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A CRITICAL APPRAISAL OF CONTEMPORARY APPROACHES IN THE QUANTITATIVE ANALYSIS OF BEHAVIOR

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For over 40 years, much research has sought to mathematically describe the relation between operant choice responding and reinforcement. This quantitatively oriented research and the accompanying theoretical interpretation of the data are embedded in the broader context of general experimental psychology, which can at times be regrettably mentalistic. Researchers and theorists in the area of the quantitative analysis of behavior may therefore need to be sensitive to mentalistic influences arising from outside the experimental analysis of behavior. One possible mentalistic influence is the psychophysics of S. S. Stevens. A second is the use of "value" as an intervening, mediating organismic variable, in the fashion of mediational S-O-R neobehaviorism. A third is essentialist thinking and the explanatory strategy of instantiation, rather than the pragmatism of the experimental analysis of behavior.

Stevens (1951) opened chapter 1 of his classic *Handbook of Experimental Psychology* with the following statement: "The stature of a science is commonly measured by the degree to which it makes use of mathematics" (p. 1). Quantitatively oriented research into operant choice behavior and the theoretical interpretation of the data that has accompanied that research over the last 40 years represent an interesting case study pertaining to Stevens's statement.

As many readers undoubtedly know, an important study in the quantitative analysis of behavior is Herrnstein (1961). Herrnstein systematically manipulated the relative frequency of reinforcement on a concurrent variable-interval variable-interval (VI VI) schedule and measured the relative frequency of responding. He found that in his experimental setting, when the relative frequencies of responding were plotted against obtained relative frequencies of reinforcement on a standard X-Y graph of Cartesian coordinates, the resulting function was a straight line with a slope of 1.0. He concluded that "the relative frequency of responding on a given key closely approximated the relative frequency of reinforcement [obtained] on that key" (Herrnstein, 1961, p. 272).

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This relation was termed "matching" and was surely an economical, parsimonious description of the data. Subsequently, Baum (e.g., 1974, 1979) systematically applied this finding to a wide variety of settings and parameters of choice experiments and developed what has been called the "Generalized Matching Law" (GML). This descriptive law uses ratios of responses and reinforcement. As expressed in nonlogarithmic form, the Matching Law is

$$B_1/B_2 = k(Rft_1/Rft_2)^a$$

As expressed in logarithmic form, the Matching Law is

$$\log (B_1/B_2) = \log k + a \log (Rft_1/Rft_2)$$

The logarithmic form is generally preferred. In practical terms, the resulting function can be represented by a straight line on log-log coordinates, with a slope that corresponds to the exponent *a* (representing sensitivity to manipulated reinforcement parameters) and an intercept that corresponds to the term *k* (representing bias, which is to say control by some factor not accommodated on a two-dimensional plot). Logarithms also yield homogeneous distributions of variance, unlike proportions or relative measures (see Stevens, 1957, e.g., p. 162).

A great deal of quantitatively oriented research has taken its point of departure from the empirical and theoretical work of Herrnstein (1961, 1970) and Baum (1974, 1979). Readers are referred to Davison and McCarthy (1988), DeVilliers (1977), Fantino (1977), and Williams (1988) for excellent summaries of this work. In addition to contributing to an understanding of basic processes as they play out in the laboratory, this work has been applied to a wide variety of other areas, ranging from behavioral economics to addiction to foraging (e.g., see Mazur, 2002, chapter 14, and Herrnstein, 1997, for a review). Taken together, this corpus of work has proved extraordinarily influential, as it argues for behavior as an orderly subject matter in its own right, in a confirmation of Skinner's (1938) early goal.

Nevertheless, this quantitatively oriented research and the accompanying theoretical interpretation are embedded in the broader context of general experimental psychology, which can at times be decidedly mentalistic. Given that behavior analysis is staunchly antimentalistic, researchers and theorists in the area of the quantitative analysis of behavior may therefore need to be sensitive to the encroachment of certain mentalistic influences arising from outside the experimental analysis of behavior. These influences pertain to the way scientists explain the behavior of their research subjects as well as to the way scientists explain their own behavior as scientists. For example, it has been argued that mentalism (a) interferes with effective prediction and control of behavior; (b) obscures important details; (c) misrepresents the facts to be accounted for; (d) impedes the search for genuinely relevant variables; (e) allays curiosity by getting people to accept the postulation of fictitious entities as explanations; (f) gives false assurances about the state of our knowledge; (g) leads to the continued use of scientific techniques that should be abandoned, such as the hypotheticodeductive method, because they are wasteful and ineffective; and (h) misrepresents the ways people think (Catania & Harnad, 1988). To the extent the items in the list above are troublesome, then research and theory affected by mentalism may be not as valid as they at first blush appear to be.

Despite the lengthy history of quantitatively oriented research, the intellectual sophistication of the individuals who carry it out, and the prestige of the institutions at which those individuals work, an analysis of the general style of this research raises three possible areas in which mentalism might impact behavioral research and theory. The first is the extent to which research and theory are influenced by the mentalistic tradition of psychophysics, such as that represented in the work of S. S. Stevens. The second is the extent to which research and theory use "value" as an intervening, mediating organismic variable, in the fashion of traditional mediational S-O-R neobehaviorism. The third is the extent to which research and theory embrace essentialist thinking and explanation by instantiation, rather than pragmatism. The purpose of the present article is to critically review these possible areas of mentalistic influence in quantitative analyses of behavior.

The present analysis pays particular attention to the verbal behavior of researchers and theorists who are active in quantitatively oriented research. Readers may well have to decide whether the possibilities outlined below and the cited instances of verbal behavior reflect casual usage that could easily be given a more precise analysis in a technical vocabulary, or whether the verbal behavior reflects the influence of mentalism and a commitment to explanation in terms of "events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner in Catania & Harnad, 1988, p. 88).

The analysis recognizes that any form of verbal behavior is complex. Consequently, the scientific verbal behavior of researchers and theorists is under multiple control. Some verbal behavior may therefore reflect nothing more than conventional linguistic practices or modes of speaking, rather than mentalism. For example, if astronomers in North America happen to talk of "sunrises" and "sunsets," they presumably are not implying that Zeus is literally dragging the sun out of the Atlantic Ocean every morning with an ethereal chariot pulled by winged steeds and then depositing the sun into the Pacific Ocean every night. Similarly, if behavior analysts talk of "changing their minds," they presumably are not implying that some nonphysical entity called a "Mind" literally exists in another dimension, changes in which cause changes in behavior. Moreover, there is danger of affirming the consequent: Although mentalism implies questionable language, the existence of questionable language doesn't necessarily imply mentalism. Nevertheless, if a pattern of verbal behavior does seem evident, one can ask whether research and theory would benefit from a sharpening of usage.

Possibility 1: An Influence of the Mentalistic Tradition of Psychophysics and S. S. Stevens

S. S. Stevens is an extraordinarily influential figure in experimental psychology, particularly in psychophysics and experimental methodology. Stevens was a contemporary of B. F. Skinner, and both were graduate students and later faculty members in the Harvard University Department of Psychology. Stevens spent much of his scientific career developing and validating a formal mathematical description of the relation between two numerical values in a psychophysical experiment. The first value for Stevens was the "objective" magnitude of stimuli that were presented to participants, as measured with the instruments of physics. The second value was the verbal report by a participant when participants were asked to estimate the magnitude of the psychological "sensation" they experienced when presented with the objective stimulus. Presumably, this value was derived from some "subjective scale" of such magnitudes, as judged by the subjects who were "observing" their own subjective reactions in some sense.

The equation that Stevens (1957) developed to describe the data was $\psi = k\ S^n$. This equation is generally known as the "Psychophysical Law." In this equation, ψ represents the strength (i.e., the magnitude) of the psychological, subjective sensation, as inferred from the participant's reported numerical estimate; k is an individual difference parameter; S is the actual magnitude of the physical stimulus; and the exponent (or power) n is the term that supposedly reflects the sensitivity of the subjective estimate of the stimulus in that modality to its objective measure. Particular values of the variables would be used to describe any particular instance of data reported from a subject, although the exponent n was held to cluster around a specific value for a given stimulus modality. Stevens (1957, p. 166) reported data from some 14 different stimulus modalities to make his case. Technically, however, the form of the equation was what was regarded as important, rather than the specific values of the variables.

This general style of research found favor with much of the community of experimental psychologists. The positivistic tradition of describing observable data mathematically was well established in science. For instance, Newton's laws of motion mathematically described the observable relation between force and movement, and his laws of gravity mathematically described the position of bodies falling in space after specified periods of time. In one sense, Newton asserted that it was sufficient to assume that gravity did exist and had the properties ascribed to it, rather than feign formal hypotheses about "action at a distance" and other properties of the gravitational force, such as an "ether" that mediated its effects. In another sense, however, Newton did appeal to his God as ultimately responsible for the properties of his mechanical universe.

One may argue that Stevens was simply conforming to this historical tradition. Stevens held that science could deal only with publicly observable data. The sensation itself was assumed to be an entity in a subjective dimension and hence not publicly observable. As a result, the subjective sensation couldn't be directly and literally included as part of science. Stevens therefore restricted his explanation to describing the experimental operations he conducted and the mathematical relations among the publicly observable data produced by those operations. Stevens was widely regarded as a champion of operationism, arguing that he was only using mathematics to describe relations between two publicly observable variables—the physical magnitude of the stimulus and a verbal report—rather than trying to make a science based on introspection. The publicly observable data were regarded as the admissible proxy or surrogate for the inadmissible phenomena from the subjective dimension. A representative passage in which Stevens expresses his underlying assumptions and the prescription for working around the limitations is as follows: "Since sensation cannot refer to any private or inner aspect of consciousness which does not show itself in any overt manner, it must exhibit itself to an experimenter as a differential reaction on the part of the organism" (Stevens, 1935b, p. 524). Thus, a particular view of the relation between mental and physical was institutionalized, along with a particular style of experimentation according to which that relation could supposedly be investigated. For example, Stevens (1957) took some pains to distinguish his approach—the validating of ratio scales of measurement was particularly important—from earlier approaches, such as

Fechner's attempts to scale "just noticeable differences" (Shepard, 1978) and the ambiguities of introspective structuralism. Moreover, the academic prestige of Harvard meant that others, at Harvard or elsewhere, embraced that general style of research and theorizing.

Not all researchers and theorists were convinced, however. For example, Kantor (1938; see also Kantor, 1945) objected bitterly to Stevens's use of operationism and verbal constructionism to implement dualism and argued as follows:

> On the one hand, the conventional sensation-psychologists have attempted to assimilate the principle [of operationism], with the result that what has been proposed as a fundamental improvement in forms of physical science has been used to implement conventional dualism in psychology. . . . As an illustration of the former attitude, we refer to Stevens' use of the principle. . . . According to Stevens everything reduces to differential reactions. . . . Obviously we have here such a truncation of the operational conception as to convert it into a thoroughgoing subjectivism. . . . Stevens' adoption of the operational principle comes to nothing more than a mentalistic psychologist's surface concession to objectivity. (pp. 14–15)

Similarly, Skinner (1945) criticized the "attempt to climb on the behavioristic band-wagon unobserved" (p. 292) and "preserve the old explanatory fictions unharmed" (p. 292). Skinner further castigated "the operationism of Boring and Stevens" (p. 292), arguing that "[t]he position is not genuinely operational because it shows an unwillingness to abandon fictions" (p. 293).

In any event, given that Stevens influenced experimental psychology generally, does the literature indicate that Stevens influenced the quantitative analysis of behavior specifically? Again, Stevens could play a role in the quantitative literature for any of several reasons, not necessarily related to mentalism, although separating Stevens from his mentalism would presumably be difficult for someone conversant with the antimentalistic stance of behavior analysis.

Research in the Harvard Pigeon Lab

The interpersonal dynamics of the Harvard Pigeon Lab and the relations among the students in the Pigeon Lab, Skinner, and Stevens are described in a special section of the *Journal of the Experimental Analysis of Behavior*, 2002, volume 77, pages 301–392. Baum (2002, p. 349) described Skinner as amiable but remote, often busy with his own writing. Skinner essentially withdrew from laboratory activity at the end of the 1961–1962 academic year, and R. J. Herrnstein became the dominant intellectual force in the Pigeon Lab. According to Boakes (2002), "For many of us, contact with Herrnstein was irregular. The unspoken attitude seemed to be that, if we were good enough to get into Harvard and to complete the prelims, we were good enough to choose our own topic and pursue it sensibly. . . . When I sought his advice, Herrnstein would give good value" (p. 375). Heyman (2002) stated that in research meetings, "Dick Herrnstein's imaginative and insightful responses to new data and new models often led the discussion. His comments came with humor and anecdotes, and he was as quick to see the positive features of a research project as well as what rested on untested assumption" (p. 382). Entering students took lab rotations and coursework that involved exposure to Stevens and psychophysics. Baum (2002) reported that "[i]n the first semester, we were subjected to a strenuous proseminar dominated by E. G. Boring and S. S. Stevens. . . . The second semester was easy sailing by comparison. Skinner took 4 weeks" (p. 350). Hineline (2002) described his experiences as follows:

> [I]n academic matters we were acutely aware that there were two major fiefdoms in the basement of Memorial Hall, clearly demarcated by invisible boundaries. S. S. Stevens, psychophysics, and the power law reigned at one end; Skinnerians with relay racks generating intricate patterns of behavior on reinforcement schedules were at the other end. . . . Smitty Stevens . . . became an even more imposing presence on the first meeting of the proseminar, when surveying the 13 newcomers arrayed around the massive table, he commented, "There are too many people in here." (p. 383)

Indeed, Herrnstein's original adviser was Stevens (see Baum, 2002, pp. 347–348), and Rachlin had intended to complete a master's thesis under Stevens (personal communication, July, 2007). A mathematics major as an undergraduate, Fantino (2002, p. 377) said he was trying to decide between psychophysics and cognition. All found real live behavior more interesting, and in recognition of the amount of energy and enthusiasm associated with the Pigeon Lab, changed over without delay to the Pigeon Lab. Data were discussed in weekly Pigeon Lab research meetings. Of 11 contributors in the special section of the *Journal of the Experimental Analysis of Behavior*, mentioned previously, five commented specifically on the quantitative analysis of behavior as the important feature of the Harvard Pigeon Lab.

Mentalism would be a problem if researchers and theorists in the quantitative analysis of behavior uncritically accepted the traditional distinction between objective and subjective, or physical and mental, dimensions that underlay Stevens's work in psychophysics and for that matter much of the remainder of experimental psychology. Some representative passages speak to this point:

> Ironically, S. S. ("Smitty") Stevens, Dick's first mentor, probably appreciated the matching law more than Skinner did, judging from Skinner's later comments. . . . Smitty and Dick continued to have a warm relationship, even though Smitty was hurt when Dick moved to the pigeon lab in his 1st year of graduate study (Baum, 1994). Through Dick, Smitty's quantitative psychophysics, particularly his psychophysical power law (Stevens, 1957), was to have a profound effect on the course of research in the Pigeon Lab. Dick once remarked to me that he thought the matching law was like the psychophysics of choice. When I proposed what came to be called the generalized matching law, which was a power law just like the psychophysical power law, I too was showing the influence of Stevens' psychophysics (Baum, 1974). (Baum, 2002, pp. 347–348)

> Herrnstein seemed ambitious for his matching law to emulate Stevens' power law. (Boakes, 2002, p. 376)

> the power law form of the generalized matching law strongly resembles S. S. Stevens' power law description of the psychophysical function. (McSweeney, 2002, p. 388)

Finally, Davison and McCarthy (1988) pointed out the important relation between quantitatively oriented research and Stevens's psychophysics in the following passage:

> Psychophysics is the study of the relation between measured stimulus input and measured behavioral output. The study of the empirical matching law . . . is no more and no less than this. At the present, the functions working for us are power functions, and the approach is similar to that taken by S. S. Stevens in psychophysics. (pp. 57–58)

As with Stevens's Psychophysical Law, a problem arises if the GML is seen as a way to mathematically describe a relation between events in the physical dimension and events in the mental dimension. The events in the physical dimension were variously the objective measurements of stimuli and responses, and the events in the mental dimension were the resulting quantities in the subjective dimension that mediated the physical events. Is the GML an attempt to express a relation between obtained reinforcement, taken as the substitute for the "subjective value" of reinforcers, on the one hand, and the objective distribution of choice responding, on the other hand? If so, this orientation would be mentalism, and would create problems from the list identified earlier. The mentalism is not in the use of a Power Law or any other mathematical expression to describe relations in a set of publicly observable data. Rather, the mentalism is in the use of a Power Law or any other mathematical expression to assert some relation between the ontologies of physical and mental dimensions, in a tradition that dates back at least as far as Fechner.

Radical Behaviorism as Philosophy in the Harvard Pigeon Lab

The brief review above raises a series of rather abstract but nevertheless critical points that lie at the heart of radical behaviorism as a philosophy. For example, radical behaviorism does not grant the assumption of two different dimensions, where one is subjective, mental, and unobservable and the other is objective, physical, and publicly observable. In addition, radical behaviorism does not grant the assumption that "psychology, in order to meet the requirements of a science, must confine itself to [publicly observable events]" (Skinner, 1945, pp. 292–293), which are to be examined in lieu of mental events for which they supposedly substitute (e.g., Skinner, 1953, pp. 280–282). In point of fact, "the radical behaviorist may in some cases consider private events (inferentially, perhaps but none the less meaningfully)" (Skinner, 1945, p. 294). However, when these events are considered, they are considered as behavioral events, rather than mental. Therefore, the function of mathematical statements is not to represent a relation between the ontologically distinct dimensions of mental and physical, even though some publicly observable measure can be asserted to substitute for the mental.

Interestingly, many students in the Pigeon Lab did not seem to spend much time engaging the fine details of radical behaviorism as a distinct philosophy of science. For example, Zuriff (2002) commented,

> To my chagrin, not only was Skinner not accessible, but Dick Herrnstein, my mentor, did not share my interest in philosophy of behaviorism. To be sure, if pushed into a philosophical discussion, he could hold his own, displaying the intellectual brilliance he

> brought to everything he approached. But his heart was with the science of behavior, not its conceptual foundations. . . . Whereas the Pigeon Lab generated two coherent research programs, one under Skinner and one under Herrnstein, no comparable coherent philosophy of behaviorism emerged, other than Skinner's own work. Undoubtedly, there were many reasons for this, but I will mention only three. First, as Skinner increasingly absented himself from the lab, the opportunities for him to enlist members of his lab into his vision of behaviorism decreased. His philosophical discussions were conducted primarily with his followers around the world rather than with members of the Pigeon Lab down the hall. Second, Herrnstein was committed to empirical behavioral research rather than the exploration of the conceptual foundations of behaviorism, and as a consequence, no institutional context existed for the development of a coherent philosophy. Third, graduate students and postdoctoral fellows came to Harvard for the empirical behavioral research not the behaviorism. Those seeking conceptual training chose to go elsewhere, for example, to the University of Nevada to work with Willard Day. Furthermore, as Skinner's interest in the philosophy of behaviorism shifted to social issues, the Harvard students had even less interest because of the tenuous connection between his social ideology and the science of operant conditioning he founded. To be sure, many an animated philosophical discussion took place in the Pigeon Lab, but no consensus emanated from them. (pp. 368–369)

Zuriff did point out that "although the Pigeon lab did not prove to be a place for the continued incubation, discussion, and development of these [philosophical] ideas as it was for Skinner's science of operant conditioning, many members of the lab made important individual contributions to the discourse on the philosophy of behavior over the years" (p. 369). However, inspection of these contributions reveals that they were not always compatible with Skinner's radical behaviorism as a philosophy. For example, Zuriff mentions that "John Staddon's (2001) *The New Behaviorism* sets out to refute nearly every aspect of radical behaviorism and to replace it with his 'new behaviorism'" (p. 370).

Thus, it appears that Skinner's philosophy of radical behaviorism was not broadly adopted among the students in the experimental analysis of behavior in the Harvard Department. A gap appeared, and commentators suggest it was filled at least partially with more traditional ideas, such as Stevens's. That Stevens could potentially influence the experimental analysis of behavior both methodologically and philosophically is ironic. By most accounts, Skinner and Stevens were not close personal colleagues. For example, as early as 1945 Skinner explicitly and publicly disparaged the "operationism of Boring and Stevens" as merely "methodological behaviorism" (Skinner, 1945, p. 292). Later, he pointed out that "it was Stevens . . . who then continued to believe in the existence of mental life" (Skinner in Catania & Harnad, 1988, p. 217), and "S. S. Stevens has applied Bridgman's principle [of operationism] to psychology, not to decide whether subjective events exist, but to determine the extent to which we can deal with them scientifically" (Skinner, 1969, p. 227). Stevens's position seemed to subscribe to the position that if science could engage the mental quantitatively, things were all right.

Despite Skinner's pointed criticisms, the operationism of Boring and Stevens has dominated traditional psychology for more than 60 years. From the radical behaviorist perspective there are no such mental causes underlying the observable data, as Stevens or any other theorist supposed. Hence, treating observable data as the expression of mental causes in a subjective dimension leads researchers to neglect the environmental factors of which the observable data are actually a function. Day (1969) commented on the palpable irony of this state of affairs in one theoretical paper about radical behaviorism as a philosophy of science, and the extent to which others, possibly including those in the Harvard Department, actually made contact with those ideas:

> Strange blends of Skinner and conventional behaviorism abound. . . . Mentalism among Skinnerians is rampant, and they are quickly trapped by the operationism of Boring and Stevens. . . . I have taken the liberty of speaking here to some of those who preach most loudly a supposedly Skinnerian line. (pp. 326–327)

Some implications of filling the aforementioned gap with Stevens's traditional ideas are discussed in the next section.

Possibility 2: "Value" as an Organismic Theoretical Term That Mediates the Relation Between S and R in the Tradition of Mediational S-O-R Neobehaviorism

Moore (2008) recently suggested that by the early 1930s the shortcomings of classical S-R behaviorism had become apparent. For instance, despite its early benefits, classical S-R behaviorism couldn't convincingly account for the variability and apparent "spontaneity" of many forms of behavior. Sometimes particular stimuli and particular responses just weren't correlated in the way that classical behaviorism required. Consequently, researchers and theorists began to abandon classical S-R behaviorism during the 1930s.

One form of behaviorism emerged and soon became dominant. In this newer form, appeals to unobservables were explicitly readmitted. The unobservables were regarded as internal, "organismic" variables that mediated the relation between stimulus and response. According to this new point of view, publicly observable stimuli (S) affect mediating, unobservable internal variables (i.e., O: acts, states, mechanisms, processes, entities), and these variables in turn affect publicly observable responses (R). The newer form of behaviorism is here called mediational S-O-R neobehaviorism. By mediation is meant that external stimuli activate some intervening, internal process or entity (i.e., a private event) that is causally connected in a complex but systematic way to an eventual response, and the mediating process or entity is the proper focus of psychological science, rather than the response itself (Moore, 2008).

Skinner was a contemporary of these various events, but he took a different path. To be sure, early in his career Skinner did seek to analyze behavior in terms of the reflex model, akin to classical S–R behaviorism (e.g., Skinner, 1978, pp. 113 ff.). However, as his own work in the laboratory progressed, he formalized the notion of operant behavior. This form of behavior does not embrace the mediational model, which others were embracing. Rather, it emphasizes the important role of consequences. Thus, although Skinner was a neobehaviorist in the chronological sense that he developed an alternative to classical S–R behaviorism in the 1930s, as did others, his alternative did not involve postulating hypothetical mediating entities (Moore, 2005).

In any case, let us return to events associated with the mediational neobehaviorist approach that developed during the 1930s. An important concern of the neobehaviorists of the time was how to ensure that researchers and theorists weren't just making something up that was specious and unscientific, particularly when they invoked "mental states" as mediating organismic variables. Moreover, how could agreement be reached on the important concepts? As suggested earlier in the present analysis, S. S. Stevens became a leader in the discussion (see Boring, 1950). For Stevens, the answer was operationism. In fact, Stevens (1935a, 1935b, 1936, 1939) contributed four highly influential articles advocating the new operational methodology. According to operationism, the meaning of a scientific concept was entirely synonymous with the corresponding set of publicly observable operations (e.g., by which it is measured). Thus, the meaning of the term "length" was determined by the publicly observable operation of measuring the distance in question. Operationism therefore was able to generate agreement about a scientific concept and promote both communication and scientific advance. As reviewed in the preceding section, with regard to psychophysics and the concept of the subjective "sensation," publicly observable responding can be taken as the surrogate or proxy for the subjective entity, so that researchers and theorists can agree on the meaning of the term. The mediating entities were designated as theoretical terms, to distinguish them from observational terms that could be directly measured with the instruments of physics.

Traditional researchers and theorists might be further concerned with "scaling" as a feature of measurement theory. The values of a variable in one scale may sometimes be transformed and expressed in another scale, to yield an equivalent measure. At heart is an assumption that some scale of units in the objective, physical dimension can be transformed into some other scale of units in a psychological, subjective, or mental dimension, where the values of the latter scale were the result of mental processes on the part of the participant in the experiment. Indeed, such an assumption was at the core of Stevens's approach to psychological science generally and psychophysics especially. As Stevens (1957) put it, "The basic principle seems to be that equal stimulus ratios tend to produce equal sensation ratios" (p. 162). (As an alternative, readers might consult Zuriff, 1972, for a different sort of behavioral interpretation of psychophysical scaling.)

At issue is whether approaches in the literature of the quantitative analysis of behavior exhibit the kind of theorizing that Stevens advocated. This kind of theorizing reflects a focal concern with hypothetical internal processes and using some measure in the physical dimension to scale what is asserted to be some corresponding measure in a subjective dimension. Again, if the literature does exhibit particular forms of language, it could do so for any of several reasons, not necessarily related to mentalism. However, a pattern of language that repeatedly appeals to internal processes and other dimensions, if only metaphorically, would seem to depart from the conventional practices of behavior analysis, in which such language is typically absent.

Consider now some passages from the literature of the quantitative analysis of behavior:

> The notion of correlation and the description of instrumental behavior as part of a feedback system require instead that we characterize both behavior (output) and consequences (reinforcement,

> punishment, and response cost: feedback) on a more molar level, transcending the momentary. . . . In a like manner, an organism can be viewed as collecting time samples of significant events in its environment (e.g., reinforcers and punishers), which it integrates and utilizes to control its behavioral output. The exact nature of this integrating or averaging process has been the subject of some recent research (Killeen, 1968; Davison, 1969; Duncan and Fantino, 1970; Schneider, J. W., 1970). (Baum, 1973, pp. 147-148)

> Our behaviorism would be emergent because it recognizes the causal relevance of mental states, and thus utility of having theoretical terms within the system to refer to those states. (Killeen, 1987, p. 231)

> Behavior is often more easily analyzed in terms of relevant psychological dimensions, rather than arbitrary physical dimensions . . . (Killeen, 1968, p. 268)

> What is left out of equation 5a is a name for the concatenation of subjective scales. "Value" may be introduced as such a name . . . (Killeen, 1972, p. 491)

> In the context of some standardized concurrent design, the matching relation may be used as a formula for defining subjective scales of reinforcement. (Killeen, 1972, p. 492)

> The hypothesis that choice and timing should be mediated by a common representation of reinforcer delay has been tested in studies using a modified concurrent chains procedure (Kyonka & Grace, 2007, p. 394)

Of interest for present purposes is whether one or more of the examples of language cited here are pedagogical, heuristic, and taken out of context (Baum, personal communication, May 7, 2007) or whether they should be viewed as metaphors that appeal to mediational processes in a different dimension. If the former, there would be no problem with mentalism, as the language is comparable to astronomers talking of sunrises and sunsets. If the latter, there would be a problem with mentalism: Publicly observable stimulus events such as reinforcement are taken to produce some mediating, hypothetical entity such as "value" according to some mediational process, perhaps involving a copy of events and parameters called a "representation," in a mental dimension, and then the mentally calculated value rather than the reinforcement itself is taken to cause some distribution of publicly observable choice responding, perhaps according to some further mental process that remains to be specified. Readers are encouraged to review the literature cited above and elsewhere, to determine which is the case.

In point of fact, the operationism of Boring and Stevens, such as might be said to validate the calculation of a subjective scale of reinforcement value, plays no role in the epistemology of behavior analysis. The operationism of Boring and Stevens arose from a particular view of verbal behavior, namely a logical, referential, symbolic view. As early as 1945, Skinner rejected this view:

> The weakness of current theories of language may be traced to the fact that an objective conception of human behavior is still incomplete. . . . Attempts to derive a symbolic function from the principle of conditioning (or association) have been characterized by a very superficial analysis. It is simply not true that an organism reacts to a sign "as it would to the object which the sign supplants" (Stevens, [1939], p. 250). . . . Modern logic, as a formalization of "real" languages, retains and extends this dualistic theory of meaning and can scarcely be appealed to by the psychologist who recognizes his own responsibility in giving an account of verbal behavior. (Skinner, 1945, pp. 270–271)

Rather, radical behaviorists look for the contingencies that control the scientific verbal behavior in question. At issue is whether that verbal behavior reflects the mentalism of "a vast vocabulary of ancient and non-scientific origin" (Skinner, 1945, p. 271), as in the explanatory fictions of folk psychology or a thoroughgoing natural science account. At issue is the source of the control over the term as an instance of scientific verbal behavior.

Possibility 3: Embrace of Essentialist Thinking and Explanation by Instantiation Rather Than Pragmatism

Pragmatism is roughly the position that the truth value of a proposition is a function of the degree to which it promotes direct, successful action, such as prediction and control. The experimental analysis of behavior embraces pragmatism. As outlined elsewhere (Baum, 2003; Moore, 2005; Smith, 1986, 1992, 1995), Skinner traced many of his ideas about scientific pragmatism to Francis Bacon and Ernst Mach.

For example, Skinner (1953) endorsed Mach's position that the first laws and theories of a science were probably rules developed by artisans who worked in a given area. As these individuals interacted with nature, they developed skilled repertoires. Descriptions of the effects brought about by relevant practices were then codified in the form of verbal statements that functioned as verbal stimuli, the purpose of which was to occasion effective action. The verbal statements, often taking the form of maxims or other informal expressions (e.g., "rules"), supplemented or replaced private or idiosyncratic forms of stimulus control. The verbal stimuli became public property and were transmitted as part of the culture, enabling others to behave effectively.

However, science progressed beyond just these lower level activities, to develop higher-order statements and concepts. A relevant passage is as follows:

> [Science] is a search for order, for uniformities, for lawful relations among the events in nature. It begins, as we all begin, by observing single episodes, but it quickly passes on to the general rule, to scientific law. . . . As Ernst Mach showed in tracing the history of the science of mechanics, the earliest laws of science were probably the rules used by craftsmen and artisans in training apprentices. . . . In a later stage science advances from the collection of rules or laws to larger systematic arrangements. Not only does it make statements about the world, it makes statements about statements. (Skinner, 1953, pp. 13–14)

Many scientific laws and theories therefore specify the relation between certain classes of responses and their consequences.

A related concept in scientific epistemology is instrumentalism, sometimes also known as conventionalism. Instrumentalism is roughly the position that scientific terms are nothing but conventional verbal devices serving a particular function. They are said to imply nothing about the existence of an actual entity. Rather, they serve only as aids in particular situations, models, or expressions. They systematize and provide an economy that would otherwise be lacking.

Moore (2008) has argued that there is a difference between pragmatism and instrumentalism. The difference is that pragmatism seeks to understand why some term or concept contributes to effective action such as prediction and control, whereas instrumentalism simply accepts the term at face value, without inquiring further as to why the term occasions effective action. On this basis, in light of the commitment in radical behaviorism to the operational analysis of psychological terms, radical behaviorism distinguishes between the two and embraces pragmatism. In contrast, traditional positions remain at the level of instrumentalism, and typically fail to analyze why effective prediction and control might occur.

As mentioned earlier in the present analysis, an important feature of the GML is the exponent *a*, said to describe sensitivity to reinforcement. Readers may note the isomorphism with the exponent *n* in Stevens's Psychophysical Law, said to describe sensitivity to stimuli in the modality being examined. One of the principal assumptions of the GML, indeed the assumption that lends the term "matching" to the name of the law, is that the value of the exponent reflecting sensitivity is 1.0. After all, the law is called the "Matching Law," not the "Proportional Law" or the "80% Approximation to a Matching Law." Presumably, the value of the exponent *a* is something that can be determined from the data that have been reported in research articles. In this regard, review papers and other analyses have examined the extent to which published data supported the proposition that the exponent is actually 1.0 (Mullins, Agunwamba, & Donohoe, 1982; Myers & Myers, 1977; Wearden & Burgess, 1982; Taylor & Davison, 1983). In many instances, such articles report a value of *a* that departs from 1.0. Interestingly, Davison and McCarthy (1988, p. 85) similarly conclude that typically sensitivity is indeed less than 1.0, often around 0.8. Baum (1974, 1979) has expertly examined cases in which the exponent for sensitivity does not equal 1.0, and suggested several reasons why it might not.

For present purposes, the important point is that many researchers (e.g., Davison & McCarthy, 1988) have already recognized that departures of sensitivity from 1.0 are in fact systematically related to independent variables in the research (cf., Baum, 1983). The question then is whether formalized theories of choice such as the GML reflect the systematic departures. At present, it appears not. The GML does have a term for sensitivity but no prescription for determining when it is and is not 1.0, as a function of the independent variables of the research. Rather, the GML seems to implicitly assume that an exponent for sensitivity of 1.0 is normative and that departures are nothing but some sort of unavoidable nuisance, to be adjusted post hoc. This assumption seems to differ from the pragmatic approach identified earlier. Indeed, it seems to suggest that the purpose of the GML or any other equation is to correctly capture the essential, Platonic nature of the phenomenon being described.

Essentialism is often supported by an explanatory strategy of instantiation. According to the explanatory strategy of instantiation, an event is said to be explained when it can be described using a general proposition, equation, or law, with some array of variables as free parameters in the statement (e.g., Kaplan, 1964; Turner, 1967). The free parameters can then take on different values in different cases (e.g., they can be "estimated" after the fact from obtained data), with the result that the statement thereby symbolically represents the data in question. Explanations by instantiation presumably get their name because the event to be explained is regarded as a particular "instance" of the class of to-be-explained events. The particular values in the expression provide a concrete realization of the more general and abstract statement. The strategy emphasizes curve-fitting and percentage of variance accounted for by some mathematical statement, equation, or model. Prediction is said to be accommodated in the superficial instrumentalist sense that one is able to predict that instances of the data can be described by deploying adjusted values of the parameters in the more general statement, rather than in the sense that a specified cause will produce a specified effect (Moore, 2008). Stevens's Psychophysical Law illustrates this form of explanation in psychology, and the relation between the Psychophysical Law and the GML has already been described. The link between essentialism and instantiation is that the form of the statement is taken to represent the essential nature of the topic being investigated.

A related concern here is prediction as an aspect of scientific behavior. Prediction can be viewed as an ingredient of explanations in terms of instantiation or from a pragmatic perspective. As an ingredient of instantiation, prediction is held to be a feature of a transcendent logical system. Primary emphasis is placed on logical status, logical relations, coherency, and so on, as if all represent evidence a researcher or theorist has insightfully tapped into a phenomenon that is of some preordained form.

In contrast, from a pragmatic perspective, prediction is relevant for practical action. Skinner (1953) commented:

> The scientific "system," like the law, is designed to enable us to handle a subject matter more efficiently. What we call the scientific conception of a thing is not passive knowledge. Science is not concerned with contemplation. When we have discovered the laws which govern a part of the world about us, we are then ready to deal effectively with that part of the world. By predicting the occurrence of an event we are able to prepare for it. By arranging conditions in ways specified by the laws of a system, we not only predict, we control: we "cause" an event to occur or to assume certain characteristics. (pp. 13–14)

Prediction is important then as a guide by which to secure reinforcers from nature. When direct control of events is possible, one can intervene or manipulate to produce desired ends. When direct control is not possible, one can nevertheless take action to produce desired ends. One obviously cannot intervene or manipulate the movement of the stars or planets, but by studying their movements one can gauge the comings and goings of the seasons, and when the planting of crops will result in a bountiful harvest.

Is it possible to view instantiation and the GML as pragmatic, leading to prediction? At issue in much current quantitative research committed to instantiation is the use of free parameters, estimated after the fact. For example, the GML uses obtained reinforcers, rather than scheduled. The pragmatic value of such an approach is unclear. Prediction involves statements in the future tense. If one must conduct the research to determine what the free parameters actually are, as in the parameters for obtained reinforcement, there is no need to "predict": The to-be-predicted data are already known, and the statement offers only a post hoc description of something that has already happened. Such statements are in the past tense, rather than future tense. To argue that a statement about behavior that has already been observed has predictive value seems curious. One could conceivably argue that future observations will conform to the general form of the mathematical statement, but one still has no idea how to "shape nature as on an anvil," as Bacon (1623/1937) put it. At best, any predictions are from effect to effect, rather than cause to effect. In cases where genuinely accurate predictions do follow from the GML, they would seem to follow from the use of scheduled parameters, rather than obtained, even though the GML is expressed using obtained reinforcement, rather than scheduled. Any predictions are valuable because obtained reinforcers correspond closely to scheduled reinforcers. At issue therefore is what discriminative stimuli set the occasion for the kind of verbal behavior called a prediction. When the verbal behavior is occasioned by obtained reinforcement, the verbal behavior is a description, rather than a prediction. When the verbal behavior is occasioned by scheduled reinforcement, the verbal behavior is then a prediction, and it has practical utility to guide effective action in the future. Often instrumentalism is cited to justify the legitimacy of some scientific statement, but the justification is only post hoc, and little attempt is made to understand the basis for any effective prediction and control.

Sometimes the GML is said to be a descriptive law, like the Gas Laws in chemistry, rather than a causal law, describing a functional relation between independent and dependent variables as ordinarily understood. To be sure, some scientific statements don't have the character of formal cause-and-effect statements (e.g., Russell, 1932). However, Skinner (1947/1972) argued that even these statements come at the end of a developmental process that included the identification of functional relations at intermediate stages. An important question is whether comparable cause-and-effect statements have been forthcoming in the quantitative literature. Perhaps the closest is melioration, advocated by Herrnstein (e.g., 1997). Momentary maximizing would seem to count as a cause-and-effect principle. However, Herrnstein (1997, pp. 68 ff.) disparaged it. Thus, there curiously seems to be no link between the GML and what causes matching, when it does occur.

In summary, if the explanatory strategy of instantiation is taken to represent some idealized mechanism, model, or substrate, as a kind of Platonic commitment to an idealized form such that variations are seen as a nuisance or failure of technique, the strategy would seem to lend itself further to essentialism. Instantiation and essentialism, by virtue of their commitment to confirming the form of the statement rather than practical action, would seem to stand in contrast to pragmatism. Of some interest, then, are such statements as in Rachlin (1971):

> It would be well, therefore, to focus future investigations on the manipulations necessary to confirm the [matching] law, rather than on whether the law is true. (p. 251)

To be sure, there may well be many reasons why such statements are made. However, the statements are potentially troublesome if they are a function of an essentialist concern with how to manipulate data to produce agreement with a theory, instead of a pragmatic concern with how to manipulate behavior to produce a beneficial outcome.

Summary and Conclusions

Behavior engendered in an experiment utilizing concurrent schedules and any resulting terms in an equation or model that describes that behavior are clearly a function of many variables. The point is that any experiment represents a locus, a particular space defined by intersection of variables that participate in that experiment. Matching may well occur in the space defined by the intersection of one set of experimental variables. Undermatching and overmatching as alternative outcomes in concurrent schedule research occur in the space defined by the intersection of a different set of variables. The sizes of these sets can presumably be determined empirically. That different variables yield different behavior is precisely the point that needs to be emphasized and that stands in opposition to essentialist thinking and the explanatory strategy of instantiation.

On what scale, then, are independent and dependent variables to be rendered? Is there to be foreclosure on anything other than a molar view, "transcending the momentary" (cf., Baum, 1973)? One would think not. Surely the question is empirical (see also Moore, 1983). Moreover, much quantitative research seems to confuse the molar–versus-molecular distinction. As McSweeney (2002, p. 389) noted, the distinction refers to the time scale of the analysis: long versus short time scales, not operant versus respondent, not behavior versus physiological reductionism. Given that the distinction refers to the time scale of the analysis, there can be either molar or molecular analyses of either respondent or operant behavior. Importantly, some recent quantitatively oriented research has begun to recognize the contribution of local relations (e.g., Davison & Baum, 2007).

Perhaps the most important consideration at this stage in the experimental analysis of choice behavior is precise identification of the causal relations that underlie that behavior. Is it matching, melioration, momentary maximizing, or optimization? At present, there seems to be insufficient identification of independent variables. A legitimate question is therefore whether the resulting scientific statements (e.g., equations, models, theories) are premature. Indeed, some years ago Davison (1984) called for a halt in model building and continued efforts at experimental analysis. At present, three models of concurrent-chains performance seem to predominate, each with particular strengths (Grace, 1994; Mazur, 2001; Squires & Fantino, 1971, see also Fantino, 1977). Whether the independent variables that control choice in concurrent chains have been sufficiently identified, so that model building is worthwhile, is a point that merits considerable discussion. In this same vein, Fantino (1984) noted, "In an era in which it seems almost every laboratory is producing its own theory aimed at describing some large segment of operant behavior, it has become easy to ignore everyone *else's* theories" (p. 380).

The present concern is not necessarily with models per se, even mathematical models. The development of models is clearly and appropriately a higher-order activity in science. Mazur (2006) has outlined several advantages of mathematical models, such as they are better than mere words, they involve more than simple curve fitting, they allow precisely testable predictions, and they unify diverse phenomena, including many in the world beyond the laboratory. At issue is whether the models have gone through a developmental process to establish a foundation in which the independent variables in the to-be-modeled behavior have been suitably identified. The proliferation of special feature equations in the literature of the quantitative analysis of choice behavior over the past 30 years or so suggests not, and testifies to the aforementioned calls by Davison (1984) and Fantino (1984). Indeed, it testifies to the usefulness of a greater pragmatic orientation among researchers and theorists.

The statement is often made that models are to be evaluated on their utility—on how well they describe a set of data and engender effective practical action. Readers will recall that the present analysis began by raising several concerns about mentalism. Three of these were that mentalism obscures important details, misrepresents the facts to be accounted for, and impedes the search for genuinely relevant variables. These concerns are directly related to practical utility. Readers are presumably aware of the proliferation of equations and models in the quantitative literature, mentioned above. If we were making systematic progress, and the models genuinely have practical utility, why have so many new terms or parameters been added so frequently? Typically, each equation or model comes about when some new data set is published that doesn't agree with an existing equation or model, and a new term is added to account for discrepancies. The new data set is often the result of the examination of a hitherto neglected variable. It may be something to do with the changeover delay or with the type of schedule that is employed. That the variable was previously neglected is precisely the point. Researchers and theorists typically don't search for other relevant variables if they are already professionally committed to a given equation or model. As a result, many resources are expended in the defense of an existing approach, when more progress would likely be made if other variables were more realistically explored. Regrettably, an approach that emphasizes model testing without a sufficient foundation for the elements of the model has significant potential to lack practical utility. This observation does not argue against the progressive, cumulative character of science. Rather, it emphasizes that science needs to be included in the sometimes untidy domain of human conduct.

A final point concerns the function of scientific language. To contend that some usage of a theoretical term such as "value" in a quantitatively oriented theory is permissible because it is merely a theoretical construct, or that it is just an intervening variable, with no surplus meaning, rather than hypothetical construct, with surplus meaning (MacCorquodale & Meehl, 1948), is mischievous and deceptive. Neither a mentalist's nor a radical behaviorist's verbal behavior is ever of either sort, because the traditional distinctions between theoretical and observational terms, between the intervening variable and hypothetical construct interpretations of theoretical terms, and between instrumentalist and realist construal of theoretical terms, are all based on a logical, symbolic, referential view of verbal behavior. Radical behaviorism rejects this traditional, referential view in favor of a nonreferential, behavioral view that involves verbal contingencies. Figure 1 is an attempt to portray this point visually. In Figure 1, discussions about the various interpretations and construal of terms follow from the initial assumption that language is inherently a referential process. As is well known, Skinner (1957) rejected this referential approach.

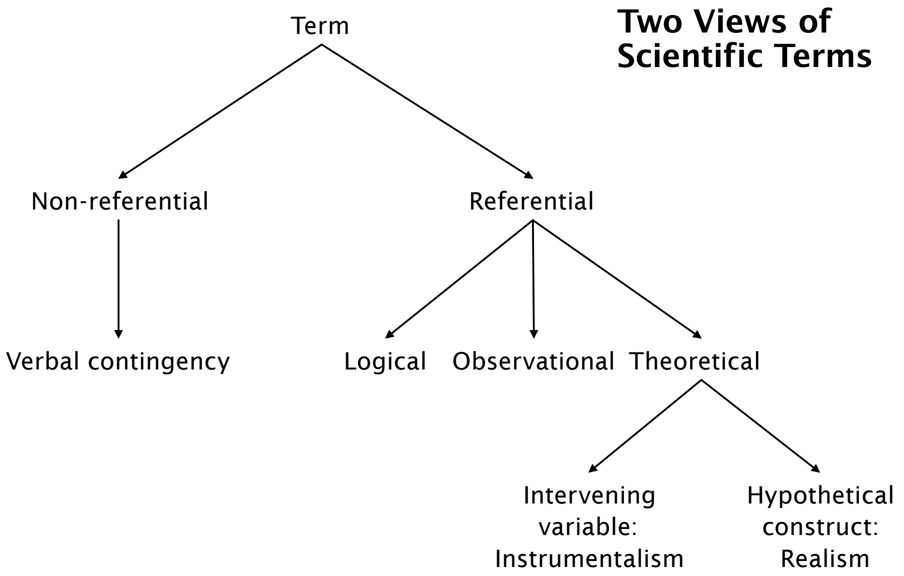

Figure 1. A schematic view contrasting the mentalistic, referential view of scientific terms with a behavioral, nonreferential view based on verbal contingencies. The referential view may be seen to lead to distinctions involving observational versus theoretical terms, instrumentalism versus realism, and intervening variables versus hypothetical constructs. Such distinctions are absent in the behavioral view based on verbal contingencies.

To be sure, a verbal product does have a discriminative function, and it is this function that is often of interest in assessments of scientific language. Skinner (1969, pp. 136–142) pointed out the effectiveness of constructed discriminative stimuli in problem solving, particularly when those constructed stimuli are verbal. As an example, one might consider lines of longitude and latitude. No line marking a plane called the equator literally exists across the ground and water at the midpoint of the Earth's surface, with parallel circles of latitude extending to the North and South Poles. Similarly, no meridians of longitude literally exist across the ground and water between North and South Poles, transecting the equator, with a conventionally important one running through Greenwich, England. These lines are constructed verbal products that have a practical value, for example, in timekeeping, navigation, and commerce. Particular coordinates of latitude and longitude are derived from the number of degrees those locations are north or south of the equator or east or west of the prime meridian.

In the same vein, we can point out that any importance of the concept of reinforcer "value" derives from its discriminative function as an abstract, constructed tact. As a verbal abstraction, it is an economical, summary term that is itself under the discriminative control of various properties of a reinforcer, such as rate, amount, delay, quality, and so on. Viewed as a verbal abstraction rather than a real entity, it does not necessarily carry any explanatory burden and is not necessarily of the sort identified as mentalistic earlier in the present analysis.

Nevertheless, we need not view any appeal to "value" in quantitative theories as an intervening variable and therefore safe. Again, to view any appeal to value as a safe intervening variable is to neglect verbal processes and hold that referential processes are involved. To so view the situation constitutes a position called epistemological dualism (Moore, 2008). Epistemological dualism holds that two dimensions are involved in accounting for knowledgeable behavior on the part of humans. The knowledgeable behavior is held to come about from manipulations involving various mental or cognitive acts, states, mechanisms, processes, or entities. In the case of scientific behavior, the knowledgeable behavior is regarded as uniquely a function of manipulations involving "theoretical" constructs in another dimension, referential theories of verbal behavior, hypothesis or model testing as a transcendental nonbehavioral activity, and so on. This entire orientation misrepresents the processes that are involved when people think. Readers will recall that misrepresenting how people think was one of the liabilities of mentalism mentioned earlier in the present analysis. Given an appropriate foundation for a theory, testing models with various terms and concepts may well be one way of doing science, but the foundation of the terms and concepts is critical. If the uses of such terms as *value* in the literature were only as an intervening variable, why are there such frequent appeals to mental processes, representations, and subjective scales in the quantitative literature? The problem is longstanding. Moore (2005) pointed out that E. G. Boring held a symposium on operationism in 1945, to clear the air about operationism. Many had thought that the restriction of a theoretical term to only one case limited more general statements and their degrees of freedom in theory construction. In partial recognition of the problem, MacCorquodale and Meehl (1948) then formalized a distinction between intervening variables and hypothetical constructs. Tolman (1949) was one of the first researchers and theorists to introduce theoretical terms to psychology, but he was quite explicit as he capitalized on the distinction, abandoning his original intervening variable interpretation and migrating to the hypothetical construct interpretation:

> I am now convinced that "intervening variables" to which we attempt to give merely operational meaning by tying them through empirically grounded functions either to stimulus variables, on the one hand, or to response variables, on the other, really can give us no help unless we can also embed them in a model from whose attributed properties we can deduce new relationships to look for. That is, to use Meehl and MacCorquodale's distinction, I would abandon what they call pure "intervening variables" for what they call "hypothetical constructs," and insist that hypothetical constructs be parts of a more general hypothesized model or substrate. (p. 49)

Thus, models that test what are presumed to be theoretical terms such as intervening variables or even hypothetical constructs are not the only way and are not always the most effective way of doing science (Moore, 2008, chapter 11). To ignore the source of a researcher or theorist's verbal behavior and to judge that verbal behavior solely by some statistical measure, say percentage of variance accounted for by the model, is to be led down the garden path of instrumentalism as opposed to pragmatism. A reasonable conclusion is that on average, we are going to advance faster and more economically if we pay more attention at the outset to sources that control verbal behavior. Entertaining explanatory fictions held to be in a subjective dimension exacts a price.

Skinner (1950) summarized his thoughts about an appropriate quantitative theoretical stance in psychology as follows:

> At the moment we make little effective use of empirical, let alone rational, equations. A few of the present curves could have been fairly closely fitted. But the most elementary preliminary research shows that there are many relevant variables, and until their importance has been experimentally determined, an equation which allows for them will have so many arbitrary constants that a good fit will be a matter of course and cause for very little satisfaction. (pp. 215–216)

Surely an appeal to the mental, subjective dimension in any research is testimony to the unfortunate attention that has been turned to fictitious and irrelevant events. The point is not to reject mathematics or models but rather to understand mathematics and models as forms of scientific verbal behavior. Once the source of control over that verbal behavior is better understood, the function of verbal behavior in science and the place of models, as well as other forms of verbally constructed discriminative stimuli, in science can be better understood.

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