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Moving Forward Without Changing Course

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It may be healthy for a scientific discipline to periodically evaluate its assumptions and practices to ensure that it is proceeding in an efficient and logically consistent path towards its goals. In this vein, Vyse (2013) has offered a critique of the current status of the experimental analysis of behavior (EAB). The paper is one of a number of recent articles that has criticized current basic behavior-analytic research. With this paper, Vyse has constructed an impassioned argument for changes he sees as essential for the viability of the field.

As basic behavioral researchers, we find ourselves in agreement with many points raised in the article. We agree that there are tangible benefits from building bridges with other sciences, adopting mathematical language, supporting research in novel areas (especially ones with clear relations to practical problems), and using the most effective research methods available. In our view, however, EAB is already doing those things. We therefore disagree with criticisms that EAB is isolated, esoteric to a fault, and limited to a small set of fixed methods. Moreover, we strongly disagree that the entire enterprise of EAB should rejoin or emulate mainstream psychology.

Many basic behavior-analytic researchers have been aligning themselves with other disciplines and publishing in a variety of mainstream journals, inside and outside psychology. The recent Association for Behavior Analysis International (ABAI) conference on behavioral economics, for example, included invited presentations by individuals who conduct research in such diverse areas as government policy and neuroscience. Some of the presenters were behavior analysts, and some were trained in other specializations. Annual programs of the Society for the Quantitative Analysis of Behavior (SQAB) typically include researchers from a variety of theoretical perspectives and research interests. Also, the addition of the B. F. Skinner lecture series to the ABAI conference program is increasing awareness among behavior analysts of compatible and complementary work of scholars from other disciplines, and vice versa. Many basic researchers also make an effort to attend and present research at psychological conferences besides ABAI, including the Conference on Comparative Cognition, the Psychonomic Society, Winter Conference on Animal Learning and Behavior, American Psychological Association, Association for Psychological Science, the College on Problems of Drug Dependence, and the Society for Neuroscience, among others.

Some of the more theoretically based behavior-analytic work is published in the *Journal of the Experimental Analysis of Behavior* (*JEAB*)

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because the journal provides a relatively unique outlet for this kind of research. Thus, the content of *JEAB* may not represent the breadth of research being conducted in the field. One can find EAB research published in many journals that appeal to broad audiences, including *Behavioural Brain Research, Behavioral Neuroscience, Hearing Research, Behavioural Processes*, *Journal of Comparative and Physiological Psychology, Learning and Motivation, Journal of Experimental Psychology: Animal Behavior Processes, Psychonomic Bulletin & Review, Animal Learning and Behavior, Behavioral and Brain Sciences,* and so on. Behavioral pharmacologists, many trained in the EAB tradition, have been especially successful in publishing their research in a wide range of psychopharmacology and substance abuse journals. Clearly, more can be done to increase the influence of behavior analysis, make connections with other fields, and gain broader acceptance, but EAB has and continues to diversify and progress in this direction.

Vyse echoes the criticism of others (e.g., Critchfield, 2011; Poling, 2010) that EAB is too esoteric and that much basic research is of little practical value or social relevance. We disagree. Basic researchers conduct research in diverse areas, including areas that have traditionally been of interest to other disciplines. Recent SQAB programs have included talks in such areas as concept learning, associative learning, neural networks, timing, memory, altruism, animal learning, gambling, as well as (as Vyse mentions) behavioral economics, choice, and impulsivity. Note also the breadth of research topics, including varieties of translational research, summarized in the recent two-volume *APA Handbook of Behavior Analysis* (Madden, 2012). We agree that the field benefits from well-designed translational research, and that some portion of basic research should focus on questions that are closely linked to social problems (as some clearly does, see Volume 2 of Madden, 2012). Much basic research however, even if it does not *appear* to address social concerns or meet usual definitions of translational research, bears on practical issues (see Pilgrim, 2011). Research on stimulus control and stimulus equivalence, delayed match-to-sample performance, extinction, resistance to change, conditioned and token reinforcement, delayed reinforcement, avoidance, and punishment, for example, is relevant to various social concerns (e.g., education, memory deficits, learning delays, self-injury and aggression, economic behavior, risk taking, addiction treatment, treatment of psychological disorders, etc.). As Pilgrim (2011) has noted, unfortunately the practical value of behavioral applications may not be sufficient to guarantee the growth and viability of the field; behavior analysis has discovered many practical applications that society has failed to adopt.

We also would argue, however, that it is healthy for some portion of basic behavior-analytic research to be completely esoteric. Part of the work of basic science is theory building. Although Vyse implies that Skinner was atheoretical, he was atheoretical only in one sense: He objected to theories that explained behavior from a different level of analysis (e.g., mental or physiological). He was a strong advocate for developing a theory of behavior that remained at the level of behavior because he believed that this enhanced prediction, description, and control of behavior (Skinner, 1947/1999, 1950, 1969). Behavior analysis has been successful at building a theory of behavior, but much work remains to be done. There are fundamental questions about the nature of behavioral processes and even behavioral units that remain unanswered. Thomas Kuhn (1962) has argued that, once a science has adopted a paradigm and enters a period of

normal science (one could argue that behavior analysis is such a paradigm), much of the work of scientists is to better articulate that paradigm. This ''mop up work,'' as Kuhn describes it, is the task of basic scientists. Some of this work necessarily is esoteric and not easily comprehensible to lay audiences because the work is specialized. That only some scientists will appreciate or even fully understand the importance of the research (at least at the time of its completion) is to be expected. This work, however, is necessary for the advancement of the science. Pure basic research enables scientists to study phenomena in close detail. Although important discoveries have and will continue to come from translational research, problem-solving research often does not have the flexibility to map out the effects of a range of values of an independent variable, track down sources of behavioral variability, or study behavior for extended durations to assess reliability. By supporting basic behavioral work, we can continue to build a theory on which we can base further extensions and applications.

Some have argued that theory building is a luxury that we cannot afford when we are faced with diminishing resources and a host of urgent social problems. We appreciate these arguments, but we do not believe that the field must choose between supporting either basic science or translational research. Both are needed. We also maintain that variability in research is good for science; it makes adaptation and evolution possible. By adopting a position that devalues research that asks unpopular questions or questions that do not have immediate practical implications, we would be reducing that variability.

It is not obvious to us how change in our theoretical perspective will improve the influence of behavior analysis. Behavior analysis has been successful in predicting and controlling behavior precisely because it has eschewed traditional views that assign causes to inner (usually mental) agents or events. It is by focusing on causes that are observable and controllable, and by identifying functional relations in individuals, that our field has been able to build a solid foundation of behavioral principles and effectively translate laboratory discoveries into practical applications. An eclectic or ''common-sense'' approach to behavior that yields a catalogue of facts and a collection of unrelated explanations may be useful or popular in the short term, but a comprehensive, internally consistent approach has been and will continue to be the most efficient path towards meaningful scientific progress.

Vyse indicates, however, that the very theory that EAB seeks to build is one that is limiting our science to a small set of fixed methods and questions, thereby minimizing its relevance. As Vyse admits, though, behavior analysts have embraced various research methods, including group designs, when those designs are useful for answering certain types of questions. Behavioral pharmacologists have long used a wide variety of designs, methods, and even apparatuses for analyzing drug effects (e.g., group designs to investigate drug effects on development, water mazes to study drug effects on spatial learning, etc.). The field's preference for studying the behavior of individuals in single-subject designs, therefore, is not simply blind adherence to precedence. Rather, the field values and advocates single-subject designs because they have proven to be effective in identifying controlling variables in individual participants (see Perone & Hursh, 2012). We maintain that it is a mistake to equate an unchanging methodology with a broken one. In phylogeny, phenotypes can sometimes remain unchanged for hundreds of millions of years because they are successful.

Similarly, if certain scientific methods seem immutable, it could reflect a successful variation.

Basic researchers have also not shied away from collecting verbal reports of human research participants. In some human studies, they provide important information about controlling variables (e.g., Hackenberg & Axtell, 1993) whereas in others they are key dependent variables (e.g., De Freitas Ribeiro, 1989; and see Fischman & Foltin, 1991). Sometimes, they are the only way to assess behavior when direct measures of behavior are impossible (for a review, see Critchfield, Tucker, & Vuchinich, 1998). A main concern with verbal reports however, is that they can be unreliable. Simply put, people do not always do what they say or say what they do. Nonetheless, we agree that verbal reports can be an important source of data, and as Critchfield et al. (1998) have argued, additional behavioral research is needed to develop effective self-report methods. Verbal reports cannot substitute for direct measures of behavior, however. For example, although we may ask people which options they would prefer on a self-control task, and their reports may resemble what they actually would do, it seems unlikely that a person could accurately describe how he or she would respond on an avoidance procedure or under a complex conditional discrimination.

We agree with Vyse's argument that basic behavioral researchers should be given greater training in mathematics, but for different reasons. Although there may be bridges that can be built though modeling and quantification, we are not convinced that mathematics provides a common language in psychology. The nature of the variables that comprise quantitative models varies across disciplines, and models are based on different assumptions about causes of behavior. Moreover, contrary to Vyse's argument, mathematification often results in criticisms of esotericism. A more important function of mathematics and quantification in behavior analysis is that it allows behavior scientists to better specify functional relations and improve the prediction and control of behavior. Modeling can therefore be a valuable tool for behavioral research, and the field would benefit from an increase in quantitative sophistication (see Dallery & Soto, 2012). Fortunately, there is already a large group of behavior analysts (including those who attend SQAB) who value and promote the quantitative analysis of behavior, and the number of articles published in *JEAB* that include quantitative models has steadily increased (Mazur, 2006).

Perhaps one of the main problems we see with EAB is that there are too few basic behavioral scientists. There are simply not enough scientists trained in basic behavioral principles to build bridges with the many other disciplines with whom we share interests, and to conduct the many forms of translational research that will lead to novel or improved practical applications. We do not know how many graduate degrees are awarded each year in EAB, but we expect that it is a very small fraction of the degrees awarded in applied behavior analysis. It should not be surprising, then, that the diversity of basic research is not as broad as one might like; it takes considerable training to gain expertise in an area, and this often requires a narrowing of focus. To accomplish its goals, the field should prioritize the training of basic researchers and increase their numbers. As numbers increase, so also will the variability from which new research directions and approaches can be selected.

On a practical level, however, one wonders whether large numbers of basic behavioral scientists will be able to find employment. As the application of behavior analysis to the treatment of deficits associated with

autism spectrum disorders has grown, so too has the need for training in behavioral principles. This growth does not appear to have resulted in many new jobs for basic behavioral scientists (many behavior analysis faculty positions, for example, require certification from the Behavior Analysis Certification Board, thus limiting opportunities for those intensively trained in EAB). Unless the field places greater value in basic behavioral research and the training provided by experts in EAB, the numbers are likely to remain small.

We appreciate Vyse's decision to focus his teaching on scientific methods and critical thinking. The prevalence of fad treatments and antiscientific views indicates that much remains to be done to help new generations of students distinguish science from pseudoscience. Training in EAB can contribute to this process. We also appreciate his enthusiasm for a science of behavior and certainly hope that behavior analysts will play a role in shaping the field of psychology and creating new behavioral technologies that solve real-world problems. It is unclear how this can be accomplished if we abandon the features of our science that have been instrumental to our success.

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