

EFFECTIVENESS AS TRUTH CRITERION IN BEHAVIOR ANALYSIS

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ABSTRACT: In this article we examine some of the relations between behavior analysis and the pragmatic philosophy of William James. We point out that the adoption of effectiveness as a truth criterion is common to both systems, which warrants a closer examination of its implications. We present the Skinnerian concepts of knowledge, science, and truth as compatible with aspects of pragmatic philosophy; at the same time they emphasize the role of social contingencies posed by the scientific verbal community in the control of scientific verbal behavior. We argue here that effectiveness, both in Skinner's radical behaviorism and in James' pragmatic philosophy, does not have the status of either independent or sufficient criterion to validate knowledge claims. We call attention to some generic and imprecise uses of that criterion in behavior-analytic literature and propose that a more consistent usage would involve subordinating effectiveness to a coherence criterion. *Key words:* radical behaviorism, pragmatism, effectiveness, prediction and control

Every theoretical system adopts or sets up criteria by which it judges different knowledge claims about its subject matter. *Effectiveness* has been elaborated in philosophy and science as one of these criteria, and we will examine it here with reference to B.F. Skinner's radical behaviorism and William James' pragmatism. Our goals are a) To characterize Skinner's use of effectiveness as truth criterion, relating it to James' pragmatism; b) To indicate that, in James' pragmatism, effectiveness is subordinated to prior assumptions or beliefs; c) To illustrate uses of the effectiveness criterion in behavior-analytic literature, which are inconsistent with some prior assumptions of behavior analysis; d) To argue that to be consistent with both Skinner's radical behaviorism and James' pragmatism, prior assumptions should be considered when applying effectiveness as truth criterion.

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Skinner and the Use of a Pragmatic Truth Criterion

Prediction and control are cited by Skinner (e.g., Skinner, 1953/1965), and before him by Watson (1913), as the objectives of science, hence as the purpose of investigation of behavioral regularity. They are a strict specification of what Skinner means by the effectiveness criterion, through which behavior analysts differentiate between acceptable and unacceptable explanations for behavioral phenomena. Such a criterion means, in general, that explanations that assist the scientist in dealing with behavior in a productive way, or that support the expert in solving the same kind of problems efficiently, will be considered valid. Adoption of that criterion brings behavioral science closer to a pragmatic approach to the scope, logic, and status of scientific assertions (cf. Baum, 1994; Lamal, 1983; Leigland, 1999; Skinner, 1974/1993; Tourinho, 1996; Zuriff, 1980), even though it may be discussed critically (cf. Staddon, 1993, 2001).

Skinner proposes that behavioral science adopt the effectiveness criterion in *The Operational Analysis of Psychological Terms* (Skinner, 1945). Rejecting the empiricist criterion of agreement based on public observation adopted by “methodological behaviorists,” Skinner states that:

The ultimate criterion for the goodness of a concept is not whether two people are brought into agreement but whether the scientist who uses the concept can operate successfully upon his material—all by himself if need be. What matters to Robinson Crusoe is not whether he is agreeing with himself but whether he is getting anywhere with his control over nature. (Skinner, 1945, p. 293)

Rejection of empiricism dates from the initial period of Skinner’s theoretical system. References, in 1931, to Ernst Mach as the original author of the notions of “explanation and causation” (Skinner, 1931/1961a, p. 337) and of “a method of criticism. . .with respect to scientific concepts” (Skinner, 1931/1961a, p. 321), used in proposing the concept of reflex as descriptive of behavioral phenomena, suggest that Skinner was already working with functional criteria in defining his scientific project. According to Leigland (1999), “it is clear that Skinner’s views on the goals of science, as well as his general view of truth, could be described as strongly pragmatic in character” (p. 483). Zuriff (1980) finds in Skinner’s work instances of both a correspondence and a pragmatic theory of truth. The latter, however, is the one that could be taken as “consistent with and characteristic of Skinner’s system” (p. 343). Zuriff adds:

Although a version of the correspondence theory of truth appears in Skinner’s works, it is neither the dominant theory nor consistent with the basic tenets of his radical behaviorism. A theory more prominent in his work and one more congruent with his philosophy of science is, in essence, a behaviorist version of the pragmatic theory of truth promoted by James. (p. 344)

What should be observed here, and will be addressed further on, is that Skinner derives a particular notion of “explanation” from Mach: the description of functional relationships, applied in his scientific project to the study of the

organism's interaction with the world around it. As Smith (1989) observed, "the Machian views of explanation and causality were directly adopted by Skinner early in his career and have continued to figure prominently in his remarks on science" (p. 271).

The argument for the instrumental criterion introduced in 1945 is supported in later texts, when Skinner covers issues such as knowledge, truth, and objectivity. Many of these passages are documented in studies that examine Skinner's pragmatism (e.g., Leigland, 1999; Zuriff, 1980). They illustrate a notion of knowledge as behavior (contingency shaped or rule governed), from which the supposition results that "to impart knowledge' is to bring behavior of a given topography under the control of given variables" (Skinner, 1968, p. 203). They also present an instrumental conception of truth, according to which the truth is "whatever yields the most effective action possible" (Skinner, 1974/1993, p. 259). Finally Skinner rejects the notion of objectivity as correspondence in favor of an interpretation consistent with the instrumental conception of truth; for him, "responses to some forms of stimulation are more likely to be 'right' than responses to others, in the sense that they are more likely to lead to effective behavior" (Skinner, 1953/1965, p. 139).

If the Skinnerian truth criterion is clear enough to promote its identification with pragmatic principles, its appropriation in the behavior-analytic literature in order to validate different propositions suggests that a more careful examination is needed. However, before we look into these possibilities, let's examine some aspects of William James' pragmatism.

William James' Pragmatism and the Truth-Processes

Radical behaviorists who discuss philosophical aspects of Skinnerian thought generally refer to James' pragmatism and, as they draw similarities between the two scholars, highlight the instrumental concepts of knowledge and truth as formulated by James and adopted by Skinner. Some aspects of the pragmatist perspective on "knowledge" were originally formulated by C. S. Peirce, the founder of pragmatism. To Peirce, the function of knowledge is to direct individuals' actions; its construction is motivated by doubt, which is appeased by new beliefs. According to Peirce, definitive beliefs, corresponding to reality and unassailable by doubt, would be possible through the application of methods used in experimental sciences, the same resource used to measure the truth or falsity of a belief. Therefore, it is in terms of doubt and belief that the notion of truth shall be approached, but "truth is the end of inquiry, that opinion on which *those who use the scientific method* will, or perhaps would if they persisted long enough, agree" (Haack, 1978, p. 97, italics added). Here, one finds a realistic component of Peirce's pragmatism, which will not be embraced by James:

the scientific method, Peirce argues, alone among methods of inquiry, is constrained by a reality which is independent of what anyone believes, and this is why it can lead to consensus. So, since truth is the opinion on which the scientific method will eventually settle, and since the scientific method is

constrained by reality, truth is correspondence with reality. It also follows that the truth is satisfactory to believe, in the sense that it is stable, safe from the disturbance of doubt. (Haack, 1978, p. 97)

Because of such realism, Rorty (1982) goes so far as to consider that Peirce's contribution to pragmatism "was merely to have given it a name, and to have stimulated James" (p. 161).¹ It is with James that pragmatism presents itself indeed as a truth theory, a functional truth theory, which emphasizes the practical consequences of the different sets of beliefs held as valid. It is not surprising, therefore, that a reference to the proximity between Skinner's radical behaviorism and James' pragmatism prevails in the behavior-analytic literature.

James' interpretation of knowledge, particularly scientific knowledge, its production and validation, will be covered only briefly below, since it is not our objective to provide a comprehensive account of pragmatic philosophy. We discuss here an aspect that has not been highlighted in the behavior-analytic literature: James' interpretation of the truth validation *process*. For James, even though effectiveness is a pragmatic truth criterion, it is not a loose one; it is, however, a criterion employed within a context of preserving prior beliefs that effectively guide practical and intellectual interaction with reality.

In the compendium of conferences on pragmatism presented by James in 1906, and published in 1907 (James, 1907/1996a), pragmatism is described as a "method of settling metaphysical disputes that otherwise might be interminable" (p. 28), by questioning the practical consequences that would result from assuming as true a certain assertion about reality. That method leads James to an interpretation of the circumstances under which a proposition is said to be true:

Any idea upon which we can ride, so to speak, any idea that will carry us prosperously from any one part of our experience to any other part, linking things satisfactorily, working securely, simplifying, saving labor; is true for just so much, true in so far forth, true *instrumentally*. (James, 1907/1996a, p. 34)

Truth, accordingly, is not an attribute of beliefs that *represent* reality in its formal or essential aspects, but a way to refer to whichever beliefs *function* productively to organize human experience. This applies to scientific knowledge: "*Theories thus become instruments, not answers to enigmas, in which we can rest*" (James, 1907/1996a, p. 32). The formulation of new theories does not mean a closer relationship between science and an essence of nature: "investigators have become accustomed to the notion that no theory is absolutely a transcript of reality, but that any one of them may from some point of view be useful" (James, 1907/1996a, p. 33). In spite of that, the notion of correspondence between a true proposition and reality does not need to be discarded, considering that

¹ According to Rorty (1982), "Peirce himself remained the most Kantian of the thinkers—the most convinced that philosophy gave us an all-embracing ahistorical context in which every other species of discourse could be assigned its proper place and rank. It was just this Kantian assumption that there was such a context, and that epistemology or semantics could discover it, against which James and Dewey reacted" (p. 161).

EFFECTIVENESS AS TRUTH CRITERION

correspondence concerns the effect that the true belief has over the effective action. According to James (1907/1996a):

To “agree” in the widest sense with reality, *can only mean to be guided either straight up to it or into its surroundings, or to be put into such working touch with it as to handle either it or something connected with it better than if we disagreed*. Better either intellectually or practically! . . . Any idea that helps us to *deal*, whether practically or intellectually, with either the reality or its belongings, that doesn’t entangle our progress in frustrations, that *fits*, in fact, and adapts our life to the reality’s whole setting, will agree sufficiently to meet the requirement. It will hold true of that reality. (p. 102)

As one considers the role of true beliefs in guiding men in the world, it is essential to remark, from now on, that James emphasized both *practical and intellectual* effects of beliefs. A belief may be true even if it does not *guide* a particular *action* upon the physical world but if it promotes a conceptual organization of the world. “True ideas lead us into useful verbal and conceptual quarters as well as directly up to useful sensible termini. They lead to consistency, stability and flowing human intercourse” (James, 1907/1996a, p. 103).

Moreover, James’ analysis of the *process* through which a belief is considered true suggests that it is necessary to go beyond an examination of the relation belief-action. James argues that a belief *is* not true; it *becomes* true; that is to say, it is made true as it is confronted with the demands following the interaction of men with reality: “Truth *happens* to an idea. It *becomes* true, is *made* true by events. Its verity *is* in fact an event, a process: the process namely of its verifying itself, its *verification*. Its validity is the process of its *valid-ation*” (James, 1907/1996a, p. 97). Truth-processes, according to James, in his 1907 work, are not limited to a confrontation of particular propositions with particular instances of actions and their effects; they involve relations to a set of beliefs that, in a given moment, organize human experience productively. This means that there are, in James’ pragmatism, elements of a *coherence* theory of truth; as pointed out by Haack (1978), since “*coherence* theories take truth to consist in relations of coherence among a set of beliefs” (p. 86), it is justifiable to confirm that “James’ account on the way one adjusts one’s beliefs as new experience comes in, maximizing the conservation of the old belief set while restoring consistency. . . introduces a coherence element” (p. 98).

In his analysis (more psychological than philosophical) of truth-processes, James (1907/1996a) stresses that “what is better for us to believe is true *unless the belief incidentally clashes with some other vital benefit*. . . . In other words, the greatest enemy of any of our truths may be the rest of our truths” (p. 43). To put it another way, “in this matter of belief, we are all extreme conservatives” (p. 35); in the truth-processes dynamics, when a new belief acquires the status of truth, it “mediates between the stock [of old ideas] and the new experience” (p. 35); besides, “an *outrée* explanation, violating all our preconceptions, would never pass for a true account of a novelty” (p. 35). In his 1907 book, James states that this dimension of the pragmatic discussion tends to be ignored by his critics:

The point I urge you to observe particularly is the part played by the *older truths*. Failure to take account of it is the source of much of the unjust criticism leveled against pragmatism. Their influence is absolutely controlling. *Loyalty to them is the first principle—in most cases, it is the only principle.* (James, 1907/1996a, p. 35, italics added)

In 1909, James published what was to be a continuation of his 1907 work, intending to respond to some of the criticism leveled against his original publication. In Chapter Eight, James deals with eight issues that he considered to be misunderstandings of the pragmatic philosophy and its concept of truth. Two deserve attention here. The first states that “pragmatism ignores the theoretic interest” (James, 1909/1996b, p. 277). As cited above, in 1907, James had already clarified that reference to practical consequences resulting from true beliefs did not mean to ignore their role in organizing a conceptual understanding of the world. In 1909, this problem is once again addressed, as James acknowledges that his initial approach to the problem, made two years before, had not been sufficiently intelligible: “Our language evidently was too careless, for by ‘practical’ we were almost unanimously held to mean *opposed* to theoretical or genuinely cognitive” (James, 1909/1996b, p. 277).

James maintains that the opposition practice-theory is not valid: “particular consequences can perfectly well be of a theoretical nature” (James, 1909/1996b, p. 279). It will be, though, in his rebuttal to another misunderstanding that James makes himself even more explicit concerning the belief-action relationship. He states that, according to his critics, “pragmatism is primarily an appeal to action” (p. 267). He begins his argument in this case acknowledging that “the name ‘pragmatism,’ with its suggestions of action, has been an unfortunate choice” (p. 267). More significantly, he explains that a set of older truths not only functions as a reference point against which new ideas will be confronted but also the agreement with it precedes the demand for agreement with another reality:

Investigations show that, in the function called truth, previous realities are not the only independent variables. To a certain extent our ideas, being realities, are also independent variables, and, just as they follow other reality and fit it, so, in a measure, does other reality follow and fit them. . . . [Our critics] ignore our primary step and its motive [relation to previous ideas], and make the relation to action, which is our secondary achievement, primary. (James, 1909/1996b, pp. 267-268)

The discussion above implies that, at least according to James’ philosophy, the adoption of a pragmatic truth criterion means more than to establish relations between particular assertions and specific individual actions when measuring its validity. The appeal to effectiveness as a property that can be tested within the limits of those particular relations does not take into account other broader processes that take place when individuals validate their beliefs, as they assume them to be true; effectiveness, from a pragmatic point of view, is not set apart from a belief system, it is not a sufficient and independent measure of the validity of knowledge claims. For James, in validating a new idea, the very characterization of

what counts as effective is largely established by older beliefs, previously determined as true. It is essential to understand that James' account of truth-processes is obviously much more refined than what we described above and involves assumptions to which behavioral psychology might well object. Apart from that, to derive from (James') pragmatic philosophy a truth criterion defined as a restrictive appeal to actions and their consequent effects is to provide a partial or incomplete reading of the processes described or hypothesized by James.

Use of the Pragmatic Truth Criterion in Behavior-Analytic Literature

In Skinner's radical behaviorism, as in James' philosophy, the appeal to effectiveness should be subordinated to an assessment of the relationship between new propositions and the ones previously assumed to be valid; on the contrary, one may be faced with inconsistencies. This subordination thus means that new explanations for behavior should be consistent with basic tenets of the behavior-analytic explanatory system (or, when these principles are contradicted, one should provide an adequate treatment of the problems which result therein). The definition of what counts as these basic principles, which function as a reference point in evaluating explanations of behavior, can be controversial; we believe, however, that two principles established with the construction of Skinner's research program reckon with reasonable agreement among behavior analysts and may be useful in the argument we intend to develop: the definition of the subject matter of a behavioral science in terms of organism-environment *relationships* and the recognition of the variability or the idiosyncratic character of behavioral relationships.

When Skinner (1931/1961a) initially proposes to define psychology as the science of behavior and reflexes as its subject matter, he differentiates the very field of his discipline in relation to physiological studies, considering the response of the organism *as a whole*, intact, not invaded by instruments. While physiology "seeks a description of the reflex in terms of physico-chemical events, [a science of behavior seeks] a description of behavior in terms of the reflex" (Skinner, 1931/1961a, p. 336). Therefore, "the term *behavior* must include the total activity of the organism—the functioning of all its parts" (Skinner, 1935/1961b), even though only some aspects are crucial to the definition of a behavioral relationship (Skinner, 1935/1961b). In addition, Skinner emphasizes the notion of reflex as an organism-environment *relationship*, a correlation between a stimulus and a response (Skinner, 1931/1961a), or, more precisely, a correlation between classes of stimuli and classes of responses (Skinner, 1935/1961b). The notion of classes of stimuli and classes of responses will be essential to accommodating behavior variability in the Skinnerian explanatory system. One response is not strictly identical to another, in the same way that different presentations of a stimulus are not identical in all aspects; even so, regularities can be identified in behavioral relationships, once the relevant properties (to the relationship) of stimuli and responses are observed. The advent of the operant concept expands this perspective of addressing the behavioral phenomenon in the sense that it leads to the

identification of other functions of stimuli in three-term behavioral relationships, with another order of complexity. In any circumstance “behavior is that part of the functioning of an organism which is engaged in acting upon or having commerce with the *outside* world” (Skinner, 1938, p. 6, italics added). In a commentary on *The Behavior of Organisms* (Skinner, 1938), made fifty years later, Skinner (1988/1989) gives a definition of radical behaviorist philosophy that again points out the relational character of its subject matter: “The philosophy of a science of behavior treated as a subject matter in its own right apart from internal explanations, mental *or physiological*” (p. 122). In the same article, he refers to a chapter on the nervous system in his 1938 book as “a declaration of independence from physiology” (Skinner, 1988/1989, p. 129).

With the systematization of the causal model of selection by consequences (Skinner, 1981/1984), behavioral variability takes on new contours, wherein it is recognized that “the behavior of the organism as a whole is the product of three types of variation and selection: . . . natural selection, . . . operant conditioning, and . . . processes through which individuals take advantage of behavior already acquired by others” (Skinner, 1990, p. 1206). Given the idiosyncratic character of behavioral relationships, the behavior analyst prefers investigative methodologies that allow him to measure the change in individual behavior. Statistical analyses will be omitted, since “a prediction of what the average individual will do is of little or no value in dealing with a particular individual” (Skinner, 1953/1965, p. 19). In Chiesa’s (1994) words, “like other psychologists, behavior analysts recognize variation in human behavior, they recognize the individuality of people and other organisms. But they approach variation from the biological perspective, not from statistical, Quetelian perspective” (p. 82).

The following paragraphs illustrate circumstances in which the pragmatic truth criterion (or, better yet, versions of such a criterion) is mentioned in the context of propositions which do not take into account or conflict with the principles mentioned above. The texts do not deal specifically with pragmatism; in some cases, the reference to pragmatism is even peripheral to the authors’ argument. In all cases, however, one observes that, when speaking of a pragmatic criterion, the authors’ only reference is the effectiveness principle. We do not intend to discuss exhaustively the positions conveyed in the examples but to point out that they work with the notion of effectiveness as an independent and sufficient validation measure. We understand that, in thus using effectiveness, the authors are led to positions inconsistent with behavior analysis or, at least, they fail to satisfactorily work out the problems that result from those positions. To a certain extent, the examples merely illustrate a usual manner of referring to pragmatic truth criterion in behavior-analytic literature and suggest the need to arrive at more consistent criteria for validating our claims to knowledge.

Restriction to “Inner Causes” Theories in Explaining Behavior

Zuriff (1979) points out that despite his criticism of internalist psychology “Skinner has. . . developed many ingenious behavioral interpretations of complex

human psychological processes, frequently reformulating them in terms of covert stimuli and responses” (p. 1). Although approached with behavioral concepts, Zuriff lists ten types of “inner” events (interoceptive stimuli in first person reports, covert verbal behavior in problem solving etc.) that would be recognized by Skinner as behavioral causes, in the sense that they may take part in processes that determine subsequent public behaviors. In conclusion, “radical behaviorism cannot be said to deny inner causes of behavior” (p. 7); nevertheless, Skinner maintains “long-standing and consistent attacks on ‘inner causes’” (p. 7), and one should ask: why?

Undoubtedly the answer is that Skinner objects to certain kinds of theories postulating inner causes. These theories are objectionable on a number of grounds. For one thing, the causal sequence is incomplete, for it is not traced beyond the inner cause back out to the external environment. This implies a second flaw in such theories. Because the causal sequence stops with the inner nonmanipulable causes, it is not possible to modify the behavior they control. (Zuriff, 1979, p. 7)

According to Zuriff (1979), the reference to inner events as explanation for behavior is unacceptable since it does not promote behavior production, that is, it does not make it possible for the professional or researcher to act upon behavioral phenomena efficiently. This standpoint is similar to Stemmer’s (1995): “inner events are irrelevant to a functional analysis because they do not contribute to the pragmatic goals of prediction and control” (p. 353).

In Zuriff’s (1979) and Stemmer’s (1995) approach, the (in)consistency of the thesis of internal causation of behavior with behavior-analytic principles is not exactly presented as central to the discussion of its validity; manipulation, or prediction and control, occupy this position. Consequently, the reader may conclude that, once an instrumental interest is accomplished, internal events may be recognized as causes of behavior, a conclusion that comes directly into conflict with the Skinnerian definition of behavior. (That this is a possible conclusion can be seen in the following topic). Zuriff (1979) and Stemmer (1995) did not quite express their conclusions in these terms, but they also did not add to their instrumental concern a requirement of consistency or coherence with previously assumed principles and do not place both of them as elements of a pragmatist attitude.

The Acceptance of Inner Causes in Explaining Behavior

Overskeid (1994) claims there is some inconsistency in Skinnerian thought concerning the non-acceptance of inner events as behavioral causes. Considering that inner events may be interpreted as stimuli and responses that interact with public responses, there should be no reason to deny them such causal status. Zuriff’s (1979) pragmatic argument should be interpreted as defining the conditions to accept inner events as causes:

According to Skinner's own reasoning, the problem of Zuriff's "second flaw" is central, then, to the question of whether private events are admissible as causes—not only intermediate ones, but *initiating* causes. . . .In other words, it is the issue of whether we can change behavior by manipulating another variable that will decide whether that variable can ultimately be said to cause behavior. (Overskeid, 1994, p. 37)

The same (pragmatic) criterion that Zuriff (1979) appeals to, as a way to resolve that Skinnerian inconsistency and theorize on the restriction of inner explanations for behavior, will be used by Overskeid (1994) to emphasize the need to accept inner causes as the only solution to the inner-world problem. As Overskeid rationalizes, the appeal to "intermediate" and "initiating" causes makes little sense since all causal sequences are infinite and all approaches arbitrary. The solution must be "pragmatic": "the causal chain behind any event in nature is endless, and identifying an event as *the* cause of another is a matter of punctuation and pragmatic considerations" (Overskeid, p. 37).

Overskeid (1994) finds a statement in Catania (1988) and Skinner (1988) that inner events cannot be *initiating* causes and argues that this is not in accordance with the adoption of instrumentality as a criterion and, subsequently, it is in conflict with radical behaviorist philosophy:

Not accepting private events as initiating causes of behavior constitutes an obvious inconsistency in radical behaviorist theory, for theoretical and empirical reasons. (Overskeid, 1994, p. 38)

Fully accepting inner causes should be a consequence of accepting the fundamentals of radical behaviorism. And what is more, thus radicalizing radical behaviorism might close some of the gap between behaviorist and cognitivist researcher—to render us with a more comprehensive psychology of both private and public behavior. (Overskeid, 1994, p. 41)

The problem with Overskeid's (1994) analysis resides in the fact that, similar to Zuriff's (1979) and Stemmer's (1995) considerations, we find emphasis being put on the criterion of effectiveness not preceded by rigorous considerations of preliminary requirements, which might lead to a critical discussion of Skinner's references to inner causes. Overskeid does mention Skinnerian concepts (private stimulus and covert responses) pertinent to his discussion, but he does not examine how they interact with the most basic principles of the Skinnerian explanatory system (for example, the definition of behavior as a relationship of the organism as a whole with the external world) and supports his views on the principle of effectiveness to take the next step and defend the validity of internal causes. Overskeid is correct in demanding a more consistent discussion on the status and functions of private events in behavioral processes; however, strictly speaking, to simply accept internal causes for behavior will not be effective or ineffective within the scope of Skinnerian science; it is more a matter of working with a different notion of behavior (other preliminary beliefs), another subject matter, compatible with another set of postulates.

The Acceptance of Methodological Alternatives Based on Group Designs in Behavior Therapy Research

Forsyth, Kollins, Palav, Duff, and Maher (1999) discuss the tendency, observed in publications by behavior therapists, towards using single subject/single case methods in contrast to group designs. The work displays the link between early behavior therapy and behavior analysis methods and principles, especially the use of single-case design as a form of control:

One hallmark of early behavior therapy was the link between basic experimental research and its application to an idiographic functional approach to addressing practical problems. . . .The single case design, a hallmark of behaviorism, illustrates an emphasis on the individual and a commitment to demonstrating both prediction and influence over behavior. (Forsyth et al., 1999, p. 206)

The results obtained from a survey of the articles published in the journal *Behavior Therapy* (study 1) demonstrate a decrease in the percentage of articles that report single subject investigations (from approximately 44% in volume 4 to around 20% across volumes 12-27), which provides “objective evidence that we are ‘drifting’ somewhat from our basic science foundations” (Forsyth et al., 1999, p. 212). The researchers add that, “similarly, the numbers of citations to basic experimental journals, though never extremely high, has decreased in the last 20 years” (p. 212). A second study was developed aiming to broaden the initial findings; four journals, namely *Behavior Therapy*, *Behavior Research and Therapy*, *Journal of Behavior Therapy and Experimental Psychiatry*, and *Behavior Modification*, were analyzed for the period covering 1974-1997. The results from that second study were consistent with the results found on the first survey: “the overall trends across all four journals sampled consistently showed declining trends in the publication of single-case designs studies from 1974 to 1997” (Forsyth et al., 1999, p. 215). Forsyth et al. provide an extensive discussion of the limits of their research and especially of social and institutional constraints that may account for the changes detected. Nevertheless, for the purposes of the present study, it is relevant to highlight one specific comment that Forsyth et al. make concerning the possible consequences of the changes observed. Considering the expanded scope of behavior therapy as a possible reason for the decline of single-subject studies, Forsyth et al. argue that:

As behavior therapy has grown, so too has its range of acceptable methods. Behavior therapy can no longer be identified by a particular use of methods, treatment techniques, theoretical frameworks, concepts, or principles. . . .This loss of identity is welcome to some, and quite alarming to others. . . .Perhaps a more pressing question is whether we are still achieving the goal of advancing behavioral science as a means to alleviate a wider range of human suffering. *As long as we are achieving this pragmatic goal, then perhaps it matters little what we call ourselves or what we ultimately do.* (1999, p. 218, italics added)

Forsyth et al. (1999) recognize that methodological, theoretical and conceptual tendencies in contemporary behavior therapy stray from the principles and methods of behavior analysis (for example, in abandoning single-case designs in favor of group designs). Even so, they mention the possibility of a science of behavior advancing towards the solution of human problems; they define this advance as its own “pragmatic objective.” In this case, the consistency of what is done with a theoretical reference can be interpreted as secondary to defining the scope of the instrumental objective. According to this commentary (but, perhaps not to the whole analysis of Forsyth et al.), even though we may be “drifting from our basic foundations,” it would be possible to achieve the objectives of predicting and influencing behavior. Putting it this way, one is supposing that the instrumental criterion is achieved even when it clearly conflicts with principles previously accepted as valid.

The Acceptance of Nomothetic Systems for Classification and Diagnosis

The development and use of nomothetic systems for classification and diagnosis in clinical psychology have drawn attention in behavior-analytic literature (cf. Cone, 1997; Follette & Houts, 1996; Hayes & Follette, 1992; Hayes, Wilson, Gifford, Follette & Strosahl, 1996; Kazdin, 1983; Sturmey, 1996) within the context of a debate over the use and scope of the Diagnostic and Statistical Manual of Mental Disorders, DSM-IV (American Psychiatric Association, 1994). It has been quite a controversial discussion, in the course of which it has been alleged that the use of the Manual conflicts with the idiographic character of functional analysis developed in clinical assessment and intervention based on behavior analysis (cf. Cavalcante & Tourinho, 1998; Cone, 1997; Hawkins, 1986; Haynes & O’Brien, 1990; Owens & Ashcroft, 1982; Samson & McDonnell, 1990; Sturmey, 1996). Hayes and Follette (1992) illustrate how, in this other reflexive context, the instrumental criterion intervenes in favor of the nomothetic systems, relegating to a secondary status the complexity, the variability, and the idiosyncratic character of behavioral relations that result from phylogenetic, ontogenetic, and cultural processes of behavior production. Hayes and Follette’s concluding statement on the use of the DSM and alternative systems is difficult to assess, since they raise quite sophisticated and diverse arguments. Only their use of the instrumental truth criterion will be mentioned below.

Hayes and Follette (1992) discuss the use of classification and diagnosis systems based on syndromes (sets of signs and symptoms), particularly DSM-IV. They acknowledge that the DSM-IV is topographically oriented, not revealing any concern for identifying behavioral *relationships*. The use of such a system is perceived as disputable within its own theoretical context:

Syndromal classification can be viewed as a topographically oriented strategy for the identification of abnormal functional units. The problem is that this strategy. . . has not been useful when the same topographical outcome can be established by diverse processes, or when very different topographical outcomes can come from the same process. (Hayes & Follette, 1992, p. 348)

EFFECTIVENESS AS TRUTH CRITERION

The current (in)utility of the DSM system, nevertheless, is treated as circumstantial, as well as the restrictions that could be raised against the Manual from a behavior-analytic viewpoint. Hayes and Follette (1992) recognize that “allowing the DSM system to organize functional analysis potentially has the effect of channeling behavioral thinking into a non-behavioral mold” (p. 353); at the same time, they propose that “syndromal classification is not necessarily incompatible with a behavioral position” (p. 349). As a result, they present a quite generic assessment of DSM-IV, based on an instrumental criterion:

An empirical failing does not translate into a failing in principle, and for that reason the utility objection to syndromal classification turns out not to be fundamental. . . . If the treatment utility of syndromal classification could be shown, the contextualistic behavioral objection to syndromal classification would largely disappear. The fact that there has been minimal progress toward this goal, however, is the major viable criticism of the DSM system from a behavioral point of view. (Hayes & Follette, 1992, p. 349)

In a more explicit statement on the acceptance of the syndromal classification, having an effectiveness criterion as its basis, Hayes and Follette (1992) assert that “syndromal classification can make sense when viewed from both major philosophical perspectives in behavior theory [relatively contextualistic and relatively mechanistic behavioral approaches]. Its main problem is not theoretical; it is pragmatic” (p. 346).

Hayes and Follette (1992) recognize that there is a conflict between the topographical, nomothetic approach to behavior that underlies DSM-IV, and the behavior-analytic approach, which uses a relational definition for behavior and allows for individual idiosyncrasies. However, for them, the objections that can be raised are those not of a “theoretical” but of a “pragmatic” nature. By “pragmatic,” Hayes and Follette mean “whatever has treatment utility,” something not necessarily achievable using the syndromal classifications of DSM-IV. It is difficult to see how syndromal classifications (that “can be viewed as a topographically oriented strategy for the identification of abnormal functional units” [Hayes & Follette, 1992, p. 348]) can be made compatible with a relational and idiographic approach to behavioral problems. In Hayes and Follette’s analysis, however, the utility of syndromal classification to the behavior analyst could be established before and regardless of that compatibility.

Limits of the Pragmatic Criterion in Behavior Analysis

The generic definition Skinner provides for instrumental criterion in 1945—“What matters to Robinson Crusoe is not whether he is agreeing with himself but whether he is getting anywhere with his control over nature” (Skinner, 1945, p. 293)—may be the origin of the diverse, and sometimes conflicting, views on the features and scope of behavior analysis. In this passage, we find a reference to two aspects of James’ pragmatism, coherence (agreement) and effectiveness (successful working). As Skinner states that what is important to Robinson Crusoe

is not agreement with himself, it becomes possible to understand that effectiveness is the only requirement postulated. However, it should be considered that, in the context of that statement, what was being rejected was a criterion (adopted by methodological behaviorism) of intersubjective agreement *based on public observation*. With the reference to Robinson Crusoe, Skinner suggested that, even in the absence of verbal interaction with others (and, therefore, in the absence of public agreement), the validity of an explanation could be checked. Whether or not it is possible to achieve agreement in the absence of verbal interaction is a question that merits discussion, but to reject the requirement of public observation should not mean that the coherence criterion could be neglected.

The insufficiency of effectiveness as a truth criterion in behavior analysis was previously pointed out by Zuriff (1980), Hayes (1993), and Hayes and Hayes (1992). Zuriff (1980) treats it as a need to relate the effectiveness criterion to the value of cultural survival endorsed by an empirical science of behavior; Hayes (1993) and Hayes and Hayes (1992) view it as a need to consider the effectiveness criterion from within a contextualist philosophic definition of behavior analysis.

According to Zuriff (1980), to simply refer to effectiveness does not define validating conditions for explaining behavior, considering that “no metric has, as yet, been provided to measure effectiveness, specially the effectiveness of an entire scientific theory” (p. 345).

Zuriff (1980) solves the problem by indicating that, in Skinner’s radical behaviorism, the use of effectiveness as the basis for assessing the veracity of an assertion about behavior does not assume a prescriptive form to be followed when developing explanations; it is, indeed, an *a posteriori* conclusion generated by theories selected according to their effect upon the scientist or professional who is conducting the behavioral study that will promote cultural survival. On the other hand, it is a criterion with which only those who share the values of a behavior-analytic culture will agree. With this interpretation, Zuriff suggests that an effectiveness criterion would be operating in an independent manner (and, further, *a posteriori*) and does not take into consideration the preliminary role that basic beliefs or assumptions play in controlling the use of effectiveness as means of assessing the validity of an explanation. This problem also appears in the analyses made by Hayes (1993) and Hayes and Hayes (1992).

Hayes (1993) and Hayes and Hayes (1992) approach the limits of a vague reference to effectiveness when they discuss the adoption of contextualism as the philosophical basis for behavior analysis. According to Hayes (1993), from a contextualist point of view, beliefs (whether scientific or not) can only have their effectiveness assessed when confronted with analytic objectives, recognized as arbitrary, clearly defined, and previously adopted. When such objectives are not explicitly assumed, or are adopted but are not conceived as arbitrary, dogmatism emerges. For Hayes (1993), Skinner would be dogmatic in the sense that his objectives (of prediction and control) are not recognized as being arbitrary. Nevertheless, Hayes (1993) is not here taking into account that, for Skinnerian behaviorism, analytic objectives or prior goals are not prediction and control as such, but a set of specific beliefs about its subject matter: behavior. Such

objectives, throughout Skinner's work, are clearly defined, explicitly addressed, and frequently recognized as arbitrary (perhaps the only exception to this is the reference to the "natural lines of fracture along which behavior and environment actually break" [Skinner, 1935/1961b, p. 347]).

Hayes and Hayes (1992) argue that the objectives of behavior analysis based on contextualist philosophy are "the description, prediction, control, and interpretation of organismic interactions in and with a context" (p. 233). In this interpretation, prediction and control are not postulated as sufficient criteria to validate a belief, but once again, there is no clear recognition of the priority of previous theoretical objectives (in the authors' version, "interpretation of organismic interactions") that define the behavior-analytic field and in the light of which the scope of other objectives can be adequately judged.

By rejecting effectiveness as a criterion that can be autonomously applied to assess knowledge claims in the field of behavior-analytic science, the analysis developed here agrees with some statements by Hayes (1993), Hayes and Hayes (1992), and Zuriff (1980), basically those that deal with the insufficiency of an appeal to "successful working." In addition to what those approaches show, the analysis we offer emphasizes the usual occurrence of a generic and imprecise characterization of the pragmatic truth criterion in behavior-analytic literature (not only in Skinnerian texts). Finally, the current analysis differs from the ones mentioned above when it reasons that imprecise uses of effectiveness as a criterion are inconsistent with aspects of James' pragmatic philosophy and Skinner's radical behaviorism. Let us add to the discussion of Skinner's position.

One may inquire what exactly would correspond, in behavior analysis, to basic tenets or to the assumptions with which behavior is treated, recognizing that a more thorough examination of Skinnerian texts may, indeed, reveal inconsistencies, and also, as Zuriff (1980) and Leigland (1999) have already observed, that it is the pragmatic concept of knowledge and truth that prevails in Skinner's metascience. A way to answer that inquiry is to take into consideration exactly what Skinner viewed as a description and explanation of behavior. As already mentioned here, for Skinner, the goal of behavioral science is to establish functional relations between the organism as a whole and events in the world around it (that can be considered a dimension of behavior-analytic "metrics"). Approaches that focus on formal properties of behavior, or that search for a cause of behavior within the organism, are not simply more or less efficacious than behavior analysis; they are alternative and competing theories, since they are originally based on a concept of the behavioral phenomenon apart from that of behavior analysis; one cannot simply confront them in order to assess which one is more effective (they require different metrics). In the best case, physiological accounts can be considered *complementary* to behavioral interpretations (although *different*, as they are used to explain other types of phenomena), as they fill in a temporal gap between the terms of the behavioral explanation.

Scientific verbal behavior is a function of the contact of the scientist with his subject matter but also of contingencies provided by a scientific verbal community. Considering that and assuming that the behavior-analytic approach to behavioral

research and intervention leads the scientist to study the relationship of the organism as a whole with its surroundings, it would not be completely accurate to state that analytic goals (Hayes, 1993) are missing when behavior analysts decide what counts as an explanation of behavior (even though, one should equally acknowledge that behavior analysts, Skinner included, sometimes make imprecise references to effectiveness as a truth criterion). Also, the use of effectiveness as a truth criterion would not be simply a “loose prediction,” or a “recommendation” (Zuriff, 1980). It would not be a loose prediction because investigation and intervention are regulated by theoretical assumptions about what is pertinent to the scientific study of behavior. It is not simply a recommendation, either, because the capacity to corroborate or expand the explanatory scope of the theoretical system will play a role in the validation process of the interpretations.

Conclusion

Pragmatism is an attempt to approach the problem of knowledge without invoking representational and foundational claims. It emphasizes functional aspects of the processes of constructing and validating our beliefs about reality. In James’ version, it means looking at the truth as a way of qualifying propositions that conform to our *intellectual* and *practical* needs. Such requirements are dealt with when we refer to the relationship of a belief to our “prior assumptions or beliefs”—*coherence*—and to their “successful working”—*effectiveness*. James recognized that his writings sometimes overvalued effectiveness as the basis for veracity; he also tries to show us that the effectiveness criterion only takes effect in the light of previous observation of the coherence criterion.

In Skinner’s radical behaviorism, the pragmatic truth criterion requires, preliminarily, agreement with its basic beliefs concerning behavior. To appeal to the successful working of a belief, not taking into consideration that coherence requirement, either leads to inconsistencies or makes it impossible to check the validity of a presumed explanation of behavior.

The antimentalist and antirepresentationalist views of scientific knowledge place behavior analysis close to pragmatism while not denying its uniqueness as a philosophy. The assumption of effectiveness as a truth criterion is a manifestation of that proximity, but not its complete attainment. When employing effectiveness as a criterion, more consistent results may be achieved if the requirements of coherence are observed and if one takes into account (whether by agreeing or by explicitly disagreeing and working out the consequences of the disagreement) the beliefs about behavior within the context of which effectiveness is being evaluated. This is in accordance with the explanatory model built by Skinner for a psychological science, and with aspects not always highlighted in James’ pragmatic philosophy.

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EFFECTIVENESS AS TRUTH CRITERION

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