Parent-mediated early intervention for young children with autism spectrum disorders (ASD) (Protocol)

Oono IP, McConachie H, Honey EJ

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Parent-mediated early intervention for young children with autism spectrum disorders (ASD)

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ABSTRACT

This is the protocol for a review and there is no abstract. The objectives are as follows:

The objective of this review is to determine the level of current evidence about the extent to which parent-mediated early intervention has been shown to be effective in the treatment of young children with autism spectrum disorders. In particular, it aims to assess the effectiveness of such interventions in terms of the benefits for both children and their parents. Another key interest we have in this review is to explore current approaches to measurement of adherence in the implementation of parent-mediated intervention strategies. This is important as we do expect that, like most forms of therapy, compliance is key to getting the desired outcome. Finally, we will also consider categorisation of the signs or features described by the various publications with the aim of exploring moderators of treatment effect.
**BACKGROUND**

**Description of the condition**

Autism is the core disorder of the pervasive developmental disorders (PDD) as defined within the International Classification of Diseases and Related Health Problems, 10th edition (ICD-10) (WHO 2010) and the Diagnostic and Statistical Manual of Mental Disorders IV, Text Revision (DSM-IV-TR) (APA 2000). It is evident before the age of three years. Children with autism have impairments in the areas of communication and social interaction, with repetitive behaviours and lack of imagination. The prevalence of core autism is accepted to be around four per 1000 (Baird 2006). Autistic characteristics are, however, understood to be on a continuum rather than being a distinct category, with clinical agreement on a range of difficulties that can be classified as ‘autism spectrum disorders’ (ASD). The prevalence for all ASD is around 11 per 1000, with a male:female ratio of 3.3 to 1 (Baird 2006). Young children with difficulties on the autism spectrum pose challenges to family members and others who interact with them: they lack understanding of how to initiate and respond to joint attention with another person, have difficulties in social timing of communication and may not understand other people’s intentions as expressed through language and gestures, even though they may appear affectionate and want to be with other people socially. They have difficulty with organising their responses and with inhibition of repetitive behaviours and interests. Children with ASD frequently pose considerable behaviour challenges to their parents and other family members.

**Description of the intervention**

Previous literature has demonstrated the effectiveness of a number of early intervention programmes (Dawson 1997; Rogers 1998a; Smith 1999), as have more recent reviews of the evidence (Osипина 2008; Rogers 2008), but the evidence base is still weak. The field of ASD remains controversial, with a range of questionable claims for the efficacy of therapies and few successful replication studies. ASD intervention programmes vary considerably in their theoretical background (Prizant 1998). Some approaches use applied behaviour analysis (ABA) or early intensive behavioural interventions (EIBI) in intensive programmes that are based at home and delivered primarily by trained therapists (for example, Howlin 1987; McEachin 1993). Others have an educational framework, such as project TEACCH (Treatment and Education of Autistic and related Communication handicapped Children), with an emphasis on structuring class environments through visual cueing, communication routines and individual tasks (for example, Lord 1994). The TEACCH project aims to increase children’s independence and is designed to work on their existing strengths rather than focusing on weaknesses. Other programmes emphasise the creation of naturalistic communication opportunities, enhancing reciprocity between communication partners, enhancing children’s motivation for social interaction and prompting specific social behaviours (for example, Rogers 1991; Koegel 1995; Aldred 2004). Research in this area has typically involved children aged between one and seven years (for example, Koegel 1996; Jocelyn 1998; Smith 2000); and although these programmes may differ in content, they all advocate treatment implementation as early as possible as a matter of clinical urgency. The age at which intervention starts has been reducing in the last decade with the advent of earlier identification and diagnosis of ASD (Charman 2010). However, a diagnosis of ASD, based on behavioural observation, is not given before the age of 12 months, as children’s impairments in social communication cannot be interpreted until the second year of life at the earliest.

Some reviews have suggested that successful programmes have important similarities, whatever their apparently different theoretical foundations (for example, Dawson 1997; Prizant 1998). Indeed, Rogers conducted a comparative analysis of the elements of apparently contrasting programmes to demonstrate how each may address the underlying neuropsychological processing difficulties evidenced by children with ASD, including, inter-subjectivity (interpersonal sharing including establishing joint attention to objects), emotional functioning and imitation (Rogers 1998b). It has been suggested that programmes tend to be successful in early intervention for ASD if the agenda is intensive, involving a significant number of hours per week and if rigorous levels of structure and instruction are introduced into the child’s world (Eldevik 2009). Programmes may thus have to effect a change throughout the family in order to successfully treat ASD.

The involvement of parents in implementing intervention strategies designed to help their autistic children has a history stretching back at least four decades (for example, Schopler 1971). Within the ASD treatment literature there have been a number of studies that evaluate specific parent training approaches in dealing with behaviour problems (for example, Howlin 1987; Viendi 2011), in improving parent-child interactions (for example, Koegel 1996; Dawson 1997; McConachie 2005), in facilitating communication (for example, Prizant 1997; Aldred 2004) and in implementing a behaviour analytic approach (for example, Smith 2000). In addition, there are evaluations of the added value of parent involvement to a day-care or nursery programme (for example, Jocelyn 1998; Rickards 2007). A review by Boyd 2010 concludes that “many of the promising focused intervention practices and comprehensive treatment models (CMTs) involve components of naturalistic interventions for teaching pivotal skills in natural environments and parent-implemented approaches where caregivers learn strategies to better support their children’s development”. Thus, there is clear need for studies of parent-mediated early intervention in ASD to be collated and summarised in a systematic review in order to evaluate the strength of the evidence.
How the intervention might work

Training parents as ‘therapists’ allows intervention to begin early, with the aim that parent interaction strategies help enhance children’s earliest social relationships. It is important, given the nature of the children’s impairments, that parents support the child in establishing shared interest in each other and in objects, and learn the power of imitation. If parents act in a way that is ‘synchronous’ with their child’s focus and intentions, then language and communication are enhanced (Siller 2008). The secondary effects may be reduced frustration for the child, as well increased parent confidence and skills. Increased parental skills allow for continual opportunities for child learning in a range of situations. Furthermore, training parents in new skills has frequently been carried out in groups, allowing for mutual support and potential reduction of parental stress.

Why it is important to do this review

In the last decade there has been an increase in publications that seek to address issues surrounding early identification and early interventions for children with ASD (Boyd 2010). From 2009 onwards, there has been a surge in the publication of randomised controlled trials (RCTs) of early intervention where parents are key to the delivery. In addition to improvement in research design, improvements in screening studies have enabled accurate detection of ASD at an age earlier than was documented about 30 years ago (that is, some children are now diagnosed as early as two years of age). However, doubt remains about which interventions are effective for young children with ASD and what constitutes an objective assessment of improvement. Furthermore, the increased number of studies raises a potential opportunity to link ASD and other characteristics to the outcomes of intervention, but these developments have not yet led to consensus on important questions for healthcare policy makers and parents alike. What intervention should money be spent on? Which children, and which parents, will benefit most from which intervention? Is there evidence of greater benefit with earlier detection of ASD and thus earlier intervention? How long should interventions last?

OBJECTIVES

The objective of this review is to determine the level of current evidence about the extent to which parent-mediated early intervention has been shown to be effective in the treatment of young children with autism spectrum disorders. In particular, it aims to assess the effectiveness of such interventions in terms of the benefits for both children and their parents. Another key interest we have in this review is to explore current approaches to measurement of adherence in the implementation of parent-mediated intervention strategies. This is important as we do expect that, like most forms of therapy, compliance is key to getting the desired outcome. Finally, we will also consider categorisation of the signs or features described by the various publications with the aim of exploring moderators of treatment effect.

METHODS

Criteria for considering studies for this review

Types of studies

We will include only RCTs in this review.

Types of participants

- Parents of children with ASD, aged between one year and six years 11 months. Studies that include child participants whose ages fall outside of this range of one year to six years 11 months may be included in the review, for example, if fewer than 5% of the children are above six years 11 months at the start of the study. The term ‘parent’ will be used to include caregivers who take on a parental role.
- Children who have a diagnosis of ASD to include Autism, Aspergers, Pervasive Developmental Disorder (PDD) and PDD Not Otherwise Specified (PDD, NOS)
- Where a study includes child participants with a variety of developmental disorders, it will be included only where results are presented separately for the ASD group.

Types of interventions

Interventions in which parents are trained by professionals in strategies designed to improve the management of their child’s ASD-related difficulties. Parents must have received ongoing supervision and support from professionals. The training may involve group or individual coaching of parents in a planned (potentially replicable) approach designed to help them promote their child’s communicative and social development, learning, skills or control of behaviour.

The control conditions will be no treatment, treatment as usual, a waiting list group, an alternative child-centred intervention not mediated by parents or an alternative parent-mediated intervention that differs in some way (for example, intensity) from the experimental condition

Types of outcome measures

A range of measures are used to characterise children and confirm the diagnosis of ASD. No particular measure or reporting of an outcome will be used as an as an inclusion criterion for this review.
Primary outcomes

Child communication and social development
- Language development (comprehension and expression)*
- Social communication skills*
- Skills in interaction with parent*

Parental outcome
- Parents’ level of stress*

It is likely that included studies will employ a variety of outcome measures, and different time points for follow-up, and analysis will be performed as appropriate.

Secondary outcomes

Child ability
- Developmental/intellectual gains
- Adaptive behaviour

Child problem behaviour
- Restricted and repetitive behaviour
- Maladaptive behaviour*

Parental outcomes
- Parents’ satisfaction with therapy
- Parents’ confidence in coping with child’s disability and behaviour problems

Cost of intervention
- Any cost information provided by the authors

The headings marked with (*) will be used to populate the ‘Summary of findings’ table. Potential adverse effects of intervention will be noted, for example, an increase in parental stress.

Timing of outcome assessment
We will collect outcome data immediately post-treatment, and at the time points closest to the following periods as appropriate: six months, one year, two years and four years,

Search methods for identification of studies
We will consider published or unpublished randomised controlled trials (RCTs), with no language restrictions.

Electronic searches
We will search the following databases:
- Cochrane Central Register of Controlled Trials (CENTRAL), part of the The Cochrane Library
- Ovid MEDLINE(R)
- EMBASE
- ERIC (Educational Resources Information Centre)
- PsycINFO
- CINAHL (Cumulative Index to Nursing and Allied Health Literature)
- Dissertation Abstracts International
- Social Sciences Abstracts
- Sociological Abstracts
- Linguistics and Language Behavior Abstracts
- Cochrane Database of Systematic reviews (CDSR), part of the Cochrane Library
- Database of Abstracts of Reviews of Effects (DARE)
- National Research Register (NRR) Archive
- LILACS (Latin American Health Sciences Literature)
- Turning Research into Practice (TRIP) database
- OpenGrey
- ASSIA (Applied Social Sciences Index and Abstracts)
- IBSS (International Bibliography of the Social Sciences)
- National Criminal Justice Reference Service Abstracts
- WHO International Clinical trials Registry Platform (ICTRP)
- metaRegister of Controlled Trials (mRCT)
- ClinicalTrials.gov

The search strategy for MEDLINE (Appendix 1) will be adapted for other databases using appropriate syntax and controlled vocabulary.

Searching other resources
We will examine other sources of information including the bibliographies of systematic and non-systematic reviews and reference lists of key articles identified through the search strategy. We will contact experts in the field by letter in order to identify unpublished studies. We will handsearch key journals to identify studies that have not been electronically catalogued in the databases searched, and in addition will use Google Scholar to search the World Wide Web.

Data collection and analysis

Selection of studies
We will transfer all citations generated through the search strategy to the reference management program EndNote (EndNote Web 3.1). Two authors (HM, an expert in early autism, and IPO, an expert in systematic reviews) will independently screen titles and abstracts identified in the search, and indicate which reports should
be retrieved in full. We will retrieve the full report of any title or abstract for which there are insufficient data. The same review authors will independently read full reports and determine whether these studies meet the inclusion criteria. Multiple reports of the same study will be identified. The two review authors above will independently assess and select studies for inclusion from the pool of remaining studies. We will resolve disagreements over inclusion by discussion where possible, and where disagreement persists, a third adjudicator (EH) will assess the paper. We will document all decisions and contact authors for further information where necessary.

Data extraction and management
For each included study, two review authors (IPO, EH) will extract and record the following data, using a piloted data collection form: study location, funding source, study design, methods, participant details (diagnostic description and severity of impairments, parent characteristics), type of intervention (including the intensity and duration of intervention), measurement of adherence, outcome measures, any reported cost data, and key conclusions of study authors. We will provide the results of the appraisal of these factors in a narrative summary.

In the event of disagreements, review authors will first discuss these with reference to the study papers or other information. Where necessary, we will contact trial investigators for clarification. We will resolve disagreements over inclusion by discussion where possible, and where disagreement persists, a third adjudicator will assess the paper. We will document all decisions and contact authors for further information where necessary. We will refer any differences that we cannot resolve to the editorial board of the Cochrane Developmental, Psychosocial and Learning Problems Group (CD-PLPG) for assistance. A list of excluded studies and why they were excluded will be generated and included in the review.

We will report the characteristics of included studies in the 'Characteristics of included studies' table. Two review authors (IPO and EH) will extract data independently using a data extraction form that we will develop. Where data are not available in the published trial reports, authors will be contacted to supply missing information.

Assessment of risk of bias in included studies
We will use the Cochrane Collaboration’s tool for assessing risk of bias (Higgins 2011). Two review authors (HM and IPO) will independently assess the risk of bias for each included study based on the following six domains, with review authors’ judgements presented as answers of ‘high’, ‘low’ and ‘unclear’ risk of bias. We will include a ‘Risk of bias’ table in the review.

Random sequence generation: we will describe the method used to generate the allocation sequence in sufficient detail to allow an assessment of whether it should produce comparable groups.

Allocation concealment: we will describe method used to conceal the allocation sequence in sufficient detail to determine whether intervention allocations could have been foreseen in advance of, or during, enrolment.

Blinding of participants and personnel: we will describe all measures used, if any, to blind study participants and personnel from knowledge of which intervention a participant received. We will provide any information relating to whether the intended blinding was effective.

Blinding of outcome assessment: we will describe all measures used, if any, to blind outcome assessors from knowledge of which intervention a participant received. We will provide any information relating to whether the intended blinding was effective.

Incomplete outcome data: we will describe the completeness of outcome data for each main outcome, including attrition and exclusions from the analysis. We will state whether attrition and exclusions were reported, the numbers in each intervention group (compared with total randomised participants), reasons for attrition/exclusions where reported, and any re-inclusions in analyses performed by the review authors.

Selective outcome reporting: we will state the possibility of selective outcome reporting by the study authors by assessing to see if any of the stated outcome was not reported in the end of the study.

Measures of treatment effect

Categorical data
Binary outcomes are likely to be uncommon but may occur. They will be analysed by calculation of the Odds Ratio (OR) with the 95% confidence interval (CI).

For meta-analyses of dichotomous outcomes that are included in ‘Summary of findings’ tables, we will express the results as absolute risks, using high and low observed risks among the control groups as reference points.

Continuous data
We will calculate mean differences (MD) (where studies use the same measurement scale) or standardised mean differences (SMD) (where studies use different scales), and 95% CI for continuous outcome measures. Most studies are likely to report change from baseline, so this will also be extracted where possible. If necessary, we will compute effect estimates from P values, t statistics, ANCOVA tables or other statistics as appropriate. We will calculate SMDs using Hedges g.

When a study provides multiple, interchangeable measures of the same construct at the same point in time (for example, behaviour outcome assessed through parent and teacher reports), we will calculate the average SMD across these outcomes, and the average of their estimated variances. This strategy aims to avoid the need
to select a single measure, and to avoid inflated precision in meta-analyses (preventing studies that report on more outcome measures receiving more weight in the analysis than comparable studies that report on a single outcome measure).

Qualitative data
It is anticipated that studies will yield some qualitative data. This will not be searched for systematically but, where appropriate, will be summarised and considered in the discussion.

Multiple outcomes
When a study provides multiple, interchangeable measures of the same construct at the same point in time (for example, multiple measures of child language), we will calculate the average SMD across these outcomes, and the average of their estimated variances. This strategy aims to avoid the need to select a single measure, and to avoid inflated precision in meta-analyses (preventing studies that report on more outcome measures receiving more weight in the analysis than comparable studies that report on a single outcome measure).

Unit of analysis issues

Cluster-randomised trials
In order to avoid unit-of-analysis errors in cluster-randomised trials, we will conduct the analysis of cluster-randomised trials at the same level as the allocation, using a summary measurement from each cluster. However this approach might considerably, and unnecessarily, reduce the power of the study (given that we may encounter studies with small numbers of participants). We may as an alternative extract from the cluster-randomised trials, direct estimates of the required effect measure (for example, an odds ratio with its confidence interval) from an analysis that properly accounts for the cluster design. Such an analysis might be based on a ‘multilevel model’, a ‘variance components analysis’ or may use ‘generalized estimating equations (GEEs)’, among other techniques. Effect estimates and their standard errors from corrected analyses of cluster-randomised trials may also be meta-analysed using the generic inverse-variance method in RevMan. We will seek statistical advice if needed.

Studies with multiple treatment groups
In cases where studies compare more than one intervention condition, we will, in the primary analyses, combine results across all eligible (parent-mediated early intervention) intervention groups and compare these with the combined results of all eligible control groups, making single, pair-wise comparisons. If this approach prevents the investigation of potential sources of heterogeneity, will analyse each group separately (against a common control group), but divide the sample size for common comparator groups proportionately across each comparison, thereby avoiding double counting of individuals (Higgins 2011).

Dealing with missing data
Where necessary, we will contact the authors of included studies to supply any unreported data (for example, group means and standard deviations (SDs), details of dropouts, and details of interventions received by the control group).

Assessment of heterogeneity
We will explore the possibility of assessing clinical variation across studies by comparing the distribution of:
- duration of intervention;
- intensity of intervention;
- type of intervention.

We will describe statistical heterogeneity by computing both $I^2$ (Higgins 2011), a quantity that describes approximately the proportion of variation in point estimates that is due to heterogeneity rather than sampling error, and by employing a Chi$^2$ test of heterogeneity to determine the strength of evidence that heterogeneity, if found, is genuine.

Assessment of reporting biases
We will draw funnel plots (estimated differences in treatment effects against their standard error) if we find sufficient studies. Asymmetry might be due to publication bias or to systematic differences between small and large studies. If a relationship is identified, we will further examine the clinical diversity of the studies to provide a possible explanation. As a direct test for publication bias, we will compare results extracted from published journal reports with results obtained from other sources (including correspondence).

We will also assess the extent of bias in the interpretation of results, for example, where the interpretation is based on results from treatment completers alone rather than the intention-to-treat analysis, or the significance of an inadequately powered studies is over-interpreted, or there is over-reliance on non-blinded measures.
Data synthesis
We plan to synthesise results in a meta-analysis if the interventions are similar in theoretical basis, method of delivery of the parent-training, duration and intensity, and have reported their results as means with standard deviations (SD). In cases where MDs (i.e. change) are presented but not accompanied by the SDs, we will deduce this (SD) from other statistics available, for example, CIs, standard errors, t values, P values and f values) using methods described in the Cochrane Handbook for Systematic Reviews of Interventions for compiling missing SDs (Higgins 2011). We will use both a fixed-effect and a random-effects model and compare to assess the impact of statistical heterogeneity. Unless the model is contra-indicated (for example, if there is funnel plot asymmetry or if there is wide difference between the results obtained from fixed-effect and random-effects meta-analyses), we plan to present the results from the random-effects model. In the presence of severe funnel plot asymmetry, we will present both fixed-effect and random-effects analyses, under the assumption that asymmetry suggests that neither model is appropriate. If both indicate a presence (or absence) of effect, we will be reassured; if they do not agree we will report this. We will calculate all overall effects using inverse variance methods. If some primary studies report an outcome as a dichotomous measure and others use a continuous measure of the same construct, we will convert results for the former from an OR to a SMD, provided that we can assume the underlying continuous measure has approximately a normal or logistic distribution (otherwise we will carry out two separate analyses).

We will perform data synthesis using the Review Manager 5.1 software (Review Manager 2011)

Subgroup analysis and investigation of heterogeneity
Subgroup analyses will be exploratory as they involve non-experimental (cross-study) comparisons and we will treat any conclusions with caution. We will carry out subgroup analysis where applicable using appropriate statistical techniques to investigate the following possible sources of heterogeneity: child’s age, child’s IQ and parental education.

Sensitivity analysis
We will conduct sensitivity analyses to determine whether findings are sensitive to restricting the analyses to studies judged to be at low risk of bias. In these analyses, we will restrict the analysis to:
- only studies with low risk of selection bias (associated with sequence generation or allocation concealment);
- only studies with low risk of performance bias (associated with issues of blinding);
- only studies with low risk of attrition bias (associated with completeness of data).

Acknowledgements
The authors are very grateful to the Nuffield Foundation, which funded both the original review and this revision.

Additional references
Aldred 2004

APA 2000

Baird 2006

Boyd 2010

Charman 2010

Dawson 1997

Eldevik 2009

EndNote Web 3.1
Parent-mediated early intervention for young children with autism spectrum disorders (ASD) (Protocol)

Higgins 2011

Howlin 1987

Jocelyn 1998

Koegel 1995

Koegel 1996

Lord 1994

McConachie 2005

McEachin 1993

Ospina 2008

Prizant 1997

Prizant 1998

Review Manager 2011

Rickards 2007

Rogers 1991

Rogers 1998a

Rogers 1998b

Rogers 2008

Schopler 1971

Siller 2008

Smith 1999

Smith 2000

Vriend 2011

WHO 2010
Appendix 1. MEDLINE (OVID) search strategy

# 1 exp child development disorders, pervasive/
# 2 Developmental Disabilities/
# 3 pervasive development$ disorder$.tw.
# 4 (PDD or PDDs or ASD or ASDs).tw.
# 5 autis$.tw.
# 6 asperger$.tw.
# 7 kanner$.tw.
# 8 childhood schizophrenia.tw.
# 9 Rett$.tw.
# 10 or/1-9
# 11 Family/
# 12 exp Parents/
# 13 (parent$ or family or families or mother$ or father$ or maternal$ or paternal$).tw.
# 14 (at home or (in adj3 home) or home based or home-based).tw.
# 15 Caregivers/
# 16 (caret$ or care-giver$ or caregiver$).tw.
# 17 or/11-16
# 18 10 and 17
# 19 exp child/
# 20 infant/
# 21 (child$ or infant$ or babies or baby or toddler$ or girl$ or boy$ or pre-school$ or preschool$ or nursery$ or kindergarten$ or kinder-garten$).tw.
# 22 or/19-21
# 23 18 and 22
# 24 randomized controlled trial.pt.
# 25 controlled clinical trial.pt.
# 26 randomi#ed.ab.
# 27 placebo$.ab.
# 28 drug therapy.fs.
# 29 randomly.ab.
# 30 trial.ab.
# 31 groups.ab.
# 32 or/24-31
WHAT'S NEW

Last assessed as up-to-date: 23 February 2012.

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HISTORY

Review first published: Issue 1, 2003

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CONTRIBUTIONS OF AUTHORS

Tim Diggle and Helen McConachie wrote the original protocol and review (2002) while Helen McConachie and Inalegwu Oono wrote the revised protocol for the updated review. Emma Honey will participate chiefly in data extraction and analysis. The final writing of the current review will be carried out by Helen McConachie, Inalegwu Oono and Emma Honey.

DECLARATIONS OF INTEREST

The funding for the review was provided by the Nuffield Foundation. The grant provided finance for Dr Inalegwu P Oono to be employed as a research associate and update this review in collaboration with Professor Helen McConachie and Dr Emma J Honey.
SOURCES OF SUPPORT

Internal sources

• No sources of support supplied

External sources

• The Nuffield Foundation, UK.

NOTES