

*TRANSLATIONAL RESEARCH IN BEHAVIOR ANALYSIS: HISTORICAL TRADITIONS AND
IMPERATIVE FOR THE FUTURE*

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“Pure basic” science can become detached from the natural world that it is supposed to explain. “Pure applied” work can become detached from fundamental processes that shape the world it is supposed to improve. Neither demands the intellectual support of a broad scholarly community or the material support of society. Translational research can do better by seeking innovation in theory or practice through the synthesis of basic and applied questions, literatures, and methods. Although translational thinking has always occurred in behavior analysis, progress often has been constrained by a functional separation of basic and applied communities. A review of translational traditions in behavior analysis suggests that innovation is most likely when individuals with basic and applied expertise collaborate. Such innovation may have to accelerate for behavior analysis to be taken seriously as a general-purpose science of behavior. We discuss the need for better coordination between the basic and applied sectors, and argue that such coordination compromises neither while benefiting both.

Key words: translational research, bridge research, the future of behavior analysis, coordinated bidirectional basic–applied research

A reader of this journal during its first 15 years would have seen many reasons for optimism about the future of the experimental analysis of behavior (EAB). This type of research had found a home in many academic institutions, including some of the world’s leading universities (e.g., Lattal, 2002). Extramural research funding was available from several sources. EAB’s influence was palpable and growing: the *Journal of the Experimental Analysis of Behavior (JEAB)* subscriptions were on the rise, and the journal was one of the

scientific publishing world’s early leaders in citation impact factor (Garfield, 1972). In some quarters, EAB was seen as illuminating behavior principles that would set the foundation for addressing the world’s great problems (e.g., Skinner, 1953).

Many laboratory advances have followed, to the point where our hypothetical early reader, awakened after a long sleep, might well be daunted by the methodological and theoretical sophistication of today’s *JEAB* articles (e.g., Nevin, 2008). Yet despite many scientific successes, EAB has seen both its scholarly and societal influence wane. On the scholarly front, *JEAB*’s paid circulation is approximately one-third of its historical peak, and on average during the period 1998–2008 *JEAB*’s citation impact factor ranked at the 47th percentile among experimental psychology journals. In some circles, this modest citation record adversely affects the hiring and advancement of EAB investigators and the funding of academic units in which EAB takes place (personal communications: M. Davison, S. Dymond, W. McIlvane, S. Provost). Perhaps not coincidentally, although no supporting statistics are available, EAB’s institutional infrastructure appears to be crumbling. At

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many universities, basic behavior scientists are not being replaced when they leave their posts, and animal laboratories—historically the field’s empirical crucible—are being closed as the faculty who once tended them retire or as institutions judge the costs of maintaining them as prohibitive. Extramural research funding increasingly is focused on priorities other than basic behavioral science (e.g., National Science and Technology Council Subcommittee on Social, Behavioral, and Economic Sciences, 2009).

On the societal front, laboratory-derived behavior principles rarely are mentioned in discussions of prominent behavior-related public policy issues (e.g., Diamond, 2005; Gershoff, 2002; Gore, 2006; Thaler & Sunstein, 2008), which instead tend to be disconnected from Psychology or dominated by the concepts and assumptions of cognitive psychology. For example, when the National Research Council of the National Academy of Sciences commissioned a blue-ribbon panel to review learning science with the goal of exploring “the relevance of basic science to education” (Bransford, Brown, & Cocking, 1999, p. v), the book-length report contained not a single reference to the basic operant learning literature (unless one counts Hull, 1943). When basic behavior principles receive public attention, unfortunately sentiments like the following are all too common: “Fundamental laws of reinforcement were derived [from] the behavior of captive starved lower animals. Such laws are ... generalizable to the conditioning of single captive starved lower animals and mentally retarded and very young human students, but not beyond” (Friedman & Fisher, 1998, p. 233–234).

HOW DID WE GET HERE?

How may EAB’s reversal of fortune be explained? Generic factors—such as shifts in psychologists’ preferred style of theory (the “cognitive revolution”) and an industry-wide decrease in journal subscriptions—may be important but are not of special interest here. More pertinent to the present discussion are specific factors related to how the mission of EAB is conceived. We propose that the fortunes of EAB have always been influenced by the extent to which a broad audience, including nonscientists and scientists who are

not specialists in the analysis of behavior, perceives it as relevant to important human affairs. This broad audience is important in providing support for both the activities of science (e.g., through research funding and academic positions) and the products of science (e.g., through article citations and journal subscriptions). The more nonspecialists who believe that EAB can illuminate matters about which they care, the greater the likelihood that EAB will prosper.

Skinner’s Advocacy

What evidence supports this view of practical relevance as an arbiter of scientific sustainability? Consider that the early flowering of EAB corresponds to a period of exceptional public relations, provided by B.F. Skinner, whose founding vision for EAB included attention to the relevance of laboratory-derived principles to human affairs. In Skinner’s inaugural treatise on operant behavior he noted, “The reader will have noticed that almost no extension to human behavior is made or suggested. This does not mean that he is expected to be interested in the behavior of the rat for its own sake. The importance of a science of behavior derives largely from the possibility of an eventual extension to human affairs.” (Skinner, 1938, p. 441) Fortunately for EAB, Skinner went on to become the “Most Eminent Psychologist of the 20th Century” (Haggblom et al., 2002), and much of his career was devoted to explaining how laboratory-derived behavior principles shed light on concerns of everyday importance (e.g., Skinner, 1953, 1957, 1971). Interestingly, the beginning of difficult times for EAB corresponds roughly with the final stage of Skinner’s career, during which his attention shifted from specific extensions of laboratory-based principles toward critique of alternative theoretical approaches (e.g., Skinner, 1977, 1987, 1990). Ultimately, of course, Skinner’s passing cost EAB its strongest advocate, and no successor has achieved the broad scholarly and societal credibility that Skinner could bring to bear on behalf of basic behavior science.

Embrace of a “Pure Basic” Science Model

Although Skinner celebrated the everyday relevance of laboratory-derived principles, EAB itself has developed primarily as “pure

basic" science (Skinner, perhaps unwittingly, set the tone by devoting his empirical career almost exclusively to laboratory work; e.g., Skinner, 1938; Ferster & Skinner, 1957). The abstract mission of "pure basic" inquiry—to advance fundamental knowledge for its own sake—has been held in high regard at least since classical Greek times (Stokes, 1997) and cannot be questioned on its own terms (most people agree that knowledge is a good thing). A possible bone of contention, however, concerns justifying the effort and resources that are required to acquire knowledge. In modern times "pure basic" science often has been defended in terms of its indirect benefits. In this view, the basic scientist creates knowledge that others (applied scientists and engineers) will employ for the practical benefit of society but, importantly, the basic scientist bears no personal responsibility for considering the practical implications of his or her research.

There are two problems with the indirect-benefits defense of "pure basic" science, the first of which is that it is not terribly accurate. While colorful examples may be cited of practical innovations that flowed directly from basic research, systematic appraisals indicate that good science at best only sometimes leads to practical benefits, and technological advances often do not wait for specific insights from basic science (Gribben, 2002; Rogers, 2004; Rutherford, 2009; Stokes, 1997). This is not to say that "pure basic" research is incapable of stimulating technological development, only that under normal circumstances it is neither necessary nor sufficient for this purpose.

Of greater relevance to the present discussion is the second problem with an indirect-benefits argument: It ignores a constellation of indirect *costs to science* of the "pure basic" approach. In order for science to make a difference, within scholarly disciplines and within society, someone must attend to its products and someone must provide the tangible resources for it to be pursued. Scientists who master only the literature, procedures, and language of basic science are unlikely to contribute to these outcomes. A primary reason concerns communication: When not forced to consider the everyday relevance of their investigations, basic scientists may not develop the skills needed to

explain the practical significance of their research. This matters because often only a few specialists possess the technical expertise to understand primary reports of basic research. Persons in a position to achieve practical benefits tend not to be basic-science specialists (e.g., Critchfield & Reed, 2009; Stokes, 1997), and thus, without assistance, may not encounter or understand relevant basic science findings. Casual inspection of *JEAB* articles suggests that EAB investigators rarely attempt to provide this assistance. As a result, perhaps, the potential audience for EAB research remains small.

In neglecting the everyday implications of basic research, it is also the case that investigators may fail to ask questions of practical significance (Cullen, 1981; Mace, 1994). In order to contribute to practical advances, basic science must anticipate key aspects of important everyday problems, but whether "pure basic" scientists are in a position to do this can be debated. Bibliometric analyses, for instance, provide little evidence that basic behavior scientists are influenced by the applied literature (e.g., Critchfield & Reed, 2004; Elliott, Morgan, Fuqua, Ehrhardt, & Poling, 2005). Not surprisingly, commentators on the applied significance of fundamental behavior principles often point to gaps in the guidance provided by the basic literature (e.g., Critchfield, in press; Cullen, 1981; Lerman & Vorndran, 2002; Mace, 1994).

Collectively, these problems help to explain the growing societal impression that basic research is irrelevant to everyday concerns and an attendant erosion of public appreciation for and support of the processes and products of science (Mace, 1994; Stokes, 1997). Overall, in order to be widely read, cited, and supported, a basic-science area like EAB must make clear how it informs the understanding of the everyday world. "Pure basic" research, whatever its merits, can be an exercise in disciplined irrelevance when basic researchers strip their work of any obvious connection to practical problems, thereby assuring that only a few academic specialists will know and care about it.

For any basic scientist who remains unconvinced that practical problems have a place in basic research, we add the following "pure basic" consideration: In neglecting the practical implications of their research, basic scien-

tists may ignore questions of *basic* significance. As Mace (1994) has noted, practical problems often set the occasion for extending fundamental knowledge. Some of da Vinci's basic-science interests were inspired by military engineering problems with which wealthy patrons presented him (Capra, 2007), and some of Pasteur's pioneering work in microbiology had roots in problems of beet-sugar fermentation (Stokes, 1997). EAB's relative inattention to practical problems may explain why a number of writers, in exploring the implications of basic behavioral research for selected everyday problems, have concluded that the basic literature contains omissions and ambiguities *at the level of basic principles*. For instance, too little is known about the dynamics of hybrid ratio-interval reinforcement schedules (Critchfield, Haley, Sabo, Colbert, & Macropoulis, 2003), choice involving schedules with ratio properties (Stilling & Critchfield, 2010); verbal relations (Critchfield, in press); punishment (Lerman & Vorndran, 2002); social behavior (Guerin, 1994); stimulus generalization (Derenne, 2010; Stokes & Baer, 1977); and behavioral momentum (Mace, et al., 2010). Questions about external validity often bring such issues into sharp focus. In short, the parameters of a complete "pure basic" science cannot be defined independently of the practical world.

TRANSLATIONAL RESEARCH

Translational research is an essential complement to "pure basic" behavioral research because it explicitly considers the generality and everyday relevance of fundamental behavior principles. Translational research is as old as science itself (e.g., Gribben, 2002) and has gone by many names. It can be defined generally as inquiry that breaks new ground by uniting a concern for fundamental principles with a concern for everyday problems and outcomes.

A skeptic can justifiably note that, following Skinner's (e.g., 1953, 1957) example, behavior analysis has always subsumed both fundamental principles and everyday problems. Yet not all behavior analysis is translational. Translation achieves *innovation through synthesis*. "Pure basic" research can innovate without attention to everyday issues (e.g., theory building) or it may focus on fleshing out existing theoretical

frameworks, which most observers would not regard as particularly innovative. "Pure applied" work may innovate without attention to basic principles (e.g., Edison; Stokes, 1997), or it may eschew innovation and focus instead on widening the distribution of effective technologies (an important problem, as Pennypacker, 1986, and others have noted, but not relevant to the present discussion).

Hake (1982) bemoaned the fact that translational investigations may please no one: They are "too basic" for applied researchers or practitioners, and "too applied" for basic researchers. It is rarely the case, however, that an investigation falls exactly into what Hake called "the crack" (p. 23) between basic and applied. Some translation has more in common with basic research. It may employ laboratory methods and address connections to the everyday world in an abstract or provisional way. It may focus primarily on a fundamental behavior principle, while exploring an everyday problem as a convenient test of generality. Other translation is more applied than basic. It may occur in the field and employ methods appropriate to field settings, or it may focus primarily on a given disorder or intervention while invoking basic behavior principles as a way to improve practical outcomes. There is no single recipe for translational investigation, and no *a priori* reason why research in either general category (more-basic or more-applied) cannot advance understanding of both fundamental principles and everyday problems (e.g., compare Mace, Mauro, Boyajian, & Eckert, 1997, and McDowell & Caron, 2010a, 2010b).

A BRIEF HISTORY OF TRANSLATIONAL EFFORTS IN BEHAVIOR ANALYSIS

A Case Study: Human Emotion

As suggested above, translational efforts in behavior science predate the scholarly movement that now is called behavior analysis. One of the earliest translations of basic research findings to human affairs was Watson and Rayner's (1920) study on conditioned emotional reactions, which was greatly facilitated by Pavlov's seminal basic laboratory research with dogs. The study would be considered unethical by contemporary standards but it nevertheless broke new translational ground. To condition a fear response to a rat in an 8-

month old infant male, Watson and Rayner used simultaneous respondent conditioning procedures very similar to those employed by Pavlov. The researchers presented the infant with a white rat and simultaneously struck a hammer to a steel bar that was positioned behind his head; repetition of this procedure resulted in conditioned fear responses to the rat in the absence of the aversive sound. They went on to illustrate the relevance of the principle of stimulus generalization to conditioned fear. Subsequent to the conditioning procedures, fear responses were observed following the presentation of stimuli with physical similarities to the rat such as a rabbit, a dog, a fur coat, a bag of cotton balls and a Santa Claus mask; no fear responses were observed to physically dissimilar objects.

Watson and Rayner's (1920) work foreshadowed considerable basic and translational inquiry that ultimately led to effective treatments for fears and anxiety. The decades following Watson and Rayner's work saw many laboratory studies in which the parameters of conditioned emotions were specified (e.g., Estes & Skinner, 1941; Jones, 1930; Kalish, 1954) as well as important early discussions of the relevance of conditioned emotions to clinical problems (e.g., Hamilton, 1927). Much was known about the conditioning of emotions by the time Wolpe (1969) developed systematic desensitization, a therapeutic approach to the treatment of anxieties and phobias that gradually and systematically exposes clients to feared stimuli. Importantly, Wolpe's approach to therapy was first modeled in laboratory studies with cats. Wolpe (1958) first conditioned fear responses to neutral stimuli in cats and then, using a procedure he termed reciprocal inhibition, he presented food simultaneously with the conditioned feared stimuli which resulted in the abatement of fear responses over time.

Basic research on human fear conditioning that employs traditional paradigms continues to this day and behavior analytic models of human emotion continue to develop in sophistication (Friman, Hayes & Wilson, 1998) by drawing especially on research on stimulus equivalence and other derived stimulus relations (e.g., Sidman, 1994). This research provides a conceptual framework for understanding how anxiety and fear (or avoidance) of harmless situations can emerge

or be derived without direct contact with aversive stimuli (e.g., Dougher, 1998; Dymond, Roche, Forsyth, Whelan, & Rhoden, 2007). Importantly, the relevant research has been conducted almost exclusively with humans in the laboratory but with the express aim of identifying functional relations that are germane to the understanding and treatment of human emotional problems (Friman et al., 1998; Hayes, Strosahl & Wilson, 2003).

This example illustrates that translation requires sustained interplay between basic and applied empirical efforts. As we shall see, however, the track record of coordination between the basic and applied sectors of behavior analysis is rather inconsistent. Below we briefly review translational traditions within behavior analysis.

The Interpretive Approach

One approach to which we will devote little attention is narrative speculation about the everyday relevance of laboratory-derived principles. Skinner's (1953, 1957, 1971) "conceptual" writings provide the paradigm examples of this approach. Perhaps because narrative interpretations can extend a behavioral analysis to topics for which no empirical work yet is available, they have stimulated the imaginations of many behavior analysts. Yet such analyses carry substantial liabilities concerning the "rules of evidence" for evaluating their validity. One issue regards how to square such interpretations with new empirical information. For example, Sidman (1989), drawing upon a sizeable corpus of laboratory research, extrapolated the possible everyday perils of influencing others through aversive control. Sidman's analysis appears to admit few exceptions to the rule that aversive control harms both controller and the individual who is controlled. Regarding the latter case, however, a recent large-scale literature review uncovered no systematic adverse effects on children of mild corporal punishment (Gershoff, 2002), a finding that appears to be at odds with Sidman's analysis. But is it really? Unfortunately, the very breadth that makes narrative interpretations appealing also can make them difficult to empirically evaluate.

Perhaps the biggest problem with narrative interpretations, however, is that they are easily disputed *without* empirical evidence (e.g., Baron & Perone, 1982). Interpretations are

logical exercises and thus their evaluation can proceed on logical grounds alone. To cite one infamous example, using just a few brief sentences, Chomsky (1959) dismissed Skinner's (1957) most elaborate interpretation, *Verbal Behavior*, simply by rejecting its foundational premise:

Skinner's thesis is that external factors consisting of present stimulation and the history of reinforcement... are of overwhelming importance, and that the general principles revealed in laboratory studies of these phenomena provide the basis for understanding the complexities of verbal behavior.... Careful study of this book (and of the research on which it draws) reveals... that these astonishing claims are far from justified.... The insights that have been achieved in the laboratories of the reinforcement theorist... can be applied to complex human behavior in only the most gross and superficial way (pp. 27–28).

Skinner did not respond directly to Chomsky but his response to critics of other interpretative writings was to expound upon and defend the strategy of interpretation (e.g., Skinner, 1963). No wonder, then, that Baron, Perone, and Galizio (1991) concluded that the main function of narrative interpretations is to “generate more interpretations and, perhaps, to evoke a sense of self-satisfaction with the apparent scope of the explanatory principle” (p. 102). Speculative interpretation may identify one source of motivation for translation, and may help to provide a conceptual framework for translational research, but it cannot substitute for empirical translational efforts.

Translation Originating in the Basic Sector

Human replication of effects first established with nonhumans. At least three types of translational laboratory investigations have originated with basic researchers. The first involves attempts to replicate in human subjects effects that were first demonstrated in nonhumans. This is a limited but critical form of translation. To show that an effect can occur in human behavior under laboratory conditions does not assure that it is important to everyday affairs, but the failure to detect an effect in laboratory tests should raise doubts about its importance outside of the laboratory (Baron et al., 1991). Because behavior analysts make fairly strong assumptions about interspecies generality of operant principles, human replications of

effects first seen in animal behavior may be underappreciated in EAB. Yet as the preceding quote from Chomsky (1959) attests, outside of behavior analysis the default assumption may be that humans are unlike other creatures. As a launching pad for other forms of translation, therefore, it is important to empirically evaluate the extent to which human behavior mirrors that of nonhuman subjects.

The first laboratories for the experimental analysis of human behavior (EAHB) were established in the 1950s and 1960s (e.g., Hefferline & Keenan, 1961; Holland, 1958; Hutchinson & Azrin, 1961; Weiner, 1962; see also Rutherford, 2009), but in some cases did not yield a great deal of published research. For a time, only a few pioneering investigators systematically explored the generality to humans of such laboratory staples as aversive control and simple schedules of reinforcement (e.g., Baron & Kaufman, 1966; Baron, Kaufman, & Stauber, 1969; Weiner, 1962, 1964, 1965). It was not until the 1980s that a sizeable community of EAHB investigators emerged (e.g., Hyten & Reilly, 1992), which in turn produced noteworthy extensions to human behavior of animal-derived principles, including the matching relation (Kollins, Newland, & Critchfield, 1997) and delay discounting (Green & Myerson, 2004).

The relevance to human behavior of certain other behavioral phenomena remains less clear. Some benchmark effects in nonhuman behavior, such as patterning on fixed schedules of reinforcement, have not been reliably reproduced in humans (e.g., Weiner, 1964). No less challenging to a behavioral account of the natural world are instances in which laboratory-based principles simply have not been evaluated in humans. For example, recent studies have revealed interesting momentary patterns of postreinforcement preference shift under concurrent contingencies (e.g., Davison & Baum, 2000), but the effects have not, to our knowledge, been examined in humans. Similarly, the Discriminative Law of Effect (Davison & Nevin, 1999), an important theoretical model that integrates the effects of consequences and discriminative stimuli and could have applied significance (Magoon & Critchfield, 2006), is based predominantly on nonhuman research.

Laboratory models of everyday events. A second use of the basic laboratory has been to develop

laboratory models of specific human problems. Such models often diverge from traditional laboratory procedures by using participants, behaviors, or controlling variables (e.g., discriminative stimuli) of everyday relevance (e.g., Borrero et al., 2010; da Silva & Lattal, 2010; Derenne, 2010; Habib & Dixon, 2010; Lionello-Denolf, Dube, & McIlvane, 2010; Milo, Mace, & Nevin, 2010), though in some cases the procedures are indistinguishable from those of “pure basic” studies, in which case only the research question belies everyday concerns (e.g., da Silva & Lattal, 2010; Ecott & Critchfield, 2004). Perhaps the most familiar example of a laboratory model is the drug self-administration procedure that uses drug doses as reinforcers and has been employed extensively to evaluate the abuse potential of various pharmacological agents (e.g., Ator & Griffiths, 1987). Also in wide use is a laboratory model of aggression called the Point Subtraction Aggression Paradigm (e.g., Lieving, Cherek, Lane, Tcheremissine, & Nouvion, 2008).

Laboratory models have been developed to examine such diverse problems as false memory (Guinther & Dougher, 2010), gambling (Habib & Dixon, 2010), say-do correspondence (da Silva & Lattal, 2010; Lattal & Doepke, 2001), alternative reinforcement as a factor in noncontingent reinforcement interventions and resistance to extinction (Ecott & Critchfield, 2004; Mace et al., 2010), cooperation (Hake, Olivera, & Bell, 1975; Schmitt & Marwell, 1968; Yi & Rachlin, 2004) and the role of conditional stimulus relations in social stereotyping, self-disclosure, and analogical reasoning (Keenen, McGlinchey, Fairhurst, & Dillenberger, 2000; Roche, Barnes-Holmes, Barnes-Holmes, & Hayes, 2001; Stewart, Barnes-Holmes, Roche, & Smeets, 2002). Many such models, however, have been embraced by only a handful of laboratories and have not yielded a large body of published research.

Studies of “uniquely human” behaviors. A third translational use of the basic laboratory is to study behavior that is associated primarily with humans. The need for this kind of human-focused research was foreshadowed in early investigations that found human schedule-controlled behavior to be influenced by factors (such as instructions, verbal mediation, and imagined consequences) that presumably do not control nonhuman performances (Baron et al., 1969; Holland, 1958; Kaufmann, Baron,

& Kopp, 1966; Weiner, 1965, 1970). Such findings remained mainly as curiosities until the 1980s (e.g., Hyten & Reilly, 1992). During this period, Murray Sidman (e.g., 1994) was reporting major advances in his work on stimulus equivalence, which from the outset he recognized might shed light on “complex human cognition” (Sidman, 1978), and Don Hake (1982) wrote with special clarity of the need to study “uniquely human” phenomena:

Common types of social and verbal behavior [are] the most critical research areas for society, the scientific community, and Behavior Analysis... because (1) they are the most common types of human behavior, (2) they comprise much of the vast middle area between basic and applied..., and (3) these new areas may lead to innovative methods, content areas, and followers that will be necessary to sustain adequate development and expansion of Behavior Analysis. (p. 25).

During recent decades, stimulus equivalence and other complex stimulus relations have been emphasized in contemporary EAHB (Dymond & Critchfield, 2001), but Hake’s seminal vision for EAHB has been less thoroughly realized on other fronts. For instance, although a number of studies have examined both cooperation/competition (e.g., Hake et al., 1975; Schmitt & Marwell, 1968; Yi & Rachlin, 2004) and the effects of verbal stimuli (rules and instructions) on nonverbal behavior (e.g., Catania, Matthews, & Shimoff, 1982; Galizio, 1979; Weiner, 1970), overall only limited progress has been made in the experimental analysis of social and verbal behavior (e.g., Critchfield, in press; Sherburne & Buskist, 1995). In this regard, EAB may be said to remain primarily an investigation of nonhuman behavior.

Summary and critical appraisal. Scholars with interests in basic research are capable of asking translational questions and have explored several ways of answering them. For each of the strategies noted here—human replications of animal-derived effects, laboratory models of everyday events, and the investigation of “uniquely human” behaviors—noteworthy successes can be identified. In these cases, multiple investigators created a critical mass of research from which useful generalizations about the world outside the laboratory can be drawn.

Although we have not stressed the point in this section, each case of translational success

originating with basic researchers combines progress toward understanding the controlling variables of everyday behavior with progress toward understanding basic principles. Consider these brief examples. Human replications of delay discounting effects have shown interspecies generality of discounting and have inspired advances in the understanding and treatment of alcohol abuse (e.g., Vuchinich & Tucker, 2003), but human extensions also have fueled the evolution of basic quantitative models of discounting (Green & Myerson, 2004). Studies employing the laboratory self-administration model of drug-taking informs the practical decision about whether drugs should be sold over the counter or only by prescription, but this model also serves as a useful assay in which to ask fundamental questions about behavioral economics (Bickel, DeGrandpre & Higgins, 1995). Work on stimulus equivalence and other kinds of "uniquely human" stimulus relations has fueled several kinds of applied interventions (e.g., Rehfeldt, in press) but also appears to demand unique theoretical accounts (e.g., Hayes, Barnes-Holmes, & Roche, 2001; Horne & Lowe, 1996; Sidman, 1994).

The preceding, though important, should not overshadow a more central point. Occasional successes notwithstanding, the most common outcome for all three translational strategies that have originated with basic researchers has been emergence of only scattered efforts, often of limited scope and influence. Most translational initiatives of basic researchers have not yielded impressive fruit, which supports the conclusion that a "pure basic" culture in EAB is not a reliably fertile environment for translational thinking.

Translation Originating in the Applied Sector

Application of operant contingencies in early behavior modification. Throughout the 1950s, psychoanalysis was the dominant approach to psychotherapy. However, two populations of psychiatric patients were notably unresponsive to this therapeutic approach: the chronically mentally ill and those with moderate to profound mental retardation. Due to the absence of effective therapeutic approaches and the fact that these two populations lived, for the most part, in institutional settings where substantial control of the environment was possible, early translations of laboratory-

derived principles focused on these two groups (Rutherford, 2009).

Early translational clinical studies were faced with making transformations of laboratory conditions with nonhuman subjects to human clinical conditions in four main areas: (a) a change in species from one with limited genetic variation and known learning histories to one with large genetic variation, unknown neurodevelopmental insults, and long and unknown learning histories; (b) a change in the physical form of response consequences and their presentation from uniform and automated to one involving variation and mediation by humans; (c) a change from near-complete control of motivating conditions via food deprivation to limited control of extraneous reinforcers in psychiatric settings; and (d) a change from automated data collection to, in most cases, data collection by human observers with the attendant risk of measurement error. Given the magnitude of differences between laboratory and clinical conditions, the early translational studies were truly experimental in nature, making a priori predictions of success highly speculative. Clearly, success was dependent on laboratory-derived principles being robust.

Most of the initial forays outside the laboratory applied behavioral principles that were firmly established in basic research, including positive and negative reinforcement, positive and negative punishment, extinction, satiation, and stimulus control. We briefly summarize here a sample of the seminal studies that comprised the foundation for the field of behavior modification.

Modification of psychotic and various aggressive, disruptive and otherwise undesirable behaviors was accomplished through the use of a wide range of differential positive and negative reinforcement procedures used with and without extinction. Lindsley (1956) used a variety of presumed positive reinforcers (such as religious pictures, auditory feedback, and music) to change the behavior of patients with chronic schizophrenia with varied degrees of success. Allyn and Haughton (1962) sought to achieve more consistent effects using the primary reinforcer food. Many of their 45 patients with schizophrenia refused to eat without considerable coaxing and persuading from staff. Allyn and Haughton discontinued staff encouragement to eat (extinction) and

within a few days the patients began eating without assistance. Afterwards, access to the dining hall (and food) was made contingent on self-locomotion to the dining hall, thereby establishing marked improvement in independent eating.

Ayllon and Michael (1959) successfully treated various disruptive behaviors in several patients with schizophrenia using procedures based on multiple behavioral principles. After observing patients' interaction with nursing staff, Allyn and Michael concluded that the patients' undesirable behavior was maintained by nurse attention. They produced large reductions in undesirable behavior by both withholding attention for these behaviors (entering the nurses' station and lying on the floor) and withholding attention in conjunction with providing differential attention contingent on appropriate approaches to nurses on a fixed interval (FI 15 min) schedule with a limited hold. In another patient in the same study, Allyn and Michael used negative reinforcement to decrease food refusal and increase self-feeding. The adult patient was significantly underweight and had a long history of being spoon fed by nurses. The nurses were asked to deliberately spill food from the spoon resulting in a smaller quantity of food available per occasion of spoon feeding. When the patient could then avoid food spillage by reaching for and requesting the spoon and self-feeding, independent feeding was established and the patient gained 21 pounds. In another group of patients who hoarded magazines, Allyn and Michael (1959) manipulated positive reinforcers by withholding attention for hoarding and giving patients access to large quantities of magazines to approximate a satiation operation. Both procedures were successful in reducing magazine hoarding.

Ayllon and Azrin (1965) conducted what remains one of the most comprehensive demonstrations of the effectiveness of behavior modification. In set of six detailed experiments with 44 patients with schizophrenia and mental retardation, Ayllon and Azrin established tokens as a generalized reinforcer and then arranged them in contingencies according to the Premack Principle. When patients could earn tokens exchangeable for tangible backup reinforcers for working as a sales, secretarial, ward cleaning, janitorial, groom-

ing, and recreational assistants, and engaging in several self-care skills, the time engaged in these prosocial behaviors increased sharply. The investigators also demonstrated that non-contingent delivery of tokens resulted in cessation of these activities in most patients. In a separate study, Allyn and Azrin (1964) used differential positive reinforcement to establish stimulus control of staff instructions. Individuals with schizophrenia and mental retardation are commonly unresponsive to instructions. The 18 patients in the study routinely failed to pick up eating utensils when going through a meal line. Providing a piece of candy contingent on picking up utensils failed to increase the target response. However, when instructions to pick up the utensils were combined with candy reinforcement, the response was established.

Finally, early applications also used various punishment procedures to reduce undesirable behavior. For example, Flanagan, Goldiamond and Azrin (1958) had individuals with stuttering speech read written passages while wearing headphones into which a loud aversive tone could be emitted. Contingent noise reduced stuttering. Flanagan et al. also showed that when fluent reading terminated the presence of the aversive tone, fluent speech increased and stuttering decreased. Baer (1962) used a negative punishment procedure to decrease thumbsucking in a 5-year old boy. The boy was positioned in front of a television that played cartoons. When the cartoons were discontinued contingent on thumbsucking and resumed when the thumb was withdrawn from his mouth, thumbsucking was significantly reduced.

The studies reviewed here along with numerous other demonstrations that laboratory-based principles could be translated into procedures to change socially relevant human behavior laid the foundation for the revolutionary change in clinical and educational practices that was to come.

Emergence of a separate field of Applied Behavior Analysis (ABA). The proliferation of studies demonstrating that laboratory-derived behavioral principles could be translated into procedures that improve socially relevant human behavior led to the creation of a new journal devoted entirely to this work. The *Journal of Applied Behavior Analysis (JABA)* was founded in 1968 and in its first issue Baer,

Wolf and Risley (1968) undertook to define the field of ABA for future generations. Of particular relevance to the present discussion, Baer et al. interpreted the term *analysis* in ABA to mean a convincing experimental design. "An experimenter has achieved an analysis of behavior when he can exercise control over it" (p. 94). However, this definition of analysis differs from a conventional definition of the term: "a method of studying the nature of something or of determining its essential features and their relations" (entry 2, dictionary.com). This conventional definition seems consistent with the historical pursuits of EAB (e.g., the generalized matching equation).

Although impossible to ascertain definitively, we argue that Baer et al.'s (1968) definition of *analysis* contributed to the pronounced technological orientation of early ABA. Just ten years after *JABA*'s inception, concerns about an over-emphasis on technology began to emerge from several quarters. Dietz (1978) was among the first to identify the problem as ABA separating from its scientific roots. He lamented a clear shift in emphasis away from analysis of the contingencies that control behavior towards a priority of intervention outcomes. Cullen (1981) observed that "there has been a drift away from the kind of 'science' that was once the hallmark of behavior modification" which may account for "many of the applications (being) trivial or transitory in effect" (p. 81). Hayes, Rincover and Solnick (1980) and Pierce and Epling (1980) exposed the problem by examining publication trends in *JABA*. Hayes et al. (1980) rated all empirical articles in the first 10 volumes of *JABA* along several dimensions. Among their findings was a sharp deceleration in intervention component analyses from an average of 22.3% in the first four volumes to an average 5.5% in the last four volumes. Parametric analyses which are common in *JEBAB* occurred rarely in *JABA*'s first 10 years, averaging only 10.3% and 4% in the early and latter volumes, respectively. Hayes et al. also found an accelerating trend in articles that made no reference to behavioral principles but were instead simple evaluations of a technology from a low of 0% in Volumes 1, 2 and 4 to a high of 22% in Volume 9. Conversely, there was a marked decline in the percentage of articles employing systematic applications, that is, those that address

questions such as 'what is the motivation for imitation' (Hayes et al., 1980, p. 278), from a high of 43% in 1968 to a low of 4% in 1976. Finally, Pierce and Epling (1980) referred to this trend as the divorce of ABA from EAB and urged reconciliation. In their empirical review of Volume 11 of *JABA*, they found low percentages of reference to basic behavioral processes (25%), citations of EAB sources (11.4%), and basic process terms used as descriptors (24.6%). Lamentations came from still other quarters (e.g., Birnbauer, 1979; Branch & Malagodi, 1980; Hayes, 1978; Mace, 1991, 1994; Michael, 1980; Moxley, 1989; Poling, Pickert & Grossett, 1981) and were consistent with the above illustrations.

Baer defended ABA's emphasis on technology over science in his Presidential Address for the Association for Behavior Analysis (Baer, 1981). After citing two personal experiences with a skilled photographer and an effective emergency room physician, neither of whom were knowledgeable of the basic science of their craft, Baer went on to be explicit about his position. In response to Michael's (1980) concern that applied behavior analysts were not quick to apply new basic findings, Baer stated "No they are not... (for the most part) that is because they have very little need to apply the newest basic findings" (p. 88). Baer argued that the vast majority of human behavioral problems can be resolved through the skillful application of positive reinforcement. However, he recognized the difficulty in realizing this objective: "...the difficulty of implementing just positive reinforcement in real-world terms is so formidable and so variable from problem situation to problem situation, that (practitioners) have their hands full" (p. 88). A devil's advocate might respond to Baer's position by noting that a sizeable portion of the difficulty in translating a positive reinforcement operation into a positive reinforcement process in real-world situations has to do with the reinforcement contingencies and their histories that maintain undesirable behavior. That is, knowing the conditions that are necessary and sufficient to translate operation into process is key and this knowledge is derived from basic research. Therefore, analyzing the contingencies that reinforce undesirable behavior and designing interventions with consideration to the variables that affect choice between desirable and

undesirable behavior (i.e., the matching relation) may well improve the success of positive reinforcement interventions (Mace & Roberts, 1993; Mace & Shea, 1990; McDowell, 1989).

Interim summary and critique. To summarize thus far, the basic and applied sectors of behavior analysis were disconnected in the late 1970s and early 1980s (Mace, 1994). In many cases, efforts to change socially-relevant behavior involved introducing reinforcement and/or punishment contingencies to an existing environment without regard for the natural contingencies that supported undesirable behavior and that interfered with performance of prosocial behavior. Baseline data on target behaviors were collected in isolation and not in the context of environmental events that could motivate and reinforce them. Most important to the present discussion, applied efforts made little conscious connection with basic behavior science, and translational research with applied origins had come to a standstill. For many, the future development of ABA in its scientific tradition was in doubt. A “pure applied” culture in ABA did not prove to be a reliably fertile environment for translational thinking.

Trends in ABA since the mid 1980s. ABA changed abruptly in the mid-1980s with the development of procedures for identifying the contingencies that maintain undesirable behavior (Iwata, Dorsey, Slifer, Bauman, & Richman, 1984/94). Known collectively as functional analysis methodologies, these procedures shifted the focus of ABA research to determining the factors that maintain undesirable behavior and using this information to promote replacement behaviors that serve the same function (e.g., see Pelios, Morren, Tesch, & Axelrod, 1999). When applied behavior analysts began to analyze behavior in this way, research in EAB became relevant to their work. It was not long before members of *JABA*'s editorial team began encouraging explicit connections with basic research. During part of the 1990s, *JABA* reprinted abstracts of selected *JEAB* articles, and around the same time Editor Nancy Neef established a regular series of explicitly translational essays (beginning with Hinline & Wacker, 1993) that discussed the applied implications of *JEAB* basic research. Two *JABA* special issues featured the general theme of translation (Friman, Lerman, & Wacker, 2003; Mace & Wacker, 1994), and several other

special sections and special issues have had a strongly translational flavor (see <http://seab.envmed.rochester.edu/jaba/jaba-specialsections.html>).

The translational emphasis in *JABA* has grown to the point where a detailed accounting is beyond the scope of this essay (e.g., see Mace & Wacker, 1994; Wacker, 2003). For present purposes, three generalities will suffice. First, much translational work continues to reflect the general mission, as outlined in early functional analysis work, of using experimental analyses to understand the contingencies that maintain problem behavior and to identify effective reinforcers for alternative behavior. From the perspective of the basic researcher this may not sound terribly innovative, but the emphasis on mechanisms of behavior control shares more with basic research than with ABA's “pure applied” technological era. Second, contact with basic research often is explicit, as evidenced by a dramatic increase in the proportion of *JABA* articles citing basic research. Whereas Pierce and Epling (1980) reported that only about one in ten *JABA* articles of 1978 cited EAB sources, by the start of the 21st century *JABA* had joined several basic-research journals as the periodicals that most often cite *JEAB* (Critchfield & Reed, 2004). Third, as might be expected based on the preceding, more *JABA* articles now invoke basic behavioral mechanisms as a framework for understanding behavior problems and interventions. For instance, a casual survey conducted in support of this essay found reference to reinforcement schedules (including extinction and response-independent schedules) in less than 3% of *JABA* article titles during the period of 1974–1981. By the years 2001–2008, the incidence had increased to more than 20% (reflecting, in part, a trend for applied work to draw inspiration from research involving complex laboratory schedules that model the competition and transitions between simpler contingencies; for a discussion of some translational implications of this work, see Waltz & Follette, 2009).

These positive developments notwithstanding, there are reasons to be concerned about the sustainability of ABA's “translational boom.” One very general observation is that, as Hake (1982) implied long ago, there are few hybrid investigators who have equal grasp of

the basic and applied literatures. For instance, Critchfield and Reed (2004) found that only five investigators accounted for more than 40% of the translational articles appearing in *JABA* during a recent interval. Given the scope of the basic and applied literatures and the strength of professional contingencies that encourage specialization, it seems likely that the typical behavior analytic researcher will remain mostly-basic or mostly-applied. If behavior analysis is to increase its translational focus, to the benefit of both basic and applied sectors, the onus may fall upon teams or networks of investigators who collectively contribute broader expertise than is likely to exist in any single individual. Indeed, innovation often results when individuals with different skill sets interact and collaborate; one advantage of collaboration is that no individual must be a translational jack of all trades (e.g., Lamb, Greenlick, & McCarty, 1988; National Institute of Mental Health, 2000).

Limited progress in this regard can be reported. For instance, many of the essays in *JABA*'s series on applied implications of *JEAB* research featured a basic-applied author team (see <http://seab.envmed.rochester.edu/jaba/jaba-implications.html>). As we noted in an earlier section, however, narrative efforts cannot substitute for translational research. Several recent *JABA* empirical reports have included a basic-research coauthor or employed laboratory procedures to address applied questions (e.g., Berens & Hayes, 2007; Ecott & Critchfield, 2004; Lattal & Doepke, 2001; Mace, Lalli, Shea, & Nevin, 1992; Mace et al., 1997). Unfortunately, such empirical efforts remain, at best, occasional, and nearly always unidirectional, with applied researchers recruiting basic-research expertise as a means of promoting better applied technologies.

THE FUTURE OF BEHAVIOR ANALYTIC TRANSLATION: THE IMPORTANCE OF COORDINATION

In our view, behavior analysis is most likely to prosper when the EAB and ABA communities are united, not just through occasional collaboration, but through *coordination* of research agendas to address questions of importance to society. By coordination we mean focusing the efforts of investigators from both sectors on problems of shared interest

and obvious relevance to society. A coordinated approach is more likely than incidental collaboration to guide basic research in elucidating the behavioral processes that are of greatest social import and, in turn, stimulate novel and effective behavioral technologies in directions that require a fuller understanding of basic behavioral processes. This proposition is inspired by other fields that have harnessed the benefits of bidirectional translational research, most notably the biomedical sciences, as the following example illustrates.

A Problem of Great Social Importance Requiring a Basic Science Solution

Acquired Immunodeficiency Syndrome (AIDS) was first identified by clinicians in the United States in 1981 and its cause isolated as the Human Immunodeficiency Virus-1 (HIV-1) in 1983 (Barre-Sinoussi et al., 1983). The virulent and lethal nature of AIDS mobilized basic and clinical sectors of the biomedical community to achieve an understanding of HIV-1 in terms of its modes of transmission, and its cellular targets, genetic constitution, and mechanisms of replication, mutation, and detection evasion in latent HIV reservoirs. What was achieved in just 25 years, in terms of the magnitude of research activity and advanced understanding of the basic nature of HIV, was unprecedented (see Gallo, 2006, and Esté & Cihlar, 2010, for a recapitulation of this history). As Gallo put it, "We have reached the end of the first 25 years of AIDS, and we can safely say that we know as much about HIV as we do of any pathogen and about AIDS as we do any human disease" (unpaginated e-journal). Relevant advances were both basic and applied. The foundational knowledge of HIV/AIDS that was derived from basic research was essential to the development of antiretroviral therapies that changed the clinical course of the disease from being an "inherently untreatable infectious agent to one being eminently susceptible to a range of approved therapies" (Broder, 2010, p. 1). As Broder also noted, antiretroviral therapies were rapidly translated from the laboratory to the clinic, and then back to the laboratory as the therapeutic limits of various antiviral drugs were identified. Active and ongoing reciprocity of information led to an unparalleled pace in modern drug development.

This example illustrates the impact that coordinated, bidirectional basic and clinical research, targeting an acute human problem, can have on both research sectors. By clarifying both the everyday relevance of basic research and the scientific foundations of application, coordinated research has the potential to alter society's investment of material resources in both. In the first 10 years of the AIDS epidemic, NIH funding rose from \$3.4 million (0.1% of NIH expenditures) in 1982 to \$804.6 million (9.7% of NIH expenditures) in 1991 (Committee to Study the AIDS Research Program at the National Institutes of Health, 1991). By 2009, nine NIH Co-Funding and Participating Institutes and Centers funded 18 Centers for AIDS Research at leading research universities nationwide.

Coordination in Behavior Science

There are, of course, no exact parallels to HIV/AIDS in behavioral science, although many acute human problems are fundamentally behavioral in nature. Moreover, precedents exist for tying behavioral research to pressing societal problems such as developmental disabilities, aging (Derenne & Baron, 2002), adaptation to space flight (Kelly, Heinz, Zarcone, Wurster, & Brady, 2005), and substance abuse (Ator & Griffiths, 1987). Only in the areas of developmental disabilities and substance abuse, however, have empirical efforts been sustained and somewhat coordinated; not coincidentally, these are rare domains in which behavioral research, both basic and applied, has achieved a measure of contemporary mainstream acceptance. These two examples, therefore, suggest that it is possible to emulate the successful practices of other fields in order to obtain material support for basic behavioral research and develop behavioral technologies that produce durable and generalized behavior change. We take up these two challenges below.

Support for and contributions of mission-driven basic research. Much of the progress in HIV/AIDS research has depended on research that focused on illuminating fundamental natural processes and employed method and theory that are typical of basic science; the choice of research question, of course, was heavily influenced by societal needs. This type of inquiry, which Stokes (1997) termed *mission-*

driven basic research, is common in other disciplines but to date has been rare in behavior analysis. Importantly, as the case of HIV/AIDS research shows, the practice of framing basic research within practical questions neither trivializes basic research nor makes it "less basic." It does, however, place basic research into a context that can be understood by educated nonspecialists, including those who set research funding priorities. It allows basic research to confront issues that might not otherwise become highlighted in "pure basic" research programs. For example, in the early 1980's, retrovirology was a small subset of the basic-science microbiology specialty of virology. Only one retrovirus was known to be the cause of human disease, the Human T-Cell Leukemia-1 Virus (HTLV-1) (Coffin, Hughes & Varmus, 1997). Yet discovery of HTLV-1 in 1981 shaped early consideration that a retrovirus could be the cause of AIDS and directly stimulated the research that led to the discovery of HIV as the cause as well as considerable research on the general properties of retroviruses (Gallo, 2006). We hold that basic behavior science can contribute to the translational agenda of behavior analysis, and thereby to its own status in society, by more often framing basic research questions in terms of the societal problems that the answers can help to address.

Promoting durable and generalized behavior change. Although it may be premature to call for a behavioral Manhattan Project to eradicate such problems as poverty, crime, and child abuse, there are problems of considerable significance that behavior analysts could tackle that would benefit from coordinated, bidirectional research. For example, ABA's successes of the past four decades include only limited progress toward achieving long-term maintenance and generalization of treatment gains (Nevin & Wacker, in press; Osnes & Lieblein, 2003). Applied behavior analysts observe that maintenance failures are often encountered when there are lapses in treatment integrity, that is, when the motivational conditions, stimulus control procedure, and schedules of reinforcement are not implemented as prescribed (Stokes & Osnes, 1989). Generalization failure can occur when a client is exposed to novel people or settings that have not been correlated with treatment, and also when a client is exposed to people and settings

that were previously correlated with reinforcement of problem behavior (Stokes & Baer, 1977; Stokes & Osnes, 1989). These failures are known collectively as *treatment relapse*.

Behavioral momentum theory (Nevin, 1992; Nevin & Grace, 2000) suggests that treatment relapse can be understood as a failure to simultaneously weaken problem behavior's resistance to change while strengthening the persistence of prosocial behavior (Nevin & Wacker, in press). Guided by behavioral momentum theory, basic researchers have been developing models of alcohol and drug relapse that may have broad application to treatment relapse in general (e.g., Podlesnik, Jimenez-Gomez, & Shahan, 2006; Podlesnik & Shahan, in press; Shaham, Shalev, Lu, de Wit & Stewart, 2003; Shahan & Burke, 2004). For example, Podlesnik and Shahan (in press) examined three models of relapse (reinstatement, resurgence, and renewal) following extinction of a target response in homing pigeons. A multiple schedule of reinforcement (VI 120-s—Lean, VI 120-s VT 20-s—Rich) comprised the baseline condition for all three models. Replicating other behavioral momentum research, responding during extinction was more resistant to change in the Rich component with the added time-contingent reinforcers in all three experiments.

The *reinstatement* model simulates the clinical situation in which response-independent reinforcers are introduced following extinction of problem behavior (Podlesnik & Shahan, in press). During the reinstatement experiment, two response-independent food presentations occurred early in the first presentation of each component during four sessions. Following reinstatement of noncontingent food, responding resumed in both components but was greater in the component correlated with Rich baseline reinforcement. The *resurgence* model is analogous to reinforcing an alternative prosocial behavior following extinction of a problem response. In this experiment, response-dependent reinforcement was introduced for a separate (right) key following extinction. During the resurgence phase, responding resumed on the center baseline key, despite ongoing extinction, but was greater in the component with Rich baseline reinforcement. Finally, the *renewal* model represents reintroducing a client to the context correlated with baseline

reinforcement of problem behavior after problem behavior has been extinguished in a different setting and extinction procedures remain in effect. This experiment presented a steady houselight during baseline and a flickering houselight during extinction. Following extinction, the steady houselight was reintroduced with ongoing extinction. Responding resumed in both components when the steady houselight was re-presented but, as with the other models, responding was greater in the component correlated with the Rich baseline stimulus.

Direct translation of these laboratory models of treatment relapse to clinical situations is the next step in the process. Preliminary evidence from a few clinical studies suggests that the models may have external validity. Ahearn, Clark, Gardenier, Chung and Dube (2003) found that stereotypic behavior in children with autism was more resistant to disruption (via access to a reinforcer that competed with stereotypy) following exposure to added VT reinforcers, a finding consistent with the reinstatement model. Also consistent with reinstatement, DeLeon, Williams, Gregory, and Hagopian (2005) introduced time-contingent reinforcers following extinction of problem behavior and found problem behavior resumed despite ongoing extinction. Finally, as predicted by the resurgence model, Mace et al. (2010) reported greater resistance to extinction following high-rate reinforcement of prosocial behavior compared to lower-rate baseline reinforcement of problem behavior.

Our thesis throughout this essay has been that parallel basic and applied investigations such as those described above may yield better outcomes through coordination of basic, translational and clinical studies. The practical reality, however, is that such coordination will likely depend on obtaining interinstitutional support.

Translational Priorities at the National Institutes of Health (NIH)

NIH is a principal source of funding for behavioral research through 19 of its institutes, centers and offices, primarily through the National Institute of Mental Health (NIMH), the National Institute of Child Health and Human Development (NICHD), the National Institute on Alcohol Abuse and Alcoholism (NIAAA), the National Institute on Drug

Abuse (NIDA), the National Institute on Aging (NIA), and the National Institute of Neurological Disorders and Stroke (NINDS). Reflecting the importance of behavioral research to NIH, the Acting Director of the NIH Office of Behavioral and Social Sciences wrote, "Behavioral research is an integral part of the NIH Mission: NIH is the steward of medical and behavioral research for the nation. Its mission is science in pursuit of fundamental knowledge about the nature and behavior of living systems and the application of that knowledge to extend healthy life and reduce the burdens of illness and disability." (American Psychological Association, May, 2009).

NIH's current commitment to behavioral research has been influenced considerably by its relatively new priority: Translational research. Much of NIH's research activity is coordinated through the National Center for Research Resources (NCRR). The NCRR strategic plan for 2009–2013 gives translational research top priority, including animal models that advance translational research. Although NCRR funds individual research projects and training at various levels, its largest enterprise is the Clinical and Translational Science Awards. These awards range from approximately \$10 million to \$35 million over 5 years and support a wide range of translational research activity in the biomedical sciences. Known as the "bench to bedside" initiative, the goal of these awards is to efficiently transform basic research focused on clinical conditions into innovative clinical remedies. Since 2006, NCRR has awarded Clinical and Translational Science Awards to 43 institutions. Of importance to the present discussion, many of these awards support a behavioral research core. In 2009, the University of Arkansas for Medical Sciences established a Center for Clinical and Translational Research supported by an NCRR award. Warren Bickel is the director of its behavioral research core, a behavior analyst with an extraordinary record of translational research. The grant provides substantial funding for Bickel's Center for Addiction Research and its eight investigators. Further funding of translational behavioral research comes from other divisions of NIH. For example, the NIA currently has a Request For Applications entitled, "Science of Behavior Change: Finding Mechanisms of Change in

the Laboratory and the Field" (National Institutes of Health, 2010). As stated in the announcement, "This initiative seeks to establish the groundwork for a unified science of behavior change, capitalizing on emerging basic science to accelerate investigation of common mechanisms that play a role in initiating or maintaining behavior change and are applicable across a broad range of health-related behaviors." The announcement specifically cites behavioral economics as a promising approach to this research.

Our point in referencing NIH and its translational research priority is to suggest that translational behavioral research that coordinates EAB and ABA studies may find substantial support from biomedical funding sources. Given the scant financial support for EAB faculty positions and nonhuman operant laboratory facilities, the opportunities that translational behavioral research affords should not be overlooked.

CONCLUSION

"Pure basic" science can become detached from the natural world that it is supposed to explain. "Pure applied" work can become detached from fundamental processes that shape the world it is supposed to improve. Neither scholars nor society as a whole should be enthusiastic about such limited endeavors. Translational research, by contrast, has the advantage of being inherently integrative. The "more basic" variety seeks to understand and test the generality of fundamental principles, with specific everyday problems as an explicit frame of reference. The "more applied" variety seeks to solve specific everyday problems, with specific fundamental principles as an explicit frame of reference. Detachment is impossible in translational research.

Within behavior analysis, the challenge concerns who will become engaged in translational research. In recent years, the applied sector has taken the lead, but behavior analysis has a long way to go before the generality of its basic research and the scientific foundations of its applied work go unquestioned by the educated nonspecialists who set funding priorities for granting agencies, universities, and public-policy initiatives. Without societal support, one can imagine a future in which basic behavioral research is conducted in only a few

laboratories, and applied research and practice address only a few social problems that are perceived as too difficult or unprofitable to attract competition from non-behavior analysts.

Disciplines like biomedical research show the way to an alternative future in which the coordination of basic and applied efforts compromises neither and benefits both. During the past two decades or so, the applied sector of behavior analysis has taken an important first step toward a coordinated future. The next step, as we hope the *JEAB* Special Issue on Translational Research foreshadows, is for the community of basic researchers to reclaim the relevance to society that Skinner so forcefully defended by directing its attention toward the fundamental problems of greatest societal importance.

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