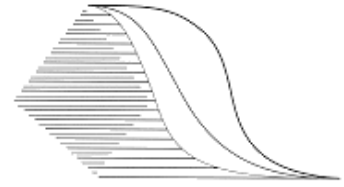


**Centre for Health Services
and Policy Research**



**Autism and Lovaas treatment:
A systematic review of
effectiveness evidence**

BCOHTA 00:1T

JULY 2000

British Columbia Office of Health Technology Assessment



THE UNIVERSITY OF BRITISH COLUMBIA

Autism and Lovaas treatment: A systematic review of effectiveness evidence

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Foreword

The British Columbia Office of Health Technology Assessment (BCOHTA) was established on December 1, 1990 by a grant to the University of British Columbia from the Province, to promote and encourage the use of assessment research in policy, planning and utilization decisions by government, health care executives, and practitioners. The Office does not participate in policy development for a requesting agency, its role is confined to appraisal of the scientific evidence.

Assessments are performed in response to requests from the public sector such as hospitals, physicians, professional associations, health regions, government; private sector groups such as manufacturers; and members of the general public. One or more of the following criteria are used to determine the priority of an assessment and the level of analysis: (1) the number of users and potential change in quality of life; (2) the acquisition and operating costs to the health care system; (3) the potential to influence provider and consumer behaviour as a result of a review; and (4) the availability of accurate information and appropriate research skills.

Health Technology Assessment projects are conducted by faculty and staff (including medical consultants) who are expert in systematic review methodology. Electronic bibliographic databases and fugitive literature (that is, literature not indexed or distributed publicly) are searched using predefined inclusion and exclusion criteria based on a specific search strategy. The critical appraisal of retrieved evidence includes the formulation of logical and defensible conclusions about the technology under study.

Reports are reviewed internally, and then sent for external review to experts from a variety of academic or clinical disciplines. Comments and suggestions are considered before a final document is produced. Distribution of reports is by request from the Office or through inclusion on our mailing list, and reports are also available for public distribution.

The strength of BCOHTA's method of systematic review lies in the process of explicitly detailing the methodology and criteria used to produce recommendations which are based solely on the research evidence. This transparent and reproducible assessment process allows other investigators to review the evidence objectively for themselves.

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EXECUTIVE SUMMARY

This systematic review examined whether early, intensive behavioural therapy for children with autism results in normal functioning, or essentially a cure. The scientific validity of this curative claim is central both to legal proceedings brought on behalf of several children in British Columbia against the Province seeking an intensive behavioural program; and to cost-benefit analyses and clinical guidelines used for planning autism treatment programs.

The report concludes that, while many forms of intensive behavioural therapy clearly benefit children with autism, there is insufficient, scientifically-valid effectiveness evidence to establish a causal relationship between a particular program of intensive, behavioural treatment, and the achievement of 'normal functioning'.

The following findings support this conclusion:

- 1) The published literature on autism contains only one report, from a controlled clinical trial, in which the authors claim that their treatment normalized or cured children with autism. (Lovaas 1987, with McEachin et al 1993). Although the study reported a benefit, it was small (19 children in the treatment group) and its findings of benefit could have been achieved by assembling a high-functioning group of autistic children. The scientific community has been reluctant to accept the results of this study, noting that while methodologically stronger than published reports of alternate comprehensive therapies, the study is inadequate to establish the degree to which this program of therapy results in children achieving 'normal' functioning, however defined.
- 2) The benefits in terms of overall functioning found by Lovaas 1987 (with McEachin et al) have not been corroborated by independent researchers. Published controlled studies involving this intensive behavioural treatment program do not report children achieving normal functioning as defined by Lovaas 1987 (with McEachin et al). Furthermore, uncontrolled studies, although a weaker form of evidence than controlled studies because they do not account for the development process outside therapy, similarly do not support conclusions of 'normalization' through Lovaas therapy.

It is recommended that randomized trials of alternative early intensive treatment programs are ethical and feasible to advance research knowledge on the treatment of autism. This research is required before effectiveness claims can form the basis of public funding decisions regarding alternative program options.

PART I • REVIEW PROCESS

INTRODUCTION

Three British Columbia provincial government ministries, Health, Children & Families, and Education, jointly requested the British Columbia Office of Health Technology Assessment (BCOHTA) to assess the effectiveness evidence regarding a program of intensive behavioural therapy known as Lovaas treatment, for pre-school children with autism. The assessment request reflects particular issues raised in legal proceedings filed against the Government of BC.

The legal action, established as a judicial review in September 1999, alleges that the Government of BC is discriminating against children with autism by denying them their right guaranteed under the Canadian Charter of Rights and Freedoms to public funding of a specific treatment program. In particular, the petitioners in the action claim that children with autism are being denied reasonable access to a program of behavioural therapy, in this report termed ‘Lovaas treatment’, of sufficient intensity and early enough in their development to achieve, in some cases, normal adolescent and adult functioning, or at least a significant reduction in later disability.

In response to the request, BCOHTA conducted a systematic search for and critical appraisal of published scientific evidence, termed primary studies or data, regarding the impact of the Lovaas form of intensive behavioural therapy on the overall outcome of children with autism. In addition, a systematic search was undertaken for published secondary analysis both critical and supportive of this primary data.

The report focuses on the effectiveness evidence of the Lovaas treatment program, not of behavioural therapy, also known as applied behavioural analysis. Behaviour therapy is accepted as a benefit to children with autism. At issue is the large program, of which behavioural therapy is an essential component, that includes 40 hours per week of one-to-one therapy with specially-trained therapists, following a particular therapeutic manual.

BCOHTA is established to undertake research of this nature, and to do so it receives an unencumbered annual block grant from the BC Ministry of Health. The Office is not named in the lawsuit, nor do any members of the Office have a financial or professional interest in the provision of autism services. The annual block grant will not be affected by the outcome of the judicial review. The presented findings are based solely on the research evidence evaluated under BCOHTA’s method of systematic review, with open and fully reported methodology and criteria.

1. CONTEXT

1.1 Autism

Definition

Autism affects an individual's ability to socially interact with others, communicate, understand language, and play.¹ The World Health Organization² definition of autism encompasses the following areas of developmental abnormality:³

- 1) Social relatedness: abnormal social relationships and social developments
- 2) Communication: failure to develop normal communication
- 3) Imagination: interests and activities that are restricted and repetitive, rather than flexible and imaginative.

Autism is a lifelong neurological disability of unknown etiology.¹ In terms of pathophysiology, it is generally accepted that:

“[t]aken together, the available evidence in autism suggests that, although certain aspects of brain functioning are often spared in autism, the syndrome nevertheless involves widespread brain dysfunction at both the cortical and subcortical levels. The originating site of the brain injury has not been identified.”^{4 (p137)}

Autism is diagnosed in approximately 15 of every 10,000 children. With approximately 40,000 live births in BC each year, this translates into about 60 new cases diagnosed per year in the province, usually prior to school entry.

Diagnosis and incidence estimates

Recent years have seen the development of consistent criteria for the diagnosis of autism spectrum disorders in both the DSM-IV (American Psychiatric Association, 1994)⁵ and ICD-10.² The National Institutes of Health (NIH) consensus group reports that based on various field trials, “the clinical diagnosis of autism remains one of the most reliable diagnoses in psychiatric or developmental research.”^{4 (p123)}

These are recent developments, however, coming after the 1970s and 1980s when most reported longitudinal outcome studies were conducted. This is not to criticize earlier diagnostic scales, but what has to be examined is the consistent application of any given diagnostic scale during the course of an outcome study.

A more recent problem has emerged in relation to estimates of the incidence of autism. It appears that, at least in the BC context, problems with the diagnosis of autism may not simply be due to problems in the application of validated diagnostic criteria. The possibility arises that children with other pervasive development disorders may nonetheless be diagnosed with autism in order to gain access to services linked to that diagnosis.⁶ The extent of this phenomenon or its influence on incidence estimates remains unknown.

Long term outcome of children with autism

It is beyond the scope of the current project to conduct a systematic review of the scientific evidence regarding the long-term outcome of children with autism. (For the purposes of this report, the long-term outcome of children with autism refers to their psychological, behavioural, and social development.) It is nevertheless acknowledged that this must represent a significant element in any framework of understanding.

Clearly, however, the long-term outcome of children with autism is not uniform, nor is it well known. The weaknesses of current knowledge are documented in a report by the US National Institutes of Health, *The State of the Science in Autism*, which notes that “Only a few longitudinal studies of children with autism have been conducted.”^{4 (p141)}

Two review articles provide reasonable overviews of published longitudinal studies.^{7,8} Howlin combines the findings from studies conducted in the two decades prior to 1970 with the two decades since, and concludes that: “over the years, there has been improvement in the levels of functioning attained by people with autism.”^{7 (p55-6)} While the majority of individuals are ranked as ‘fair’ or ‘poor’, 10% - 20% of people in the latter years are in their own homes and in work. Howlin qualifies the conclusions by noting that “direct comparisons between studies are complicated, because of differences in methodology, in the subjects involved, and in data analysis.”^{7 (p55-6)}

Nordin and Gilberg⁸ provide a review article summarizing the findings from an unspecified number of longitudinal studies of children diagnosed with autism, childhood psychosis or autistic-like conditions, some of which were reported as early as the 1960s. Their summary corroborates the findings of Howlin that:

“These studies all yield remarkably consistent results ... In all of the studies published, a poor or very poor outcome with regard to social adjustment was seen in 60 - 75% of cases followed up to pre-adolescent or early adulthood. A good outcome (with near normal or normal social life and acceptable functioning at work or school despite certain difficulties in social relationships and oddities in behavior) was seen in 5 - 15% of cases.”^{7 (p55-6)}

This conclusion is supported by the work of Kanner. As reported by Howlin,^{7 (p55-6)} “[i]n his account of over 90 young adults who had first been diagnosed in childhood, Kanner noted that 11% to 12% of the group had done relatively well, despite receiving little in the way of specialist intervention or support”.

An appropriate conclusion from this longitudinal research is that, while most individuals with autism have poor to fair outcome, some individuals diagnosed with autism in early childhood ultimately do relatively well. A conservative assumption based on these available data is that any research regarding treatment effect bears the burden of proving that their study is not biased by preferentially assigning children among treatment groups.

It is worth dwelling on this point briefly for those unfamiliar with its significance to assessment of scientific validity. Any treatment will appear effective or perhaps curative if it is applied to the (relatively small) subset of children who will do well regardless of specific treatment. The appearance of treatment effect will be further enhanced if, at the same time as

children likely to benefit are assigned to receive treatment, children with poorer prognoses are assigned to a 'matched' control group.

Moderate bias in favour of the treatment group could have a very significant impact on conclusions of treatment effectiveness, especially when the study group is small. For example, biased assignment of 2 children out of twenty (10%) could have a 20% effect on assumption of treatment benefit, if a child in the treatment group achieves normal functioning, while the child in the control group remains significantly impaired.

The only valid safeguard against this form of bias, known as selection bias, is random assignment of children to treatment and control groups. Random assignment of children to treatment and control groups has been recently shown by Jocelyn et al⁹ to be both feasible and ethical in Canada, the most relevant context for this review. This study, although only 12 weeks in duration, is particularly relevant because it shows randomization is suitable for empirical validation of an early (24-72 months), intensive community-based integrated treatment program.

While their study is ground-breaking in this area of research, it was, as the authors note, too short to determine ultimate treatment effect. The authors also point to the need to assess timing, intensity and duration of their community-based program before drawing any effectiveness conclusions.⁹

1.2 Behavioural modification therapy

Behavioural modification therapy has its roots in the theory which holds that most behaviour is learned through interaction between an individual and the environment. The therapy applies techniques designed to affect this interaction and to improve behaviour. The so-called 'time-out', for example, is designed to provide a child with a mild negative experience so as to reinforce rules against disallowed behaviour.

Behavioural modification is to be distinguished from psychological or medical therapy. Psychological therapies aim to improve behaviour indirectly through developing individuals' understanding of themselves, their needs and desires. In the classic Freudian psychoanalytic model, successful intensive therapy would lead to pervasive and long-lasting change. But this therapeutic approach, in contrast to behavioural modification, is regarded as requiring intensive training by specialist therapists. Behavioural therapy on the other hand is more adapted for training parents and teachers to be part, and often the primary providers, of the therapy itself.

Medical therapy emphasizes biological determinants of human behaviour. Some proponents of this approach assert that most or all behaviour is dependent on physiological factors. The more generally-held and moderate view is that behavioural determinants include a combination of biological and learned elements. Pharmacotherapy, widely used in the US and Canada for behavioural disorders¹⁰⁻¹² is a medical therapeutic approach, but almost exclusively considered symptomatic treatment.

Applied Behavioural Analysis

Applied Behavioural Analysis (ABA) is a type of behavioural modification therapy. It was first defined in 1968 by Baer, Wolf, and Risley as “the process of applying sometimes tentative principles of behavior to the improvement of specific behaviors and simultaneously evaluating whether or not any changes noted are indeed attributable to the process of application.”¹³ It emphasizes social (as opposed to clinical outcome) measures, focussing on changes in an individual’s social interactions, rather than a score against a self-concept inventory. ABA also emphasizes ongoing ‘analysis’ of the behavioural therapy so as to ensure that the behavioural change is due to the therapy itself, and not to confounding factors such as influences from family or peers.

The actual method for this ‘analysis’ is not clearly defined. Rather, it is stated as a general principle of ongoing critical analysis of effect. Behavioural modification therapy therefore seems to use what might be described as an ecological model. Both individual behaviour and the context in which it occurs are regarded as amenable to change, with an open possibility of altering either or both so as to effect improvement in behaviour.

1.3 Scope of review

Under the terms of the request mentioned above, this assessment has a defined scope of inquiry.

At issue in this review are comprehensive behavioural treatment programs designed to alter the outcome in autism, and improve the overall functioning of affected individuals. These programs address multiple symptoms, involve various professionals and parents, and require thousands of hours of work over a period of several years.

Issues not addressed

This systematic review does not consider the literature on focal treatments directed either at reducing specific behavioural problems associated with autism, such as sleep disturbances and escape behaviours; or at increasing behavioural successes such as social interaction with peers and symbolic play. Following Rogers, among others, it is accepted that,

“(t)he literature on effective focal treatment in autism is plentiful and published in a variety of journals, in the fields of developmental disabilities, applied behavioral analysis, and discipline-specific journals ... Behavioral treatment approaches are particularly well represented in this body of literature and have been amply demonstrated to be effective in reducing symptom frequency and severity as well as in increasing the development of adaptive skills.”¹⁴

Matson et al¹⁵ draw similar limited conclusions in favour of behavioural therapy for children with autism. The authors describe 271 published studies evaluating behavioural techniques directed at target behaviour, which are divided into categories of aberrant behaviour, social skills, language, daily living skills, and academic skills. (It may be noted that among the techniques included is home-based, parent-mediated therapy - of particular interest here as one of a variety of parent-mediated therapies.)

Nor does this review address several important issues related to the provision of medical diagnosis, treatment, or support services and educational opportunities for children and youths with autism.

For example, as mentioned above, there may be significant problems associated with labelling children specifically with a diagnosis of autism, as opposed to other pervasive development disorders. Children so labelled may have preferential service access not available to those with other diagnoses, thereby raising potential for an exaggerated incidence of autism diagnosis.

Also unexamined are the challenges of sustaining standardized services across widely-dispersed geographic areas, such as exist in the province of British Columbia, other than to acknowledge, as the involvement of no less than three government ministries in developing an Autism Action Plan¹⁶ suggests, that a wide range of specialists and special services are required in this field; and that the demands placed on these resources are likely to be increased by the need to integrate the Autism Action Plan with government commitments to other special needs children and their families.

The focus on overall benefit

The Autism Action Plan which outlines the Government's current and proposed commitments to children and youth with autism and their families, highlights "Early intervention and treatment" as its first "Special Activity". It states:

"Research supports what many parents have known instinctively: that early intervention for children between the ages of 0 and 5 can provide tremendous and lasting benefits."¹⁷

The implication that early behavioural therapy can help to alleviate autistic symptoms in many if not most children and provide significant relief for parents is a relatively modest claim and as such is not in doubt. (Early intervention is here taken to mean starting therapy at ages 3 to 5 years when autistic symptoms have become manifest, and which is earlier than would otherwise be possible within regular, publicly-funded educational services such as kindergarten and main school.) To reiterate, the literature supporting assertions that behavioural treatment alters many symptoms associated with autism is not considered in this report, except to note that it is extensive, with the weight of evidence favouring benefit.

At issue in this report, however, is the stronger assertion, conceivably also to be inferred from the Autism Action statement, that scientific evidence supports behavioural therapy as a means to alter beneficially the natural course of the condition, or all the symptoms of the condition, and as a result a substantial number of children will achieve 'recovery' or 'cure'.

At this point, the type of scientific evidence needed to support this assertion is briefly discussed and illustrated to assist those unfamiliar with standard evaluative frameworks.

1.4 Evaluation framework

If the long-term outcome of a condition is well known, and predictably poor or fatal, then to prove any therapy capable of altering that outcome requires only a carefully documented case series observing treatment effect.

A relatively simple example of such an approach is to be found in Tay-Sachs disease, a rare ganglioside-storage disease, hitherto uniformly fatal in infancy. Any treatment for this condition resulting in increased longevity or a moderate level of intellectual and social functioning could be assumed to be due to treatment effect, and would therefore require no comparison group to substantiate the scientific validity of an effectiveness claim. The only issue of scientific validity is whether individuals receiving treatment were correctly diagnosed as having the relevant condition at the inception of the case series.

As the long-term outcome of a condition becomes more complex and unpredictable, however, studies designed to assess that condition require increasing complexity if they are to sustain validity. With autism for example, if some children ‘recover’, in the sense that they enter regular schools, and ultimately live independently in productive careers, proponents of a particular therapy must prove an effectiveness claim has not been biased by selecting for treatment those children who would have recovered with minimal, alternate, or less intense therapy.

To guard against such selection bias, study researchers should, theoretically, randomly select patients from a population under study. However, because random selection is seldom feasible for complex childhood developmental disorders, the best that researchers can achieve is to make explicit the extent to which their population is representative of a cross-section of individuals with the condition.

An additional opportunity to minimize selection bias arises through random assignment of children, however selected, to treatment or control groups. If researchers have systematically biased their study through selection of children who will do well regardless of treatment, this will become apparent in an exceptionally good outcome found in the control as well as the treatment group. In almost no circumstances can strong conclusions be drawn in the absence of random assignment.

The Task Force on Promotion and Dissemination of Psychological Procedures examined a broad range of interventions for childhood disorders, and concluded as follows:

“Given the advances in outcome methodology within the past 25 years ... it seemed appropriate to establish an absolute set of criteria for determining adequate study design and empirical support. For instance, because of threats to the internal validity of studies not using random assignment, random assignment in a group study was considered an absolute criterion.”^{18 (p141)}

A study design that includes randomized treatment and control groups, however, while necessary, is not sufficient to establish scientific validity. Several additional conditions need to be met, as called for by the NIH:

“Research is needed that uses robust experimental designs to evaluate and compare various approaches to treatment. Methods are needed that (a) involve random assignment to different treatment conditions; (b) use standard intervention protocols that capture a wide range of skills and symptoms, under both laboratory and ‘real life’ situations; (c) make use of outside evaluators who are not invested in the outcome of the research; (d) assure high compliance with the defined treatment protocol to be sure that the intervention as designed is actually and consistently implemented; and (e) use

longitudinal designs that evaluate treatment effects, both during the treatment itself, and at set points after the intervention has been accomplished.”⁴ (p149)

The importance of these additional conditions to the scientific validity of studies of children with autism is presented in section **2.2** below, and the extent to which the individual studies meet them presented in the subsequent analysis, **Part II**.

1.5 Research evidence versus human drama

The research issues examined above should not be viewed as being only of academic interest, with no bearing on the realities of life for those affected. Indeed, as has already been mentioned, the cost to individual children of poor research methods is not to be dismissed lightly.

One difficulty confronting appraisal research, of considerable importance in the present case, is that the complexity of detailed health technology assessment reporting is unlikely to achieve the impact of mass-media accounts of dramatic ‘cures’.

In the case of autism, the human drama projected by the popular press can be especially compelling. If a specific therapy or program holding out hope of ‘cure’ is presented to families facing daunting challenges, the impact on them is likely to be significant. And with sufficiently high a public profile, the potential exists to influence funding-decisions and ultimate program design, through policy-makers and program-providers, who after all are family people too.

In these instances, however, it is important to recognize that what may seem a compelling ‘association’ does not necessarily signify a ‘causation’. Causation requires substantiation through an analysis of a group effect, not by individual cases. This, in turn can only be derived from soundly-based research evidence. It is therefore the strength of the evidence that is at the heart of this review.

The authors hold the view, that while argument for or against any particular position may be advanced legitimately, if any intervention in a field which presents so many puzzling features is to be scientifically and impartially assessed, the integrity of the process must be paramount. The claims of individual need and the demands of public responsibility deserve no less.

2. METHODS

2.1 Search protocol

I. RESEARCH QUESTION

What is the effectiveness evidence that early, intensive behavioural treatment programs for pre-school children with autism result in improved overall outcome versus alternative management strategies ?

II. INCLUSION CRITERIA FOR PRIMARY DATA AND SECONDARY ANALYSIS

The following selection criteria were used to identify the primary clinical study reports:

1. Population of Interest

Human pediatric (pre-school) populations with autism; no exclusion due to presence of co-morbidity.

AND

2. Intervention

Reports were included if interventions were described as early, applied behavioural analysis, behavioural therapy **or** intensive, home-based program.

All studies examining some form of overall autism treatment program were included.

‘Early’ behavioural therapy was accepted as meaning initiation of therapy when the diagnosis is made and prior to age 5 when kindergarten services are available.

AND

3. Outcome measures

Reports were included if the study measured overall function, including:

- intellectual functioning;
- language;
- social interaction and play;
- adaptive or self-care skills;
- mal-adaptive behaviour;

Studies were excluded that were limited to “training trials.” Training trials are short intensive efforts to alter a child’s communicative or social skills in a particular domain.

AND

4. Types of studies

Reports were included if the study design included a treatment and a control group. Individual case reports and case series were excluded.

Articles providing critical appraisal of the primary study reports were also gathered. Critical appraisal is defined as: a systematic examination of the methodological quality of the reports providing primary study data.

III. SEARCH STRATEGY

Study reports were identified by searching computerized bibliographic databases covering traditional medical literature: Current Contents, Embase, HealthStar and Medline. A search strategy designed to identify primary analyses was combined with terms specific to autism.

The following Medical Subject Headings (MeSH with first letter capitalized) and text words (keywords with no capitalization) were used in the Medline and HealthStar searches for concepts which were combined.

1. Autism terms: “Autistic Disorder;” “autis*,” “asperger*,” or “kanner*,” “lovaas*” or “lovaas-oi [as author];
2. Co-authors of Lovaas or authors citing Lovaas: mceachin-j* or smith-t* or koegel-r* or russo-d* or rincover-a* or newson-c* or wilhelm-h* or reynolds-b* or litrownik-a* or mann-r* or simmons-j* or leiken-s* or schaeffer-b* or perloff-b* or anderson-s* or avery-d* or dipietro-e* or edwards-g* or christian-w* or birnbrauer-j* or leach-d* or rogers-s* or lewis-h* or reis-k* or sheinkopf-s or harris-s or kanner-l* or mesibov-g* or mundy-p* or perry-r* or cohen-i* or decarlo-r* or boyd-r* or gresham-f* or macmillan-d* or wainwright-sharp-j* or bryson-s* or howlin-p* or goode-s*
3. Intensive Behaviour Treatment terms: “Explode Behavior Therapy;” “Early Intervention (Education);” “early intervention;” “intense* or “intensive;” “discrete” or discreet” “trial learning;” “applied behavio* analysis;” “young autis*” “project” or study.”
4. Controlled and Uncontrolled Trials terms: “Prospective Studies;” “Multivariate Analysis;” “Risk Factors;” “Odds Ratio;” “Evaluation Studies;” “Clinical Trials;” “Clinical Trials, Phase I;” “Clinical Trials, Phase II;” “Clinical Trials, Phase III;” “Clinical Trials, Phase IV;” “Multicenter Studies;” “Comparative Study;” “Sampling Studies;” “Program Evaluation;” “Case-control Studies” “Cohort Studies;” “Cohort Effect;” “Longitudinal Studies;” “Follow-up Studies;” “Prospective Studies;” “Retrospective Studies;” “Cross-sectional Studies;” “Survival Analysis;” “Research;” “Research Design;” “multivariate analys*,” “risk factor*,” “odds ratio*,” “evaluation” “study or studies or trial*,” “comparison group design;” “historical cohort*,” “clinical or controlled” “study or studies or trial*,” “multicent* or multi-cent*” “study or studies or trial*,” “comparative” “study or studies or trial*,” “sampling” “study or studies or trial*,” “program* or programme*” “evaluat*,” “case control;” “cohort” “study or studies or analys* or trial* or effect*,” “longitudinal” “study or studies or analys* or trial*,” “follow-up or followup” “study or studies or analys* or trial*,” “prospective” “study or studies or analys* or trial*,” “retrospective” “study or studies or analys*,” “concurrent” “study or studies or analys* or trial*,” “incidence” “study or studies or analys* or trial*,” “cross-sectional” “study or studies or analys* or trial*,” “case series;” “case” “study or studies or analys* or trial*,” “case design;” “time series;” “survival” “study or studies or analys*,” “uncontrol* or uncontrol*” “study or studies or analys* or trial*,” “non-control* or noncontrol*” “study or studies or analys* or trial*,” “observational” “study or studies or trial*,” “research design*,” and, as document type, “clinical trial;” “clinical trial, phase I;” “clinical trial, phase II;” “clinical trial, phase III;” “clinical trial, phase IV;” “controlled clinical trial;” “multicenter study.”

The equivalent database-specific subject headings were used in the other databases searched. Text words only were used in Current Contents, as no subject headings are applied in this database.

Databases searched were * Medline (1966-1999), HealthStar (1975-1999), Embase (1988-1999), CINAHL (1982-1999), Current Contents (1996-1999), and combined Science and Social Sciences Citation Index (1989-1999).

References of retrieved articles were reviewed to identify further relevant citations.

The search results were reviewed independently by two reviewers. Inclusion criteria given above were applied. When differences arose as to whether an article was relevant to the review, the disagreements were resolved by discussion. All articles that appeared to meet the criteria were requested in full text form. The **Bibliography** at the end of this report lists the articles obtained.

IV. FUGITIVE LITERATURE SEARCH

A search strategy similar to that adopted in the mainstream search was applied to the fugitive literature search, designed to identify primary analyses which combined terms specific to autism. Where possible, Medical Subject Headings (MeSH) and text words were used, but other subject headings were also applied to accommodate the indexing used in specific databases. These subject headings were derived from Library of Congress or in-house indexing terms.

Commercial databases:

The following commercial databases were searched:

1. Cochrane Library
2. HSTAT (technology assessment guidelines)
3. HSRProj (NLM)
4. Dissertation Abstracts
5. Article1st (OCLC)
6. Papers1st (OCLC) – conferences and paper abstracts
7. TRIP database (evidence-based medicine)
8. Ebsco Academic Search
9. Ebsco Canadian MAS
10. Best Evidence
11. CPG Infobase
12. CRISP
13. National Guideline Clearinghouse Database
14. HTA Database

In-house databases:

1. In-house catalogue

Web library catalogues:

The following Web Library Catalogues were searched using Library of Congress or National Library of Medicine (MeSH) subject headings:

1. UBC Library Catalogue
2. BC Ministry of Health Library Catalogue
3. Canadian Institute of Scientific and Technical Information (CISTI) Catalogue
4. Belinda Database (Buckinghamshire Health Authority Library)
5. WorldCat
6. GAO Web Catalog
7. LocatorPlus (NLM Catalog)
8. Health Canada Library Catalog
9. New Zealand Health Library Catalog
10. Royal Society of Medicine Library Catalogue

Internet peer-reviewed sites:

1. UK Academic Web Directory
2. UK Social Science Information Gateway
3. OMNI (Organising Medical Networked Information)
4. Medical Matrix
5. Health Communications Network
6. Global Health
7. Health Index
8. Medweb Public Health

Internet search engines:

1. Northern Lights
2. Google
3. Altavista

Directories:

1. ECRI. HealthCare Standards
2. UHC Technology Assessment Monitor

Organizations contacted:

1. Autism Society of BC
2. Centre for the Study of Autism
3. National Institutes of Health
4. British Columbia. Ministry of Social Services
5. American Speech, Language and Hearing Association
6. National Autistic Society
7. Families for Early Autism Treatment
8. Clark Institute of Psychiatry
9. CMHS National Mental Health Services Knowledge Exchange Network
10. United Kingdom. Department of Health
11. Autism Research Institute
12. National Alliance for Autism Research
13. National Institute of Neurological Disorders and Stroke
14. National Institute on Deafness and Other Communication Disorders
15. National Institute of Mental Health
16. New York. Department of Health

2.2 Critical appraisal of study methods

BCOHTA critical appraisal methods require two independent researchers to assess study validity using predetermined standard criteria. The criteria applied are those highlighted by the NIH^{4 (p148)} and emphasized by other authors offering critical appraisal of the primary effectiveness data,¹⁹ namely that a study should:

- 1. Compare various approaches to treatment**
- 2. Involve random assignment to different treatment conditions**
- 3. Use standard intervention protocols that capture a wide range of skills and symptoms, under both laboratory and ‘real life’ situations**
- 4. Make use of outside evaluators who are not invested in the outcome of the research**
- 5. Assure high compliance with the defined treatment protocol to ensure that the intervention is actually and consistently implemented as designed**
- 6. Use longitudinal designs that evaluate treatment effects, both during the treatment itself, and at set points after the intervention has been accomplished.**

Detailed appraisal of each criterion follows accepted clinical epidemiology standards as outlined by Sackett et al.²⁰

Study details were extracted independently by the two BCOHTA researchers. Complete agreement was achieved regarding study design features. Effectiveness data was also extracted independently by the two BCOHTA researchers, also with complete agreement regarding results. Both BCOHTA researchers also read, discussed and agreed on the incorporation into the BCOHTA report of the critical appraisal findings of other authors.

PART II • SEARCH & CRITICAL APPRAISAL FINDINGS

3. SEARCH FINDINGS

The systematic search (electronic and reference lists) resulted in the identification of approximately 1200 abstracts and citations in total. These were reviewed independently by two researchers. Approximately 150 articles met the minimum inclusion criteria and were retrieved. The criteria were then re-applied to the full published reports to determine appropriateness for full critical appraisal.

General features of the retrieved articles meeting the criteria are summarized in **Table 1**. Part **A** of the **Table 1** lists research contributing primary data, and these studies are further described in **Table 2**.

No studies comparing alternative behavioural intervention programs, or randomized trials of behavioural intervention programs were identified as meeting the inclusion criteria.

Four controlled studies of treatment programs were identified that reported overall outcome for children.²¹⁻²⁵ The primary data from Lovaas (1987)²¹ and McEachin et al (1993)²² are appraised in section **4**. The two study reports will be reported and discussed together, since the report by McEachin et al is a long-term follow-up study based exclusively on the research reported in 1987 by Lovaas. The three further studies identified are appraised in section **5** below.

Studies contributing secondary analysis are listed in Part **B** of **Table 1**. The first section includes systematic reviews of effectiveness data in which the authors conducted critical appraisal. ‘Critical appraisal’ is defined broadly so as to include any reasonable evaluation of study design, procedures, analysis and outcome measures. The requirement for critical appraisal did, however, serve to exclude most review articles, which uncritically cite original primary research findings.

The second section of Part **B** includes all the published critiques and comments regarding the primary study by Lovaas (1987)²¹ and McEachin et al (1993).²² This latter category is considered essential because Lovaas (1987)²¹ and McEachin et al (1993)²² are the only authors to date claiming to have evidence that their treatment program resulted in children attaining ‘normal functioning’. The material serves both to determine the place of these effectiveness claims within the autism research community, and to obtain additional study details not originally included in the published trial reports.

Table 1: Summary of search findings

A. PRIMARY DATA		
Trials comparing alternative behavioural intervention therapy	Randomized trials of behavioural intervention therapy	Studies of early (pre-school) intensive behavioural therapy with a control group
None	None	<ol style="list-style-type: none"> 1. Lovaas 1987;²¹ (McEachin et al '93)²² 2. Birnbauer and Leach (1993)²³ 3. Sheinkopf and Siegel (1998)²⁴ 4. Ozonoff and Cathcart (1998)²⁵
B. SECONDARY DATA		
Systematic reviews that include critical appraisal of the primary data	Critical appraisal debates following Lovaas 1987²¹ with McEachin et al 1993²²	
<ol style="list-style-type: none"> 1. Rogers (1998)²⁶ 2. Tregear et al (2000)²⁷ 3. Howlin (1997)⁷ 4. Green (1996)²⁸ 5. Smith (1998)²⁹ 	<ol style="list-style-type: none"> 1. Schopler et al (1989);³⁰ response by Lovaas et al (1989)³¹ 2. Boyd (1998);³² response Smith and Lovaas;³³ response Boyd (1998)³⁴ 3. Gresham and MacMillan (1997);¹⁹ response Smith and Lovaas (1997);³⁵ response Gresham & MacMillan (1997)³⁶ 4. Gresham and MacMillan (1998)³⁷ 5. Schopler (1998);³⁸ 6. Mesibov (1993);³⁹ Mundy (1993);⁴⁰ Kazdin;⁴¹ Baer (1993);⁴² Fox (1993);⁴³ response by Smith et al (1993)⁴⁴ 7. Connor (1998)⁴⁵ 	

Table 2: Description of studies providing primary data

REPORT	DESIGN	SUBJECTS	INTERVENTION	OUTCOME
Lovaas 1987 ²¹ (McEachin et al '93) ²²	Prospective: 3-7 yrs. Assigned based on location / therapists	Experimental: n = 19 Control 1: n = 19 Control 2: n = 19 Age less than 40-46 mths.	Min. 40 hrs. Lovaas therapy	Blinded Experimental: 9/19 normal functioning Control: 0/19 normal functions
Birnbaauer and Leach (1993) ²³	Prospective: 2 yrs. Matched control	Experimental: n = 9 Control: n = 5 Mean age 39 mths.	Mean 29 hrs. Lovaas therapy	Not blinded Experimental: no normal functioning 4/9 achieved IQ ≥ 89
Sheinkopf and Siegel (1998) ²⁴	Retrospective: 21 mths. Matched control	Experimental: n = 11 Control: n = 11 Mean age 33 mths.	Mean 27 hrs. Lovaas therapy	Intake blinded Experimental: no normal functioning mean IQ 25 pts higher
Ozonoff and Cathcart (1998) ²⁵	Prospective: 10 -12 wks. Assigned first 11 children to Experimental; next 11 children Control	Experimental: n = 11 Control: = 11 Mean age 33 mths.	10 home sessions TEACCH therapy	Not blinded Experimental: no normal functioning Experimental: improved 9 months on developmental scale

4. CRITICAL APPRAISAL OF LOVAAS (1987)

The principal citation identified in the search, and the study which underpins the claims made for the Lovaas treatment, is the 1987 research report by Lovaas (1987),²¹ together with the long-term follow-up reported by McEachin et al (1993).²² This section provides a detailed account of this study and its critical appraisal using the criteria listed in 2.2 above.

The remaining reports identified as offering primary data from controlled studies are examined in section 5.

4.1 Lovaas (1987), McEachin et al (1993) study appraisal

Study design

The description presented here includes details found both in the Lovaas (1987)²¹ and McEachin et al (1993)²² study reports as well as material presented earlier as part of other program accounts, and subsequently, in response to questions and criticism.

This study was a prospective, non-randomized group study, comparing an active treatment group to two control groups: one selected from children referred from the same source as the treatment group and one group selected from the same source, but not referred to the Lovaas treatment centre.

Assessment at enrollment was conducted by psychology graduate students, ‘independent’ from the investigators. Assessment at enrollment was not described as blinded; that is, acceptance or rejection for active treatment could be known by parents. In the case of 5 children, assessment was conducted in conjunction with the children’s parents.

Assessment at the initial outcome stage, school entry, in contrast to enrollment, was conducted by researchers blinded to treatment allocation.

Final outcome of school placement, IQ, and social functioning was conducted by research staff not blinded to treatment allocation; the 9 children who achieved the best outcome, termed ‘normally functioning’, were also reassessed using independent, blinded outcome assessors.

Inclusion criteria

As stated in the original trial report: “Subjects were enrolled if they met three criteria: (a) independent diagnosis of autism from a medical doctor or licensed PhD psychologist; (b) chronological age (CA) less than 40 months if mute and less than 46 months if echolalic^[*]; and (c) prorated mental age^[**] (PMA) of 11 months or more at a chronological age (CA) of 30 months. The last criterion excluded 15% of the referrals.”^{21 (p4)}

* echolalia means automatic repetition of what is said

** prorated mental age is defined by Gresham and MacMillan^{19 (p189)} as “a psychometric scaling procedure. The PMA adjusts for variations in mental age scores as a function of the child’s chronological age at the

Almost all children had clinical diagnoses of autism, made by an independent agency prior to contact with the project.

Group allocation method

“... subjects were assigned to the experimental group unless there was an insufficient number of staff members available to render treatment (an assessment made prior to contact with the family).”^{21 (p4)}

“The assignment to groups was made on the basis of staff availability. At the beginning of each academic quarter, treatment teams were formed. The clinic director and staff members then determined whether any opening existed for intensive treatment. If so, the next referral received would enter the experimental group; otherwise the subject entered the control group.”^{22 (p361)}

McEachin later adds detail that: “the first referrals for the study were all assigned to the experimental group due to the fact that referrals came slowly (7 in the first 3.5 years) and therapists were available to treat all of them.”^{22 (p362)}

Population selected

- An experimental treatment group of 21 children diagnosed with autism (2 lost to follow-up after 6 months).
- Population selected over a 15-year period.
- First control group consisted of 19 children referred for therapy, but for whom there were not sufficient therapists available for a full intervention program; received 10 hours or less of 1:1 treatment. Two children assigned to this group because they lived more than a one-hour drive from the treatment centre.
- The second control group consisted of 21 children selected through a chart review from the same referral source, and who did not receive any of the intervention program.

Study groups were found not to have any statistically significant differences in the following dimensions:

- chronological age at first diagnosis
- chronological age at onset of treatment
- prorated mental age
- sum pathology (total of 8 measures of severity derived from parent interviews)
- abnormal speech
- self-stimulatory behaviour
- appropriate toy play
- any recognizable words

The only significant difference reported was a difference in the chronological age at onset of treatment. Control group 1 subjects were 6 months older on the average than experimental subjects (mean chronological age of 35 months vs. 41 months, respectively).

time of test administration. The PMA assumes a chronological age (CA) of 30 months, irrespective of the child's actual chronological age, and is calculated according to the formula: $PMA = MA/CA \times 30$.”

Therapeutic intervention

The intervention was reported as 40 or more hours per week of 1:1 behavioural therapy for 2 or more years delivered largely by university students. A succinct summary is provided by Rogers:

“The therapy consisted of operant teaching techniques, mostly reinforcement but also some punishment techniques, used to teach a wide range of social, language, cognitive, and self-care skills, as well as to reduce inappropriate behaviors. The initial phase of treatment was delivered largely in the homes, with parents also trained and carrying out the treatment. As the children progressed, behavioral teaching was delivered in community settings and typical preschools as well, with considerable focus on generalization and appropriate social behavior. The treatment protocol was written up in a manual,^[46] and instructional videotapes were also developed. The students delivering treatment were trained didactically in the principles of applied behavior analysis in college courses and then trained to deliver treatment to the children through advanced trainers. There was tight control of the treatment delivery by those who had developed the treatment program.”^{26 (p172)}

Lovaas adds that therapists require extensive theoretical and practical experience, as well as noting that the “treatment effects could not be replicated without (contingent adversives)”.^{21 (p8)}

Outcome measures

Initial outcome measurements were conducted between the ages of 6 and 7, when a child would normally have completed first grade. IQ was measured using seven different IQ tests. Subjects were also scored as to their extent of normal functioning. Subjects received a ‘3’ for normal functioning if they received an IQ score in the normal range, completed first grade in a normal class in a school for normal children, and were advanced to the second grade by the teacher.

Later follow-up of children in the experimental group and control group 1 (McEachin et al 1993)²² consisted of:

- school placement ascertained from parents;
- intelligence test (differing between children with and without verbal ability);
- the Vineland adaptive Behavior Scales (administered to parents to assess child’s ability to cope effectively with the everyday environment);
- the Personality Inventory for Children (administered to parents to assess child’s psychological state and presence of disturbances).

Findings

i) *At school entry, age 6-7* (Lovaas 1987)²¹

Clinically and statistically significant difference between groups on both overall outcome variables:

- IQ = 25-30 point increase favouring treatment versus either control groups;
- functioning well in typical first grade classrooms without any special support: experimental treatment group (47%) versus controls (2%).

Note that nearly half of the experimental group were termed “recovered”, having IQ scores in the normal range (mean = 107; range 94-120) and passing through first grade in a regular classroom. School personnel described these children as “indistinguishable” from their peers. However, no data was provided to substantiate this latter claim.

ii) *Late childhood/early adolescence* (McEachin et al 1993)²²

- IQ = 30 point increase favouring experimental treatment (mean 84.5); control group 1 (mean 54.9)
- education in regular placements: experimental treatment 47%, controls (0%)
- adaptive behaviour scores favour the experimental treatment group.

McEachin et al conclude that the best outcome group of children sustained their beneficial overall outcome.

4.2 Critical appraisal findings of Lovaas (1987), McEachin et al (1993)

For consistency with the published literature in this area, the critical appraisal issues will be presented as answers to the NIH criteria^{4 (p148)} set out in section 2.2. The discussion includes the perspective found in the published literature (systematic reviews and debates) as well as that of the BCOHTA reviewers.

1) The study should compare various approaches to treatment

The Lovaas study does not compare two different therapies, but only compares two levels of intensity in a single form of behavioural intervention. There were insufficient details provided from the second control group to determine what therapies were applied. The BCOHTA researchers noted that the 10 hours of 1:1 therapy was not found to result in any children achieving normal functioning.

2) The study should involve random assignment to different treatment conditions

Group assignment was not randomized. Lovaas et al³¹ assert that the only criterion for assignment was availability of therapists and facilities. The research team determined if sufficient resources were available, and if so, then the next referred case was assigned to the experimental treatment group.

Baer^{42 (p373)} offers the only independent support for the group assignment method used in the Lovaas study. He states his conviction that the Lovaas method of group assignment, if based solely on predetermined resource availability, was essentially equivalent to random assignment. The researchers would have no input to group assignment and bias could only result from referring physicians.

He concludes that, because the children were so young and autism is so unpredictable, that referring physicians could not be “sensitive enough” to bias the treatment group to contain higher-functioning children with autism. However, support for unbiased group assignment is negated by Lovaas and his colleagues who admit that group assignment was also based on family factors, geographic location and stage of research.

All other authors providing critical appraisal of the Lovaas study raise serious concerns regarding drawing effectiveness conclusions, in the absence of randomization. Rogers raises

the most general and arguably most important point regarding randomization in studies with small sample sizes. She notes: “Non-randomization is particularly problematic in studies with small sample sizes which are inherently at greater risk of group inequivalences that cannot be detected using the usual statistical measures.”^{26 (p172)}

Schopler et al³⁰ provide the most aggressive challenge to Lovaas (1987) in terms of the timing and method of group selection. They ask whether this study should be termed an experimental study at all, or should more correctly be termed a ‘*post hoc* analysis’, that is, an incidental finding in a trial designed to answer a different question.

Schopler et al³⁰ point to an earlier publication by Lovaas⁴⁷ which states that patient assignment was based on differences in the abilities of the family to provide the treatment. Families with restrictions such as divorce, personal problems, or full-time employment commitments were assigned alternate treatment placement of children in facilities. The implication is that the families may have differed between the treatment and the control groups, contrary to what is claimed by Lovaas. If so, the differences in findings may equally be the result of differences in families rather than in the amount of treatment applied.

In response to these criticisms, Lovaas et al asserted^{31 (p166)} that the only criterion for assignment was availability of therapists and facilities. They explained that low-income and working families accepted the intervention because therapists were in their homes during most of the day providing support and care for their children.

However, despite this response by Lovaas, the BCOHTA researchers consider Schopler’s point to be significant. Questions remain about how the study was actually conducted at various stages:

- Did the researchers follow a strict research protocol, established prior to enrollment of the first child, which included details of group assignment under various circumstances? In other words, how did the researchers deal with multiple more or less simultaneous referrals?
- How was one child selected for a single available experimental therapy placement?
- Similarly, how did the researchers deal with subsequent group assignment if all 7 of the initial referrals over the first 3.5 years were entered in the treatment group?
- Were later stages of the research project influenced by earlier stages? Several of the initial seven subjects assigned to treatment would have entered school before subsequent students were enrolled. An excess number of students were evidently assigned to the control group owing to the unbalanced initial assignment, and the authors do not explain how this was achieved.

Subject selection bias

One of the primary concerns in attempting to replicate the treatment effect outside the original study is the ability to select similar children for treatment.

The children studied by Lovaas were not randomly sampled from the population of children with autism. Rather they were referred to the treatment program following clinical diagnosis in an affiliated, but independent institution. Therefore, they may not represent a cross-section of children with autism.

Of greater concern is whether Lovaas provided experimental treatment to ‘high functioning’ children with autism, 47% of whom achieved normal functioning. In other words, were the enrolled children representative of the population or were they selected consciously or semi-consciously by the referring centre because they were seen to have exceptional potential?

Schopler et al³⁰ list characteristics of the experimental treatment group that, in their words “skewed (it) toward relatively high-functioning autistic children”:

- i) Lovaas used a prorated mental age to “exclude many subjects with intellectual functioning higher than the profoundly retarded range”^{21 (p4)} that Lovaas suggested was the only group excluded.
- ii) Lovaas only included children between 40-46 months if they had echolalia, “a symptom widely recognized (also by Lovaas 1981)⁴⁶ as a characteristic of autistic children with a better prognosis”. Lovaas notes, in response to Schopler,³¹ that 4 children met these criteria for a better prognosis, but they were equally distributed among the experimental and control groups.
- (iii) Lovaas selected children with an IQ of 63, “considerably higher than any random sample of autistic children”.³⁰ Schopler also claims that Lovaas used IQ measurements that under-estimate IQ. Lovaas and Schopler disagree regarding normal epidemiological IQ levels, and whether the Lovaas experimental group were representative.

Mesibov³⁹ raises similar concerns about subject selection. He states:

“Concerns about the representativeness and comparability of the sample group are raised because (a) different cut-off ages were used for ecolalic and mute children, and the authors did not state how these ages were selected; (b) the control group had fewer higher functioning clients that one would expect in groups of this size: typically, 20% to 30% of people with autism are higher functioning, irrespective of the services that they receive; and c) different testing protocols were used for clients in different groups.”^{39 (p380)}

Boyd (1998)³² provides the most cogent and unanswered criticism of selection bias found in Lovaas 1987. Boyd noted that the sex ratio in control group 1, the group for which there is most outcome data (McEachin 1993)²² does not reflect the sex ratio found in a standard autism population. The sex ratio, accepted by both Boyd and Lovaas, is 3 or 4:1, boys to girls.

Of the 19 children in each group, there are 3 girls (16%) in the experimental group and 8 girls (42%) in control group 1. The ratios are 5.3:1 for the experimental group, and 1.4:1 for control group 1. Boyd goes on to cite evidence, not refuted by Lovaas or his colleagues, that “a number of studies have shown autistic females to have lower IQ scores than males”.^{39 (p212)} One study, Volkmar et al (1993), cited by Boyd showed: “Significant sex differences were evident with IQ but not on measures of autistic symptoms, with girls having significantly lower IQ scores. The sex ratios for IQs below 35, 35-69, 70 or above were, respectively, 2.84:1, 3.41:1, 2.7:1.”^{32 (p212)}

Boyd argues that, when the general population sex proportions are known, the control group sample should be compared to that population proportion. Using this assumption and a simple chi-square test statistic, he demonstrates that control group 1 is statistically significantly different from a population of children with autism. Indeed, he argues (forcefully, in the opinion of the BCOHTA researchers) that the much higher percentage of girls in the control group could account for most of the difference in the experimental and control groups.

Boyd³² and Smith & Lovaas³³ agree that the effect of sex on the outcome of active treatment is unknown. However, Smith and Lovaas provide no adequate explanation for how they dealt with the biased control group 1. Furthermore, Smith and Lovaas argue incongruously that the differences between the experimental and control groups were not significantly different, ignoring the need to establish the cell frequencies based on population sex ratios.

Smith and Lovaas compound the inadequacy of their response by noting individual effects, not group effects. They state: “the girls with autism who received intensive treatment in the Lovaas (1987) study fared quite well. Two of the three achieved ‘normal functioning’ at age 7, and at age 12 (McEachin et al, 1993).”^{33 (p343)} In other words, the fact that two girls achieved normal functioning does not diminish the importance of recognizing that, on average, girls do not do as well as boys.

In summary, the issue of whether the Lovaas (1987) experimental treatment sample was representative is unanswerable without additional research, using random selection if possible, or at minimum, random treatment allocation. At this point it is only possible to say that the best outcome children made very significant gains over the course of the study.

3) The study should use standard intervention protocols that capture a wide range of skills and symptoms, under both laboratory and ‘real life’ situations

This criterion is met by Lovaas et al and has not been at issue in the published literature, nor for the BCOHTA analysts. The Lovaas method of intensive behavioural intervention relies on a published manual providing extensive details of treatment definitions and protocols.⁴⁶ Therapists providing Lovaas treatment can also receive extensive theoretical and practical training. In addition, the Lovaas method manages a wide range of intellectual and social skills in various residential, community and home situations. Indeed, the Lovaas method is credited with helping to move therapy beyond the clinic and the laboratory into real life settings such as homes and the community. It should be noted, however, that while criterion 3 is clearly met, criterion 6 (details of the application of that method) is not.

4) The study should make use of outside evaluators who are not invested in the outcome of the research

The extent to which outside evaluators were used by Lovaas at baseline has remained controversial. Gresham and McMillan note that, at intake, 5 children were not evaluated by outside evaluators, but by the parents.³⁶

Smith and Lovaas claim outcome assessment at school entry “the evaluators were hired by a psychologist unaffiliated with the UCLA project and told that they would be working on a study of child development. As an additional precaution against their discovering the purpose of the study, the evaluators assessed equal numbers of children with no history of clinically significant behavioral disturbances.”^{35 (p202)}

Gresham and McMillan note that outcome in terms of school performance and behaviour was reported by parents and teachers, not by outside evaluators. Outside evaluation was limited to IQ testing of the 9 children from the active treatment group who achieved the highest level of functioning.³⁶

5) The study should assure high compliance with the defined treatment protocol to ensure that the intervention is actually and consistently implemented as designed

Treatment integrity, also known as reliability of treatment implementation, refers to the extent to which a treatment was actually applied. Gresham and MacMillan provide the most detailed critique of this aspect of the Lovaas 1987 study. They state that the “failure to monitor the degree to which treatment procedures were implemented as planned across therapists and subjects calls into question the internal validity of these findings”.¹⁹

Gresham and MacMillan¹⁹ consider the lack of detail in actual adherence to treatment protocols as a significant problem in terms of ‘construct validity’, that is, the causal explanation linking independent treatment variables to dependent variables in terms of autism outcome. Clearly-defined treatment activities are essential if treatment effect is to be distinguished from extraneous variables influencing the outcome of children with autism.

Gresham and MacMillan observe, “The (Lovaas) 40-hour-or-more treatment protocol was supposedly responsible for dramatic behavior change, and the 10-hour-or less treatment produced no effects. This suggests that the dramatic treatment effects for the experimental group were due primarily to the amount or dose of treatment rather than to the discrete trial training procedures per se. Without integrity data, we cannot know what aspects of the (Lovaas) treatment supposedly make it work”.^{19 (p196)}

BCOHTA researchers concur with Gresham and MacMillan. In neither the Lovaas 1987 study report, nor in the subsequent discussions do Lovaas et al provide details of which elements and how much of the therapeutic intervention individual children received. Also omitted are details of the type and amount of alternative (perhaps pharmaceutical) therapies received by the experimental group. For example, the study authors do not detail how many children in control group 1 received 10 hours per week of treatment and how many children received less; nor do they detail what additional community treatment the children may have received.

Replication of the Lovaas (1987) treatment benefit requires replication of the comprehensive treatment model including parents, therapists and pre-school institutions. This could be extremely problematic, as Gresham and MacMillan observe. “Absent the same level of expertise, training, and close supervision, one can only wonder whether favorable outcomes, let alone ‘normal functioning’ can be achieved at sites being required to provide the [Lovaas, UCLA treatment program].”^{37 (p10)} Further research is clearly needed to refine the treatment instrument and determine a relationship between specific treatment activities, duration and child benefit.

Of equal concern in attempting to reconstruct the treatment effect is the timing of treatment. Lovaas explains that the experimental group, on average, began treatment approximately 6 months earlier than the control group (34.6 months versus 40.9 months). However, no details were provided regarding the time when individual children started treatment; how long they had treatment; or when follow-up assessment occurred. It is possible that a child began treatment at age 2, had treatment for two years, but was not examined in school until 3 years later.

In summary, despite the detailed material regarding the Lovaas method, the research reports provide virtually no details regarding the actual application of that material. In fact, the *absence* of description of the amount, quality, and consistency of the applied behavioural analysis techniques is one of the most striking aspects of Lovaas and McEachin's work.

6) The study should use longitudinal designs that evaluate treatment effects, both during the treatment itself, and at set points after the intervention has been accomplished

The Lovaas study meets this criterion in that it is a longitudinal design which evaluates treatment effects. At issue, however, is how the treatment effect was evaluated.

Several authors strongly criticize the way that Lovaas evaluated treatment effect. Schopler et al see an excessive use of classroom placement and IQ as outcome measures. They state: "Classroom placement may have less to do with changes in the child than with the policies of the school system toward special-needs children".³⁰ Lovaas et al respond that placement is determined independently in this geographical area. If children had autism, then the school district would have placed them in separate schools.

Schopler et al also note that "Improvement on IQ measures may reflect improvement in compliance rather than in cognitive functioning. Higher scores on follow up IQ tests will then reflect improved test-taking skill rather than improved intellectual functioning."^{30 (p162)}

Mesibov similarly notes that "Many skills required for normal functioning have not been measured by McEachin et al (1993). They did not report on the students' social interactions, friendships, conceptual abilities, and social communication skills, skills likely to differentiate children with autism from their peers without handicaps. These are important aspects of the autism syndrome and deserve more scrutiny in further studies."^{39 (p380)}

Mundy similarly states: "In summary, the interpretation of the McEachin et al (1993) data set should be done cautiously and be constrained by the limits of the outcome measures. The outcome measures used may not have been of sufficient breadth or specificity to address the issue of whether the best-outcome group displayed remission of autistic symptoms or displayed symptoms typically presented by high functioning children with autism."^{40 (p383)}

Gresham and MacMillan³⁷ raise different concerns regarding the measurement instruments used in Lovaas 1987, particularly at baseline. While it is outside the expertise of BCOHTA researchers to interpret the validity of this critique or the responses by Lovaas et al, these comments are included because they raise important issues regarding the baseline characteristics of the study population.

Gresham and MacMillan^{19 (p190)} provide a technical critique (similar to that of Schopler³⁰) of the use of a scaling procedure termed the 'prorated mental age' (PMA). They challenge Lovaas et al to explain several details of the procedure, so that its validity might be assessed. Gresham and MacMillan¹⁹ note that the original trial report does not provide sufficient detail to determine individual children's ages, mental ages and how these were combined with the IQ tests.

Lovaas (1987) only reports averages for each of the groups. Gresham and MacMillan summarize their concerns: ‘we simply do not know how to interpret the posttest IQ results given that posttest measures primarily were scaled as deviation IQs and pretest scores were based on a PMA using a potpourri of development scales’.^{19 (p189)}

Several authors^{30,31,37,40,44} discussed the relative merits of various tests of psycho-social development. Again, these issues are beyond the expertise of BCOHTA researchers, who confine comment to agreeing with the more generally valid methodological observation made by Gresham and MacMillan that: “all pretest measures should have used the same scale and that all posttest measures and follow-up measures should have used the same scale”.^{36 (p221)}

4.3 Summary of critical appraisal of Lovaas et al (1987)

The BCOHTA critical appraisal agrees with other commentators that the study by Lovaas (1987)²¹ and McEachin (1993)²² suffers from several major methodological limitations. These limitations include non-random assignment of children to treatment and control, an unrepresentative control group 1 in terms of sex ratio, and inadequate documentation of treatment integrity.

In addition, different assessment tools were used on the children at baseline and administered under non-standard conditions; and follow up measures limited to IQ tests may have missed residual problems. As a result, the BCOHTA reviewers conclude that it is very difficult to rely on the effectiveness claims made by Lovaas et al.

BCOHTA reviewers and other commentators also raise serious concerns regarding the external validity (reproducibility of treatment effect in contexts outside the original study setting) of the Lovaas study. The principle concern is the extent to which Lovaas therapy requires restriction to a higher functioning sub-group of children with autism. Lovaas admits that the poorest functioning 15% of children referred were excluded, while Schopler et al estimate that applying the Lovaas inclusion criteria in their context would eliminate 57% of referrals.^{30 (p163)}

Of equal concern in reproducing the Lovaas treatment effect is that, while the Lovaas intervention model offers extensive documentation, the Lovaas report provides almost no details of actual treatment activities, of staff training, or of quality control.

5. CRITICAL APPRAISAL OF OTHER PRIMARY STUDIES

This section appraises three additional studies reporting primary data on early (pre-school) intensive therapy, with a control group. The first two, Birnbauer and Leach (1993)²³ and Sheinkopf et al (1998)²⁴ both evaluated the effectiveness of Lovaas therapy. The third, Ozonoff et al (1998)²⁵ evaluated the effectiveness of a home-based therapy program.

The following critical appraisal of these studies utilizes the same NIH conference criteria applied in the previous section (listed in section 2.2 above). Comments from other published systematic reviews with critical appraisals are added.

5.1 Birnbauer and Leach (1993)

Description

This was a prospective study that treated 11 children, but only reports on 9 (5 boys), with a mean age of enrollment of 39 months, all with DSM-III diagnosis of autism, or pervasive development disorder. The non-treated control group consisted of 5 boys who were ‘younger’, but ‘matched’ on all other variables.

At intake, children were assessed in terms of IQ, adaptive behaviour, personality, and parental stress. They were repeated every 12 months during therapy. Final outcome data were collected at 24 months.

The authors did not conduct any group analysis. They report that, after 24 months, 4 of the 9 treated children had non-verbal IQs of 89 or higher; the language levels of the treated group doubled that of controls. All children in both groups continued to have symptoms of autism at the end of 2 years. For example, both groups continued to show marked levels of stereotypic behaviour and poor toy-play. Of the 5 controls, one made marked improvement and 4 made mild to moderate improvement.

1) The study should compare various approaches to treatment

No comparison of treatment options.

2) The study should involve random assignment to different treatment conditions

Not randomized. In fact, the sample size is so small and the method of determining a control group so weak, this study more closely resembles an uncontrolled case series than a controlled experiment.

3) The study should use standard intervention protocols that capture a wide range of skills and symptoms, under both laboratory and ‘real life’ situations

The intervention utilized the Lovaas treatment manual,⁴⁶ and reports a mean of 29 hours per week of therapy at home. However, the therapy was carried out by a variety of trained volunteers.

4) The study should make use of outside evaluators who are not invested in the outcome of the research

The authors state that all the children had clinical diagnoses of autism made by an outside agency. The authors provide no details regarding diagnostic tests, criteria or reliability.

The standardized tests and behavioural assessments during the study period were carried out by examiners blinded to patient assignment. Behavioural assessments were tested for inter-rater reliability.

5) The study should assure high compliance with the defined treatment protocol to ensure that the intervention is actually and consistently implemented as designed

No description of compliance; no testing of treatment application.

6) The study should use longitudinal designs that evaluate treatment effects, both during the treatment itself, and at set points after the intervention has been accomplished

The study makes no effort to relate the amount or quality of therapy to clinical outcome. It is prospective, but limited to the time of active treatment. The study does not report on individual patient trends over the course of therapy.

Other systematic reviews with critical appraisal:

In her systematic review, Rogers provides this summary comment regarding Birnbauer and Leach:

“This article represents an independent replication of Lovaas’ model, and reports positive outcomes associated with the treatment. However, some difficulties limit its usefulness as a replication. The children received less than half of the number of treatment hours that Lovaas reported. There was no information regarding the fidelity of the treatment model delivered by the therapists. No long-term follow up data were reported, and there were no statistical analyses of group differences on pre- and post-treatment measures. The high-improvement group was defined by their non-verbal IQ scores in the 85 or above range. However, non-verbal IQ is a problematic outcome measure. Positive changes in non-verbal IQ do not necessarily reflect widespread gains in children with autism because the measures tap an area of strength in autism - visual-perceptual reasoning - and do not measure areas of definitive functioning. There was also a considerable amount of missing IQ data. Two of nine treated children and three of five control children had no IQ data at all, and full IQ protocols were reported on only four of nine treated children and one of five controls. Moreover, the mean developmental rate of the treated group in the area of adaptive behavior was actually below that of the control group.”^{26 (p173)}

In his systematic review, Smith notes:

“Children ... received less intensive therapy than children at UCLA (an average of 18-25 hr/week, instead of 40). Also, service providers received less supervision from senior staff and, at one site (Birnbauer et al) appeared to have substantially less experience, typically leaving after 13 weeks of providing 2.5 hours of treatment per week (a total of 32.5 hours) ... (T)he investigators reported that their group of nine children made significant gains, but when the results are expressed as IQ, it appears

that the group showed little improvement ... (It is evident that, at least under some circumstances, behavioral analytic early intervention programs may fail to yield clearly positive results.”^{48 (p38)}

BCOHTA comment

The BCOHTA researchers noted that this study report is remarkably similar to Lovaas 1987²¹ and McEachin 1993²² in that, while both cite references to Lovaas treatment with its extensively documented protocols and manuals, they provide remarkably few details on what therapy actually took place. The lack of validation of treatment integrity undermines claims of a causal link to overall outcome and limits the ability to replicate the study in an independent context.

Summary of Birnbauer and Leach

This study is considered too small, too short and too methodologically weak to provide strong evidence for or against the effectiveness of Lovaas therapy. Particularly noteworthy is the fact that it does not validate the remarkable success rate reported in Lovaas 1987²¹, which reported that half of subjects achieve ‘normal functioning’. In fact, none of the children in the study reported by Birnbauer and Leach functioned in the normal range on any parameter, in some cases following 24 months of therapy.

5.2 Sheinkopf and Siegel (1998)

Description

This is the second published study, along with Birnbauer and Leach, which used a controlled study methodology to assesses independently Lovaas treatment effectiveness.²⁴ This is a retrospective study of 11 children, all diagnosed with autism or pervasive developmental disorder, with a mean age of 33 months and a mean IQ of 63.

The authors did not use a prospective, experimental design. In fact, the researchers had no role in group assignment at all. Parents ‘independently’ chose to instigate Lovaas therapy. The researchers then ‘selected’ these children retrospectively for inclusion from a larger longitudinal study.

The study report provides no indication of the total number of children who were independently started on Lovaas therapy, to what degree, or how many of this larger set formed the subset for inclusion. The active treatment children (receiving Lovaas treatment) were matched pair-wise with community controls.

Study findings

The treatment group had a mean IQ 25 points higher than the control group (mean of 90 versus mean of 64; $p = .01$). All 10 children in the treatment group with outcome data improved on IQ measures, usually by 20 or more points. In the control group 6 children improved by small amounts to moderate amounts, four had lower scores, and one stayed the same.

The authors found no group differences on number of symptoms; the treated group had decreased symptom severity ($p = .01$). Ten of 11 children in the treatment group and 11/11 in the control group still had a diagnosis of autism at the end of therapy. No information was provided regarding language development, adaptive behaviour, academic or social functioning.

1) The study should compare various approaches to treatment

No comparison of intensive treatment alternatives. The control group received ‘community’ therapy, poorly defined.

2) The study should involve random assignment to different treatment conditions

No random assignment. This was not an experimental design, but a retrospective observational study. Moreover, as set out, the method of control group construction is incomprehensible: “Matching was accomplished by reverse serial selections of any case matching an index case, with controls drawn from a database of approximately 1,000 children surveyed from most recent cases, backward.”

3) The study should use standard intervention protocols that capture a wide range of skills and symptoms, under both laboratory and ‘real life’ situations

The intervention is described as consisting of 1:1 instruction based on the Lovaas manual⁴⁶ and the supervision of one of three community therapists and parents.

4) The study should make use of outside evaluators who are not invested in the outcome of the research

The initial diagnoses and assessments were made by the primary researchers for all children. The authors note that the initial data on the children was collected prior to the researchers’ awareness that the parents in the treatment group had, presumably independently, decided to begin Lovaas therapy. Post-treatment assessments were also made by the researchers. No statement is made on the issue of blinding final outcome assessments.

5) The study should assure high compliance with the defined treatment protocol to ensure that the intervention as designed is actually and consistently implemented

The report describes use of Lovaas behavioural treatment in home and regular community treatment; mean 27 hours per week for a mean of 21 months. However, there is no documentation of treatment fidelity. Data regarding treatment was gathered retrospectively through “phone interviews with one parent for each child.”^{24 (p18)}

The issue of treatment fidelity is made even more problematic in terms of training and quality control, as “(t)he authors of (the) paper were not involved in treatment implementation.”

All children also received a wide range of community and professional services, “in an equal amount.” The presumed difference leading to the subject benefit was that the treatment group received a mean of 20 hours per week of one-to-one treatment. The control group received a mean of 11 hours of treatment (not necessarily one-to-one).

6) The study should use longitudinal designs that evaluate treatment effects, both during the treatment itself, and at set points after the intervention has been accomplished

The study used only IQ measures (mainly non-verbal), number of symptoms, and severity of symptoms. Rogers' remarks²⁶ on the difficulties posed by non-verbal IQ measures are noted in the commentary on Birnbauer and Leach (p29 above).

Comments of other systematic reviews with critical appraisal:

In her systematic review Rogers states: "As with the Birnbauer and Leach (1993) study, there was some evidence of reduced severity of symptoms of autism in the experimental group after two years of treatment, but virtually all children continued to meet criteria for autism."^{26 (p174)}

By contrast, the systematic review by Smith⁴⁸ provides no critique of the study methodology used by Sheinkopf and Siegel. Instead he uncritically cites the findings by Sheinkopf and Siegel as showing IQ benefits for the treatment group using Lovaas therapy.

The systematic review by Green, similar to Smith's, accepts the findings of Sheinkopf and Siegel as providing "evidence that intensive behavioral intervention increases the intellectual functioning (as measured by standardized, objective tests) of many young autistic children."^{28 (p36)} She notes, however, that the study provides no details regarding the treatment applied, and that there is no evidence that any children achieved normal or near normal functioning.

Additional BCOHTA comment

The study uses a very weak, partially retrospective, observational study design. Although providing some support of benefit from the Lovaas method, Sheinkopf and Siegel²⁴ did not report any children as 'normal' or approaching normal functioning. Indeed, owing to the virtual absence of description of treatment effect or of central co-ordination of the intervention, it is impossible to determine any causal inference relating to the limited gains reported for these children.

5.3 Ozonoff and Cathcart (1998)

This is a study of 11 children treated with a 10-week, home program service based on a form of therapy termed "Treatment and Education of Autistic and related Communication-handicapped Children" (TEACCH).²⁵ TEACCH is a long-established special education agency formed in the 1960s and located in North Carolina, serving children with autism. Most children receiving the TEACCH program are involved in classroom settings.^{49 (p46)}

TEACCH, in contrast to Lovaas therapy, considers its method involves "structured teaching" as opposed to behavioural therapy. As Smith explains: "In contrast to behavioral treatment, Project TEACCH is aimed primarily at designing sheltered settings that help children make use of the skills they already possess, rather than at helping children to enter more 'normal' or 'typical' settings."^{49 (p46)}

In the Ozonoff study, the home program service was added to the regular day-treatment programs attended by all the children.²⁵ The regular day-treatment programs in this part of the USA are described by the authors as consisting of the ‘discrete trial method’ of applied behaviour analysis used in Lovaas therapy. Children in this study thus were receiving intensive therapy following two different treatment orientations.

The home program involved 10 treatment sessions, in the clinic and home, designed to assist parents in becoming co-therapists. All families received treatment during the same 4-month period. Children were age 2 - 6 (2 girls and 9 boys).

Findings

Children in the treatment group gained on 4 of 7, and on total score in the Psycho-educational Profile - Revised developmental test. The treatment group gained 9 months on the developmental scale during 4 months of therapy.

The study seems to have been published too recently for consideration in other systematic reviews of controlled trials of early intensive treatment. Therefore the critical appraisal comments are almost exclusively from BCOHTA reviewers.

1) The study should compare various approaches to treatment

No comparison of home-based approaches. In this instance, the researchers compared the addition of home-based treatment to regular school-based treatment.

2) The study should involve random assignment to different treatment conditions

Not randomized. The study enrolled the first 11 families to respond to an advertisement to the treatment group, and the next 11 to the control group.

3) The study should use standard intervention protocols that capture a wide range of skills and symptoms, under both laboratory and ‘real life’ situations

The home-based program, although based on TEACCH philosophy, “did not follow a specific time-line, packaged protocol or manual, but emphasised individualized, structured teaching.”^{25 (p28)}

4) The study should make use of outside evaluators who are not invested in the outcome of the research

The study relied exclusively on internal evaluators, aware of treatment allocation, to determine intervention effect. The study does not report the method of clinical diagnosis.

5) The study should assure high compliance with the defined treatment protocol to ensure that the intervention is actually and consistently implemented as designed

Parents were not taught a pre-defined set of skills.

6) The study should use longitudinal designs that evaluate treatment effects, both during the treatment itself, and at set points after the intervention has been accomplished.

The study appropriately used the same test for pre- and post-intervention assessments. (Psycho-educational Profile - Revised developmental test). The study was limited to assessment of cognitive and developmental parameters. It did not examine adaptive behaviour, behaviour problems, or social functioning.

Additional BCOHTA comment

The BCOHTA authors agreed that this study is worth considering because, although the intervention period is only 10 - 12 weeks and it suffers from many methodological flaws, it is a prospective controlled trial using overall outcomes that studied an intensive, home-based treatment alternative to Lovaas therapy.

The authors' conclusion seems appropriately conservative given the methodological weakness found: "The results of this study suggest that auxiliary home interventions increase developmental functioning in young autistic children, above and beyond gains due to school-based services."^{25 (p31)}

5.4 Summary of critical appraisal of corroborative research

The effectiveness claim found in Lovaas et al (1987),²¹ that half the children achieved normal or near-normal development and placement in schools, remains uncorroborated by independent research.

In fact, while improvement in IQ and autism symptoms were described for most children, *none* of the children in two other controlled trials (Birnbauer and Leach;²³ Sheinkopf and Siegel²⁴) achieved a normal development stage by the end of therapy. It should be noted, however, that in both these instances the children received about half (20 hours per week) the intervention intensity described in the original Lovaas study (1987).²¹

There are no controlled trials comparing the Lovaas method with alternate intensive, one-on-one therapies. The controlled study by Ozonoff,²⁵ although short and small-scale, provides at least some evidence that alternate, home-based programs, in this instance emphasizing parent teaching as opposed to applied behavioural analysis, need further evaluation both independently and in comparison with Lovaas treatment.

The published effectiveness evidence does not include any studies which examined overall outcome following early diagnosis using a screening manoeuvre. That is, studies to date have not followed a population of children diagnosed 'early' in the natural history of the condition to determine both the benefits from early treatment and the costs associated with false-positive and false-negative diagnostic labelling.

The need for adequate outcome trials will increase as screening tools are proposed. To date, the cost and benefit of screening tools remain completely unknown, although the literature does show preliminary work toward establishing the cost and benefit of early (age 2) versus later (age 3) diagnosis.

For example, Lord⁵⁰ followed for one year a cohort of 30, age-2 children referred for possible autism to determine how many received false-positive and false-negative clinical diagnoses. Other researchers have conducted similar analyses of the validity of early diagnostic programs.^{51,52} None of these studies, however, is linked to treatment or to overall outcome.

Support for ‘early’ therapy should therefore be directed toward early treatment of children known to have autism, and not toward therapy of children receiving an ‘early’ diagnosis. Rogers similarly concludes: “The hypothesis that age at start of treatment is an important variable in determining outcome has tremendous implications for the field and needs to be tested with methodologically-rigorous designs.”^{26 (p176)}

6. DISCUSSION

The published effectiveness evidence on overall outcome from intensive behavioural therapy for pre-school aged children with autism gives rise to two problematic issues. The first is the inconsistency between the Lovaas effectiveness claims and subsequent accounts of similar treatment success. The second is the degree to which the effectiveness claims have been advanced despite these inconsistencies.

6.1 Lovaas effectiveness claim

Lovaas 1987²¹ (with McEachin et al 1993²²) in essence claim that by applying their method for 40 or more hours per week to higher-functioning children with autism (that is, omitting the most retarded 15%), approximately half of the children will achieve IQ measures in the normal range and placement in regular schools, and will also be described by their teachers and peers as indistinguishable from other children.

Although providing insightful self-criticism of his study methodology and the need for additional research, Lovaas nevertheless uncritically promotes the assertion that his method virtually results in normal functioning. The following exemplifies this stance:

“Like any other study, ours needs to be replicated by independent investigators, and it could have been improved in several ways. For example, we assigned subjects to groups on the basis of therapist availability rather than on the basis of a more arbitrary procedure (such as tossing a coin) because parent advocacy groups protested arbitrary assignment. However, arbitrary assignment might have precluded the suspicion that we somehow biased our groups. Also, further studies may show how to simplify or shorten the intervention and how to increase the proportion of autistic children who attain normal functioning.”^{31 (p167)}

Mesibov raises the principal concern with this approach: “... readers might jump to the conclusion that the children have been cured. This has been an unfortunate consequence of other presentations and studies published by these authors. Although their results are impressive, they fall far short of demonstrating normal functioning.”^{39 (p380)}

In addition, Rutter, an influential researcher in the autism field, concurs that the effectiveness claim is overstated:

“The [Lovaas (UCLA) treatment program] claims have given rise to controversy, sometimes heated, but existing empirical evidence does not provide a resolution. There are reasons for being cautious about the acceptance of these strong claims (if only because of the uncertainties of diagnosis in very young children and because the claims of ‘cure’ run counter to both clinical experience and what might be expected from prevailing theories) ... doubts about the claims should lead to further studies to test them rather than a dismissal on the basis of existing evidence.”⁵³

The BCOHTA researchers accept the later, more moderate statement by the authors. In this passage, Smith, McEachin and Lovaas summarize points of agreement with critics of the Lovaas 1987 study:

“First, our best-outcome subjects appear to have made significant gains. Second, we have made a plausible case for attributing these gains to our treatment instead of extraneous factors such as spontaneous recovery. Third, this finding needs to be replicated because no single study by itself can give conclusive evidence on the efficacy of a treatment. Finally, replications should not simply rehash what has already been done.”^{44 (p385)}

6.2 Failure of corroborative efforts

Published controlled studies, as documented in section 5 above, do not report children achieving normal functioning as defined by Lovaas 1987²¹ (McEachin 1993).²² Furthermore, uncontrolled studies, although a weaker form of evidence than controlled studies because they do not account for the development process outside therapy, similarly do not support conclusions of ‘normalization’ through Lovaas therapy.

For example, Anderson et al,⁵⁴ who applied the Lovaas treatment method more rigorously than any but the original trial itself, concluded that while 6 of 14 children studied showed IQ gains of 20-22 points over 2 years, all children continued to need special education services and none were mainstream in a regular kindergarten or school.

Two other uncontrolled studies, Harris et al (1991)⁵⁵ and Fenske et al (1985),⁵⁶ reached similar conclusions. Harris et al report on a centre-based intensive behavioural therapy program for nine children after one year, and conclude that “all treated children continued to demonstrate impairments after treatment”.^{26 (p175)} Fenske et al reported on nine pre-school children after 2 years of intensive treatment at the Princeton Child Development Centre. Rogers notes that, “no information was given about the children’s level of functioning in public school classes or any other outcome variables.”^{26 (p175)}

Systematic reviews with critical appraisal similarly fail to find support for Lovaas’ effectiveness claims:

- a) Rogers: “[Studies] did not demonstrate the level of improvement in multiple areas of functioning or the sustained long-term effects of the treatment that Lovaas reported. The field awaits a full, independent replication of the Lovaas study.”^{26 (p176)}
- b) Tregear et al (ECRI*): “Although it appears possible to improve some aspects of function in autistic children, it is not clear that any one program is more effective than another.”²⁷
- c) Howlin: “For the present, Lovaas programme clearly confirms the power of behavioral interventions. The true extent of the benefits, however, still requires greater exploration and longer term evaluations, covering many other aspects of functioning are needed if the true cost-effectiveness of the time, effort and energy expended by families is to be adequately assessed.”^{7 (p61)}

* ECRI is a non-profit health-services research agency whose reports are not in the public domain but are available for purchase. <<http://www.healthcare.ecri.org>>

- d) Green, in the systematic review that conducted the least critical appraisal, draws the strongest conclusions in favour of Lovaas treatment. She states that “applied behavioral analysis is the treatment of choice, it should have at least 30 hours of 1:1 therapy per week by individuals with extensive training, and it will result in very significant cost savings over the lifetime of individuals with autism.”^{28 (p42)}
- e) Smith, a co-author of the McEachin study along with Lovaas, provides a somewhat cautious interpretation of the research evidence in terms of achieving normal functioning:

“It is encouraging that debates over how much and what kind of early intervention children with autism should receive have largely replaced debates over whether such intervention merits particular attention at all.”^{48 (p45)}

However, he also states in relation to his study (McEachin et al 1993)²² “... some outcome studies indicate that a major breakthrough may have occurred. Nevertheless, these studies require replications with improved research methodologies and, even if interpreted in the most favorable possible light, reveal substantial shortcomings in the interventions under investigation. Therefore, much research remains to be done.”^{48 (p42)}

The absence of corroborative evidence of ‘recovery from autism’ does not devalue the effectiveness of early, intensive and comprehensive treatment programs in achieving significant developmental gains. However, Lovaas and his research colleagues have not limited their effectiveness claims to achieving developmental gains. Instead, they have permitted and even fostered the premise that appears throughout the published literature, associating their therapy with a notion of achieving ‘normal functioning’ for as many as half a given population of children with autism.

Despite the lengthy existence of the treatment report (available for almost thirty years) and the dramatic effectiveness claim based on it (first aired thirteen years ago), while the published literature does offer some support for the Lovaas effectiveness claims through case reports of individual success (some well documented^{28 (p36)}), the literature has virtually no accounts of groups of children entering programs and ‘recovering’ from autism.

6.3 Does the evidence point a way forward?

As asserted at the outset, the aim of this review has been to evaluate the available evidence regarding the effect of overall treatment programs on the long-term outcome of children with autism. The BCOHTA authors do not promote or discredit any of these complex programs, developed in particular contexts for a wide range of social, professional and economic reasons. The simple point to make here is that the outcome evidence does not support adopting a single entire program, whether the Lovaas or alternative models, based on the likelihood of achieving normal children.

It should be mentioned at this point that the source of current difficulties does not lie principally with the Lovaas and McEachin study reports, nor should the shortcomings identified here and elsewhere be taken as a dismissal of the primary research.

On the contrary, this work deserves significant commendation. The researchers have taken steps, unprecedented at the time of the research, to ensure objective and independent

assessments of their subjects, examined long-term outcome, and established control groups under what seem to be extremely difficult study circumstances. It is also to be noted that the research and the treatment program described were ground-breaking in their orientation toward early, intensive, home (non-institutional) treatment of children with autism.

Because of the major methodological flaws, however, the findings of these researchers cannot be regarded as conclusive. Rather, their contribution might have been expected to fuel further hypothesis generation. While this has occurred to some degree, the work has to date been insufficient to bring the science to what might be described as an authoritative level.

Nevertheless, the notion that Lovaas therapy, if properly applied, can result in normalization of children with autism has gone so far as to become the rallying point for planning autism services (in BC and elsewhere), rather than, as might be expected, the central focus of further research aimed at examining an uncorroborated effectiveness claim.

Yet it is the very absence of subsequent research, and, in that absence, extrapolation from this single provocative study to reach generalized conclusions regarding treatment effect, (at the extreme, the possibility of cure for half of children with autism) that has given rise to the present program-planning dilemma. A scientific claim of effectiveness is being used to force action, but the same scientific evidence is inadequate to provide any guidance on how to proceed.

Given the gulf that seems to separate the sides in this debate, how are the courts, policy makers, and most importantly the families whose daily lives are doubtless filled with unanswered questions, to make practical use of the available evidence?

To help overcome the uncertainties, payers and program-planners may benefit from following two steps. First, it will be important to identify an authoritative, objective and reliable means of monitoring the state of effectiveness evidence. Second, in the absence of scientifically-valid studies of overall autism programs, it seems prudent to revise existing autism services using incremental, not programmatic steps.

6.4 Monitoring the state of effectiveness evidence

To monitor the state of effectiveness evidence, it is suggested that program-planners and public-payers identify:

- i. a group that uses explicit methodology to appraise the validity of individual clinical trials and to situate any valid effectiveness evidence in a rigorous evaluative framework; but
- ii. a group that is *not* asked to create clinical practice guidelines.

The American Psychological Association, Task Force on Psychological Intervention Guidelines¹⁸ is one such group. The Association has provided a clear methodology to determine effectiveness evidence and to draw conclusions regarding the quality of that evidence, without seeking to develop guidelines based on clinical opinion.

With regard to Lovaas therapy, the Task Force group state that this program of therapy has not been established as efficacious according to their criteria: “To date, no comprehensive integrated intervention for autism has sufficient support to achieve either well-established or

probably efficacious status partially because of concerns and problems with conducting randomized assignment group designs and a lack of strong replication studies.”^{18 (p142)}

Although not supporting a Lovaas program, the Task Force supports specific, non-comprehensive behavioural interventions (not dealing with overall outcome) as having sufficient empirical support. In other words, they support the position that applied behaviour analysis has been shown to alleviate the symptoms of autism, but not the condition itself.

The problems associated with developing guidelines, as opposed to clarifying the state of scientific evidence, are exemplified by The New York State Department of Health, Clinical Practice Guidelines.⁵⁷

The New York Guidelines give their strongest endorsement (Level A)* that “principles of applied behavioral analysis and behavioral intervention strategies be included as an important element of any intervention program for young children with autism.”^{57 (Ch4, p3)} Although no actual evidence is specified, it seems reasonable to accept this recommendation is based on the strong evidence of symptomatic improvement, but not cure rates.

However, the Guidelines go on to make several more Level A recommendations regarding frequency, intensity and duration of intervention that seem essentially to accept uncritically elements of the Lovaas treatment program. They recommend that intensive behavioural programs include as a *minimum* approximately “20 hours per week of individualized behavioral intervention using applied behavioral analysis techniques (not including time spent by parents).”^{57 (Appendix C, p1)}

On the other hand, they recommend excluding certain aspects of the initial Lovaas treatment program that have become socially unacceptable. For example, the authors recommend against use of physical aversives as part of the applied behavioural analysis program, but without noting that this may conceivably have been an essential component of the initial Lovaas (1987)²¹ treatment success.

Nor do the Guidelines provide effectiveness evidence to support their recommendation for either 20 hours per week of intensive therapy, or the requirement of a 1:1 therapist-child ratio. The guideline authors state that the precise number of hours should be adjusted for individual children, but provide no guidance on what outcome measures should be used to increase, decrease, or discontinue behavioural therapy altogether.^{57 (Appendix C, p1)}

This is not to direct particular criticism at the NY Clinical Practice Guidelines. In fact, they are similar to most guidelines developed by clinical experts, in that while acknowledging the limitations of the clinical effectiveness evidence, they tend to make recommendations that extend far beyond what the evidence itself will support. As noted by national organizations responsible for clinical practice development, unsubstantiated extrapolation from limited clinical effectiveness evidence is the major drawback of the clinical practice guideline movement.⁵⁸

* Level A = Strong evidence is defined as evidence from two or more studies that met criteria for adequate evidence about efficacy and had at least moderate applicability to the topic, and where the evidence consistently and strongly supports the recommendation.^{57 (Ch1, p4)}

6.5 Taking incremental, not programmatic steps

Public payers faced with an urgent need, therapeutically, politically, and legally, to invest in comprehensive, pre-school services for autism have no way of knowing which elements of which form of early, intensive treatment are necessary and sufficient to best benefit children and their families. Therefore, the only evidence-based direction forward requires incremental, not programmatic steps. Ultimately, specific behavioural therapies, shown effective, can be matched to local needs and the abilities of local therapists.

Taking incremental steps based on available evidence may in fact already be the primary force shaping autism services. Dawson and Osterling,⁵⁹ for example, describe a convergence of approach found in US programs. They note that regardless of divergent philosophies of therapy, eight independent programs have developed around a common model of early, intensive therapy for children with autism, which include parental involvement.

The “elements of effective programs” Dawson and Osterling describe have been further defined in a prospective study, funded by the Office of Special Education, US Department of Education.⁶⁰ Based on expert opinion and survey results, they identify several key elements shared by all “nationally known models or program” as the “earliest possible start to intervention; individualization of services for children and families; systematic playful teaching, specialized curriculum; intensity of engagement; and family involvement.”^{60 (p21)}

7. CONCLUSIONS

The authors of this systematic review concur with the observations of the American Psychological Association Task force (6.4 above), and in addition draw the following conclusions:

1. The Lovaas et al (1987)²¹ and McEachin (1993)²² study, while methodologically stronger than published reports of alternate comprehensive therapies, is inadequate to establish the degree to which this form of therapy results in children achieving 'normal' functioning, however defined.
2. There is insufficient effectiveness evidence to establish a relationship between the amount (per day and total duration) of any form of early comprehensive treatment program and overall outcome.
3. Randomized trials of alternative early intensive treatment programs are ethical and feasible to advance research knowledge.
4. There is insufficient effectiveness evidence to conduct a cost-benefit analysis of early, intensive treatment programs in terms of 'normalization' of children diagnosed with autism. It remains the case that without a soundly-based determination of the extent to which the intervention may result in benefit, and the degree of any such benefit, cost-benefit analyses have no basis on which to proceed.

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