

## Letters to the Editor

### Lovaas study questioned

#### To the Editor:

I was dismayed to read your uncritical reporting of Ivar Lovaas's dramatic claims to have made normal or cured almost half of the children he treated with intensive behavior modification. His cure rate for autism, greater than any other claimed in recent history, has resulted in exaggerated and misleading media coverage. The source of this misunderstanding can readily be traced to three areas of methodological error in the Lovaas (1987) study. They include Lovaas's (1987) biased selection of children, his so-called control group, and his inappropriate outcome criteria. The cumulative effects of these errors is to leave his claim of a 45% cure rate without supporting evidence.

1. Misleading subject selection: Lovaas invented the statistic, "prorated mental age" (PMA) just for his report. Why he used this PMA rather than the ratio IQ or the deviation IQ used by most other investigators is never explained. However, it does seem to provide lower appearing IQ scores. [When PMA scores are translated into ratio IQ scores] it then becomes a ratio IQ of 37 or greater [required] for the subject's inclusion in his study. This criterion excludes many subjects with intellectual functioning higher than the profoundly retarded range Lovaas suggests as the only group he excluded.

Lovaas's second cutoff criterion excludes even more low-functioning children. He included children who had chronological ages of 40 to 46 months at the time of their initial evaluation only if they demonstrated echolalia, a symptom widely recognized as characteristic of autistic children with a better prognosis. Taken together, Lovaas's PMA criteria and his echolalia criteria appear to create a sample significantly higher functioning than any random sample of autistic children.

The evidence that his sample is, in fact, higher functioning than most autistic children, is quite clear according to his own figures. He reports a mean PMA for his treatment group of 18.8. This translates to a ratio IQ of 63. I know of no epidemiological study reporting such high average IQ for autistic subjects.

The evidence presented in Lovaas's study supports the conclusion that his subjects were a nonrepresentative, relatively high-functioning group of autistic children, with the best prognosis regardless of treatment.

2. Misleading control group: Lovaas

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claims that experimental and control groups were "comparable at intake" [and] then leaves out the all-important family connection...Children and families in his experimental group seemed to have more resources than those in his control group. Moreover, in his (Lovaas 1978) publication, Lovaas enumerated the bases on which he excluded children from his treatment group if parents could not meet his demanding requirements for participation.

Also, the study raises the question: was the positive treatment effect Lovaas reported caused by the techniques he taught his students, or was it caused simply by having interested students around the house to help with a difficult child, regardless of operant conditioning procedures?

3. Inappropriate outcome measures: In Lovaas's article he did not explain why he did not use outcome measures based on the cure of the specific social, behavior, and communication problems of each child in the group. Instead, he relied on placement in normal classrooms. Clearly, such placement has less to do with the changes in the child than it does with the philosophy of the school system in placing special-needs children, and with the advocacy efforts made by Lovaas's staff and the children's parents.

In summary, the evidence for Lovaas turning 45% of a typical autistic group of children into a group of normal ones simply isn't there in his study.

[Furthermore] there probably are any number of programs more cost effective for a wider range of autistic children, over a longer period of a child's life, than covered in Lovaas's project, such as any number of progressive public school programs designed for autistic children. In Lovaas's New York Times interview he stated, "It's the kiss of death to be in a class with other autistic kids. They won't learn anything useful." Since no comparison group of children served in a classroom for autistic children was included in Lovaas's study, this was clearly a grossly exaggerated statement even for a newspaper interview. It could have most damaging effects for autistic children who could have public school services denied to them on the basis of his claims.

Anyone wishing to know more about an effective state program for autistic children can read Schopler, E., Specific Diagnosis and Specific Treatment Factors, American Psychologist, Vol. 42, No. 4, April 1987. For a more detailed documentation of the above critique, an unedited copy can be obtained by request to Eric Schopler, Department of Psychiatry, CB #7180, Med. School Wing E, University of North Carolina, Chapel Hill, NC 27599-7180.

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#### Dr. Lovaas and Dr. John McEachin reply:

We believe that Dr. Schopler is mistaken in his analysis of the study, and wish to respond to his three areas of criticism.

1. Subject selection: The statistic, "Prorated Mental Age" (PMA) was devised as a means of analyzing the relationship between intake Mental Age (MA) and IQ following treatment where, of necessity, tests used at follow-up were different than at intake. The best measures of IQ, including the WISC and the Bayley, use a deviation IQ score which must be looked up in norm tables and cannot be calculated as a simple ratio. The ratio IQ suggested by Schopler yields a higher score than the deviation method. Ratio IQs—and the derivative PMAs actually reported—make the children in our study appear to have higher IQs than if we had reported deviation scores. The subjects in our study had a mean deviation IQ at intake of 49. The mean IQs reported by other researchers have ranged from 45 (M. K. DeMeyer) to 60 (B. J. Freeman) to 62 (M. Rutter).

We were careful to ensure that our sample would be representative of the type of children that most researchers consider autistic. Only 15 percent of all referrals were excluded because of low MA and these were subjects for whom retardation far overshadowed autism as the primary diagnosis. The reason we raised the chronological age limit for echolalic children was on the clinical hunch that such children would respond faster to treatment and we would have more time to prepare them for a normal school placement. There was an equal number of echolalic children in the intensive treatment group as compared to the control group. Yet, despite our original hunch, of the four children accepted into the experimental group because of being echolalic, only one achieved normal functioning. Therefore, if echolalic subjects were deleted from the data analysis, the percentage of normal functioning subjects at follow-up would increase rather than decrease.

2. Appropriateness of control group: Several steps were taken to ensure that group assignment was not biased. We will mention here only those questioned by Dr. Schopler. First, it was not up to the families to recruit therapists. We determined ourselves, before any contact with the family, whether we had enough staff available, and no one ever paid for the treatment received. Treatment was given by trained student therapists who worked for course credit. Secondly, to verify that there was no socio-economic bias, the SES status of the fathers was calculated and showed that scores slightly favored the control group. Thirdly, there was no significant difference between the experimental and con-

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