

THE DEVELOPMENT OF A TREATMENT-RESEARCH PROJECT FOR DEVELOPMENTALLY DISABLED AND AUTISTIC CHILDREN

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This paper describes the development and main results over the last 30 years from the treatment-research project with developmentally disabled (autistic) children in the Psychology Department at the University of California, Los Angeles (UCLA). Three important dimensions in treatment research are addressed. The first pertains to the role of serendipity or accidental discoveries, the second to the importance of pursuing inductive rather than theory-driven research, and the third to the importance of adding in a cumulative and step-wise manner to improve treatment adequacy. Data from various areas of treatment research have been used to illustrate new directions for the project. These illustrations center on early and successful attempts to isolate experimentally the environmental variables that control self-injury, failure to observe response and stimulus generalization with subsequent loss of treatment gains, and the main results of intensive and early behavioral intervention in the child's natural environment. Effective treatment for severe behavioral disorders is seen to require early intervention carried out during all or most of the child's waking hours, addressing all significant behaviors in all of the child's environments, by all significant persons, for many years.

DESCRIPTORS: inductive treatment research, generalization, early intervention, home-based treatment, children

I will take this opportunity to describe, in an informal and personal manner, the more critical observations and turning points that have helped determine new directions in treatment research with developmentally disabled and autistic children at the UCLA Psychology Department's Autism Project. Space does not allow for a comprehensive review of the empirical studies that form the basis for behavioral treatment of developmentally disabled (including autistic) children. For such reviews the reader is referred to Lovaas and Smith (1988), Newsom and Rincover (1989), and Schreibman (1988). One of the best known and earliest presentations of the research procedures pursued in this paper is presented in detail by Sidman (1960). Thompson (1984) has discussed certain instructive parallels between the inductive and discovery-oriented research of Claude Bernard (the father of physiology) and that of applied behavior analysis. Bernard attempted to isolate helpful from harmful (medical) treatments, as is the goal of psychological treatment research. The reader may also want to

become familiar with the work of Chamberlin (1897), a geologist who advocated a procedure that he labeled "the method of multiple working hypotheses" and warned against the seductiveness of the "grand theories" in misleading inquiry, much as is advocated in this paper. I will therefore lay no claims to originality in the observations presented here because others have discussed similar observations more succinctly.

People often ask me how I became interested in working with children diagnosed as autistic. I know that the route that took me there was not one that I designed for myself, but one that my environment arranged for me, in a rather fortuitous manner. My interest in environmental determinants stemmed from the German occupation of Norway during World War II. As a child, I wondered whether such destructive behaviors were genetically or environmentally determined. I hoped for the latter. By luck, I was assigned an adviser in the graduate program at the University of Washington, Professor Edwin Esper, who was of "the old school" of behaviorists. Esper was upset with Boring's favorable coverage of Wundt and Titchener and ended up writing his own history of psychology to correct Boring's "mistakes" (Esper, 1964). He was a stu-

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student of A. P. Weiss, had studied linguistics with Bloomfield, favored behavioral interpretations of language, was strongly critical of Chomsky, and had published one of the first experimental investigations in linguistics (Esper, 1925). Pioneers in psychology, like Thorndike, Hall, Yerkes, Meyer, Weiss, and Jastrow, were behaviorally oriented and initiated applied psychology, addressing educational and clinical problems. O'Donnell (1985, pp. 209–291) provides an interesting account of this development as it was welcomed by psychiatry with its ties (at that time) to biology. Behavioral psychology was considered to preserve “the experimental rigor of laboratory science” (p. 235), which Freudian theory was seen to threaten. At the same time, psychologists who worked within universities were under some pressure to give help to the society that financed them (“the university . . . belonged to the people,” p. 217), and behavioral psychology offered a framework for studying how to change behavior. O'Donnell provides a quotation from Lightner Witmer that is as true to behavioral sentiment today as it was 90 years ago: “To ascribe a condition to the environment, is a challenge to do something for its amelioration; to ascribe it to heredity often means that we fold our hands and do nothing” (p. 234).

Given this promising start, one may wonder why it took so many years before behavioral psychology fielded large-scale treatment programs. In fact, by the 1950s, behavioral psychologists seemed antagonistic towards those who addressed social problems. One of the few attempts to unite behavioral and clinical psychology was presented by Dollard and Miller (1950), who translated psychoanalytic concepts and clinical observations into Hullian theory. As was the case with so many conceptual efforts at the time, the gap between theory and empirical observations was too large, and there were little or no data. Today, some 100 years after the pioneers expressed their hopes of providing a scientific basis for treatment and educational interventions, the gap is beginning to narrow. A major source of joy in my work has been to help narrow the distance between research and practice.

EARLY WORK ON LANGUAGE

I had been offered a postdoctoral position by Sid Bijou in his Child Development Institute in 1958, and was placed in an environment that eventually would help me contribute to bridging the gap between behavioral psychology and clinical application. At the Child Development Institute, I began my professional career by introducing supervised training in psychodynamic therapies to the clinical students there (which included Robert Wahler and Ralph Wetzel). I was beginning to feel the futility of such efforts, and Sid Bijou placed me under increasing pressure to “do research” with the preschool children. Not knowing what to do, I set out to test whether one could demonstrate reinforcement control over the vocal utterances of preschool children, trying to replicate Greenspoon's experiments. I was not very enthusiastic about that project, but it seemed better than doing nothing. It is noteworthy that had it not been for a very accidental discovery at that time, my research career may have been very different or nonexistent. The full story is too long to tell here (but see Lovaas, 1977, pp. 119–126). In short, a child subject was seated in a playroom in full view from where I sat behind the one-way screen in an adjoining observation room. He had been instructed to sit and then to talk to a box that dispensed toy trinkets as reinforcers. At some point he said, “What shall I say?” As per protocol, he was reinforced for making that utterance. To my surprise, he then got out of his chair and walked over to the attending adult to ask this question of her. Halfway through his walk across the room, I was about to tell my assistant to “Get the kid back in his chair,” when it dawned on me that the child was leading me to an important discovery. In short, I reasoned that the discriminative stimuli (SD) generated by his immediately preceding verbal response (“What shall I say?”) exercised SD control over his own nonverbal behavior (walking across the room to address the assistant). He had not been instructed to get up; rather, he had been told to sit. In short, he probably would not have walked across the room, had it not

been for his question "What shall I say?" (The immediate causes of behavior are likely to be found in the seconds preceding or accompanying a response. It may help to think of a reflex, where the latency between a stimulus and a response is a matter of milliseconds.) It was this kind of observation that I needed to make sense out of my discussions about Benjamin Whorf (1956) with Professor Esper some 5 years earlier. Whorf had proposed that a person's particular language may influence the way that person perceived, thought about, and behaved (nonverbally) in his or her physical environment. Similarly, Dollard and Miller (1950) had proposed that the response-produced stimuli from a client's "new language" (at the end of treatment) would have to control new and "healthy" behaviors outside of treatment, in order for such treatment to be effective. My interest in these matters stemmed from my clinical practice. Like most clinicians, I administered "talking therapies." My clients would ask me, "Do you mean that by sitting here talking to you, I will get better?" I answered "yes," placing my faith on the hypotheses of Whorf and Dollard and Miller.

At the end of a series of studies on verbal control over nonverbal behavior (Lovaas, 1961, 1964a, 1964b), there was some evidence that, for example, if one reinforced verbally aggressive statements, then one could observe an increase in non-verbal aggressive behavior. However, I could only conclude that the amount of verbal control over nonverbal behavior varied considerably across children, and when present, it seemed rather short-lived. The degree of stimulus control would vary across children as a function of their particular reinforcement history, and could be easily extinguished. Subsequently, Meichenbaum was to learn of these studies (Meichenbaum & Goodman, 1969, 1971), which contributed to a foundation for what is now labeled cognitive behavior therapy. For reasons that I cannot understand, Meichenbaum deleted the learning-based interpretations of these verbal-nonverbal interactions that I had provided. Instead, he substituted developmental and cognitive terminology, much of it from lay terminology, to account for

what was hoped to become a substantial effect in psychotherapy. Dropping the experimentally based learning interpretations may have been a mistake. A review of the literature suggests that more data are needed to support whether, and with what kind of clients, cognitive-behavioral techniques generate strong and lasting therapeutic effects (Barlow, 1988, on anxiety; Elkin, Parloff, Hadley, & Autrey, 1985, on depression; Meador & Ollendick, 1984 on conduct disorders and hyperactivity in children; Whitman, 1990, on persons with mental retardation). In his discussion of the Whorfian and similar hypotheses, Jordan (1982) points out that few hypotheses in anthropology, psychology, and linguistics have been more appealing and, at the same time, supported by less evidence.

However limited the verbal control turned out to be, this early research helped to lead me away from research in highly contrived laboratory settings and into real-world environments. Two very significant events took place in the late 1950s, when Ted Ayllon and Israel Goldiamond presented programs and data on how to intervene on the real-life behaviors of schizophrenic clients and persons with speech dysfluencies. They were clinicians and researchers "in one," presenting as good examples of reinforcement and stimulus control as anyone could want. Psychoanalytic theories of stuttering and Harry Stack Sullivan's theory of schizophrenia were both more entertaining to me. Sadly, they were both failing to generate effective treatments. By the early 1960s, a number of studies had been published that represented the first steps in gaining experimental control over behavior in real-life environments, perhaps the most important achievements in the history of applied behavior analysis. It was a development that helped the early behaviorists, and later B. F. Skinner, to realize their hopes of applying experimental designs to solving social problems.

MEETING WITH AN AUTISTIC CHILD

There were a number of confounding and uncontrolled variables in the early work on the inter-

action between verbal and nonverbal behavior. One of these was the subject's history with language, which of course was essentially unknown. After I arrived at UCLA in 1961, my students and I went out looking for children who were chronologically of an age at which they were expected to talk, but who evidenced little if any language. Our hope was to build language in these children under controlled and known circumstances, and then to observe closely how their social, emotional, intellectual, and other behaviors might concurrently change. My attempts to isolate the pivotal role of language in controlling other behaviors still had not extinguished.

In searching for a child who was old enough to talk but had failed to do so, my students and I were referred to a clinic for autistic children. As if in a dream, I had found the ideal persons to study. They had been diagnosed with autism and were hypothesized to suffer from a unique form of (mental) illness that needed discovery of a special form of treatment in order to help them. If such treatment was to be discovered and correctly applied, all their problems would quickly vanish. Fortunately, by now I was beginning to learn not to conceptualize clinical problems in that manner. Instead, the fascinating part to me was to observe persons with eyes and ears, teeth and toenails, walking around yet presenting few of the behaviors that one would call social or human. Now, I had the chance to build language and other social and intellectual behaviors where none had existed, a good test of how much help a learning-based approach could offer.

SELF-INJURIOUS BEHAVIORS AND MORE SERENDIPITY

We wasted no time in breaking down the complex construct of autism and developed objective and sensitive measurement systems of the various behaviors of children so diagnosed (Lovaas, Freitag, Gold, & Kassorla, 1965b). In inductive work, the most important first step for any investigator is to secure accurate and sensitive measures of the de-

pendent variables, that is, the client's various behaviors. Sensitive measures will help to assess the impact, if any, of the various independent treatment variables one presents to one's client. The diagnosis provides only a rough, yes/no assessment. In a field like psychology, where so little is known about what variables affect behavior, sensitive and on-line assessments are critical in order to detect (that is, discover) those environmental variables that may be functional in altering behavior.

In working with children who cannot talk, one soon discovers that they have other problems as well, and one of the most salient of their behaviors is self-injury. Although we had initially intended to establish language, we were forced to search for those variables which controlled self-injury. Otherwise, our study on language could not proceed. Again, we were fortunate because the first client we selected, Beth, had an abundant amount of self-injury in addition to extremely limited language. Attempting to help Beth overcome her self-injurious behaviors turned out to be richly instructive. The second break came when we were referred only 1 client during our 1st year. To fill up our laboratory space, we had to work with her from 9 a.m. to 3 p.m. daily, 5 days a week. This gave us ample time to closely observe her and make discoveries. Because we had only 1 subject in our study, we were forced to use a single-subject replication design. This also turned out to be a major break. Group designs, considered to be the appropriate and acceptable design at that time, would not have helped us isolate the critical variables. Finally, self-injury is an ideal dependent variable because it can be recorded accurately and laid out over time, becoming sensitive to manipulation of treatment variables.

The study on Beth has been reported elsewhere (Lovaas, Freitag, Gold, & Kassorla, 1965a). I would like to summarize the main lessons we learned, because they came quite unexpectedly and had a profound effect on our subsequent treatment-research project. Beth's self-injurious behaviors had lasted for about 10 of her 13 years and left her with major scar tissue on her scalp (from banging)

and hands and face (from biting and scratching). She had been institutionalized and received state-of-the-art psychodynamic treatment for at least 1 year, without any apparent change. We discovered that the treatment of choice at that time, derived from psychodynamic theory and supported by common sense (but no data), centered on delivering sympathetic comments and demonstrations of affection when Beth engaged in self-injurious behaviors. Contrary to predictions from this theoretical position, this treatment did in fact accelerate her self-injury. On the other hand, withholding that treatment and building alternate behavior lowered the rate. As we continued to probe with other interventions, we found that major physical and social changes in her environment (such as non-contingent demonstrations of affection) seemed to have no effect on her. Then, by accident, we discovered that an apparently insignificant change (changing nursery school songs to those not associated with extinction) caused her self-injury to suddenly drop to zero, an excellent example of stimulus control.

The amount of reinforcement we received from Beth during the experimental manipulations was massive, and we came to appreciate and need her more than ever. Her "psychotic episodes" turned out to be rational and social behavior, controlled by known laws that regulated "normal" behaviors. I had up to that point been a "doubting Thomas" when it came to the explanatory power behind reinforcement theory. Increasingly, we came to view the behaviors of developmentally disabled and autistic individuals, not as instances of pathology, but as belonging to the natural order of things (Lovaas & Smith, 1989). Itard (1932; see also Lane, 1977) had worked from this position some 160 years earlier, unencumbered by the many theoretical detours that were to follow him. The research of Wolf, Risley, and Meese (1964) and Hewett (1965) paralleled our own and was soon to be followed by similar discoveries from other investigators across the country.

One more accidental observation may be worth mentioning, and that concerns the "decision" to

use contingent aversives. Persons often ask me how that came about. When one sees a client (or "subject") once or twice a week, one develops a relationship with that person that is very different from seeing a person 6 hours a day, 5 days a week over most of a year. Also, by the time I saw Beth, I had helped raise four children and learned a great deal about how to raise them. By now I was spending much more time with Beth than I had with my own children, and I had come to consider her as one of my own. One day, while I briefly interrupted Beth and her teacher's play to make a short comment, Beth walked away from us to a steel cabinet, bent over, and violently banged her head against the sharp corner. I would not let any of my own children act like that. Quite impulsively and without any contemplation, I reached over and gave her a whack on her behind with my hand. She stopped suddenly and looked at me, as if to ask, "Is this a psychiatric clinic or isn't it?" I experienced intense fear and guilt as to what I had done. However, Beth paused for about 1 minute, then as if to test me, hit her head once more. I mustered up enough courage to give her one more slap on the behind. At that point, Beth came back over to the teacher and me, and acted very affectionate and sociable. There were no other acts of self-injury that day, or in my presence thereafter. This incident was never planned, and in fact, I would not have planned to do what I did. It was experiences like these that gradually led us to use the average environment and average children as a model for how to construct a treatment program. These experiences also taught us to change the natural environment only enough to isolate those variables which would make it both therapeutic and educational. As we were learning more about how to help these children, we became increasingly certain that Freud, Bettelheim, and others had, by their comprehensive, popular, and easy-to-understand theories, led themselves and others into an enormous blind alley.

The use of contingent aversives has become a controversial issue that warrants more attention than space allows in this paper. Some concerns and precautions that should be exercised when one con-

templates the use of such interventions have been presented by Lovaas and Favell (1987). Shortly after the work with Beth, we sought out (and were referred) some of the most severely self-injurious clients in the Southern California area. This led us to the large state hospitals where such clients resided. The severity of the problems one can observe in such settings is appalling, and most instructive for anyone who wants to work with developmentally disabled individuals. In short, our observations on the effects of contingent social attention and aversives were replicated (Lovaas & Simmons, 1969). It became increasingly clear that we could severely damage a client by attending to his or her self-injury. The use of contingent aversives quickly suppressed self-injury for most clients, who subsequently could be taken out of restraints and introduced to a large range of educational environments. I remember vividly, even some 30 years later, a client who learned to walk again after having been tied to his bed for so many years that his tendons had shortened (secondary to disuse).

Another significant observation occurred when we tried to teach socially appropriate behaviors to these clients. We discovered that most of them, like Beth, were teachable. One client learned the alphabet (receptively) in less than 2 hours! Then, observing the bedlam around them in these large institutions, the thin ratio of staff to clients, and the profound lack of appropriate teaching and treatment skills on part of the staff, one could see it as inevitable that self-injury would develop under such circumstances. The clients had little or no language, and there seemed no other way in which they could control their environment. They all seemed to want some measure of control, much like the rest of us.

One additional illustration of the importance of testing observations from the average environment in designing treatments: In giving one's child a spank for some destructive behavior, most parents will not leave it at that, but eventually will "make up." That is, once the child stops the unwanted behavior, the parent waits for (or prompts) some socially appropriate behavior and, when that behavior occurs, follows it by assurance to the child

that all was well (a hug and reassurance to the effect that "I love you"). In doing so, two important acquisitions may take place. First, socially appropriate behavior may be strengthened through negative reinforcement, because the parent removed all signs of aversives. Second, parental expressions of love may acquire secondary positive reinforcing properties, becoming "safety signals" by being associated with reduction of aversive stimuli. Such use of aversive events is potentially more therapeutic than the mere use of their suppressing properties. Although data supporting these hypotheses were reported long ago (Lovaas, Schaeffer, & Simmons, 1965), these have apparently been ignored or overlooked by those who later implemented aversive interventions.

Given the potential dangers involved in using aversives, we decided to carry out such work with the clinic open to parents, professionals, and the news media. We have maintained an open-door policy since that time, independent of whether we use aversives or not. Such a policy helps to ensure that everyone works hard, tries to be helpful, and keeps in touch with social ethics. Subsequent involvement of the clients' parents as active participants helped to extend that policy and will be discussed later.

Ferster (1961) had postulated that the behavioral deficit of autistic children was due to their deficiency in social reinforcers. Establishing social reinforcers by associating adults with the reduction of aversive stimuli (Lovaas et al., 1965) and/or the presentation of primary positive reinforcers (such as food) was indeed accomplished (Lovaas et al., 1966). However, this did not by itself result in a concurrent increase in socially appropriate behaviors, which had to be separately shaped. Another misleading simplification centered on the possibility of observing major gains in appropriate behaviors with the reduction of self-injurious behaviors. This failed to occur. We were to pursue many other misleading treatment strategies before fully recognizing the complexity of the problem facing us.

Although the causes and treatment of self-injurious behaviors are becoming reasonably well un-

derstood, there are other behavioral excesses among developmentally disabled persons that are not. One such large group of behaviors has been labeled "self-stimulatory," as observed in the ritualistic, repetitive, stereotyped, and high-rate behaviors of rocking, pacing, jumping up and down, gazing, lining of objects, and so forth. We have proposed the possibility that such behaviors may be operant behavior, maintained by the sensory-perceptual reinforcers that appear to be generated by such behaviors (Lovaas, Newsom, & Hickman, 1987). Based on our low success rate in guessing at what the causes of behaviors may be, it is important to keep in mind that this is only one of several possibilities. Some of the treatment implications of these behaviors have been presented elsewhere (Epstein, Taubman, & Lovaas, 1985). These kinds of behaviors are of particular interest in treatment research, because they appear durable and, unlike most kinds of socially inappropriate behaviors, do not decrease in frequency by withdrawal of social reinforcers. Given the high relapse rate of so many behaviors established by the use of socially medicated reinforcers, it would be a boon to treatment research if one was to discover the variables that created and maintained socially appropriate "higher levels" of self-stimulatory behaviors. These variables may not be of the operant kind.

THE 1973 TREATMENT STUDY

The first comprehensive treatment study was begun in 1964 and reflects many of the blind alleys of our own design (Lovaas, Koegel, Simmons, & Long, 1973). First, we worked under the belief that if we removed the children from their natural environment and placed them in an institutional setting, we would be able to obtain very accurate measures of the children's behaviors on a 24-hour schedule and better control all relevant aspects of their environment. Second, we thought that 1 year of intensive one-to-one treatment (2,000+ hours) would be enough and that treatment gains would last. Finally, we focused our major efforts on developing language, because we still considered lan-

guage to be pivotal in facilitating improvement in other nontreated behaviors.

Numerous gains in treatment were made during these years. We observed major increases in complex behaviors like language, helped to develop learning strategies like verbal and nonverbal imitation, reduced tantrums and self-injurious behaviors, and so on. But one makes perhaps the most progress by recognizing one's mistakes. Our assumption that increases in language would be associated with concurrent improvements in other areas of functioning was not supported. This was a major disappointment because we had hoped that once the children learned to talk, they would develop the kind of response generalization that would "push them over" into normalcy. Instead, the children revealed themselves to be without much prior knowledge. A "little child" did not seem to be hiding on the inside, waiting for the opportunity to come out from his or her autistic shell, as so many theoretical formulations had postulated and still do. Nevertheless, the acquisition of language did give the clients access to future educational environments, where additional appropriate behaviors could be built (a kind of "successive" response interaction).

The second lesson that we learned during this time concerned the lack of generalization across environments, including posttreatment environments. When we discharged the clients to the state hospital from which they had come, they inevitably regressed. It was heartbreaking to observe Pam and Rick, who had gained so much with us, slowly but surely lose the skills they had acquired. When we brought the children back for treatment a second time, they recovered many of the gains they had made during the first treatment, only to lose them again after their second discharge. The data we secured before, during, and after treatment served as a most important guide to future development of the project. It may have been possible for us to fool ourselves without such data; others apparently had. By now we knew that there seemed to be no shortcuts and, instead, a lot of hard work ahead.

At about the same time, we were to learn another

bitter lesson: how difficult it is for colleagues from other orientations to consider adopting and testing the treatment programs that were developed. We pleaded with the staff at the hospital to which the children were discharged to allow us to continue treating them there. This was met by an immediate rejection on their part. The children were to be regressed so that they could recover those childhood experiences that they had missed (because of their bad mothers). Then, they would easily develop into normal individuals. How could one ever imagine that this behavior modification program, designed to train dogs and pigeons into robots, would ever work with human beings?

On the other hand, the children who were discharged to their parents, who wanted to be informed about our treatment, did better. The important role that parents were to play as colleagues in treatment will be discussed below.

THE 1987 EARLY INTERVENTION PROJECT

There were six observations made during the 1973 treatment-research study that were to play a major role in the design of our next effort (Lovaas, 1987). First, we had by accident discovered that the youngest children in the 1973 study made the greatest progress. Second, we learned that treatment effects were situation specific. We therefore moved treatment away from a hospital or clinic setting and into the children's home and everyday environment. Third, we found limited evidence for response generalization and designed treatments for most or all of the children's behaviors. Fourth, we learned that the parents could become skilled teachers of their children, and they were the best allies one could want in helping accelerate and maintain treatment gains. Fifth, we offered treatment for most of the child's waking hours, for 2 or more years, and taught the children to develop friendships with average ("normal") peers, in order to continue treatment at that level as well. This arrangement would more closely resemble that available to average ("normal") children who learn from their

environment from morning to night, vacations notwithstanding. Finally, and most importantly, by the 1970s we possessed a large range of data-based procedures that could be put together, in a cumulative manner, so as to expand and enrich the treatment program. Our treatment came to consist of hundreds of separate teaching programs. It is a compliment to applied behavior analysis that it has been possible for literally hundreds of investigators to generate thousands of replicable studies that add in a cumulative manner to a vast array of useful knowledge. I know of no other area within clinical psychology, special education, or other helping professions within the social sciences that have accomplished this. At the same time, there is every reason to believe that progress in other fields (such as psychotherapy) will not occur until such a strategy (of replicable and cumulative findings) has been established.

The Early Intervention Project apparently generated major and lasting increases in intellectual, educational, social, emotion, and other behaviors (McEachin, Smith, & Lovaas, 1993). The question is often raised as to why the young children did so much better than the older ones. There could be several reasons for this, but my main guess would be that intensive (40 hours per week of one-to-one) treatment was started early enough so that a sizable minority (47%) acquired an adequate amount of language, social, play, and self-help behaviors after 1 year. This group of children could be successfully mainstreamed among average ("normal") children in regular preschools. Once kindergarten was successfully passed, the children went on to successfully pass the first grade and subsequent classes in public schools. The development of friendship by these children with average children may have helped to build further prosocial behavior and protect against relapse. Of course, there could be many other reasons why early intervention works, but it is beyond the scope of this paper to discuss these.

The latest follow-up received favorable reviews (Baer, 1993; Foxx, 1993; Kazdin, 1993; Mesibov, 1993; Mundy, 1993). The follow-up was con-

ducted in a double-blind manner, and the test battery was very comprehensive, involving normed tests of emotional, social, intellectual, self-help, and other functioning. (Such assessments are sadly missing in most behavioral treatment research. The presentation of outcome data in that manner is quite understandable and important to those who may want to adopt our treatment procedures.) We had entertained many guesses as to what the outcome data would show. For example, we had feared that many clients would relapse after treatment had been terminated, as in the 1973 study. This did not happen. We had also anticipated that the best outcome group would look somewhat homogeneous, perhaps socially withdrawn and emotionally flat, as in "residual signs of autism." This did not turn out to be the case. The 9 best outcome subjects appeared very heterogeneous. From theories of autism and schizophrenia, one would also suspect that their Wechsler scores on subscales assessing comprehension of social events and abstract language would be lower than those of average children. This also proved not to be the case.

An observation on the commonly used constructs of intelligence and autism may be relevant here. Our treatment was not directed at modifying either construct. Rather, the treatment focused on building the many behaviors that would facilitate the clients' interactions with, and learning from, the average, real-life environment. The intensively treated group gained and retained 20 IQ points. The best outcome subjects appear to be indistinguishable from average persons on the tests employed. The success of behavioral treatment seems to make the constructs of intelligence and autism superfluous.

The achievement of lasting average (or normal) functioning in almost half of the intensively treated group was an extremely gratifying and unexpected finding. However, the reinforcing efforts of such success do not last all that long. What is left to do is to be of more help to the other half who did not achieve average and normal functioning, and this may be a much more time-consuming and challenging job than that of helping create the best

outcome results. My guess would be that the group that did not gain average scores on IQ and other tests is very heterogeneous and that progress toward recovery of these children will occur in much smaller steps, perhaps awaiting discoveries from basic research in areas other than reinforcement theory. The best outcome subjects tended to be those who acquired verbal imitation within the first 3 months of intensive treatment, whereas the remaining subjects failed to acquire this discrimination. Those who fail to acquire verbal imitative behavior often show an ease in matching visual stimuli, appearing more like "visual learners" rather than "auditory learners." The data from the reading and writing program that we are currently testing for the visual learners may give us some clues about how to proceed.

It is important that unexpected findings, such as those of the 1987 study, be replicated. Over the last 6 years, we have placed major efforts into helping other sites set up similar programs. This has been unexpectedly difficult because of the numerous misunderstandings that most professional persons harbor about behavioral treatment. Those who do want to replicate often experience inadequate subject referrals, inadequate financial support, and, perhaps more important than any other variable, the difficulty of being a young investigator who may risk tenure by being involved in a project that will yield so few and immediate publications. Nevertheless, there are three sites that are well on the way to test the replicability of our procedures. In contrast, it has been relatively easy to replicate the treatment across families who present their request for services directly to the UCLA project. Over the last 2 years, the project has trained some 200 such families across the country, and requests for services are growing at a very rapid rate. Differences between parents and professionals as potential colleagues and innovators will be discussed in more detail below.

It may be helpful to elaborate somewhat on the complexity of the problem children we have studied over the last 30 years and the importance of remaining skeptical about treatments based on sim-

plistic approaches. A paper by Lovaas and Smith (1989) presents our view of a behavioral model of children diagnosed as autistic. In short, the problem is much more complex than most writers suspect. For example, topographically similar behaviors like self-injury may have three separate functional relationships, requiring separate forms of treatment (Carr, Newsome, & Binkoff, 1980; Favell, McGimsey, & Schell, 1982). Furthermore, there is little support for Kanner's (1943) hypothesis that the group of children labeled autistic is a homogeneous one. Rather, there appears to be wide individual differences within the diagnostic grouping, with the possibility of as many different etiologies and treatment outcomes as there are clients so diagnosed. Additionally, the behaviors observed in autistic children are not unique to them, and the shape of the learning processes do not appear to differentiate them from average persons or persons with other diagnostic labels. It is possible that autism as a diagnostic category may have been created as an arbitrary collection of certain low-frequency behaviors.

THE IMPORTANCE OF STUDENTS

I have been extremely fortunate in being able to work with young, bright, and open-minded students as colleagues. The importance of having a group of persons around you with whom to collectively look at problems is that no one person can possibly find the answers to what has turned out to be a very complex array of problems. There are no "experts" in this field. Working inductively and in an exploratory manner takes a great deal of time and effort; nature does not seem to give up its secrets all that readily. In making guesses or hypotheses about how to change behavior, be prepared to have one success for every 25th guess, and for that success to contribute in only a minor way to the solution of one of the problem behaviors of one particular client. It is like a thin schedule of reinforcement, but the reinforcers are substantial.

The number of students who work on the project has gradually increased. Today, some 60 to 80

undergraduate students and 4 to 8 graduate students work as therapists/researchers in one-to-one relationships with clients for 6 to 20 hours per week, each for a minimum of 6 months. New students enter each quarter and receive training by working in an apprenticeship fashion with experienced students. After 3 months on the project, students will help train incoming students (UCLA is on the quarter system). After 6 months, unusually talented student-therapists can be appointed as "senior therapists" and may stay for another 6 to 12 months, supervising a team of 4 to 6 students that are assigned to each family. Graduate students serve as "clinic supervisors" and will demonstrate and help to train complex behaviors like advanced language. The clinic offers training in individualized research, and many students take advantage of that, once their tenure as therapists has been completed. The clinic operates 12 months a year, year in and year out. Given the richness of the staffing, the project delivers upward of 300,000 hours of one-to-one treatment per year, not counting supervision of out-of-town treatment. As the director of the project I have to keep track of all developments, and that can only be done by delegating. I work with very responsible persons, who grow on the job, and that includes parents.

PARENTS AS COLLEAGUES

Traditional clinical training and theories of treatment did not encourage parent participation. Remnants of that approach may delegate parents to a spectator role, as in placing them behind one-way screens to observe others carry out treatment of their child. Or the parents are viewed as very vulnerable and anxious, and are excused from active participation on that basis. There is every reason to believe that trying to raise a developmentally disabled child is stressful, but what data suggest that such stress is reduced by becoming a spectator? It is possible that the best stress reduction takes place when a parent experiences some control over his or her children, and discovers that he or she can help the disadvantaged child grow and develop.

Fortunately, I met Bernard Rimland early in my career. Rimland is a psychologist and himself a father of an autistic child, and his book helped diminish the monopoly that psychoanalytic treatments exercised in the 1960s (Rimland, 1964). Unlike most professionals, Rimland came to visit the project (in 1964) in order to evaluate whether behavioral treatment could help his son and others like him. During the course of his visit, I was invited out to dinner with parents of autistic children in the Los Angeles area. It was with considerable reluctance that I accepted the invitation. Meeting one's "clients" in such an informal manner was considered to be against clinical practice and ethics ("They may remove your teeth," a colleague warned me). In any case, I accepted, and over wine and Italian food we got along quite well. It was clear to both Rimland and me that should the parents break the stronghold exercised by psychodynamic therapists over treatment, it could be achieved only through joint action. The next year, Rimland started the National Society for Autistic Children (NSAC), which became the forerunner of the Autism Society of America (ASA).

Parents continue to be the leaders in disseminating behavioral interventions. A recent publication by Maurice (1993) provides an informed and insightful presentation of the hardships parents endure in attempting to secure data-based interventions for their young children diagnosed with autism. Maurice also provides the first popular, personal, and sympathetic account of behavioral treatment. In contrast, most professionals who work with autistic and other developmentally delayed children have resisted behavioral treatment, or actively spoken out against it. Some examples of this will be provided next.

ADVERSITY

All is not going to be happiness and smooth sailing. We would have been in a better position to protect ourselves had we been forewarned of adversity. Protection for a researcher is particularly important when it appears that one has to stay in

for the long effort, as in trying to help severely disadvantaged children and their parents.

There are many disappointments. One pertains to dissemination. There must be at least a 25-year delay between what is now known about how to teach developmentally disabled children and what has been adopted. Paradoxically, special education teachers appear eager to receive training in behavioral treatments, as well as being in a position to deliver such treatments. It is difficult to know where the obstacles are. When a treatment is adopted, it is often in a "watered-down" version. Watering down of treatment has happened before (cf. Lane's, 1977, description of Itard's work). I hope that with data and parent support we may prevail. One helpful first step could be made within our own Association for Behavior Analysis pertaining to systematic update, quality control of treatments and treatment outcome across sites.

A second and related obstacle centers on pronouncements by colleagues representing other areas of investigation. These take numerous forms. There is an alarming tendency in psychological treatment research to attribute failure to the client, as in invoking organic limitations when treatment fails. For example, in their review of research with autistic children, DeMeyer, Hingtgen, and Jackson (1981) concluded that "infantile autism is . . . accompanied by . . . permanent intellectual/behavioral deficits" (p. 432), adding that no one would even give lip service to changing that. Zigler and Seitz (1980) suggested that one would fail in one's efforts to alter IQ scores to any substantial degree. Some attribute failures to defects in the investigator. Thus Spitz (1986) characterized those who had reported increases in intellectual functioning accompanying educational enrichments as "fools, frauds, and charlatans." Or the treatment provided is harmful. Thus Bettelheim (1967) attacked behavioral treatment as follows: "Perhaps we may say of the operant conditioning procedures what has been said of lobotomy: that 'lobotomy changes a functional disorder that is potentially recoverable into an organic one for which there is no treatment'" (p. 411). More recently, Greenspan (1992) presented "be-

havioral schools of thought" as an example of a "common unhelpful approach" that "ignores the delayed child's many needs" and would result in "disorder patterns to become more stereotyped and more perseverative as [the children] grow" (p. 5).

There will also be personal attacks, particularly after the publication of unexpected findings, such as those reported in the early intervention study (Lovaas, 1987). The pronouncements of two professionals, both senior officials in ASA (an organization I helped start) illustrates such behavior. One circulated letters and pronounced in meetings with large groups of parents and colleagues that I could not identify what was fraud. The other, a colleague at UCLA, described the results as "totally not true" and expressed regrets that "that stuff comes out under the label of UCLA."

As a scientist, one can take some comfort in the fact that the majority opinion, and a show of hands, does not determine truth. Bacon (1905) expressed this view as early as 1620: "If the multitude assent and applaud, men ought immediately to examine themselves as to what blunder or fault they may have committed" (p. 101). Although such statements may provide comfort for the scientist, erroneous and derogatory statements about treatment research cause delay in dissemination. Such delays have been of great concern to the practitioner, considering the many lives at stake.

CONCLUDING COMMENTS

As the UCLA project developed over the last 30 years, we have been struck by a myriad of observations, and a short comment about treatment delivery and research may be appropriate. As a practitioner, it may be wise to set aside more time for treatment than traditional clinical theory would suggest. In traditional practice, emphasis is placed on treating a hypothetical process, which, once fixed, is supposed to generate change across a large number of behaviors and situations. If this were the case, it would provide much support for the practice of short-term treatments in offices, clinics, and the like.

If data from behavioral research hold up and such generalization does not exist, then major revisions in clinical and educational practice need to be instituted. One may have to intervene on all behaviors, in all environments, with the help of all significant persons. The treatment may have to start early in life and continue for most or all of the clients' waking hours, for a long period of time. There are not enough professionals to deliver the necessary treatment. This means that we will have to give away our professional skills to lay persons, and the sooner the better. It is to everyone's advantage that we have some functional treatments to disseminate, and that some democratic control over treatment goals will thereby be insured. Given that we work inductively and within an open system, it is equally important to alert persons to the limitations of what we have discovered so far.

With respect to research, much value has been attached to proceeding inductively and to freeing oneself from the comprehensive theories championed by the majority. Furthermore, developments over the last 35 years have enabled researchers to specify socially meaningful environmental and behavioral variables in sufficient detail and objective manner so as to render these replicable. As functional relationships between such variables are discovered outside of contrived laboratory settings, the future for large-scale treatment programs looks favorable. In short, experimental psychology is becoming increasingly relevant in helping solve social problems, as hoped for by the pioneers in behavioral psychology introduced at the beginning of this article.

Based on treatment research with developmentally disabled children, it is apparent that it takes a great deal of time to help to change one's clients and to add to the knowledge on how to do so effectively. One may wonder if this slow pace is to our benefit or disadvantage. Should we hope for and try to promote larger and more rapid change? In this regard, it may be helpful to recall Darwin's comment, that nature does not make leaps. If nature gave us trial-and-error behavior, then it treated us kindly, because errors seem to far outnumber

successful trials. A large step in the wrong direction may have put a stop to our existence. There is safety in small steps.

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