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Some Things We Learned from Sidman and Some We Did Not (We Think)

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To begin, we want to place our title in context. Many people that we have met throughout our careers assumed that we were Murray's graduate students – because (we think) the first author was a student in his research group in the 1970s, both of us received our graduate degrees from Northeastern University, and we have pursued some of the same research interests that interested him. In reality, that is an honor that we cannot claim. We were not his students in the strictest sense of that relationship.

Our advisor was Larry Stoddard. Our research with Larry had a somewhat different focus than the main thrust of Murray's work. We focused mainly on stimulus control processes involved in functional communication of persons with little or no functional language. We published many papers with Larry as professional colleagues, but we have no joint authorships with Murray. Moreover, when we were junior researchers, Murray was burdened with administrative jobs and split work sites (Northeastern University in Boston and the Shriver Center in Waltham). We saw him only rarely – mostly in those occasional research meetings that he could attend. After he retired, we saw him only a little more. Thus, we cannot even claim a *de facto* post-graduate student relationships (much as we would have liked such an arrangement if circumstances had permitted).

Despite this history, we do claim Murray as our teacher – because that is a fact. In the new APA *Handbook of Behavior Analysis*, the first author (2012) wrote this dedication:

“This chapter is dedicated to Murray Sidman, who taught us **how to think scientifically** [emphasis added here] through his writing and our too-infrequent personal contacts over many years... I am not certain that Murray would agree with everything I have written here, but I am fairly certain that he would agree that analyses such as these may help move our field forward.”

The purpose of that chapter was to amplify on Murray's (1986a) chapter in which he first talked about the 2-, 3-, 4-, 5-, and n-term behavior analytic units. In that outlet, it was possible to explore that rich subject in a manner not usually available in a typical chapter or journal article. We fully expect some psychologists to describe the chapter as “ponderous” or “impenetrable.” If so, they should blame Murray in part for that quality; he taught everyone in his sphere of influence to ponder and truly penetrate problems – even when a superficial analysis would be sufficient to meet current academic contingencies. Murray always demanded more of himself and led the rest of us by example. As the dedication implies, Murray taught us *how to think*, but not *what to think*.

We claim that we learned some things but not other things from Murray with some trepidation, hence the “we think” disclaimer in our title. Since he taught us how to think and not what to think, our thinking may sometimes differ from his as a result of considering and/or weighing evidence differently. Also, Murray likely taught us things that we have not yet

realized that he taught us. Finally, we probably have missed or forgotten aspects of his writings that bear on the problems that we are going to consider here. He cast a wide net in his writing, and we are not sure that the net always caught our attention and/or understanding.

Research Translation

These days, a survey of professional journals in the biomedical or behavioral sciences would often encounter a term that was unheard of twenty years ago – *translational research*. When a new term is introduced and broadly affects fields of inquiry, it is natural to ask what it means. Here is one definition from an NIH source:

“Translational research includes two areas ... (1) One is the process of applying discoveries generated during research in the laboratory... to the development of trials and studies in humans. (2) The second ... concerns research aimed at enhancing the adoption of best practices in the community. Cost-effectiveness of prevention and treatment strategies is also an important part of translational science.”

We attribute much of the success that we have had professionally to the fact that Sidman, Stoddard, and their immediate colleagues were prime movers in creating the field of translational behavior science research in the neurodevelopmental disabilities. Before and after their work, there was certainly basic or quasi-basic research as well as applied research in that area. Much research as the Sidman group did it, however, was of the first type mentioned in the NIH definition.

Perhaps the best example concerned their development of a nonverbal stimulus control shaping program to teach people with little or no understanding of spoken language to discriminate circles from ellipses (Sidman & Stoddard, 1966, 1967; described below). One purpose was to provide a means for assessing visual perception in individuals who could not follow verbal instructions. Another purpose (then implicit) was to model teaching and assessment procedures that could go beyond the limits of then-extant (and much current) medical and educational practice (see Sidman, 1986b for a follow-up treatment of behavioral methods for assessing behavioral development).

In the late 1950s and early 1960s, researchers in other branches of the behavioral sciences had begun to use simple and conditional discrimination procedures to study learning and remembering processes of persons with intellectual disabilities (Ellis, Girardeau, & Pryer, 1962; Zeaman & House, 1963). Such research identified atypical attending and remembering processes that seemed to account for differences between people with intellectual disabilities and their normally capable peers. While Sidman surely knew about and might have been influenced by that work, the greater influence was the *errorless learning* research of Terrace (1963a,b) with pigeons. Although that work was done largely to address then-current theoretical issues in learning, it also served as a laboratory model illustrating the potential utility of stimulus control shaping procedures to promote rapid discrimination learning without the use of verbal instructions. Sidman and his colleagues then translated and extended those procedures to show potential for meaningful clinical and educational applications (e.g., Mackay & Sidman, 1968; Sidman, 1970) – clearly in line with the first area of research translation mentioned in the NIH definition.

Initially, Sidman’s group was not much concerned with the second aspect of the NIH definition – dissemination to broad audiences in the community, best practices, and cost-effectiveness. There were certainly efforts made and helpful hints in their work, but their successors struggled (and continue to struggle) with these problems. Only recently have we

begun to see how research in the Sidman tradition might ultimately align itself broadly with the second part of the NIH definition.

Briefly, we have long felt that translation of Sidman-style behavior analyses has been hampered by lack of commitment to and support for developing methodologies that are designed explicitly for implementation with broad audiences of potential consumers. Most university environments that we connect with still tend to encourage pursuit of traditional academic objectives (e.g., preparing students for academic positions, publishing in professional journals, securing grant support for basic or quasi-basic research, illustrating applications of scientific principles on a small scale without efforts to scale up to large populations, etc.). There have been exceptions such as the *Headsprout* initiative to improve outcomes of early primary grade reading instruction (e.g., Layng, Twyman, & Stikeleather, 2004), but these are rare.

Sidman did not teach us how to take the next logical step in the program that he initiated – translating our work in a manner consistent with the broad applications described in the second part of the NIH definition. However, he did show us a programmed instruction approach for reaching a broad audience of more capable learners – the Sidman and Sidman [1965] *Neuroanatomy* text (subsequently updated to take advantage of newer technological support [Gould & Brueckner, 2007]). Regrettably, this type of instruction also remains the exception. Although there have been efforts at wider-scale programmed educational applications of stimulus control procedures that he pioneered (e.g., de Souza et al., 2009), that approach also has yet to reach large-scale implementation. The first author's group at the Shriver Center also has been trying to follow Sidman's lead. They have a certain amount of progress to show for it, but they are struggling with realities like those that have impeded research translation in other branches of clinical and educational science (cf. Lenfant, 2003).

Stimulus Control

In this short paper, we cannot cover all we learned about stimulus control from Sidman. The recent re-publication of his *Remarks* series in this journal summarized many key points, but there were many more. Among the most important concepts, we learned to think of stimulus control relations as quantal entities that were obscured by “learning curves” that were/are commonly presented as data (see Sidman, 1977a for a discussion of the relevant issues).

In quantal analyses (cf. Bickel & Etzel, 1985), stimulus control outcomes at levels less than those desired by the experimenter/teacher indicate not *weak* control but rather (1) *different* control than was intended or (2) desired stimulus control relations mixed with undesired ones. In this conception, learning of stimulus control relations need not require the gradual strengthening suggested by learning curves but can occur virtually instantaneously after laying a foundation of prerequisite stimulus control relations. It provides a straightforward way to analyze phenomena such as one-trial learning, learning set, and errorless learning.

Although he taught us much about analyzing stimulus control (e.g., through stimulus variation to identifying controlling relations), there remains a need for a comprehensive account of how new stimulus control relations emerge from their prerequisites. Long ago, Ray and Sidman (1970, p. 199) wrote the following:

“All stimuli are complex in that they have more than one element, or aspect, to which a subject might attend [and thus] ... we may never have a generalizable formula for forcing subjects to discriminate a specific stimulus aspect. We may have to settle, instead, for a combination of techniques, each of which is known to encourage stimulus control.”

For most of our careers, we and colleagues at Shriver have been pursuing research aimed at developing and implementing a combination of techniques that might lead to a “true technology of stimulus control development.” We have made progress – perhaps more than is generally known – but we still have much to do before we can claim that we have such a technology. However, we are starting to think that we might achieve a first approximation of a generalizable formula for encouraging students to attend to the stimuli that we define as relevant.

Our recent approach has been to develop computer algorithms that (1) essentially query the participant about the nature of the stimuli to which s/he attends and (2) make alterations in the teaching sequence to encourage the development of the stimulus control relations that we are trying to establish. This approach is new more in degree than in basic concept. Well-programmed instructional sequences have always had an objective similar to ours, but developers evolved practices over decades that were more a concession to the limits of technology than a reflection of limits in understanding what could/should be done to promote learning. We will illustrate this point using the circle-ellipse program mentioned earlier.

Summarizing the program: Starting in the upper-left panel of Figure 1, the participant was to touch a lit key containing a circle (S+) and not dark keys (S–). On subsequent trials of the program, the initially large stimulus difference was gradually made less distinct (by gradually increasing illumination of the S– keys), and ultimately was eliminated (upper-right panel of Figure 1). At this point, the participant had to select a lit key with a circle (S+) and not lit keys lacking one (S–). Thereafter, initially faint ellipses appeared on the S– keys, gradually becoming more distinct over trials (“fading in”) until both the circle (S+) and the ellipses (S–) were equally distinct, thus requiring a circle vs. ellipse discrimination. In the final stages of the program (lower row of panels in Figure 1), the ratio of the major and minor axes of the ellipses was changed gradually such that they became more and more like a circle – allowing the program to evaluate how fine a circle vs. ellipse difference could be resolved by the participant.

Sidman and his colleagues intended their circle-ellipse program to find application to a broad class of participants who might require nonverbal teaching/assessment procedures to reveal their true perceptual abilities (e.g., individuals with severe intellectual disabilities, neurological patients, preschool children, nonhumans, etc.). To broaden the reach of their program, they developed numerous variations and potential improvements that were applied with an ever-expanding cadre of participants (cf. Sidman & Stoddard, 1966). Ultimately, they arrived at a circle-ellipse program that did have fairly broad success and a low failure rate.

Did the circle-ellipse program inspire whole classes of comparably effective nonverbal teaching and assessment programs? Regrettably, it did not. Its main effect, we think, was as an illustration of what *could* be accomplished given sufficient time, focus, and resources. Moreover, this program and others in the same vein (e.g., Sidman, 1977b) did serve as models for others to emulate when designing instructional methods for special education applications. That said, Sidman’s careful, evidence based approach was apparently deemed impractical for routine classroom applications by teachers who had (1) very limited time for and expertise in materials development, (2) many different student needs to serve, and (3) few resources beyond the minimum necessary for their jobs. Under the *parsimony* principle of Etzel and LeBlanc (1979), teachers were encouraged to use simple methods first (e.g., trial-and-error, “extra-stimulus” (or “noncriterion-related”) prompting procedures [Schreibman, 1975], etc.) and resort to advanced stimulus control shaping methods only after the simple methods had failed. That principle, however, seemed to de-emphasize the

demonstrated fact that error histories from failed methods may capture undesirable forms of stimulus control that can interfere with subsequent learning (e.g., Stoddard & Sidman, 1967).

We think that failure to translate findings of stimulus control research by Sidman and others doing similar work has been due mainly to a mismatch in timing. Most of the foundational behavior analytic research was done between 1960 and 1990, whereas the engineering and information technology necessary to support research translation has become truly accessible only in the past decade or so. For example, early programs deriving from the circle-ellipse program were time-, labor-, and cost-intensive due to the use of hand-prepared photographic, tabletop, and early computer-generated materials (e.g., Dube & McIlvane, 1989). Today's computer graphics are much more powerful, easier to manipulate, and supported by software applications that greatly reduce the burden of stimulus preparation. Cumbersome desktop computers have been largely supplanted by laptops, tablets, and handheld devices. Increases in computer speed and memory capacity in today's computing equipment (and further increases to come) render real-time analyses of learner performance in relation to behavioral history achievable, even trivial, from a computing perspective – thus setting the stage for highly individualized program branching and greater pedagogical efficiency and effectiveness.

If ever-expanding capability is combined with current and probable future development of stimulus control technology, then translation and broad application of methods pioneered by Sidman will become not only achievable but perhaps inevitable. Indeed, we may come to the point where we must recast the parsimony principle: Why use error-prone, often inefficient methods such as trial-and-error or non-criterion-related prompting when computer-managed or computer-based teaching algorithms with high efficiency and effectiveness are available?

As one example from our recent research, we have been developing computer-managed procedures for translating the “learning by exclusion” (LBE) approach developed at the Shriver Center (e.g., McIlvane & Stoddard, 1981; McIlvane, Kledaras, Lowry, & Stoddard, 1992) to overcome frequently encountered learning challenges in picture-aided augmentative/alternative communication systems such as the Picture Exchange Communication System (PECS, Frost & Bondy, 2002). In the PECS system, children exchange visual symbols to request or label items and activities. Phase 1 of PECS training is to use symbols to request desired items. Children are taught initially to locate the picture of a desired item and give that picture to a communication partner in order to receive the item. When the child learns to do this independently, s/he is taught to be persistent in his/her requests, and to locate a communication partner even if one is not immediately available (Phase 2). In both phases, the child operates with a single picture, and s/he need not discriminate any of the features that distinguish one picture from another. Picture discrimination training is undertaken in Phase 3 in which learners are required for the first time to attend to and to discriminate the defining physical features of individual picture symbols in order to differentiate their different meanings. The first instances of conditional discrimination represent a pivotal skill that is necessary to move picture/symbolbased AAC forward.

Standard PECS methodology is simple differential reinforcement (i.e., trial-and-error) with error correction (thus following the parsimony principle of Etzel and LeBlanc). Consideration of “errorless” teaching techniques is limited to one page (Frost & Bondy, 2002, *p.* 134), and the techniques considered are very elementary (e.g., incorrect choice size or position fading). We suspect that Frost and Bondy were concerned that more advanced, often more complex methodologies might be difficult to communicate to practitioners and/or present other logistical challenges in implementation.

LBE is one such method. Carr and Felce (2008) showed that it is substantially more effective than the error-correction procedures of the PECS curriculum. Tables 1 and 2 show their most salient findings. Table 1 reproduces Table 4 from their report with some minor changes to make the nomenclature consistent with that used in this article. It shows that the LBE methodology is superior to the standard methodology both in (1) enhancing overall accuracy on both LBE training and learning outcome trials and (2) reducing inter-participant variability.

Table 2 is our re-analysis of their data to show that the difference in procedure efficacy was actually greater than their Table 4 suggests. Because they used a two-choice task, their “chance” performance could range from about 25%–75% correct (cf. Sidman, 1987). Thus, the ~73% mean correct performances and the broad score ranges with the PECS standard error-correction methodology suggests little or no control by the teacher-specified stimuli for many children. In trying to make an educated guess about the actual degree of teaching effectiveness of LBE vs. the standard method for the groups, we think that a better metric is to (1) set the midpoint of the 25%–75% “chance” range (50%) as a strong indicator of no stimulus control by teacher-specified stimuli and (2) calculate the percentage increase across the range beginning from 50% and ending at 100%. For example, the standard method 72.8% mean raw accuracy score yields only a 45.6% stimulus control estimate for the group (i.e., $72.8 - 50 = 22.8$, $22.8/50 = 45.6$). We calculated the ranges similarly as deviations (positive or negative) from the 50% chance-range midpoint, and our recalculations show that the LE method was associated with even less variability than was suggested in Table 1. Notably, we recently replicated one arm of the Carr and Felce (2008) study, showing that a computer-managed LBE algorithm could potentially enhance learning outcomes and reduce variability even further.

Data of the type presented here buttress a point we made earlier concerning the parsimony principle. If an LBE algorithm (1) can be implemented via a computer-managed protocol that is easily followed even by nonprofessionals, (2) minimizes errors, and (3) is more effective/efficient than “simple” differential reinforcement/correction procedures, then what is the justification for preferring the latter? We argue that parsimony demands the use of the most efficacious *readily deliverable* technology – not merely the technology that seems most straightforward procedurally or easy to understand with only superficial analysis.

Stimulus Control Shaping

If what we have just argued rings true, then the implications are clear: We and our colleagues with similar interests should (1) accelerate efforts to develop effective, readily deliverable methods of stimulus control shaping and (2) re-commit ourselves to the challenge of understanding more fully the variables that determine shaping successes and failures. Concerning the latter, we think it useful here to point out that there is still much to be done in understanding the behavioral processes that operate in successful and unsuccessful stimulus control (and response) shaping.

The circle-ellipse program has a series of discrete steps aimed at transferring initial control by a lighted (S+) vs. dark keys (S–) to a circle S+ vs. no form S– to a circle S+ vs. S– flat ellipses to, ultimately, a circle S+ vs. S– ellipses that were hard to distinguish from circles. We learned that when errors occurred in teaching sequences like this, the general solution was to make stimulus changes smaller. That solution worked often, but not always. The “whys” of program failures seemed of little importance in the context of the many successes of the approach. In the general case, we think the great level of success enjoyed in teaching many people who were historically considered unteachable has led us to underemphasize development of solutions to the exceptionally difficult challenges in teaching some

individuals – analogous to the challenge of developing “orphan” drugs for rare disorders. What follows are a few things that we have learned that we do not think we learned from Sidman.

Smaller program step sizes may have unintended negative consequences

One longstanding goal of our program has been to develop effective, efficient methodology for teaching the first instances of arbitrary (sometimes called symbolic) matching to sample in participants with meager-to-nonexistent verbal repertoires. One approach was first reported by Zygmont, Lazar, Dube, and McIlvane (1992). They taught children with and without intellectual disabilities to match nonrepresentational forms to one another using a procedure they called sample stimulus control shaping. Participants began with an identity-matching task. Over trials, samples were gradually altered in form until they no longer resembled their formerly identical comparisons, and the task became arbitrary matching to sample.

The sample stimulus control shaping method has been used subsequently to establish such baselines with both nonverbal humans (e.g., Carr, Wilkinson, Blackman, & McIlvane, 2000) and *Cebus apella* (e.g., Brino et al., 2011). In both illustrative studies, however, the programs broke down (i.e., led to incorrect selections) at points along the way. In the general case, the prescription to make programmed stimulus changes more gradual has been at best intermittently successful in applications such as these, perhaps because it fails to address unmanaged behavioral processes that caused the breakdowns.

As we have studied relevant stimulus control management problems over the years, we have come to hypothesize that procedures that try to transform identity matching into arbitrary matching can fail when they shape restricted stimulus control (or *stimulus overselectivity*; Lovaas & Schreibman, 1971) by unchanged features of the sample stimuli across shaping trials. In Carr and colleagues' (2000) study, for example, one child (DJB) required 80 shaping sessions to master a 3-sample:3-comparison visual-visual arbitrary matching baseline. This lengthy training regimen was needed mainly because the child showed protracted failure at the end point of shaping when the difference between program steps supporting accurate and inaccurate matching was only a few pixels that differentiated contrasting forms comprised of many hundreds.

In subsequent studies, we investigated varying shaping steps not only in size but also in quality. These procedures varied stimulus features across trials within program steps with the goal of eliminating the possibility of restricted stimulus control by specific features (e.g., Serna, 2004). That approach appears helpful but does not entirely eliminate program breakdowns. Our current research is using a *dynamic* stimulus control shaping method implemented by computer in which qualitative stimulus changes occur *within* rather than *across* trials; this allows not only substantial stimulus variation but also observation of stimulus changes as they happen (i.e., by apparent motion) (Gomes et al., 2011).

Reinforcement variables may interact with stimulus control variables in unforeseen ways

A more recent development in our program on discrimination learning has concerned a subset of participants who pass conventional reinforcer function tests (e.g., McIlvane, Dube, & Callahan, 1996) but nevertheless show substantial (sometimes almost unbelievable) indifference to schedules of reinforcement that are assumed to be effective in supporting learning during typical stimulus control shaping (e.g., Dube & McIlvane, 2006). Our research was inspired in part by the voluminous behavior analytic research on choice processes (e.g., Herrnstein, 1970). According to the matching law, behavior to concurrently available alternatives is distributed in the same proportion as reinforcements are distributed

among the alternatives. The law has been investigated most widely with concurrent schedules in which two responses with independent schedules are constantly available. It is widely replicated and applicable to human and nonhuman populations.

When conforming behavior is plotted in the standard way, it looks like that shown in the upper portion of Figure 2 – approximating a 45-degree slope on a logarithmic plot and showing that Participant MRM (and numerous others we have studied) allocated his behavior in conformance with 3:1 and 5:1 disparities in the concurrent schedules. By contrast, our Participant JST (and numerous others) did not show conforming behavioral allocation. Very shallow slopes indicate virtual indifference in allocation of behavior to the schedules.

Such findings have relevance for analysis our discrimination learning findings and to methods like them in ABA therapy: Virtually all procedures for establishing simple and conditional discrimination embed within them concurrent schedules (typically continuous or intermittent reinforcement vs. extinction). In order to acquire and/or maintain discrimination, the individual must allocate behavior in relation to those schedules. For example, the circle-ellipse program provides continuous reinforcement (CRF) for responding to stimulus aspects that define the circle and variable ratio 8 (VR8) for responding to irrelevant features such as position. If the program does not succeed in effectively directing attending to relevant stimulus differences *and* the participant is indifferent to the CRF-VR8 consequence difference, then program failure should be anticipated (i.e., the error consequences would not be effective).

Because we suspected that some stimulus control shaping failures occurred because some participants were minimally sensitive to concurrent schedule disparities, we reasoned that our reinforcement methodology might need augmentation via a training procedure to increase sensitivity to schedule disparities. Data in Figure 3 represent a great deal more that was collected by the Shriver group. One training method used *concurrent fixed vs. progressive ratio schedules*. The former followed a fixed number of responses with reinforcing consequences. The latter increased the required number of responses as sessions progressed. Data in the upper left portion of the figure show responding of the type expected – greater allocation of responding to the fixed alternative as the progressive ratio requirement increased (from 3–96). By contrast, the participant shown in the lower left portion of Figure 3 showed virtual indifference to the schedules – allocating behavior to both alternatives in an inefficient manner. Indeed, in the later stages of the procedure, the participant was just as likely to select the alternative requiring 96 responses before reinforcement as the one requiring only three.

Data to the right in Figure 3 show outcomes of a program to teach children with schedule insensitivity to increase their behavioral efficiency. SPW showed the desired outcome – increasing selections of “richer” alternatives as the training progressed. Other plots show growing or continuing indifference to even large schedule disparities. Notably, reinforcers retained potency as measured by reinforcer function tests and demonstrated as children continued to respond to the schedules despite apparent indifference to their disparities.

The Problem of the First Instance

Ray and Sidman (1970) made the point that reinforcement could only make more probable stimulus control that had occurred for other reasons – but what are those other reasons? To make progress on this critical issue, we have ventured outside the realm of behavior analysis into cognitive science that concerns itself with such matters. Much research has shown that some stimulus presentation arrangements are better than others for promoting certain desired forms of attending and relational learning (Serna & Carlin, 2001). For example, presenting a

large number of identical S– stimuli and a single S+ has been shown to facilitate both oddity learning and matching-to-sample. Perhaps not coincidentally, the circle-ellipse program used that type of arrangement, and that may have been a variable in its widespread success.

Participant variables are also becoming increasingly important in behavioral analyses of attending. For example, Dickson, Wang, Lombard, and Dube (2006) reported that a diagnosis on the autism spectrum was associated with greater overselective attending than other diagnoses associated with intellectual disability. Although much of Sidman's work did not emphasize variables such as these, we think that his program on the behavioral consequences of stroke (Sidman, Stoddard, Mohr, & Leicester, 1971) was clearly heading in that direction.

Conclusion

There appears to be much that behavior analysts can contribute by following paths pioneered by Sidman and his colleagues. We recommend that our colleagues review the Sidman program that evolved between 1960 and 2000. By itself, we think such a review could constitute an excellent graduate seminar. By examining the directions and problem choices of the Sidman program, we think that students might achieve a comprehensive understanding of the objectives, processes, and challenges in conducting a translational research program. Moreover, we think that they might become inspired by truly understanding what Sidman was after, what he achieved, and what his students (i.e., all of us) can achieve by following his lead.

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Figure 1.
Selected steps from the circle-ellipse program developed by Sidman and Stoddard (1966).

Figure 2.
Examples of behavioral profiles conforming or not conforming to the matching law.

Figure 3.

Left portion: Examples of expected and atypical response to fixed vs. progressive ratio schedules. Right portion: Illustrative data from a program designed to teach children to increase the efficiency of behavior allocation.

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Table 1

Data reproduced from Table 4 of Carr and Felce (2008)

	LBE (Raw Accuracy)		Error-Correction (Raw Acc.)	
	Mean	Range	Mean	Range
<i>Exclusion (LBE) Trials</i>	92.7%	70%–100%	72.8%	30%–97%
<i>Learning Outcome Trials</i>	91.7%	72%–100%	72.5%	39%–100%

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Table 2

Data reanalyzed from Table 4 of Carr and Felce (2008)

	LBE %> Chance		Error-Correction %> Chance	
	Mean	Range	Mean	Range
<i>Exclusion (LBE) Trials</i>	85.4%	40%–100%	45.6%	–40%–94%
<i>Learning Outcome Trials</i>	83.4%	44%–100%	45%	–22%–100% 24