

Comments on "Building Social Behavior in Autistic Children by Use of Electric Shock"

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Comments are directed primarily at two points: the use of autistic children as Ss and the theoretical problem of transfer of behavior from specific learning experiences. With regard to the first, it is argued that the present work consists of the development of techniques for training these specific, highly aberrant, children and that extending findings to others would be a dubious procedure. The difficulties of handling transfer within a strict S-R theory are discussed. The present study attempts to establish generalization of a social approach response. It is argued that the results demonstrate simple shock avoidance learning but that little transfer of this behavior is indicated.

As is the case with most work in the Skinnerian or operant tradition, the present paper by Lovaas, Schaeffer, and Simmons is, in effect, a case study of a small number (two) of Ss exposed to intensively applied experimental training. While this does not yield data that are amenable to elegant, statistical analysis, I feel it is, in many ways, a more valuable approach to problems like the present one than are approaches which use large numbers of Ss studied less intensively. Further to its credit, the present work samples behavior in more than one situation, which is rarely the case in operant work, though it is obviously important in making general statements about the S's behavior. Finally, Lovaas *et al.* have freed themselves from the Skinnerian dogma concerning punishment (i.e., the belief that only "positive reinforcement" produces learning, punishments leading only to temporary response suppression), a belief that never made much sense of the learning experiences of human beings, and that is now clearly dethroned on the animal level as well (Solomon, 1964).

Now let me turn to some critical comments which may be grouped under two headings: (a) problems in the choice of autistic children as Ss, and (b) the problem of transfer.

Problem of Subjects. First, a point of clarification: are the five-year-old, identical twin boys in this study autistic or schizophrenic? The authors use the terms interchangeably, as have a number of others before them, but Kanner's work (1943) and Rimland's recent book (1964) point up a number of important ways in which the autistic child differs from the schizophrenic child. Some of these differences, such as the autistic child's inability to process certain types of sensory input in a meaningful way, which Rimland argues is due to a basic neurological disturbance, are of direct relevance in interpreting the results of the present study. Unfortunately, not enough evidence is provided in the paper to make the distinction. However, since the Ss display the autistic characteristics of lack of speech, social unresponsiveness, repetitive, stereotypic behavior, and since they are male identical twins (characteristics associated more frequently with autism), I will assume for the sake of argument that they are the syndrome of early infantile autism in other respects as well, keeping in mind that their description might be consistent with other diagnoses, including childhood schizophrenia, severe retardation, or organic brain damage.

It might be argued that differential diagnosis

noses of this sort are unimportant, since differential psychiatric treatment so frequently is not associated with differential diagnosis, and that a better way to proceed is to approach the autistic child like any other "organism," apply operant techniques, and see what works. I am not willing to grant this argument. I think the facts indicate that the autistic child is not like any other organism. The behavior of children such as those used in this study represents a rare and extremely pathological phenomenon. Their lack of normal speech and apparent inability to acquire it under a variety of training programs sets them apart. For whatever reason, these children react to their environment in an extremely aberrant fashion, an observation borne out by their behavior in the present study as well as by a number of other investigators who have worked with them.

Thus, I think it crucial in this case that careful attention be paid to the specific characteristics of these Ss and to the observations of others who have worked with autistic children. I find no reference to the work of others in the present paper. Furthermore, the present authors, like others of the Skinnerian orientation (see Ullman and Krasner, 1965), are too readily inclined to accept a psychiatric diagnosis as an adequate description of their Ss. A preferable approach, and one more in keeping with their professed behavioral orientation, would be one which tries to collate present observations with the observations of other investigators who have worked with the same type of Ss. Any explanations then put forth should account for all of the known evidence relevant to the phenomena in question.

Why did the present investigators choose to work with autistic children? Two general reasons suggest themselves: (a) children manifesting autistic characteristics are particularly suitable for the testing of some theoretical notions; or (b) the investigators are specifically interested in developing treatment techniques that will be effective with autistic children. The first reason applies, I think, only insofar as the use of electric shock as a punishment is con-

cerned. That is to say, because of the extreme degree of their disturbance and the failure of other techniques to have any effect on these Ss, an argument can be made for trying out a technique that would ordinarily lead to serious objections on ethical grounds.¹ Beyond this, however, the investigators seem to have little interest in the theoretical implications of their work. There is little reference to why autistic children behave as they do or why some methods of treatment might be more effective than others. Thus, I am led to conclude that their main interest is in working out techniques for treating autistic children.

The following familiar Skinnerian counter-argument might be posed, however: Theory should await the establishment, under controlled laboratory conditions, of ways of shaping behavior. Once this is accomplished, then theorizing can proceed and the work can be extended.

I don't think this argument is valid in a general sense, and it certainly seems inapplicable to the present case. How can anything of general relevance about the effects of punishment on establishing social bonds, for example, come out of nontheoretical work with Ss that are so aberrant in this very dimension of the development of social bonds? The only way that it can, of course, is if theoretical concepts bridge the gap, as in the case of Rimland's speculations on the role of reticular formation dysfunction in autism. Such an approach makes the rare autistic S of general interest as an example, in the extreme, of some particular phenomena. However, this does not seem to apply in the present case.

Thus, I think it can be argued that the present work represents the testing of techniques for training autistic children, and that extending the relevance of the findings

¹I think there is a real ethical problem here when pain is deliberately inflicted on subjects, without their consent, in an attempt to "control" their behavior. The argument that nothing else has worked is really beside the point, though it has been evoked in the past to justify such ethically dubious practices as the widespread use of prefrontal lobotomies.

to other children would be a dubious procedure.

Problem of Transfer. The problem of explaining how generality of behavior can result from specific learning experiences is a central one for any peripheral S-R learning approach. McGaugh and I have discussed this and related issues in detail in another paper (Breger and McGaugh, 1965). In brief summary, we suggested that the behaviorist conception of learning as specific response acquisition is unable to account for the facts of perception (e.g., perceptual constancies), transposition, response equivalence, language acquisition and usage, and related phenomena. A view positing the acquisition of central mediators (schemata, cognitive maps, strategies, programs, etc.) is better able to account for the learning process and is coming to prevail in the field. In this view, generalization of behavior to novel situations is a function of mediating structures which organize sensory input in relation to past experience or memory, leading to the generation of new responses. This contrasts with the attempt to explain transfer as a function of physical stimulus similarity, i.e., the classical notions of stimulus and response generalization.

This issue is of direct relevance to the problem of autism since autistic children are markedly lacking in the ability to generalize. In a sense, the autistic child seems to be that rare organism that behaves according to strict S-R principles. He cannot make use of past experience in any *integrative* fashion. As Rimland puts it, "We again find autistic behavior understandable in terms of an impairment in the ability to attach memory to sensory input" (p. 98); or, "*in early infantile autism stimuli are apprehended, but not comprehended*" [italics in original, p. 86]. Rimland argues that autism is a cognitive disturbance, that the autistic child is incapable of developing the sort of central mediating structures discussed above. This manifests itself in the lack of language or the "echoic," reflexive use of words, in stereotyped, repetitive behavior patterns, and in the inability to develop social responsiveness. This last,

because the mother, and people more generally, never comes to *symbolize* nurturance and the relief of discomfort.

This brings the discussion around to the present study, which is, in effect, an attempt to create social responsiveness by associating people with relief from pain. Two kinds of results are reported: Learning in the experimental situation itself, as in Study 1 in which the Ss learn to come to K to avoid being shocked; and, second, transfer of this learning to different situations. It is the latter which is of crucial importance, of course, and it is to the credit of the present investigators that their work focuses experimentally, if not theoretically on this problem.

What are the findings? In general, approach to E seemed to generalize to situations close to the original one, especially when continued electric shock is applied (see Figs. 2 and 5) but, even in the original shock room, this eventually extinguishes after nine sessions and had to be reinstated with additional shocks. Similarly, in Study 3, E eventually lost his "secondary reinforcing" power and by the eighth and ninth sessions, despite shocks, the social responsiveness had dropped out.

Of greatest interest as a test of transfer are the reports on social responsiveness to people other than the E (nurses) in situations distinct from the experiment (in the ward). If, in fact, the Ss were learning to associate approach to people with relief from pain, rather than a simple avoidant response, then they should have shown greater social responsiveness to nurses on the ward. The results here were equivocal. In Study 1 it is reported that, "The changes in behavior outside were most noticeable during the first 14 days of shock training after which Ss apparently discriminated between situations in which they would be shocked and those in which they would not." The nurses' reports, in Study 2, which are difficult to interpret, seem to indicate little generalized change.

The authors take note of these findings when they cite "... the highly situational and often short-lived nature of the effect of shock." However, they subsequently do

clude that "... the most therapeutic use of shock will not be primarily in the suppression of specific responses or the shaping of behavior through escape-avoidance training. Rather, it would seem more efficient to use shock reduction as a way of establishing social reinforcers, i.e., as a way of making adults meaningful in the sense of becoming rewarding to the child." While this type of learning is obviously of greater value to the child, it does not seem to me that this conclusion is consistent with the reported findings. The findings demonstrate the shaping of specific responses, but very little lasting generalization to people as meaningful sources of reward. Rimland's hypothesis that autistic children lack the neurological equipment to integrate stimulus input with past experience in the formation of such meaningful, generalized, social response patterns seems consistent with the present findings. Unfortunately, it is possible that autistic children are incapable of the type of learning that in-

volves integrated generalization; in any case, I see little in the present findings to contradict such a hypothesis. Perhaps the best training for these "S-R children" would involve learning by strict S-R principles, i.e., teaching specific responses for each specific situation. It is fortunate that most people can learn so much more in less laborious ways.

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