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Changing Course

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Although the field of behavior analysis provided the formative experiences and much of the profession training that have fueled a long and satisfying career, I now find that I am turning away from behavior-analytic research and teaching. My reasons for making this change stem from concerns about the costs and potential benefits of basic behavior-analytic research strategies and the isolation of behavior analysis from mainstream science. I offer some suggestions for strengthening the profession.

Key words: experimental analysis of behavior, professional issues, research methodologies, graduate training

After many happy and fruitful years in the field of behavior analysis, I find myself turning away. It is not because I've discovered weaknesses in the principles of behavior. Nor is it because I've decided that respondent and operant processes represent only a tiny fraction of what humans do, and, as a result, I prefer to swim in deeper waters. On the contrary, I am convinced that the basic principles of behavior are strong and their influence on human life is far more pervasive than is understood by most who have not studied behavior analysis. Rather, my turn to other pursuits is spurred by what I believe to be weaknesses in the profession and the relation of those weaknesses to my current career goals.

Although my change of direction began much earlier, recent commentaries on the future of behavior analysis (Poling, 2010; Schlinger, 2010) and the potential benefits of translational research (Branch, 2011; Critchfield, 2011; Mace & Critchfield, 2010; Neuringer, 2011; Pilgrim, 2011; Poling & Edwards, 2011) in this journal and elsewhere lay the groundwork for this article. My shift in focus is inspired by a utilitarian philosophy. After being a psychology professor for almost three decades, I am clear about one thing: Above all else, I am a science teacher; not a behavior analyst but a behavioral scientist and an advocate for scientific thinking. Behavior analysis provided my introduction to a scientific approach to human behavior. It was the science in behavior analysis that grabbed me: the imposition of order on the apparent willfulness and idiosyncrasy of human behavior; the power of an independent variable to reveal important aspects of nature that would otherwise go unseen. But as I became a mature professional, I was more and more convinced that the science *in* behavior analysis had greater value for me than the science *of* behavior analysis.

Behavior analysis does science very well. It is arguably the most thoroughly rigorous approach to human behavior ever devised. But there are many ways to do science. Some are better than others, but many get the job done. At this point in history, it would be foolish for behavior analysts to cling to the notion that traditional psychological science has produced little of value. As someone who teaches at least one section of introductory psychology every year, I see little evidence of what Branch (2011) calls "the ongoing disintegration of psychology as a field" (p. 21). Psychology remains one of the most popular college majors in the United States. According to the U.S. Department of Education (2011), 97,200

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bachelor's degrees in psychology were awarded in 2009 and 2010, up from 74,200 in 1999 and 2000. With a recent Nobel prize going to a traditional experimental psychologist, and a remarkable number of best-selling books being produced by academic behavioral scientists (e.g., Ariely, 2008, 2010; Kahneman, 2011; Zimbardo, 2007), the field seems to be doing quite well. From my vantage point, the perennial problem for departments of psychology is staffing: finding enough instructors to handle the ever-increasing enrollments.

But popularity is an unreliable measure of value. Some very bad ideas have been enormously popular, and psychology has had its fair share of bad ideas. Nonetheless, I see considerable evidence of advances in other areas of psychology. Here are just two examples from fields that are most familiar to me:

Loss aversion. Behavior analysts have been studying rewards and punishments for many decades, but the simple concept of loss aversion—that a loss has much greater effect than a gain of equal magnitude—was discovered by behavioral economists. Loss aversion is one of the principles of prospect theory (Kahneman & Tversky, 1979; Tversky & Kahneman, 1981; Wakker, 2010), and the application of this principle, combined with other techniques, has led to promising applied research (e.g., on the payment of teacher bonuses, Fryer, Levitt, List, & Sadoff, 2012) and to the development of Save More Tomorrow, an intervention used by over half of the large companies in the United States to encourage employee retirement savings (Benartzi, 2012; Thaler & Benartzi, 2004). Loss aversion is a powerful principle that appears to have important implications in a wide variety of contexts (Baumeister, Bratslavsky, Finkenauer, & Vohs, 2001).

The illusion of conscious will. Although B. F. Skinner (1971) was famously associated with the belief that free will is an illusion, this persistent scientific and philosophical question has received little attention from behavior analysts. For example, behavior analysts played a central role in the debunking of facilitated communication (Green & Shane, 1994; Jacobson, Mulick, & Schwartz, 1995), but it was researchers outside the field of behavior analysis (Wegner, Fuller, & Sparrow, 2003) who took up one of the most baffling questions to come out of this episode: How could so many facilitators be unaware that they were doing the typing? In recent years, psychologists have returned to the question of free will and have identified a number of the variables that lead participants to believe they have caused events they have not caused and, conversely, to believe they have not caused events they have caused (Ebert & Wegner, 2011; Wegner, 2002; Wegner & Wheatley, 1999).

Offering an undergraduate student a good behavioral science education does not always produce someone who thinks scientifically about human behavior, but this is the challenge I believe is worthy of my time and effort. In a country where, according to recent polls, only 40% of adults believe in evolution (Miller, Scott, & Okamoto, 2007), promoting scientific thinking seems like the best use of my remaining years. Science does not provide the answer to everything, but a society informed by evidence is likely to do better in the long run than one guided by intuition and superstition.

Fortunately, I am in a position to make a contribution to the understanding and value of science. I teach introductory psychology, and because the course satisfies a general education science requirement, the majority of my students are nonmajors for whom Psychology 101 represents their only college-level—and often their last ever—science course. It is essentially a behavioral science course for a very important target group: the-soon-to-be college-educated general public; I approach it with a great sense of responsibility. Teaching Psychology 101 is arguably the most important thing I do in my working life.

Ironically, in the interest of being a more effective teacher of and contributor to science, I am turning away from the field that started me on my path as a scientist. I am moving on to other research and teaching topics, other conferences, and other journals. For over 20 years I have offered an upper level undergraduate course for psychology majors called "Conditioning and Learning: Theories and Applications" with a required rat laboratory, but I recently retired this offering in favor of a new course on behavioral economics. I make these changes with some reluctance because I am grateful for all that behavior analysis and its people have given me over the years, but the current state of the experimental analysis of behavior (EAB) makes it more useful for me to spend my time in other ways. Here are two reasons why.

The Costs and Likely Benefits of the Experimental Analysis of Behavior

I am convinced there is still great potential for the application of behavior analysis to an increasing array of social issues. Any visitor to the annual convention of the Association for Behavior Analysis International (ABAI) will discover a large and vibrant group of practitioners and applied researchers who are finding new ways to address a variety of psychological conditions and societal problems, and I suspect there is enough work of this kind to keep the field busy for the foreseeable future.

However, my interests are in the experimental domain, and in stark contrast to the applied area, basic EAB appears to be painting itself into a corner. Although he has much greater authority than I to say so, I agree with Poling's (2010) assessment that EAB has become the "esoteric analysis of behavior" (p. 8) and with his statement that, "In fact, many established areas of basic research involve little or nothing more than a progression of well-controlled studies that clearly demonstrate the influence of unimportant independent variables on trifling outcome measures" (p. 9). In my opinion, this rather harsh assessment is more a statement about scientific methodologies and scholarly isolation than it is about the unexplored behavioral universe. Undoubtedly, there are many things (perhaps an infinite number of things) that still wait to be discovered through the methods of behavior analysis, but in my opinion, little of what is left to be discovered using the current methodologies is worth the time, expense, environmental impact, and laboratory animals needed to acquire it. I fully admit that this is something of a religious statement based on things that are inherently unknowable (e.g., the future). But the operant chamber has been with us for over 80 years, and I believe it has long ago yielded its most important secrets. Those secrets were powerful and pervasive, but like several recent commentators (Critchfield, 2011; Neuringer, 2011), I believe the future will be more productive if behavior analysts begin to address different questions using different methodologies.

In the publishing year beginning in November 2011 and ending in September 2012, 30 empirical articles appeared in the *Journal of the Experimental Analysis of Behavior* (*JEAB*). Of these, 21 used classic operant methodologies (rats and lever presses or pigeons and key presses), nine used human participants, and one used other species. (These counts do not sum to 30 because one study used both human and nonhuman subjects.) Of the nine human experiments, three were studies of stimulus equivalence, and one each addressed choice, multiple-schedule performance, verbal behavior, behavioral momentum, self-control, and pharmacological effects. Two of the most unusual studies were one that employed a prisoner's dilemma methodology to study self-control and another that used stimulus equivalence techniques to explore false memories.

I argue that these totals, taken in the journal's 54th year of publication, show very modest methodological expansion or development. There has been considerable topical development, of course. Stimulus equivalence, the matching law, and behavioral momentum had not been discovered in 1958 when *JEAB* was launched, but by now these topics have been extensively researched. And yet, with a few exceptions, behavior analysts continue to plumb many of the same waters they were exploring in 1958.

To be certain, the kind of triviality that Poling (2010) points to can be found throughout mainstream psychology, but behavior analysis is bound to a limited group of methodologies and, as a result, a relatively restricted set of experimental questions and publishing outlets. At the beginning of the field's history, there may have been good reasons for behavior analysis to establish itself as a separate discipline, but as it has become a more mature science, many of the distinguishing characteristics of the EAB now stand as barriers to the goal of greater influence in the larger scientific community.

Isolation from Mainstream Science

Behavior analysts have been worried about their small numbers and limited influence for at least 30 years (Lattal & Harzem, 1984; Skinner, 1981). The burgeoning popularity of applied behavior analysis in the treatment of autism spectrum disorders (ASD) seems to have secured the position of the applied wing for the time being, but as recent discussions in this journal suggest, hand wringing continues in the basic research area. Much has already been said about this topic; however, the recent commentaries have not touched on some of the important ingredients that might lead to greater popularity and influence. To illustrate this point, I briefly turn to one of EAB's striking successes: delay discounting.

In her contribution to the recent translational research discussion, Pilgrim (2011) pointed out that delay discounting has managed to capture the attention of scholars in other fields and achieve the valued status of grant worthiness. Similarly, Critchfield (2011) expressed the view that delay discounting "clearly has gone mainstream" (p. 8). Pilgrim also contrasted delay discounting with stimulus equivalence which, despite a comparable 40-year history, has drawn little attention outside behavior analysis.

Why are these two cases so different, and in particular, how has delay discounting managed to be so successful? I believe there are a number of reasons. Hyperbolic discounting was first empirically demonstrated by Ainslie (1974) while he worked in Herrnstein's laboratory. In the 1970s and 1980s, Herrnstein and his colleagues published a number of papers on the matching law, melioration, and delay discounting in prominent mainstream psychology journals (e.g., de Villiers & Herrnstein, 1976; Prelec, 1982), and by the 1990s, his group was publishing delay discounting papers in equally prominent economics journals (e.g., Herrnstein, 1991; Herrnstein & Prelec, 1991). What made this progression possible?

One factor is mathematics. Economics is a discipline replete with mathematical models, and Herrnstein and his colleagues spoke that language. Herrnstein began his career at Harvard working with S. S. Stevens, who was fitting mathematical functions to psychophysical data; Howard Rachlin came to Harvard with an undergraduate degree in mechanical engineering; and one of Herrnstein's later students, Dražen Prelec, had a degree in applied mathematics. So one reason for the success of this group was that they possessed the methodological skills required to communicate with their target audiences: mainstream psychologists and economists.

But mathematics is not the entire story. Melioration, which Herrnstein developed with Will Vaughan (Herrnstein & Vaughan, 1980; Vaughan, 1981), was also presented in economic journals (e.g., Prelec, 1982) and was one of the most important theoretical developments to come out of Herrnstein's laboratory, yet it has captured far less attention than delay discounting. Why? I can think of three possible reasons.

First, hyperbolic discounting challenged a basic assumption of the "standard economic model" (Wilkinson & Klaes, 2012). The economist Paul Samuelson (1937) first proposed the exponential decay function for delayed rewards that became the accepted model of discounted utility. In 1955, Strotz challenged the notion of exponential discounting and noted that it was inconsistent with commonly observed preference reversals among rewards of varying delays ("the intertemporal tussle," p. 180). But it was not until behavior analysts began to address the problem in the laboratory that hyperbolic discounting and the experimental control of preference reversals were convincingly demonstrated (Ainslie, 1974, 1975; Ainslie & Herrnstein, 1981; Chung & Herrnstein, 1967). As a result, the discovery of hyperbolic discounting refuted a basic assumption of the field of economics. Furthermore, the existence of predictable preference reversals flew in the face of standard notions of the economic person (*homo economicus*) and rational choice.

Second, although much of the experimental work on delay discounting has been done with nonhuman species using lengthy steady-state strategies, the basic phenomenon of preference reversals is a pervasive aspect of nature and is not tied to this experimental procedure. There is now an extensive human research literature on delay discounting that has used both hypothetical and real rewards (see Frederick, Loewenstein, & O'Donoghue, 2002, for review). Furthermore, it is quite easy to demonstrate preference reversals in a 3-min classroom exercise: "I have in my hand one envelope containing $10 and another containing $12. Raise your hand if you prefer the $10 envelope. Now, raise your hand if you prefer the $12" (see Vyse, 2008, for a description of the full exercise). Many demonstrations of preference reversals have been developed that can easily kindle the interest of college classes, lecture audiences, children, and adults. Few other behavioral phenomena are this easy to demonstrate and so pervasive in everyday life.

Finally, myopic, impulsive choice is at the core of a wide array of pressing social problems, including obesity (Scharff, 2009), personal financial security (Vyse, 2008), and substance abuse (Rachlin, 2004). The discovery of hyperbolic discounting has made a substantial contribution by revealing that our struggles with self-control result from a basic feature of human nature and not from individual weakness of will.

So what lessons can we take from this example of remarkable research success? One moral of the story is to make sure you have good luck. It is not every day that you will stumble on a line of research that challenges a cherished notion in another discipline and is at the heart of a host of important social problems. This much was, for the most part, a fortunate turn of events. But Ainslie, Herrnstein, Rachlin, and the others in the Harvard group who pioneered this line of research were also very well prepared to recognize the importance of hyperbolic discounting and to communicate their findings to other audiences. They read widely in other disciplines and had the mathematical and statistical skills needed to publish in economics and mainstream psychology journals.

There is also great value in phenomena that are not tied to a single laboratory methodology. Herrnstein and colleagues went on to look at preference reversals in humans using procedures drawn from psychophysical research (e.g., Kirby & Herrnstein, 1995), and beginning in the 1980s, a great number of economists and behavioral scientists began to demonstrate hyperbolic discounting using a wide variety of methodologies (Frederick et al., 2002). That delay discounting is such a relatively simple phenomenon to reproduce is a tremendous strength for its success as a scientific principle.

A parallel example can be seen in another area of behavioral economic research: prospect theory (Kahneman & Tversky, 1979; Tversky & Kahneman, 1981). Like Herrnstein and his group, Kahneman and Tversky possessed the mathematical sophistication to publish in economic journals, and like hyperbolic discounting, prospect theory challenged the standard rational economic model. Finally, although, several decades later, prospect theory has been very elaborately enumerated (cf. Wakker, 2010), many of its basic principles, such as loss aversion and framing effects, can be demonstrated with very simple examples (e.g., the classic "Asian disease" case from Tversky & Kahneman, 1981). Indeed, as a research team, Kahneman and Tversky seem to have had a talent for devising simple but very clever tests of their theories (Kahneman, 2011).

Hyperbolic delay discounting and prospect theory were topics first studied by psychologists that have now achieved a kind of canonical status in the fields of both psychology and economics. They were developed by researchers with very different research paradigms but who nonetheless shared two important characteristics: reading widely and mathematical sophistication.

Unfortunately few behavior analysts are able to achieve this kind of reach to wider audiences. Some may be plugging away at the kind of EAB esoterica that will never be of much interest to those outside our little group. Others may be inadequately trained in the languages of other audiences or unwilling to speak those languages. Communication with economists or natural scientists will often require the use of mathematics, and the relatively modest goal of interaction with our nearest neighbors in mainstream psychology usually involves the use of group designs and statistical analyses. These are simply the facts of the behavioral science world as they have existed for some time. In my opinion, as long as behavior analysts continue to cling to what appears to be their most treasured goal, the promotion of a particular brand of science based in a unique set of methods, their influence on a much more worthy objective, the discovery and dissemination of scientific knowledge, will be greatly diminished. In summary, I find myself in complete agreement with Neuringer's (2011) suggestion:

> Both forms of translation, to societal goals and to related fields of inquiry, require broadening of focus. Replace method-oriented research, in which the researcher limits his or her reading to EAB journals and experiments to operant chambers, with experiments directed at answering questions or solving problems that are common to many fields. To do this, researchers may need to reach out to acquire new skills, construct new types of equipment, learn new fields of study, explore new paradigms, and push the boundaries of our science. (p. 29)

And now we have arrived at what is the crux of the matter for me. In my view, too much of the recent debate about translational research and the future of behavior analysis has been political. For example, Critchfield's (2011) suggestion that effective translational research might involve the more frequent use of human participants, group designs, and statistical methods was met with vigorous defenses of the status quo from several commentators (Branch, 2011; DeLeon, 2011; Pilgrim, 2011). I expect this article will engender similar responses. But these dismissals based on traditional arguments serve to maintain a kind of scientific identity and hinder the simple goal of revealing the shape of the natural world, and I believe this is a great misuse of our intellectual resources.

Two reviewers of this article requested that I provide examples to back up my disagreement with Branch's "ongoing disintegration" statement and to defend my assertion that psychological science is a productive and valuable discipline. In making these requests, one reviewer asked for evidence of a "coherent corpus of knowledge" and the other for "a cohesive scientific approach and integrated set of findings." Framed as they are, these requests further reveal the political nature of our enterprise. At its core, science is simply a set of tools that, when used effectively, can dramatically alter our view of the world and, in some cases, pave the way for practical benefits. Cohesion and theoretical order are desirable aspects of a science, but they are not necessary for successful exploration and discovery. If behavior analysts choose to erect barriers that cast their own science in a positive light and separate them from the activities of other investigators (e.g., Machado, Lourenço, & Silva, 2000; Schlinger, 2004), they risk a kind of scientific parochialism. It is somewhat ironic that the theoretical approach most closely associated with the author of "Are Theories of Learning Necessary?" (Skinner, 1950) now stands as one of the few remaining grand theories of psychology. In my opinion many behavior analysts have become distracted with the goal of defending that grand theory and the methodological strategies that built it. I think we would be better served by returning to the view that Skinner proposed in that article:

> But it is possible that the most rapid progress toward an understanding of learning may be made by research that is not designed to test theories. An adequate impetus is supplied by the inclination to obtain data showing orderly changes characteristic of the learning process. An acceptable scientific program is to collect data of this sort and to relate them to manipulable variables, selected for study through a common sense exploration of the field. (p. 215)

It is perfectly reasonable to ask for evidence of "progress towards an understanding" in the field of mainstream psychology, and in response to the reviewers′ requests, I have offered the examples of loss aversion and the illusion of conscious will. But spitting contests about your grand theory versus mine are a distraction from the simple yet essential task of gathering new knowledge. Scientific discoveries can reveal important natural processes whether clothed in a coherent theoretical framework or not. Sometimes theories can be useful heuristic devices, but they can also serve to narrow our vision and encourage confirmation bias. Behavior analysts have been wise to avoid the pitfalls of the hypothetico-deductive approach, but, in my view, we spend too much time defending our particular theoretical and methodological corner of the world and too little time looking for deeper pools of scientific potential. We—and, more important, the goal of scientific discovery—would be better served if we left the theory wars behind and returned to a more common sense exploration of behavior.

SUGGESTIONS FOR THE FUTURE OF EAB

Stated simply, I believe it is time for EAB to rejoin mainstream psychology. In the beginning, there were good reasons for creating a separate scientific profession, but unlike Branch (2011), I believe behavior analysis is no longer a young science. The field is desperately in need of change and innovation, and a continued strategy of isolation will not help us achieve that goal. Here are my thoughts about how a new phase of EAB would look with respect to new methodologies, graduate training, the professional organizations, and the journals.

New Methodologies

Several other authors have urged us to explore new methodologies (Critchfield, 2011; Neuringer, 2011) without, in my opinion, going very far in the direction of speculating on what those new methodologies might be. I think the elephant in the living room is self-report. Of course, behavior analysts do study verbal behavior, but following Skinner's (1957) lead, they seem more concerned with language acquisition and a functional analysis of language than with the enormous swath of verbal responses made by competent speakers. Many of the most important things we do in life—voting, choosing a health care or retirement plan, endorsing a verdict in a trial, lying or telling the truth—involve verbal responses, and because behavior analysts typically reject self-report as a methodology, they have effectively ceded all these topics to non-behavior-analytic researchers. In my view, making peace with self-report and broadening our methods of studying human behavior in general would go a long way toward producing more valuable, less esoteric findings and would give our work greater relevance beyond the boundaries of our little clan.

An expanded study of human behavior will undoubtedly require more frequent use of group designs and statistical methods. These more traditional research strategies are occasionally found in the pages of *JEAB* now, so the objection to these approaches is not categorical within behavior analysis. But greater openness to the publication of human research that has employed group designs, in our journals and in mainstream journals, would help to expand the field.

It is somewhat ironic that the new methodologies I am recommending are, in fact, quite old methods in the larger behavioral science community. They are the very same methods that behavior analysts eschewed as they endeavored to create a unique discipline for themselves (Sidman, 1960). But behavior analysts need not look further than their own field to see the potential power of between-subjects designs. If asked to identify a single journal article that has had the greatest influence on the growth of ABAI, I suspect most people would point to Lovaas (1987), a between-subjects design published in a high-impact mainstream psychology journal. This was, of course, an applied study, and the current topic is the experimental domain. But it is likely that many phenomena of interest to behavior analysts are amenable to group designs. Furthermore, there is the hope that behavior analysts will discover as yet unexplored methodologies that are hybrids of EAB procedures and more mainstream methods or are entirely new approaches. But, in my view, combining the use of simple group designs with some of the less objectionable forms of self-report measures would be an initial step in the right direction.

Graduate Training

In recent decades, behavior analysis has gone through a professionalization movement, establishing certification standards and procedures for bachelor's, master's, and doctoral level behavior analysts and promoting state licensing laws to regulate the profession (Behavior Analyst Certification Board, 2012). In parallel with the certification movement, a number of graduate programs have emerged to meet the growing demand for certified behavior analysts, primarily in response to the success and popularization of applied behavior analysis for the treatment of people with developmental disabilities. The majority of students in these programs hope to pursue professional careers that provide services to various client populations or other consumer groups, and, as far as I can tell, these behavior analysts are being served quite well by the current graduate education system.

At this point in our history, the same cannot be said for behavior analysts who hope to achieve academic positions and research careers. Some of these students are pursuing doctorates in dedicated behavior analysis programs that stand alone or exist apart from the university's psychology graduate program, and others are working with behavior-analytic faculty within traditional psychology departments. In my opinion, dedicated programs populated exclusively by behavior-analytic faculty no longer serve the EAB scientific profession well. These programs are unlikely to give students the kind of exposure to the diversity of research topics in the larger behavioral science community that would help to expand and enliven behavior analysis.

Most important, whether EAB students are in dedicated behavior analysis programs or in psychology programs, it is essential that they take all the same methodology and data analysis course work as their cohorts in mainstream experimental psychology. Furthermore, if they hope to work in more quantitative fields (e.g., economics), they should acquire the mathematical skills that make publication in those fields possible. If this means that EAB students take more methodology courses than students in psychology (because they might also be required to take behavior-analytic methods courses) then so be it. In that case, EAB students will be even better prepared than students in psychology.

Neither this recommendation for more methodology courses in graduate training nor the foregoing suggestion of methodological expansion in behavior-analytic research should be taken as a theoretical endorsement of statistical methods and group designs, about which I have previously expressed criticism (Vyse, 1988). It is merely a recognition of the conditions for extending our work to new topics and publishing it in mainstream journals. Behavior-analytic researchers who acquire a broad background in statistics and methodology will also be better prepared to succeed in the job market. In my case, work experience in applied behavior analysis was my introduction to a scientific approach to human behavior, but it was statistical skills that got me a tenure track job. On a very basic level, the EAB will not grow unless new researchers are able to secure academic positions. More extensive methodological training can only help.

The Professional Organizations

Currently basic behavior analysis finds itself in an unusual place with respect to the various professional organizations. ABAI is a very strong body with a large membership and an extensive network of chapters and special interest groups, but it is increasingly dominated by applied researchers and practitioners. In the years since the big ASD boom in applied behavior analysis, the relationship of basic researchers to ABAI has become somewhat awkward. On the one hand, there is the Society for the Quantitative Analysis of Behavior (SQAB), a small band of researchers who typically meet the day before ABAI gets under way. When the main conference begins, EAB researchers become a small minority in a sea of applied professionals. Meanwhile, there is Division 25, Behavior Analysis, of the American Psychological Association (APA); the Association for Psychological Science (APS); and a collection of smaller organizations, such as the Psychonomics Society and the Society for Judgment and Decision Making.

Over the years, Division 25 has played the role of a tiny subgroup of a much larger organization featuring a very modest schedule of activities and presentations. Membership in Division 25 has declined from a peak of 1,642 in 1976 to 624 in 2011 (American Psychological Association, 2012). APS has no division structure, but it is worth noting that a number of behavior-analytic researchers have managed to achieve much wider audiences for their work by publishing in the APS flagship journal *Psychological Science* (e.g., Estle, Green, Myerson, & Holt, 2007; Jones & Rachlin, 2006; Kirby & Herrnstein, 1995; Neuringer & Voss, 1993).

If behavior-analytic researchers hope to present their work to wider audiences, they would be well served to move their professional activities to those audiences. ABAI is dominated by applied researchers whose common theoretical viewpoint is both a blessing and a curse. On a purely social level, it is enjoyable to spend time each May with a group of like-minded people, many of whom have become close friends. But the very philosophical uniformity that is such an appealing aspect of ABAI is a constraint on the flow of information. We both speak to and hear from the same happy few.

A more intellectually productive approach would be to move SQAB and the rest of EAB to either APA, APS, or both; effectively, if not actually, completing the transformation of ABAI into an applied organization. If experimentalists were to recommit themselves to Division 25 or make a home for themselves in APS, they would be afforded the possibility of much greater interaction and collaboration with scientists from related fields.

There will undoubtedly be many objections to such a suggestion. For example, ABAI is an international organization with membership from all over the world, and APA is not, making it a less hospitable environment for behavior analysts from outside the U.S. True, but APS is an international organization. What about people from other disciplines and those without PhDs? In fact, most basic science research is done by doctoral-level professionals (and their graduate and undergraduate students) at academic institutions. This is a reality of the behavioral science world. As a result, the limitations posed by the requirement of a doctorate should not be a barrier for the overwhelming majority of behavior-analytic researchers.

If travel expenses are limited and researchers are forced to choose between ABAI and one of the larger mainstream organizations, initially it will be an uncomfortable choice to make. But if behavior analysts continue to avoid other psychological audiences and presenters simply because it is more comfortable to do so, they will be engaged in a kind of meliorative process that will not help to maximize the influence of their research endeavors.

The Journals

I see no need to make substantial changes in the existing behavior-analytic journals. They serve a very useful function and will continue to do so. There is hope that in a revitalized environment for basic behavior-analytic science, other research methodologies will be acceptable for publication in behavior-analytic journals, and the trivial kinds of research Poling (2010) points to will become less common. If, sometime in the distant future, ABAI were to become an explicitly applied organization, there might be a logical argument for moving *The Behavior Analyst* and *The Analysis of Verbal Behavior* under SEAB. But the issue at hand is not our journals so much as theirs: achieving publication in mainstream journals that will have greater readership and increased influence. Accomplishing this goal will depend on the kinds of changes suggested by Critchfield (2011), Neuringer (2011), me, and others.

CONCLUSION

Beyond this article, my change of course will hardly be noticed. My research has been very eclectic, with relatively few publications appearing in behavior-analytic journals. For much of my career I have served on the editorial board of one or more of the journals, but at the moment, I am without a board assignment. I have never been a consistent conference goer in any of the professional organizations to which I belong. I will not leave a big hole.

But I suspect that some of what I have expressed here will resonate with others. Age and a utilitarian approach to my remaining time as a college professor have helped bring me to this change of direction, but I suspect many professionals, regardless of age, share my interest in doing as much as they can with their working lives. Furthermore, if behavior-analytic researchers hope to increase their numbers and achieve greater prominence and influence in the larger scientific community and in society in general, I believe changes like the ones I have suggested are more likely to be effective than merely urging each other to read more widely and redouble our efforts to get published in mainstream journals. As Critchfield (2011) suggests, "we cannot simply will EAB into greater social relevance" (p. 16). Reshaping the experimental analysis of behavior will require broadening our methodological repertoires and reconstructing the environments in which behaviorally oriented scientists are trained and nurtured.

In addition, strengthening behavior analysis may require some further discussion of what it means to be a behavior-analytic researcher. If we choose to define ourselves by a set of methodologies, then I believe we are in trouble. If, on the other hand, we are united by the view that "a science of behavior is possible" (Baum, 2004, p. 3) and by the kind of conceptual rigor that has served us so well, then I believe the future is bright. There is still much more we can contribute to the understanding of behavior and the promotion of science as a method of inquiry.

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