intellectual* constructs [theory] the formulation of which they suggested. The latter—concepts—must be *consistent* with observation, and must in addition be *logically* in accord with one another. Now these two requirements can be fulfilled in *more than one* manner, and hence the different systems of geometry. (Bradley, 1971, p. 86)

Phillip Frank (1950), who was among the original founders of the Vienna Circle, tells us that such Machian viewpoints became the "principal background" (p. 7) of the early logical positivists. Though they also had points of disagreement with Kantian philosophy, the fledgling logical positivists greatly favored what we have been calling the naturalism of Kant's position, as follows: "We saw much truth in Kant's statement that the recording of observations is not a purely passive act but that a great deal of mental activity is necessary in order to formulate general statements about sense observations . . . . Our whole group understood and fully agreed that the human mind is partly responsible for the content of scientific propositions and theories" (*Ibid.*). Frank goes on to show how for several years the early logical positivists placed their emphasis on the phenomenal, conceptualizing side of things, with Carnap a leading author in this vein. However, around 1930 Neurath was to shift the locus of meaning's source from the phenomenal understanding of the physicist to the *physical* existence of objects. Here is Frank's summary of what took place:

> Perhaps the most striking effect of the cooperation of Schlick and Carnap with the old Viennese group was the shift to "physicalism" and to the "unity of science." Neurath had been particularly eager to prohibit any establishment of a metaphysical doctrine by a tactic of infiltration [i.e., through emphasis on the physicist's phenomenal conceptuali-

> zations]. He suggested that sense data should be dropped as elementary concepts of the logical structure of the world and replaced by physical things. Instead of building up the system of human knowledge upon concepts like "red spot" or "feeling of warmth," one should use elementary symbols expressing concepts like "rock" or "table," and define "redness," or "warmth" as derived conepts. As the starting point in sensation had a certain tint of idealism, so the new starting point had a tint of materialism [realism]. Carnap had in his "Logical Structure of the World" spoken of "methodical materialism" as a possible language for his system. But he had come to prefer "phenomenal language," statements in terms of sense impressions. Neurath worked out a system based on physical things as elementary concepts and called by him "physicalism." Carnap refined Neurath's physicalism to a precise logical structure and even constructed a "physicalistic language" for the field of psychology. (Frank, 1950, pp. 35-36)

I interpret this physicalistic development in logical positivism as comparable to the point made above concerning the fact that experimental psychologists *begin* in a realistic-materialistic language. It seems to me that the physicalistic language adopted by the positivists shifted the source of meaning to the palpable events, making it possible to circumscribe meanings that supposedly really and truly *exist*, independent of the phenomenal conceptualizing abilities of the human being. This is consistent with the view that *method* is where we can find the ultimate source of meanings, as opposed to the *theories* of the person. I believe that it would have been more in line with the experimental advances of modern, sub-atomic physics if the positivists had retained their initial quasi-idealistic, Machian emphasis. But this was not to be, and hence as Leahey has shown, a philosophy of science ready-made for the rabidly empirical behaviorism of the 1930s was brought to the shores of America for our leading experimental psychologists to pounce upon.

Operationism in Psychology

There can be no question but that behavioristic psychology, the most influential school of thought in academic psychology's history, was committed to a physicalistic-realistic interpretation of meaning from the very beginning (Watson, 1913). Hence, operationism and verifiability were adapted to an already existing metaphysical frame of reference. When S.S. Stevens (1935) interpreted Bridgman's views to psychology by suggesting that "A term or proposition has meaning (denotes something) if, and only if, the criteria of its applicability or truth consists of concrete operations which can be performed" (pp. 517-518) he was (a) focusing upon instrumental operationism to the exclusion of paper-pencil (symbolic) operationism, and (b) leaving things general enough so that the source of a term's meaning was not specified. That is, based on this definition, presumably a freely created conception might be applied to empirical test, in the manner of Einstein's comments on free creativity (refer above). When he formulated his hypothetico-deductive method, Hull (1937) tightened the screws down on reality as something more than simply a criterion of applicability by suggesting that: "Whenever a theorem fails to check with the relevant facts, the postulates which gave rise to it must be ruthlessly revised until agreement is reached" (p. 8).

If we consider the total scientific effort, it is just as probable that some error in methodological design might account for the inability of a theorem framed speculatively to "meet the facts." Revision on the side of method is just as necessary as revision on the side of theory. There are numerous cases in the history of science—involving for example Galileo and even Einstein—in which theoreticians freely admitted that they put more stock in their mathematical ratiocinations than they did in the experiment conducted on the basis of such reasonings. Indeed, Kuhn's (1970) fundamental argument is that theoretical (including metaphysical) assumptions have regularly taken precedent over empirical observation in the history of science. Validating evidence is not without its faults. There is a fundamental logical error in the course of empirical tests such as the hypo-thetico-deductive method. D.K. Adams (1937, p. 215) pointed this out to Hull (who responded very ineffectively).

Thus, to suggest that "If my theory is correct, the data will array as it predicts" and then to find the data arraying as predicted is to *affirm the consequent* of an If-then hypothesis, which proves nothing with absolute certainty or necessity. This is like saying "If a man (*antecedent*) then a mortal (*consequent*)," affirming in time that "This is a mortal" and concluding "Therefore, this is a man." As there are more organisms with the quality of mortality than human beings, so too there may be more than one theory "accounting for" the facts as predicted and observed. Indeed, it is routinely accepted today among sophisticated (critical) realists in *this* sense of the term (see Kendler's meaning, above) that for any fact pattern there are *in principle* up to N possible theoretical explanations that might conceivably explain the predictable and repeatable series of events known as an experimental finding. This does not mean that more than one explanation actually has been or will be formulated, of course. We are speaking "in principle" to highlight the fact that even falsification has its limitations (see Rychlak, 1980).

Such arguments fell on deaf ears among the behaviorists. Though these views were commonly accepted by the "new" physicists of the 1930s, by 1941 we find Bergmann and Spence discussing operationism in terms of how they presumed a "primitive physicist" approached the task of science. With unabashed but misplaced confidence, they completed the job begun by Hull, leaving no doubt as to where the ultimate source of meaning resides: "All scientific terms are derived terms, derived from and retraceable to what one might call 'the hard data,' the 'immediately observable' or what Stevens calls the 'elementary operation of discrimination'" (Bergmann and Spence, 1941, p. 6).

Though Israel and Goldstein (1914) subsequently complained that psychologists were misusing Bridgman's concept, the stage was set for research psychologists to limit their thinking to "reality" in a way that would have greatly upset Mach. At the very least, Mach would have expected psychologists to conduct a phenomenal examination of the paper-pencil (symbolic) activity of the scientist. Experimental psychologists have been ill-disposed to conduct this type of operational analysis because of their commitment to "reality" and the consequent denigration of theories as mere "verbal reports," or epiphenomena, the "effects" of reality's influence on the individual. We see a remnant of this attitude in Kendler's (1983) reference to talented guesses about the nature of reality being preferred to relying upon supposedly irrelevant analogies in the mental gymnastics of a theoretician. By midcentury, Skinner (1950) was even asking "Are theories of learning necessary?" so convinced was he that he could lay his hand on reality and make pigeons and people alike dance to his manipulative tune. The upshot is that psychological scientists in our time have been much less psychological about their area of study than have theoretical physicists been about theirs (Zukav, 1979).

It is this commitment to method and the ultimate source of meaning underlying the measurements involved—i.e., reality—that has negated opportunities for the growth of teleological theory in psychology. We noted at the outset Leahey's claim that intentionality (purposivity, telic theorizing, etc.) is not capable of being studied if we conform to operationism, because an intention cannot be operationally defined. I have devoted considerable effort to dispelling this charge by conducting research on a teleological theory of behavior for some 20 years to date (Rychlak, 1977, Chaps. 9 and 10; 1981b). I have succeeded in doing so, I believe, by keeping clearly in mind what is my (telic) *theory* and what is my atheoretical *method*. I think of my theory and the metaphysical assumptions from which it draws as standing at one end of our two-way street, and my method of empirical validation and the metaphysical assumptions from which it draws as standing at the other. In fact, I have terms for both contexts, each of which relates to the same conceptualization. Thus, I speak theoretically of *affective assessment* but operationalize this construct as *reinforcement value*.

When I conduct a study and find evidence for the effects of reinforcement value on a dependent variable of some type, I fully appreciate that an alternative theoretical explanation of the "facts" is possible. Affective assessment is not the only explanation of what has been observed and cross-validated many times. However, in order to dislodge my theoretical term it is necessary for the non-teleologist to propose countering theoretical arguments, redefine terms, and introduce data from related studies to buttress his or her case. Neither of us can claim total proof for a point of view—it is rare that we find a study completely validating one line of theory in opposition to another. But gradually, as Hempel (1961, p. 66) has taught us, the complete theoretical outlook of either my approach to the data or the critic's approach will tend to be more instructive, fruitful of new suggestions, and helpful to those who seek an understanding of psychological factors in behavior.

What makes an affective assessment a kind of intentionality? This is not the place to go into the broader ramifications of my logical learning theory, but suffice to say that I begin theorizing on the assumption that human beings are *telosponders* in addition to being responders. To telospond is to behave "for the sake of" an intention, reason, or purpose, and in so doing to select alternatives based upon a dialectical reasoning capacity (Rychlak, 1977, p. 283). I have many theoretical arguments suggesting that (a) people predicate reality, lending meaning to it rather than "inputting" meaning from it; (b) meanings are dialectically bipolar as well as demonstratively unipolar, hence it is possible for the human being to learn from what was *not* input by reasoning to the opposite of what *was* input; (c) the person is consequently *always* an agent of behavior, directing his or her course through a precedent-sequacious line of meaning-extension rather than following a stimulus-response or input-output course of efficient causation.

How then do I operationalize my "intentional" construct of telosponsivity as affective assessment? I ask subjects to render a judgment of *likability*, based on my assumption that they *can* take an item in mind and render a judgment of "like versus dislike." The item—a word, a painting, a person's name or face—becomes the "that" for the sake of which they render an affective assessment. They record such a judgment (verbal report) on a scale that I make available to them. This is done on two occasions, and an item which the subject says he or she "likes" or "dislikes" on *each* of these occasions (*reliability*) is then entered into the materials I will be asking subjects to deal with in some way (e.g., memorize, recognize). The marks on my scale are what I then refer to as the *reinforcement value* of the item in question. There are alternative explanations of why a subject might like or dislike an item. Traditional learning theory has attributed such "choices" (*sic*) to past reinforcements, efficiently-caused effects totally outside the individual's personal control, except only as mediators. A frequent claim is that frequency of past contact influences such choices, with familiar objects liked and unfamiliar objects disliked. I have conducted many experiments to discount this simplistic explanation (Rychlak, 1977, Chap. 9). Recently, Zajonc (1980) has come to the same conclusion—that affection is independent of familiarity—but he continues to promulgate a non-teleological theory based on data of the sort I use to support a telic explanation.

So, in order to operationalize telosponsivity I must take verbal report seriously, and then from that point I do what everyone does—and that is to extend a line of theoretical development from my metaphysical assumptions, and the theory they underwrite, "to" my method of validation (using procedural tests along the way). Since my logical learning theory is in my head, and not in the "hard data" or some "elementary operation of discrimination" (see above), I am always free to accept or reject what I judge to be a relevant operational definition of my concept(s). And if I cannot operationally define each and every term in my theory, then I can at least make an effort to extend a line of theoretical development to some logically plausible implication of my style of thought. What would my line of theory suggest would take place if "such and thus" occurred? Is there anything unique in this prediction? How instructive is it? What would other theories suggest might occur—the same thing? Something different? If the same outcome might be predicted, why would this be said to be likely? Are there any intermediate points in the explanation where my theory might prove more instructive than the competing theories?

Beginning in such tests of procedural evidence I can then construe an experimental design (methodological context) which will likely support my view and challenge but never really demolish "the other side"—theories to which I do not subscribe. If my data align as predicted, I cannot claim certainty (due to the affirming-the-consequent limitations discussed above), but I am one step beyond where I was. My theoretical hunch is falling into line with my empirical demonstrations, so that I have the weight of *both* a coherence and a correspondence view of truth behind my arguments. It is not easy successfully traversing these alternative but not mutually exclusive interpretations of the truth.

Coming down to earth from this abstract rumination, let me say that the empirical tests of logical learning theory have been rewarding enough, with probably 80% of some 85 or 90 experiments conducted over the years coming out—partially or fully—in the predicted direction (Rychlak, 1977; 1981b). I have most recently applied logical learning theory to a large-scale developmental study of the lives of young business managers over an eight-year period (Rychlak, 1982). I am presently focusing on the dialectical aspects of human reasoning, trying to devise experimental designs which can be understood (operationally) to test whether it is true as I claim (theoretically) that human beings learn according to this process of oppositionality in their associative processes.

As the reader doubtless appreciates, modern cognitive psychology has absolutely *no* appreciation of the dialectic in human reason whatsoever! I have a theory which employs explanatory principles that are totally foreign to alternative cognitive explanations. It seems clear to me that I would get nowhere if I simply tried to argue with my colleagues over the merits of my thinking, compared to theirs. I may still get nowhere, but so long as I can continue devising empirical proofs of my view, and possibly come up with a design that is both highly convincing of the role of dialectic in human mentation and also highly difficult for traditional cognitive theories to encompass, then I might conceivably begin to attract some interest.

The Leahey/Kendler Debate: A Closing Comment

It is not for me to resolve or judge the Leahey/Kendler debate, a pretty hopeless task in any case. I think we have learned that what these two excellent scholars are disputing are their fundamental assumptions concerning their work as psychological scientists. But as I have already taken the liberty to expound on the issues involved the reader may rightfully expect me to particularize. I hope that my discussion above shows that my attitudes come down somewhere between what I understand the attitudes of Leahey and Kendler to represent. I agree with Kendler that there is nothing inherently wrong with operationism, if we understand what Bridgman and the positivists who said similar things were really driving at—a clear understanding of what our claims on knowledge come down to. As we have seen, Kendler's commitment to operationism is underwritten by realism, but I have always considered myself an *objective idealist* or, alternatively, I would also consider myself a *critical realist* in the style of Mach.

It is unquestionably the case that what our minds "fill up with" over a lifetime is knowledge gleaned from experience, and experience is in large measure a relationship with that immutable, palpable, hard-rock thing called reality. But to suggest that the *ordering into meaning* of that reality is done by sources in reality is to take a step that I am not prepared to accept. Hence, I favor Leahey's critique at this point. Our understanding of physical things still demands a conceptualizaiton from our point of view. I am a complete Machian at this point—as I understand his basic attitude to be (refer above). And it is particularly in the social realm of human experience that a more idealistic view of things is called for. Human relations are made contingent upon, *not* what the Skinnerians claim as an "effect" of some operant response, but the assumptive "causes" that we refer to as premises, attitudes, affections, and so forth. Indeed, the debate between Leahey and Kendler is an excellent example of two human beings, wrestling over assumptions that are discernible and, depending upon which ones we affirm, determine our behavior in a way that "environmental inputs" could never hope to do.

Now, Leahey has done an excellent job of presenting the shortcomings of operationism, when it is based upon realism and focused upon instrumental sources of meaning. But I would not call operationalizing a "myth," because I think that in the way Bridgman was trying to use this concept he was, in effect, doing a "job analysis" of the physicist in action. Operationism becomes a myth only when we believe that it is some one, sure-fire way in which to derive meanings from a source completely free of our intellects (points of view, assumptions, etc.). This is when we begin anguishing over surplus meaning and related nonsense; if meanings are framed by precedents, there will always be some "surplus" meaning in the framework.

If we merely see this operational process as one step in what we have to do in order to make our ideas clear via validating evidence, then I think the mythology drops out. There is no more an operation-ISM in this case than there is an experiment-ISM. These are simply words we use—operational definition, experimental design—to convey what it means to validate our theories. This would free up debate to confront issues that are more to the point of theoretical analysis. Psychologist A, a realist (or naive realist), operationalizes in the belief that meanings contained in his or her theory are derived from reality. Psychologist B, an idealist (or critical realist), operationalizes in the belief that meanings he or she has conceptualized can be demonstrated in the empirical realm. But both psychologist A and B are *also* accruing evidence in this process of operationally defining terms, evidence which will either support or fail to support a contention that they have made (within bounds of the affirming-the-consequent fallacy). It seems to me that it is this way of gathering evidence that unifies the so-called natural sciences, not the physicalistic data which they all supposedly tackle.

Although I agree with Leahey that psychologists have ideologies, I do not agree that operationism is or ever has been an ideology *per se*. I would put the ideological or paradigmatic case more in the realm of British Empiricism, with its attendant realism and reductionistic thesis. I say this because, as noted above, there was a strong bias in behaviorism toward such realistic and reductive explanations even before operationism or verifiability was framed. Hence, I believe that Bridgman's thought as logical positivism was merely adopted by those influential psychologists of our history who already had agreed upon a style of looking at and interpreting what they presumed to be an implacable, traceable, and manipulable reality. I am one of a minority of psychologists who believe that psychology *does* have a paradigm today and that it is *not*, as Robert I. Watson (1967) claimed, in some preparadigmatic stage of its development. Our "received view" paradigm since at least 1920 in the rise of experimental psychology involves a Lockean style of explanation, with realism, reductionism, and empiricism framing the way in which we are taught to think about and describe data (Rychlak, 1981a).

The problem with attacking operationism *per se* is that it cuts off a measured understanding of what it is that we *must* do if we are to validate our theories. Thus, after suggesting that we do not have to continue the framing of operational definitions, Leahey (1980) says: "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 be devoted to improving the abilities of their theories to do just that" (p. 40). If we were now to analyze what "the best efforts" of scientists to understand, explain, and predict our experience entails, I feel certain that at some point we would be considering the definition of terms within an experimental format. This is where we attempt to control alternative interpretations and predict what is likely to flow from our unique and distinctive theoretical claims on the area of knowledge we happen to be addressing. And though Leahey might not like the terminology, at that point it would surely be proper to speak of the framing of definitions operationally.

Of course, I have no lasting commitment to sheer terminology. The ideas behind our terms are what count. If operational definitions have been equated with some notion of "tracing the real and unquestionable truth" then I would be not only pleased but enthused to drop this term. If I *must* believe in an immutable reality to claim the title of psychologist then I like Mach before me am prepared to forego my professional identity.

I think of Kendler's efforts to break meanings down into four types as his personal operational analysis (symbolic) of the way in which he believes that we all conduct our work as scientists. I think that implicit in his analysis is the distinction between theory and method, as when he stresses that "intuitive meaning [theory] must be contrasted with operational meaning [method]" (Kendler, 1981, p. 339). Of course, Kendler distinguishes between intuitive meaning and theoretical meaning, something that I would not find useful. Intuition is framed by informal theory, and doubtless metaphysical assumptions engage the person's intellect at this point as well. Indeed, there are intuitions about the theory and intuitions about what sort of instrumentation is equal to the task of operationalizing the theory. That is, we sometimes forget that the scientific method is also framed by a *theory of knowledge*. Scientists accept this theory, with its reliance upon validating evidence and the correspondence theory of truth. Given a method of framing proof based upon this theory of science, the scientific community can now entertain a *common* grounds of testing their myriad interpretations of even the *same* empirical facts.

Something I worry about, and I think Kendler may have a similar concern, is that when psychologists criticize the scientific method—an aspect of which I am now contending is the operational definition of a theoretical term—they are likely to give the impression that there is an alternative method to this validational effort (see Rychlak, 1981a, pp. 449-456). Many psychologists who share my dissatisfaction with the mechanistic accounts of behavior that currently dominate the profession believe that the problem is with the *method* adopted by psychologists and not with the *theories* being put to test by that method. They interpret the Kuhnian analysis to mean that science revolutionizes its methods of doing scientific investigating from time to time. I agree with

Leahey that, because of their poor attention to metaphysics, psychologists have made a metaphysics of their method (Burtt, 1955, p. 229). But this is *not* the fault of the method. It is the fault of those who do not know when they are theorizing and when they are proving what they are theorizing about. That Kuhn (1970) thinks of revolutions occurring in the theoretical realm and *not* the methodological is clear from many sources in his book (p. 7, pp. 17-18, p. 77, and especially p. 182).

Scientists change designs, they acquire new instruments so that their experimental work takes on new dimensions. But the fundamental logic of validation has *never* changed in science, and it never will. To rely exclusively on what we mentioned above as a talky-talk approach to psychology would mean that procedural evidence would become the exclusive grounds for belief in a subject matter. As I said above, famous scientists like Galileo and Einstein placed so much credence in their mathematical ratiocinations that they actually bordered on this attitude. But science has ever demanded that someone put such ideas to empirical test in a properly controlled, operationally understood environment. Then, of course, we have the phenomenon occurring of some inexplicable but highly reliable effects taking place in the methodological context for which no background theory can account.

And so it goes, a two-way street, two *contexts* of knowledge-generation as Reichenbach (1938) called them. This is what we are left with, and it is to my way of thinking quite enough. Science is a distinctive manifestation of our human potential, reflecting our strengths as well as our limitations and weaknesses. It is, so far as I am concerned, a marvelous demonstration of the belief that we are teleological organisms. But then, I have not operationally framed this theoretical hypothesis in a properly controlled experimental setting; hence, my telic claim is based exclusively on procedural evidence. But then, so was Einstein's theory of relativity—to begin with!

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