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# The Gifts of Culture and of Eloquence: An Open Letter to Michael J. Mahoney in Reply to His Article, "Scientific Psychology and Radical Behaviorism"

A. Charles Catania
University of Maryland Baltimore County

In what seems to be a response to a paper by Skinner (1987), Mahoney (1989) provides evidence of unfamiliarity with and intellectual intolerance toward radical behaviorism by presenting a critique of it that includes a variety of improper and counterfactual attributions. For example, he argues that radical behaviorism is Cartesian rather than Baconian when the historical record shows the opposite, that it is fundamentally associationist when in fact it is selectionist, and that its philosophy of science is essentially that of operationalism and logical positivism when instead it moved on to other criteria decades ago. The details of Mahoney's history are sometimes flawed and sometimes unsubstantiated, as when he provides a distorted account of the origins of the Association for Behavior Analysis or when he makes undocumented claims about the banning of books. On examination, many of his arguments are couched in stylistic terms that share their rhetorical features with racial, ethnic, and religious stereotyping.

*Key words:* Bacon, Chomsky, Darwin, Descartes, Skinner

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Dear Michael:

I have read your paper in *American Psychologist* (Mahoney, 1989), and the extent to which it is laden with errors of fact and interpretation compels me to respond. You may wonder about my title. It comes from W. H. Fremantle's account of the 1860 Oxford debate over Darwin's *On the Origin of Species* (Darwin, 1892/1958, pp. 251–252). According to that version of the exchange between Wilberforce and Huxley, Wilberforce said, "I should like to ask Professor Huxley . . . as to his belief in being descended from an ape. Is it on his grandfather's or his

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The following have asked to be cosigners of this open letter: Mary H. Aangenburg, Galen Alessi, Larry A. Alferink, Jack Alvord, Donald M. Baer, Jon Bailey, Beatrice H. Barrett, Anthony Biglan, Sidney W. Bijou, Joseph V. Brady, Marc N. Branch, T. A. Brigham, Ann K. Brown, Bruce L. Brown, John L. Brown, Brenna H. Bry, Jose E. Burgos, M. Michele Burnette, Don Bushell, Steven D. Bynum, Eric L. Carlson, Aleeza Cerf-Beare, Daniel D. Cerutti, Samuel M. Deitz, Alyce Dickinson, John W. Donahoe, Mark S. Drusdow, David A. Eckerman, Janet Ellis, Timothy F. Elsmore, George T. Endo, John Eshleman, Barbara Etzel, Edmund Fantino, Howard E. Farris, Steven Fawcett, William C. Follette, Gregory Galbicka, Mark Galizio, Patrick M. Ghezzi, Sigrid S. Glenn, Israel Goldiamond, Bram Goldwater, Lewis R. Gollub, Gina Green, Joel Greenspoon, Peter Harzem, Steven C. Hayes, Nancy S. Hemmes, Derek P. Hendry, Bruce E. Hesse, Philip N. Hineline, James G. Holland, Bill L. Hopkins, Jane Howard, Tor Jenssen, J. M. Johnston, Peter R. Killeen, Norman A. Krasnegor, Gerald D. Lachter, Victor G. Laties, Kennon A. Lattal, P. Scott Lawrence, Judith LeBlanc, Kenneth Lloyd, Ivar Lovaas, David Lubinski, Charles A. Lyons, John C. Malone, Nora McGonigle, Mary Ann Metzger, Jack Michael, L. Keith Miller, J. Moore, Edward K. Morris, Bobby Newman, David C. Palmer, Joseph A. Parsons, Slobodan Petrovich, David Polson, William K. Redmon, Ellen Reese, Jon Ringen, Elias Robles, Ted Schoneberger, Laura Schreibman, Evalyn F. Segal, James A. Sherman, Eliot Shimoff, Charles P. Shimp, Richard L. Shull, Murray Sidman, Howard N. Sloane, Norman E. Spear, William C. Stebbins, Beth Sulzer-Azaroff, Douglas C. Taylor, James T. Todd, Richard D. Torquato, Rocio Vegas, William S. Verplanck, Robert G. Vreeland, Barbara A. Wanchisen, Edelgard Wulfert, W. Joseph Wyatt, Louis Wynne, G. E. Zuriff.

Many have contributed useful comments about the manuscript. It would be impractical to list them all, but I especially appreciated or made particular use of those from Joseph V. Brady, John L. Brown, Paul Chance, Jeanine Czubaroff, James A. Dinsmoor, Lewis R. Gollub, Robert P. Hawkins, William J. McGill, Allen Neuringer, Slobodan Petrovich, Howard Rachlin, Eliot Shimoff, B. F. Skinner, James T. Todd, and W. Joseph Wyatt (though they did not have an opportunity to review the outcome).

For reprints, write the author at the Department of Psychology, University of Maryland Baltimore County, 5401 Wilkens Avenue, Catonsville, MD 21228-5398 USA.

grandmother's side that the ape ancestry comes in?"; and Huxley replied, ". . . [as to the descent from a monkey,] I should feel it no shame to have risen from such an origin. But I should feel it a shame to have sprung from one who prostituted the gifts of culture and of eloquence to the service of prejudice and of falsehood." I think the quotation is relevant here because, as I will try to show, a strong case can be made for a very substantial measure of intellectual prejudice in your arguments.

You are not alone, and that is one reason why I regard it as so important to respond to you. Behaviorist bashing seems to have become increasingly popular in recent years. Its rhetoric is typically characterized more by style than by substance, and it is usually accompanied by both intentional and accidental misrepresentations (for example, I refer you to recurring announcements of the death of behaviorism, and to persistent errors of fact in introductory texts: for documentation of some examples, see Catania, 1982, 1987; Czubaroff, 1988; Sherrard, 1988; Shimp, 1989; Todd & Morris, 1983). Your paper makes it necessary once again to correct the record.

You've provided references to bolster some of your arguments and you've offered interpretations of the histories of science and of psychology, but your references do not support your claims for them and much of your interpretation of history is inaccurate. I will document these conclusions simply by describing some of the errors of fact and interpretation that became apparent to me in my reading of your paper.

*Radical Behaviorism Is Baconian, not Cartesian*

You praise Bacon's role in the history of science, and then you argue that Descartes rather than Bacon "holds a revered place in the history of behaviorism" (Mahoney, 1989, p. 1373). You continue by elaborating on Descartes' mind–body dualism. Your justification is a treatment of Descartes at the beginning of a brief introduction to modern behaviorism (Rachlin, 1970).

The reader who reads past Rachlin's introductory material will discover that his primary concern was to provide some historical background. Historical references do not constitute endorsements. Rachlin did not advocate Descartes' dualism, and though he discussed Descartes' concept of the reflex, rather than making it the foundation of his subsequent treatment Rachlin argued that the concept is not adequate in helping us to understand the properties of operant behavior. (You know, of course, that operant behavior is not elicited as in the reflex relation, and that the elaboration of the distinction between operant and respondent behavior was one of Skinner's crucial early contributions to behavior analysis.)

Probably you will not be convinced by this argument alone, so let me offer some additional and even more persuasive evidence. Those who are familiar with Skinner's writing know that he credits Bacon with a formative influence on his thinking about the nature of science. Consider the following from Skinner's autobiography: "I read biographies of Bacon, summaries of his philosophical position, and a good deal of the *Advancement of Learning,* the *Essays,* and *Novum Organum.* This was stretching my abilities pretty far, and I doubt whether I got much out of it at the time, but Francis Bacon was to serve me in more serious pursuits later on. (Skinner, 1976, p. 129; see also pp. 294–295); "I took the history of science seriously . . . . I also planned to observe the history of science as it unfolded and, following Francis Bacon a little too closely, to take all knowledge to be my province" (Skinner, 1979, pp. 49–50); and, most important, "Three Baconian principles have characterized my professional life. I do not mean that they have governed it. The facts of my life have confirmed them, and my early acquaintance with Bacon may have improved the chances that they would do so" (Skinner, 1983, p. 406). Excerpts would not do justice to Skinner's subsequent discussion of those principles, which takes several pages.

As if that were not enough, consider the following: "The effect of an eliciting stimulus is relatively easy to see, and it is not surprising that Descartes' hypothesis held a dominant position in behavior theory for a long time, but it was a false scent from which a scientific analysis is only now recovering" (Skinner, 1971, p. 18). You can learn about Skinner's debt to Bacon in these and other writings of his, but nowhere in them will you find a comparable treatment of Descartes.

The more I contemplate your equation of Descartes with radical behaviorism, the more unbelievable I find it. It isn't just that radical behaviorism is thoroughly incompatible with Cartesian dualism. Don't you remember that one of the most outspoken contemporary Cartesians of them all is Noam Chomsky? A book of his (Chomsky, 1966) is probably the most vigorous defense of Cartesian thinking that you will find in recent literature. You must be familiar with Chomsky's position with regard to contemporary radical behaviorism. His widely cited book review (Chomsky, 1959) missed the point that *Verbal Behavior* (Skinner, 1957) is about the functions of verbal behavior and not its structure; a concern with the functions of verbal behavior is largely orthogonal to the structural matters with which Chomsky has dealt. It is curious that Chomsky shares with you many of the misconceptions about what is entailed by a behavior analytic approach and that, like you, he has helped to perpetuate misrepresentations of it (e.g., cf. MacCorquodale, 1970).

You devoted a substantial part of your text to building up to the contrast between Bacon and Descartes, presumably to provide relevant historical context, but the material just presented demonstrates that even the main historical foundations of your arguments are seriously flawed. I trust that this account will at least persuade you to reverse the historical roles you have assigned to Bacon and to Descartes.

### *Radical Behaviorism Is not Operationism*

Again and again you characterize radical behaviorism as operationist, but that too is wrong. Maybe you came to this conclusion because you are familiar with the title of one of Skinner's papers: "The operational analysis of psychological terms" (Skinner, 1945). But in reading past the title, one discovers that the paper was a renunciation of operationism. Some quotations may be helpful.

Skinner's first paragraph argues that the contributions of operationism have been negative: "No very important positive advances have been made . . . because operationism has no good definition of a definition, operational or otherwise." The second paragraph deals with some problems in the vocabulary of operationism: "a few roundabout expressions occur with rather tiresome regularity . . . . We may accept expressions of this sort as outlining a program, but they do not provide a general scheme of definition, much less an explicit statement of the relation between concept and operation."

Skinner's position was in transition at the time, and he occasionally used terminology that he would later find inappropriate. For example, here is how he describes the circumstances that led to the 1945 paper, which was his contribution to a symposium on operationism: "Although I had lost interest in the operationism of the thirties, I still called myself an operationist and thought that certain parts of the manuscript were suitable for the symposium" (Skinner, 1988a, p. 162). Even if you wanted to base an argument about Skinner and operationism on a quotation like this (and you shouldn't), you would have to note that the line refers only to the Skinner of 1945. You will find no support at all for your interpretation in later writings (e.g., cf. Skinner, 1953, pp. 281–282).

### *Some Other* -Isms *Also Don't Qualify*

You like to characterize positions in terms of *-isms*. That makes it difficult for me to react to some of your characterizations, because many *-isms* have definitions that vary drastically as a function of context. Objectivism is certainly one of those, but it hardly matters which version you are concerned with because objectivism as you described it is in no way compatible with a radical behaviorist position. To assert that "an objectively separate 'real world' lies behind the organism and exists independently of being perceived" (Mahoney, 1989, p. 1374) is to imply a distinction between the organism's subjective and objective worlds that is equivalent to a commitment to mind-body dualism.

Methodological behaviorism, the sort that grew out of the behaviorism of John B. Watson and the sort that you should have called orthodox, can be squeezed into this dualistic and objectivist mold. But, as I thought you knew, radical behaviorism quite explicitly eschews dualism. It does not deny that events take place inside the skin (you erred by identifying it with the psychology of the black box or the empty organism); instead it maintains that we should call those events private rather than mental, and that they are the same sorts of events as those outside the skin. It assumes that we have different sorts of access to the world inside our skin and the world outside but that they are both made of the same sort of stuff, and it attempts to work out the implications of that assumption.

Don't just take my word for it: "It is particularly important that a science of behavior face the problem of privacy. It may do so without abandoning the basic position of behaviorism. Science often talks about things it cannot see or measure .... An adequate science of behavior must consider events taking place within the skin of the organism, not as physiological mediators of behavior but as behavior itself. It can deal with these events without assuming that they have any special nature or must be known in any special way. The skin is not that important as a boundary. Private and public events have the same kinds of physical dimensions" (Skinner, 1963). The question is not whether important events occur in the brain, but whether those events are part of the same physical world as those outside it.

Associationism is one more *-ism* that you mistakenly identify with radical behaviorism. It is somewhat less ambiguous and therefore somewhat easier to deal with than those mentioned earlier. Historically, it was the basis for conditioning theories and for theories of stimulus–response connections. Unfortunately for your case, those theories are not among the primary concepts of contemporary behavior analysis. Skinner has been quite explicit about it. Consider the evidence of the following quotations: "[The] effort to associate my position with 'early association theory inherited from the epistemology of the British empiricists' resembles that of current theorists who try to explain operant conditioning in terms of Pavlovian conditioning, which is much closer to associationism . . . [and] seems to miss entirely the notion of selection by consequences and the parallel between operant conditioning and natural selection" (Skinner, 1988c, p. 140); "It is hard to reply to anyone who . . . regards me as a stimulus–response psychologist. I have not been one for more than 50 years. The essence of operant conditioning . . . is that behavior is not triggered by the environment but selected by it" (Skinner, 1988d, pp. 460–461).

With regard to logical positivism, you cite L. D. Smith (1986), but I don't see how you can read Smith's chapter on Skinner without acknowledging the conclusion that Skinner's early behaviorism showed the influence of logical positivism but later diverged from it in radical and significant ways. Smith argues that Skinner's position should not be identified with logical positivism.

I don't know what to make of your metaphysical behaviorism, and there still remain determinism and evolutionism and pragmatism, among others. How can I be sure of what you mean by each of these? If you think the door is closed on debates over the implications of quantum mechanics among physicists, you are mistaken (e.g., Mermin, 1989). As for chaos theory (Gleick, 1987), certainly it overturns cherished assumptions about deterministic systems, but I have not seen any resistance on the part of behavior analysts to its implications; in fact, they seem to be consistent with current research directions within behavior analysis (e.g., Neuringer, 1986; Page & Neuringer, 1985). Evolutionism comes in many guises, but all I can guess is that, whichever the kind you referred to, it is one that you regard as a bad kind. And pragmatism is a bit like eclecticism; it is often convenient and it may be congenial to American culture, but it does not provide the foundations for a systematic science. If these are your criteria for identifying radical behaviorists, it is no wonder that you have trouble finding them.

One more *-ism* must be considered. I assume that strict environmentalism corresponds to your "exclusive environmental determinism" (Mahoney, 1989, p. 1375). Your discussion suggests that you are unfamiliar with several accounts by Skinner of the role of evolution, such as his "Phylogeny and ontogeny of behavior" (Skinner, 1966). Skinner (1974, pp. 4–5) lists as "an extraordinary misunderstanding" of behaviorism the belief that it "neglects innate endowment and argues that all behavior is acquired during the lifetime of the individual."

The reference is to Skinner's *About Behaviorism,* and there are nineteen other misunderstandings on Skinner's list. By my reading almost all of them are implicit in your manuscript, and many are explicit. You present yourself as someone knowledgeable about radical behaviorism and therefore about Skinner's writings. Furthermore, you included *About Behaviorism* in your references. Given that Skinner's list was in the first chapter, are we not forced to conclude that you did not even get as far into this book as you did into Rachlin's?

What function then is served by all your labelling with *-isms*? Mainly rhetorical, because the labelling disposes the reader to work out the syllogism, for example as in the following: Objectivism is bad, and radical behaviorists are objectivists; ergo, radical behaviorists are bad. Well, such syllogisms work but their premises don't; ergo, the conclusions don't follow. I am sure that you would find such rhetorical devices objectionable if applied to racial or ethnic or religious affiliations; it is therefore ironic that you do not reject their application to intellectual ones.

As the etymology of the word shows, prejudice is prejudgment, and it implies an insensitivity to new evidence. Although you cited some relevant evidence, very little in your presentation suggests that you gave serious attention to that evidence. On those grounds, I am compelled to charge you with a level of intellectual prejudice that has led you to careless scholarship and to misrepresentation.

### *Historical Matters, Such As the Roots of ABA and Banned Books*

You place much emphasis on history, and that should imply that you have taken care to get your historical facts straight. Yet in discussing the origins of the Association for Behavior Analysis (ABA), you claim that it was a spinoff of the Association for the Advancement of Behavior Therapy (AABT), whose "most orthodox members have left and have expanded a regional group into the national (sic) Association for Behavior Analysis (ABA)" (Mahoney, 1989, p. 1375). Isn't it curious, then, that the published history of the ABA (Dinsmoor, 1979; Peterson, 1978) documents how its roots can be found within the Midwestern Psychological Association (MPA) as far back as 1969. It officially became the Midwestern Association for Behavior Analysis (MABA) in 1974, and later expanded from its region origins, not nationally but internationally, into the Association for Behavior Analysis. The account does mention some activities at the AABT meeting in 1974 (the circulation of an announcement of the 1975 MABA meeting, and some planning for that meeting), and some activity within Division 25 at the annual APA meeting. You may wish to claim that you mentioned a regional group in the above quotation even though you didn't identify it as MABA. Nevertheless, your attempt to inject your recollections of events within AABT into the history of ABA without acknowledging the discrepancies between those recollections and the documented history of MABA is hardly consistent with careful historical scholarship.

The ABA issue is important because the quality of the evidence bears on another of your historical points: you speak of "the development and circulation of a list of 'banned readings,' books that faculty and students in some radical behavioral departments were instructed not to read" (Mahoney, 1989, p. 1375). But is it appropriate to offer such a slanderous accusation without a shred of documentation? And you want us to believe that not just the students but also the faculty were so constrained? What group was responsible for imposing this on the faculty as well? Maybe the university administration?

I have been involved with behavior analysis for more than 30 years. As past-Editor of the *Journal of the Experimental Analysis of Behavior* and past-President of ABA and of Division 25 of the APA, I have participated in a wide variety of professional activities. I have had contact with many colleagues within my discipline and have visited many campuses. Yet in all of those interactions over all of that time, I have never heard of such a list (and certainly would have denounced it if I had).

Now maybe an extremist somewhere once did some such thing; or maybe a disgruntled behavior analyst, fed up with too many misrepresentations of the field by those who should have known better, had the bad judgment to make up such a list as a joke. But your wording, "development and circulation," implies something more systematic, probably involving several individuals. The charge is most serious, and it is therefore incumbent upon you to produce the list, to name its source, and to document that sanctions were indeed threatened for violating the ban (to brandish a list without revealing its contents smacks too much of McCarthyism). Given your treatment of other behavior analytic matters, I am not inclined in the absence of concrete evidence to believe that any such thing ever happened.

One last comment on this matter. To prove that such a list existed is to vindicate the accuracy of your historical claim. But if you generate the proof of such bizarre behavior, please do not assume that your conclusions about the status of contemporary radical behaviorism are justified by it. The burden remains yours of showing that such idiosyncratic and uncharacteristic sanctions have played a significant role in the history of behavior analytic practices.

*Style Versus Substance*

I've already mentioned your use of the stylistic device of labelling. Your equation of radical behaviorism with orthodox behaviorism is another example, as is your use of the term *scientism* (cf. Czubaroff, 1988). And when you say that it "is essential that a distinction be drawn between scientific psychology and radical behaviorism" (Mahoney, 1989, p. 1376), your provisos that follow do not cancel your implication that radical behaviorism is not scientific.

You speak of some scientists as respected on the grounds that they "challenged or revised radical behaviorist accounts of learning" (Mahoney, 1989, p. 1374) without seeming to entertain the possibility that some of these might also be respected by or even regarded as behavior analysts. And when you find a behavior analyst who has done something you approve of, that individual is suddenly no longer numbered among true behavior analysts in your eyes but has become "evangelical" (Mahoney, 1989, p. 1375). That is a powerful rhetorical device; according to it, no radical behaviorist, no mater how ingenious or imaginative or creative in research, can by your definition ever do anything right. The very act of doing so makes the individual ineligible for the radical behaviorist label.

Let me offer you a different alternative. Instead of relabelling them, why not ask Hayes or Killeen or Neuringer or Zuriff or others you mentioned whether or not they still wish to be regarded as behavior analysts or radical behaviorists? And why don't you also examine the experimental literature that is available in relevant behavior analytic journals, such as the *Journal of the Experimental Analysis of Behavior.* You will find papers there even on those research lines that you regard as forcing the revision of and challenging radical behaviorist accounts. For example, with regard to biological constraints, the first experiments on autoshaping appeared in that journal (e.g., Brown & Jenkins, 1968; Williams & Williams, 1969), and despite the claims by Garcia (1981) that his work on bait-shyness was suppressed by behaviorally disposed editors, a paper he submitted there had received enthusiastic reviews but Garcia withdrew it rather than providing an abstract and making other stylistic revisions.

I could try to draw up a list of citations that are inconsistent with your stereotypes (one of my favorites is the report of an ingenious procedure for teaching a pigeon to describe its feelings: Lubinski & Thompson, 1987), but it may be sufficient to point out to you that the November 1988 issue of that journal was devoted to biological factors in behavior, and the November 1989 issue to the experimental analysis of cognition.

Another rhetorical catch-22 involves consistency over time. An interesting example occurred in an exchange between Herrnstein (1977a, 1977b) and Skinner (1977): Skinner suggested an expansion and refinement of his taxonomy of behavior classes, but Herrnstein argued that it would be inappropriate for Skinner to diverge from his taxonomy of the 1940's and 1950's. If one adheres to something one enunciated decades ago, one may be described as inflexible and rigid; but if one's views have changed, one may be described as vacillating and inconsistent. Of course, it can as easily go the other way around: the former may be described as faithfulness to tradition and the latter as openness to change. The point is that such characterizations indicate the value judgments of those who invoke them, but they do not provide an adequate account of the history of a subject matter though they may sometimes pass as one.

I mentioned earlier the similarity of such discourse to instances of racial or ethnic or religious stereotyping. One property of such stereotyping is its insensitivity to contrary evidence, and that property is shared by many of the examples I cited above (e.g., the repeated inappropriate identification of radical behaviorism with associationist theory). The consequence of such stereotypes is intellectual discrimination (in the pejorative rather than the technical behavioral sense of that word). And, given that one can change one's intellectual identification more readily than one's racial or ethnic or religious ones, intellectual intolerance is at least as much to be feared as other varieties. If you doubt that, consider what adherence to creationism has done to the national tolerance for the teaching of evolution in this country over most of the past century.

### *B. F. Skinner's Uncompromising Stance*

You speak of "scientistic (sic) intolerance" (Mahoney, 1989, p. 1374) and you chastise Skinner for his assertions in "Whatever happened to psychology as a science of behavior" and other papers. But can you blame him? He has made many truly profound contributions to our understanding of behavior: the treatment of reinforcement as a behavioral phenomenon and not as a theory to be tested (its status is comparable to that of osmosis in biology; it isn't everywhere, but it is in many places and you should be able to spot it when it's there); the three-term contingency; the modification of behavior through shaping; the operant as a class of responses; schedules of reinforcement; the distinction between contingency-shaped and rule-governed behavior (to mention only a few). The applications of these fundamental concepts, in improving the quality of life, in teaching, and even in saving lives, are amply documented (if you doubt it, I refer you to more than 20 years of the *Journal of Applied Behavior Analysis*). But did your diatribe imply that you regard any part of that body of work as meriting a respected place in contemporary psychology? Hardly!

If your article really represented the position of contemporary psychology, it should come as no surprise that some radical behaviorists have moved to more congenial environments, but intellectual ghettoes are as objectionable as racial or ethnic ones. Such behavior might be called paranoid if the prejudice is imagined, but it is eminently sensible if the prejudice is real. You say that "radical behaviorism isolated itself from and came to lag behind changing perspectives on the nature and practice of optimal scientific inquiry" (Mahoney, 1989, p. 1373), but you have the directionality wrong. The isolation originated with the sort of intolerant and uninformed treatment that is illustrated by your article.

In a special issue of the journal *Behavioral and Brain Sciences* that has since appeared as a book (Catania & Harnad, 1988), Skinner replied to roughly 150 commentaries on some of his classic papers. In a summing up, he remarked upon his reactions to the project as a whole: "... it has been my experience that when I write something in one setting at one time and come back to it in a different setting at a different time I see other implications and relations. I had thought that something of the same sort would happen when other people read these papers.... Too often, this has not happened.... I have been unable to avoid spending time and space on the simple correction of misstatements of fact and of my position, where I would have welcomed the opportunity for a more productive exchange" (Skinner, 1988b, pp. 487–488). One doesn't have to agree with Skinner, but criticisms based on elementary misunderstandings of what he has said can carry little weight.

*Coda*

Oh yes, there could be more. Whether confirmatory bias is a good thing or a bad thing in science might be debated (e.g., is it wrong for a physicist to be more concerned about a reported demonstration of cold fusion when that demonstration seems inconsistent with other things the physicist knows, than about a reported failure to produce such a demonstration?). It might be useful to elaborate on the important differences that distinguish methodological behaviorism from radical behaviorism, or radical behaviorism as a philosophy of science for psychology from behavior analysis as a body of research methods and findings. The conclusions that you draw from your claim that "one does not find the assumptions or assertions of radical behaviorism ... in modern texts on scientific methodology within the social sciences" (Mahoney, 1989, p. 1373) could be challenged by showing how you can find them instead within the biological sciences (cf. Provine, 1988; T. L. Smith, 1986).

Darwin wrote: "Great is the power of steady misrepresentation; but the history of science shows that fortunately this power does not long endure" (Darwin, 1872/1962, p. 421). Surely you will help to confirm his statement by getting your characterizations of radical behaviorism and behavior analysis right in the future and by making every endeavor to avoid the misrepresentations that were evident in your paper.

You may argue in extenuation that the professional literature continues to mushroom and that the literature of behavior analysis and radical behaviorism has grown very large, but those facts do not free any of us from the obligation to be familiar with the citations in our own writings. I especially hope that you will make the necessary corrections in your forthcoming book. I can think of no more convincing way for you to demonstrate that you are indeed prepared "to transform ideological swords into conceptual plowshares and to risk trusting in the harvest of open dialectical exchange" (Mahoney, 1989, p. 1376).

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## POSTSCRIPT

The publication in *The Behavior Analyst* of my reply to Mahoney warrants an account of its history. It is perhaps best to begin with a paper by B. F. Skinner. In its August 1987 issue, the *American Psychologist* published his piece entitled "Whatever happened to psychology as the science of behavior?" In it Skinner speculated on the obstacles standing in the way of "efforts to explain behavior as a subject matter in its own right rather than as the effect of internal processes, mental or neural" (Skinner, 1987, p. 780). He identified features of humanistic psychology, the helping professions, and cognitive psychology as such obstacles.

With respect to humanistic psychology, he argued that many "find the implications of a behavioral analysis disturbing [because the] environment takes over the control formerly assigned to an internal, originating agent [and] some long-admired features of human behavior are then threatened" (Skinner, 1987, pp. 782–783). With respect to psychotherapy, he referred to "exigencies of the helping professions" that create circumstances under which practitioners "must ask people what has happened to them and how they feel . . . instead of investigating the early lives of their patients or watching them with their families, friends or business associates"; he concluded that "it is not surprising that they should then construct theories in terms of memories, feelings, and states of mind" (Skinner, 1987, p. 783). With respect to cognitive psychology, his case was that the search for rules had come to substitute for the observation of behavior: "Rather than observe what people actually did, one could simply ask them what they would probably do" (Skinner, 1987, p. 784); this tactic led to mathematical, machine or neural models that had the properties of rule-governed rather than contingency-shaped behavior.

In its November 1989 issue, the *American Psychologist* published a piece by Michael J. Mahoney that appeared to be a reply to Skinner's article, though it only indirectly addressed the three major points just outlined. It was entitled "Scientific psychology and radical behaviorism," with the subtitle "Important distinctions based in scientism and objectivism" (Mahoney, 1989). In it the behaviorism of behavior analysis was characterized as orthodox behaviorism, and radical behaviorism was hardly distinguishable from the behaviorism of John B. Watson. It suggested that behavior analysis was Cartesian rather than Baconian, it espoused many of the misunderstandings of behaviorism that Skinner had listed in *About behaviorism* (Skinner, 1974), and it even claimed that radical behaviorists had participated in the banning of books.

Mahoney's paper engendered perhaps a dozen replies that were submitted to the *American Psychologist*. Mine, an early draft of the piece now published here, was submitted in mid-December 1989; at about the same time I sent a copy to Mahoney and also circulated it to several colleagues for comment. Given that my reply was in the form of an open letter, I invited those to whom I had sent it to be co-signers and suggested that if they wished to co-sign they might write directly to the journal and ask to have their names added to my paper.

But the timing was not right. Late in January 1990 I was informed that my reply was not accepted, primarily on the grounds of length. I had intended my piece as a full manuscript (I had included an abstract), but it was treated editorially as a letter. Several factors contributed to this decision, including an editorial transition: Raymond J. Fowler, as the new Executive Director of the American Psychological Association, had only just assumed the *American Psychologist* editorship.

Meanwhile, co-signer letters began to arrive at the editorial office. Roughly thirty had been sent by the end of January and, as word spread about the editorial disposition of my reply, more followed in February and March. Some who had submitted their own replies (including Sigrid Glenn and Edward K. Morris) even withdrew or offered to withdraw their letters in favor of mine, and one colleague offered to spearhead a drive to raise money to disseminate my reply, either through the mails or as a paid advertisement (the idea was eventually dropped because so distributed the reply would have had no archival status).

But the significance of these events was overshadowed by another. I had prepared a revision based on the feedback I had received on the first draft I circulated. One copy went to Skinner, who soon after wrote me a letter, dated 15 January 1990. It began:

> Dear Charlie:
> The revised version is perfect. I hope they publish it . . . .

But the final paragraph was the following:

> The leukemia is something of a nuisance. I have to go over once a week or so for transfusions and whatnot and have recently had a vascular access catheter put in on my chest to avoid getting needles into large veins on each occasion. I'm feeling fine and enjoying life, though I'm not getting as much work done as I once did.

I had heard about the leukemia. This letter was the first firm information I had received, however, and the task of setting the record straight suddenly took on special urgency.

In the week or two after learning about the rejection of my reply to Mahoney, I decided to phone Ray Fowler and discovered that, on the basis of the co-signer letters, he had been about to send me a letter inviting me to re-submit my reply so that it could be reviewed as a manuscript submission rather than as a letter; if it were accepted, Mahoney would be given an opportunity to respond to it.

These events provided a welcome occasion for further revision of my reply, especially given that I had by then received very many helpful suggestions about both style and substance. To incorporate them all was impossible, however: some were incompatible (e.g., one suggestion for softening the tone was likely to be counterbalanced by another for making it firmer). Nevertheless, I was able to improve the paper substantially and in May was able to try out a little of it on a symposium audience at the annual meeting of the Association for Behavior Analysis (the other participants were James T. Todd, Edward K. Morris, William L. Heward, and John O. Cooper; Samuel M. Deitz and Jack Michael were discussants).

By 18 June I was able to include the following line in a letter to Skinner:

> As I think you know, Andy Lattal has been appointed by Ray Fowler as an *ad hoc* Editor for a special section in the *American Psychologist* that will address the Mahoney piece and other issues involving the relation between behavior analysis and psychology.

It had become clear that Ray Fowler and the *American Psychologist* were indeed responsive to the concerns of the behavior analytic community. (I had earlier resigned from the American Psychological Association, but I am pleased to report that on the basis of the events recounted here I have reinstated my membership.)

Time was also working other changes, however. In a letter of 12 July 1990 Skinner wrote "I am glad to know that Andy Lattal is organizing the replies to Mahoney. I wish I had my current paper ready to send to him with a suggestion that it might be part of a single presentation." But the letter had also mentioned that a "critical episode in the leukemia a couple of weeks ago has put me back."

On 10 August 1990, Skinner accepted an award from the American Psychological Association for a lifetime of achievement in psychology. It was his last public appearance. On 17 August 1990, he put the finishing touches on his last paper and sent it off to the *American Psychologist.* He died on 18 August 1990.

As of this writing, both Skinner's remarks at the APA meeting and his last paper are in press in the *American Psychologist,* and have been moved up in the publication schedule so that they will appear this year. And Andy Lattal's editorial charge has been revised: he is now working to organize a special issue of the journal devoted to B. F. Skinner and the field that has grown out of his work.

That journal and especially that special issue seem no longer to provide an appropriate forum for my reply to Mahoney. If I do contribute something to that enterprise, it will be about Skinner and his work, and not about Mahoney's opinions of them. Yet it may still be of value to have the reply archivally available, and the contributions of those who provided comments and/or co-signed it deserve acknowledgment. For that reason, I have accepted Sam Deitz' gracious invitation to submit my reply to Mahoney to *The Behavior Analyst*. Some might argue that here it will be available mainly to those who least need to read it, but if its contents are useful it at least can now be cited conveniently.

Had I revised the reply for the *American Psychologist,* I undoubtedly would have changed it in several ways. For example, probably I would not have retained the direct addressing of Mahoney as "you" and instead would have switched to third person. For the present purposes, however, I felt it would be more fitting to leave the manuscript in the form it had reached by the summer of 1990, which would have been the last version that Skinner could have seen. Except for the correction of a few minor errors and stylistic infelicities, that is the version published here.

It is also closer in this form than it otherwise would have been to the version or versions that Mahoney has seen. The replies to Mahoney's article that had been accepted by the *American Psychologist* have appeared (Lonigan, 1990; Morris, 1990; Proctor & Weeks, 1990; Wyatt, 1990), and Mahoney has written a response to them. In that response he refers to my open letter, and calls it "a direct attack on my integrity and intelligence" (Mahoney, 1990, p. 1183). Yet nowhere does he acknowledge even the most thoroughly documented of the misunderstandings that he has helped to perpetuate, such as his reversal of Bacon and Descartes. Despite all his rhetoric, he still seems not to have recognized that he has gotten some things wrong. Until he can do so, there seems little hope that he will be able to reconcile his views with those of contemporary radical behaviorism.

A. Charles Catania
University of Maryland
Baltimore County
28 October 1990

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