

Why Morris, Lazo, and Smith (2004) Was Published in *The Behavior Analyst*

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The Fall 2004 issue of *The Behavior Analyst* published an article, “Whether, When, and Why Skinner Published on Biological Participation in Behavior” (Morris, Lazo, & Smith, 2004). We are its authors. Before and after it was published, several colleagues asked why we submitted it to the journal and why it was published there. They noted that its readership was already aware of Skinner’s views and that the paper should have appeared in a journal whose readers had more to gain. We agreed (and agree). This is our response.

ORIGIN AND SUBMISSION

We submitted our manuscript to *The Behavior Analyst* because, to be candid, it had been rejected by five other journals—four published by the American Psychological Association (APA), two of them generalist journals and one a journal for reviews of empirical research. The fourth APA journal and the non-APA journal were nonhuman animal behavior journals that have an evolutionary and ethological focus. Not only was our manuscript rejected, the editors did not even send it out for review. Although disappointed, we are not complaining. Our manuscript was rejected for

straightforward reasons, and the editorial correspondence was instructive, although it did convey one unsettling implication. The reasons had enough nuance, though, that along with the instruction and implication, they might interest the journal’s readers.

The manuscript’s origins lay in Morris’s suggestion to Lazo that she write a paper on the behavior-analytic view of biological participation for his Fall 2001 history and systems of psychology course. On the basis of Smith’s then-developing bibliography of Skinner’s works (Morris & Smith, 2003), she decided to focus on Skinner. After her paper was written, we collected more data and revised the manuscript for behavior-analytic presentations (e.g., Lazo, Morris, & Smith, 2002). In due course, we also decided to submit a manuscript version to a U.S. journal outside the field of behavior analysis. We submitted it to the five journals in succession, revising it where we could for the next submission, but always refining and updating it.

In what follows, we present, describe, and reflect on the editorial correspondence. First, we present the editors’ reasons for rejecting the manuscript and suggestions for alternate journals. Second, we describe an unsettling implication of their reasoning and suggestions, and comment on how it might be corrected. Third, we relate briefly why the manuscript was accepted by *The Behavior Analyst*. And fourth, we pass along lessons we learned throughout the process and offer some accounts of the editors’ actions that speak to the place of behavior analysis in contemporary psychological science.

We thank Carol Pilgrim for permission to quote her correspondence with us by name, Richard Fyffe for his advice about confidentiality in scholarly correspondence, and Matt Normand for sending us Steven Pinker’s Web site address.

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REJECTIONS

Reasons

Although the manuscript was uniformly rejected, the editors always conveyed their decisions in a professional manner. Albeit sometimes pro forma, their correspondence was also sympathetic and helpful. As for their reasons for rejecting the manuscript, they cited problems in its breadth for their readers, issues concerning space limitations, and its appropriateness for their journal's categories of publications and audience.

Breadth. The editor of the first generalist APA journal to which we submitted the manuscript rejected it for not having enough appeal for the breadth of his journal's readership. On March 2, 2004, he wrote,¹

I am sorry to inform you that [your manuscript] will not be sent out for formal peer review. Since [the journal] functions somewhat differently from many in our field, I would like to explain some of the factors at work in the editorial process. . . . The limited space we have available constrains us, and we can accept only articles that are of interest to a broad range of psychologists and that have broad consequences for the science and practice of psychology. Also, because of the many purposes the journal must serve, the number of original articles we can publish is quite limited. The rejection rate, as a consequence, exceeds 85%. We often have to reject perfectly good manuscripts simply because the manuscript is too technical (or for too specialized an audience).

The paragraph was fairly pro forma, but understandably so. The editor probably has to state this reason many times over in managing the range of manuscripts his journal receives.

Breadth notwithstanding, Skinner was recently ranked the most eminent psychologist of the 20th century in a

study published in an APA journal (Haagbloom et al., 2002); thus, the association should want to have his views accurately portrayed. They reflect on psychology as a whole and its definition as a science, as well as its subject matter, purview, and interdisciplinary relations. Misunderstandings about Skinner hinder collaboration across research programs and are mischievous in their effects on relations among scientists. We perhaps erred by not making these points more strongly. They are pertinent to APA's broad readership and hence to the readership of a generalist APA journal.

In the next paragraph, the editor addressed our manuscript more directly, noting that it was "not right" for his journal:

The editorial committee that initially reviews all manuscripts submitted to [the journal] decided against sending your manuscript out to an associate editor for a full editorial review. This is no reflection on the quality of your paper—it just is not right for [the journal], and in our judgment, sending it out for review likely would have had the same results but taken much longer, thus delaying its publication elsewhere.

Of course, not all manuscripts are right. Perhaps we erred by failing to argue that a journal's readership should not be measured just by its breadth, but also by the depth of their knowledge, in this case, of the field's most eminent member. Surely, depth is also a mission of generalist journals.

Space. Not only are manuscripts rejected for reasons of breadth, but also for space, if the two can be disentangled. Lack of space was the basis for our manuscript's rejection from the other generalist APA journal. Its editor began his April 7, 2004, correspondence with us this way:

I have read your articulate paper in which data are presented regarding the criticism of Skinner that he ignored the biological foundations of behavior. Although I found you and your co-authors to have done perfect work on the issue (and although I personally believe that you are right in pointing out the frequency and incorrectness of these criticisms of Skinner), I am sorry to say that I don't think the work is appropriate for [the journal].

¹ Although we have not identified the journals from which our manuscript was rejected, we do refer to their "editors." We use this term, though, to refer to the source of the editorial correspondence, not necessarily to the journal's listed editor. The correspondence might have come from an associate editor, guest associate editor, or managing editor. Readers should not assume that the material we quoted is attributable to any particular person.

He then described why the work was not appropriate—lack of space:

Putting aside the fact that your paper goes by the heart of the matter that has long concerned me, given the recent page reductions made by APA, I must choose papers on the basis of their potential interest to the audience that reads [the journal]. However much I appreciate your thought and work (and, on a personal level, however much I approve of your findings), I can't see devoting the page space to an issue, no matter how well considered, that seems to me to be beyond the interests of the readership [see also the correspondence from the editor of the first generalist APA journal on "limited space"].

The editors of the two nonhuman animal behavior journals offered similar reasons. One wrote, "I receive many more papers than I can publish, and thus must turn away much good work." The other noted, "The manuscript is . . . considerably longer (6,785 words) than the maximum length we recommend (5,000)." Of course, no journal can publish all the good work it receives. Nonetheless, we were disappointed that the editors did not regard a pervasive misrepresentation of the 20th century's leading psychologist, especially the founder of a significant program of research on animal behavior, worth the space to correct it.

Appropriateness. Our manuscript was also rejected for being inappropriate to some journals' purposes and purviews and categories of publication. The editor of the APA literature review journal, for instance, noted that our manuscript failed as a "review." He was right; we had erred. The editor of the APA animal behavior journal made a similar point. Her letter of May 16, 2004, began this way:

While it is very interesting to read, I do not think it fits into any [journal] category. The most likely category would be a theoretical review, but there is little theory discussed, except in many bits and pieces of reviewing Skinner's words. . . . I regret not being able to accommodate your request but I think I am really only saving you time as I think [the journal's] reviewers would also question the appropriateness of the paper.

She then asked, rhetorically, "A likely question for a reviewer or reader would be, 'Now, that we know Skinner

thought more about biology, how does that change the nature of his theories and their ability to contribute to comparative psychology?" Knowing more about Skinner does not change his theory. What it changes is the accuracy with which the theory is portrayed. A correct portrayal allows his contributions to become integrated with comparative psychology and ethology, thus increasing the likelihood of collaboration. We should have elaborated further on this point. This editor also made an interesting observation about her journal's younger readers:

The paper also assumes that the reader is fully familiar with the nature of the charges that Skinner's work was un-biological. I fear that such familiarity is rare in younger scientists and thus I do not think many [of the journal's] readers would be able to put your efforts into perspective. Thus, the paper would have to be expanded to illuminate Skinner's history of thinking as a backdrop.

We are not so sure. As we noted in Morris et al. (2004), leading public intellectuals today charge that Skinner's work is unbiological, as do well-known primatologists, ethologists, and comparative psychologists. Steven Pinker's Web site, for instance, contains a section on psychological theories in which, under behaviorism, he lists this talking point: "*Equipotentiality* as a keystone of behaviorism" (<http://pinker.wjh.harvard.edu/>; see the "Teaching" link to his course on "The Human Mind," then to "The canonical version of your course Web site," then to the course handouts). Morris et al. specifically refuted this point by quoting directly from Skinner's published works (e.g., Skinner, 1977). Younger readers of the foregoing journal are presumably familiar with Pinker and de Waal, but perhaps not. Perhaps they read only empirical literatures in which these issues are moot, but we doubt it. Indeed, we hope not. This is a point we could have addressed, but did not.

The final journal to which we submitted our manuscript was the non-APA animal behavior journal, whose editor wrote to us on July 13, 2004:

I have now read the manuscript through carefully and feel that it is not appropriate for publication in [the journal]. . . . [It] does not fit within the stated "Aims and Scope" of [the journal] as posted at the Web site:

The journal publishes original contributions from all branches of behavioral research on all species of animals, both in the field and lab. It contains scientific articles of general interest in English language that are based on a theoretical framework. A section on "Current issues-perspective and perspectives and reviews" is included as well as theoretical investigations, essays on controversial topics and reviews of notable books.

On receiving this, our fifth rejection, Morris protested, replying via e-mail the same day,

I understand that my manuscript was not a good fit for [the journal] on purely structural grounds. It is also not good for the zeitgeist of "what everybody knows about Skinner" (and modern behaviorism), so you can imagine some of my consternation. . . . In any event, thank you for taking the time to read it over. If you like, feel free to pass it on to anyone who might be interested.

The editor replied, "Thank you very much for your note. It was very kind of you. And I did enjoy reading the paper. I will try to think of an alternative journal. Would you mind if I did pass it on to someone who might have a good suggestion?" Morris responded,

Thank you for your kind comments in return. Yes, I would welcome suggestions. I have been to the appropriate APA journals (e.g., [the APA animal behavior journal]), but structurally, it is also not a good fit for them either. As for the more general APA journals (e.g., [the first of the two generalist journals]), they claimed that the manuscript was not of broad enough interest to their readership to warrant publication. They have their priorities in this regard. I cannot fault them for that—where this is the reason. . . . I can . . . publish the present paper in a behavior analysis journal [he wrote, broaching on hubris], but that would be preaching to the choir. Behavior analysts have been insular enough this way. My skin is thick. I thought I would venture out.

The editor wrote back, "I need to get a grant proposal off (Monday) but then will see if I can get some suggestions. I hated saying no without a review, but I just thought it would not take us anywhere, with all of the concerns. I will try to find help though." Morris re-

plied, "I have been a journal editor and can much appreciate your not burdening [the reviewers] with an ms unlikely to be published for structural reasons. Good luck with the grant!" That was that—the end of their correspondence. Their respective duties and responsibilities took them in different directions.

Suggestions

Although all five editors rejected the manuscript, four of them recommended alternate journals. One wrote, "you might consider submitting your manuscript to a more specialized journal such as *History of Psychology*, where I believe you are more likely to find your audience." Another confided, "I suppose it is a little inappropriate to suggest other journals, but in the interest of seeing your work available to other scholars, I would suggest *History of Psychology* or *Philosophical Psychology*." A third editor commented similarly, "I have read papers that have analyzed important figures in science and found them useful and think this paper is useful as well. But I read the articles in journals on the history and philosophy of science, which seems the natural niche for your work." The fourth wrote, "I wish you luck in finding a journal that focuses more on the history of science, or on the history of behavioral study. I am not sure, if, for example, the journal *Behaviour* would be more appropriate. I believe at least, that they sometimes publish longer papers."

The editors' good intentions notwithstanding, their recommendations were overly optimistic about our likely success and were inconsistent with our intent. We doubted *History of Psychology*, *Philosophical Psychology*, or history and philosophy of science journals would have found our manuscript suitable to their purposes and purviews as it was written. *Behaviour* is not an American journal and thus not one to which we would have submitted our

manuscript, but the editors could not have known this.

AN UNSETTLING IMPLICATION AND A CORRECTIVE

Skinner Is History and Skinner in History

The editors' reasons for rejecting the manuscript, along with their observations and suggestions, implied an unsettling implication. One editor wrote that the manuscript was "essentially historical in nature." Another thought that her journal's younger readers would be unfamiliar with the charge that Skinner's work was unbiological. Two others recommended the *History of Psychology*. Consciously or not, they may have been saying that Skinner is "history."

If so, then to some extent, the editors are right. Skinner is a major figure in the history of psychology (see Leahey, 2004), the most eminent psychologist of the last century (Haagbloom et al., 2002), and the founder of a science of behavior and its philosophy (Skinner, 1938, 1945) who vied with other notable psychologists in an earlier era (e.g., Hull, 1943; Tolman, 1932). The implication that Skinner is no more than history, however, is troubling. His proposals for a natural science of behavior, a naturalized philosophy of science, and their applications for improving the human condition are as fresh and cutting-edge as ever. What Dews (1970) described of Skinner's contributions in the 20th century remains true today:

Massive advances in science can affect society either by changing man's views of himself or by leading to substantive changes in his environment. The contributions of Copernicus and Darwin profoundly affected society through their philosophical implications, though they have made little difference to the contents of one's house or how one does things. Dalton's Atomic Theory and Faraday's Electromagnetism had little influence on the nineteenth century Establishment, although they led, through chemistry and electricity, to profound changes in man's surroundings. The work of a few people has affected society both ways; Pasteur's germs affected both people's view of life and also their beer, wine, and medical treatment. Skinner's dis-

coveries in the field of the transaction of a higher organism with its environment will have a greater and more enduring effect on man's view of himself than the views of Freud. Meanwhile, slowly but increasingly, education is being influenced by Skinner's findings, and perhaps some day they may influence broadly how men dispense justice and punishment, raise children, handle neuroses, organize an economic system and conduct international relations. (pp. ix-x)

A Corrective

We have already noted several ways in which we could have made our manuscript a more compelling to the editors. These were that psychology should accurately portray the views of its most eminent member; that a broad readership should have some depth in their knowledge of their discipline; that misunderstanding is mischievous to relations among scientists; and that misrepresentations hinder collaboration and integration across research programs. However, Reviewer A of the present manuscript suggested an even more compelling and effective strategy for countering the implication that Skinner is history:

Despite how annoying it is that erroneous views of Skinner's writings persist, if the goal is to promote understanding of behaviorism, maybe what needs to be presented to folks in other areas of psychology are current empirical approaches that are consistent with radical behaviorism. Skinner's views would be presented in the context of their current relevance. The theme of such a paper would more explicitly address an understanding of behaviorism of the present. This might be a better hook. . . . Another way of looking at these ill effects [of misunderstanding Skinner] is that it is a misunderstanding of *current behaviorism* [italics added] that hinders collaboration.

This recommendation would have been difficult to incorporate into our manuscript as written, so it clearly calls for another paper. In any event, although Skinner's contributions may be eclipsed for the moment (see Catania, 1987; cf. Henriques, 2003), this does not make him history. The promise of those contributions still awaits. It awaits a synthesis of (a) his natural science of behavior and (b) a natural history of behavior, the latter of which in-

cludes ethology, comparative psychology, and psychology's content domains (e.g., cognition, development, emotion, motivation, perception, personality, social)

ACCEPTANCE

As for why our paper was accepted by and published in *The Behavior Analyst*, we defer to Carol Pilgrim, the journal's editor at the time, who wrote to us on August 30, 2004,

As you have pointed out [in the cover letter], many readers of this journal will be familiar with Skinner's ongoing discussions of biological influences, but your paper makes the tremendous contribution of addressing the issue with substantive data. I . . . believe that your novel treatment provides much that will be of interest to even seasoned members of our field. In addition, our readers will now have an important and convenient response to offer to naive . . . colleagues, and a delightful route to further educating our students.

If our manuscript serves these purposes, then we are pleased that it was published in this journal, yet we remain unhappy not to have reached a broader audience (cf. Catania, 1991).

LESSONS AND ACCOUNTINGS

When colleagues asked why we submitted Morris et al. (2004) to *The Behavior Analyst*, we replied that it had been rejected elsewhere. With this present manuscript, that reply is more complete. When asked why the article was published in *The Behavior Analyst*, we answered with Pilgrim's reasoning. That answer now has Pilgrim's concurrence. In addressing these questions in the foregoing material, we have described the manuscript's origins and submission and reasons for its rejection. Although its history is not unique among manuscripts, the process taught us some lessons and led us to consider a broader account of the editors' actions.

Some Lessons

What we learned was not rocket science. The lessons are fairly obvious.

Heeding them might increase any manuscript's probability of acceptance. First, manuscripts must fit a journal's mission and its categories of publications. We did not always attend sufficiently to this point. Second, manuscripts written for behavior-analytic audiences are not likely to conform to the expository and interpretative style of journals outside the discipline (see Himeline, 1980). They have to be thoughtfully crafted and carefully placed (see, e.g., Whitehurst & Valdez-Menchaca, 1988). Third, when breadth may be an issue, behavior analysts should clearly describe how their manuscripts relate to a journal's readership and contribute to the depth of its literacy. Fourth, manuscripts written for a behavior-analytic audience are less likely to be convincing to readers outside the discipline because of a lack of shared history and assumptions. Thus, authors should consciously keep the outside reader in mind as they prepare their manuscripts, perhaps even to the point of having a copy of the journal at their side as they write. As an added benefit, what makes a manuscript convincing to readers outside the discipline will likely make it even more effective for readers within the discipline.

Accounting for the Editors' Actions

Although the manuscript was rejected for straightforward reasons, some of the editors' observations and suggestions implied that Skinner was history. Granting that the subject is always right (Skinner, 1948, p. 274), we cannot blame editors who hold this view. They are right, too. We can, though, offer interpretations about the source of the view that suggest how to correct it.

Individual experience. Some of the sources may be idiosyncratic. For example, among his capacities at APA, the editor of the first generalist APA journal also oversaw the development of Division 25's *PsycSCAN Behavior Analysis and Therapy*, and then its

eroding subscription base as the division lost members. The latter does not speak well of the discipline's vitality. An APA division that is losing members is not obviously of interest to a broad APA readership. The eroding subscription base may also have been irksome because the *PsycSCAN* lost money for APA. (The *PsycSCAN*, by the way, is now available through *PsycINFO*; see <http://online.psychinfo.com/psycscans/>.)

The visibility of behavior analysis. A more common source of the view that Skinner is history may be the dearth of behavior-analytic submissions and publications in the journals the editors edit and read. This may easily lead them to think behavior analysis is of little interest to their readership. To be sure, behavior analysis has become isolated (Coleman & Mehlman, 1992; Krantz, 1971, 1972), even "ghettoized" (Leahey, 2000, pp. 528–531). This may be due to disregard—even disrespect—by other behavioral, social, and cognitive scientists because of the field's countercultural views (Hineline, 1980). It may be due to the natural process of specialization within the sciences, as subdisciplines build on subdisciplines (Lee, 1989) or, in the case of APA, as its minisciences proliferate into disunity (Sternberg, 2005). And, it may be due to the founding of behavior-analytic journals, leading to the field's reduced visibility in psychology as a whole.

This may be, in part, "what happened to behaviorism" (see Roediger, 2004). In addressing these issues, Hineline (1990a) has argued,

that well-chosen behavioral work be placed in non-behavioral journals, to enhance our visibility to the world at large. As our own journals have developed, most of us have ceased attempting to do this, which is understandable when such attempts risk punishment and a lowered probability of reinforcement. However, a result has been that respected mainstream scholars, including textbook authors and editors, seem honestly to believe that there has been no recent progress in behavior-analytic work, or even that such work has ceased to occur. An article placed in an outside forum may not be as warmly received as those placed in our own journals, but

it plays an important role in letting the world know that we are still here and making progress. (p. 7; see also Hineline, 1990b, on making presentations at conferences outside behavior analysis)

For almost a half-century, behavior analysts have struggled with the tension of how to obtain informed peer reviews of their manuscripts, yet at the same time have those manuscripts appear in mainstream journals. To accomplish this, behavior analysts have to publish in journals both inside and outside of their discipline—a bit of a response cost.

Mainstream commissions. A still more general source of the view that Skinner is history may lie in today's normative scientific and professional psychology. Among the accepted views is that behaviorism was overthrown by a cognitive revolution (see Baars, 1986; Gardner, 1985), in part because Skinner denied biology (see Garcia & Garcia y Robertson, 1985; Mahoney, 1989). This view appears in books, journals, and newsletters published by psychology's leading associations and societies, even in the face of constructive appeals on behaviorism's behalf (e.g., Roediger, 2004; see Baars, 2004; Colombo, 2004). If journal editors believe these views, then they are right to reject manuscripts such as ours. Even when manuscripts correct these views, they may simply be beside the point, except as historical exercises, because behaviorism is presumably dead.

Today, a literature that rebuts this view of the cognitive revolution is slowly growing. Scholarly research variously points out (a) that the revolution was more linguistic and sociological than paradigmatic (O'Donohue, Ferguson, & Naugle, 2003); (b) that there was no revolution—the behaviorism that cognitivism replaced was mediational, and thus cognitive to begin with (Greenwood, 1999; Leahey, 1992); and (c) that, in any event, not all of behaviorism died—radical behaviorism survived (Leahey, 2000, pp. 528–531; Moore, 1995). A longer lived literature also rebuts the misrepresentations

tations of Skinner's views and of behavior analysis (e.g., Catania, 1991; Skinner, 1973, 1977; Todd & Morris, 1983, 1992). Most of this literature, though, lies outside mainstream psychology, except perhaps in special issues of journals, but the latter are easily set aside when the material lies outside the interests of the general readership (e.g., Leahey, 1992; Todd & Morris, 1992). The misrepresentation of Skinner should not be a special interest, however, but an interest of any scholar in the behavioral, social, and cognitive sciences.

CONCLUSION

We agree with the colleagues who challenged us about publishing our manuscript in *The Behavior Analyst*. It should have appeared in a journal whose readers had more to gain. This was an opportunity lost. It was lost through the manuscript's crafting and placement, and when Skinner became, in part, history. The editors of five prestigious journals, however, are now more knowledgeable about Skinner's views. This does not mean that they have become friends of behavior analysis, but they are at least better informed about Skinner's views. Further advances for behavior analysis await our field's publications in their journals—publications that make Skinner's contributions relevant to the future of psychological science.

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POSTSCRIPT

The evolutionary psychologist David Barash e-published an article at the end of March, 2005, in *The Chronicle of Higher Education*, titled “B. F. Skinner, Revisited.” It appeared in print on April 1, 2005 (Barash, 2005). Barash praised Skinner’s (1971) book, *Beyond Freedom and Dignity*, endorsing its account of the difficulties Western culture has had in accepting the implications of thoroughly naturalistic approaches to behavior. Among the implications are the denial of self-action, free will, and responsibility as tradi-

tionally conceived. Barash (2005) began his article, though, by distancing himself from behaviorism, writing,

When I teach or write about animal behavior, I often counterpoise B. F. Skinner’s work in particular as the intellectual antipode of my own perspective, which emphasizes the importance of built-in, prewired, evolutionarily generated mechanisms. For Skinner and his disciples, living things (including human beings) are tabula rasa, blank slates upon which the contingencies of reinforcement write as they will, thereby constituting the crucial—indeed, only—determinant of behavior: the experience of each individual. (p. B10)

On March 28, 2005, Barbara Wanchisen (Federation of Behavioral, Psy-

chological, and Cognitive Sciences) alerted me (Morris) and Phil Hine (Temple University) to Barash's article, which I forwarded to Hank Schlinger (California State University–Northridge). Barb, Hank, and others said they would write letters to the editor and urged me to do so, too. At the time, though, I chose not to and, instead, sent Barash a copy of Morris et al. (2004).

In the process, I looked back over our article and saw that Barash (2005) had perhaps been disingenuous in his criticism of Skinner. What he wrote was inconsistent with some of his earlier remarks. So, I did write a letter to the editor, as follows:

I resonated strongly to David Barash's essay on the philosophy of a natural science of behavior in his "B. F. Skinner, Revisited" (April 1). However, his rhetorical device of distancing his own views from what he alleged was Skinner's purely environmentalist stance was unnecessary and divisive. The views he attributed to Skinner were wrong. If Barash had never read Skinner or only *Beyond Freedom and Dignity* [Skinner, 1971], I could understand how his views might have been influenced by the generally poor scholarship in the secondary and tertiary literatures on Skinner. However, Barash has read Skinner. In a 1984 comment in *Behavioral and Brain Sciences* on Skinner's 1966 article, "The Phylogeny and Ontogeny of Behavior," he wrote, "Skinner shows . . . much greater sensitivity to species differences than I had previously appreciated" [p. 680]. In the 1966 article on which Barash commented, Skinner [1966] wrote, pointedly, "No reputable student of animal behavior has ever taken the position 'that the animal comes to the laboratory a virtual tabula rasa,' that species differences are insignificant, and that all responses are equally conditionable to all stimuli" [p. 1250]. When rhetorical devices trump the facts of the matter, this is a great hindrance to collaborative research across research programs and mischievous in its effects on mutual understanding and respect among colleagues.

Thinking that my letter might be a little mean, I sought counsel from my colleagues, among them, Sigrid Glenn (personal communication, April 3, 2005). She suggested that I tone down the final sentence, replacing its last phrase with "this may preclude fruitful collaborations and collegial relations

among scientists." I did. By that time, my colleagues had submitted their letters, properly praising Barash's support of the implications of Skinner's science but also correcting his misrepresentation of Skinner on biological participation. They also copied their letters to Barash, along with personal notes, to which he responded with great grace. In this context, they thought my letter might do more harm than good. I agreed, and did not submit it. Three letters were eventually published (Black, 2005; Fallon, 2005; Schlinger, 2005), along with one critical of both Barash and Skinner for assuming that psychology could be a science (Groot-huis, 2005).

Still, though, I thought that Barash's inconsistency regarding Skinner was worth pointing out. So, on April 3, 2005, I e-mailed him the following:

I loved your recent *Chronicle of Higher Education* article on the difficulties faced by purely natural science approaches to behavior in our culture. However, I thought the opening gambit of distancing yourself from Skinner in re the tabula rasa was, ah, unnecessary? Indeed, in your previous work, you expressed a contrary view regarding Skinner in BBS comments you made on an article in which Skinner stated just the opposite of what you related in TCHE. I wrote a letter to this effect for TCHE, but have decided not to submit it. First, I am aware that others have already submitted letters on the general inaccuracy of your point. Second, although accurate, my letter was a little mean. And third, in the long run, I would rather make friends than enemies. Nonetheless, I thought you might like to see the evidence I would have brought to bear in this matter. I sent you an article [Morris et al., 2004] via snail mail that addresses this point of misrepresentation in greater detail. Again, though, you wrote a great and brave article.

In a matter of hours, Barash e-mailed back, "Hello Ed, you are right! The reality is that in my eagerness to make a point, I slipped into a caricature of Skinner's views, and for that I apologize." I wrote in return the same day, "You are a nice guy . . .," to which he responded, "Not really; just someone who has been wrong so often that I've gotten used to admitting it!" I wrote back that this had made me laugh, not at him, but with him. That was the end of our exchange. I include it here, with

Barash's permission, to let him explain what had happened to behaviorism in his article that otherwise praised Skinner. I also include it to reinforce the point that younger scientists will easily find misrepresentations of Skinner's views on biological participation in the contemporary literature.