

## **Title**

Treatment foster care for improving outcomes in children and young people

## **Reviewers**

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## **Dates**

Date edited: 19/10/2007

Date of last substantive update: 18/10/2007

Date of last minor update: 19/10/2007

Date next stage expected 01/12/2007

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## **Contribution of reviewers**

William Turner (WT) and Geraldine Macdonald (GM) both contributed to the writing of the protocol and the development of a search strategy (assisted by Jo Abbott the TSC of the Cochrane Developmental, Psychosocial and Learning Problems Group).

WT and GM will both review titles and abstracts of potential trials and extract relevant data on methodological quality and treatment outcomes. WT will enter data and both authors will contribute to analysing results and writing up the review. The authors will share the responsibility of updating this review every two years.

## **Internal sources of support**

University of Bristol, UK

## **External sources of support**

Nordic Campbell Center, DENMARK

## **Published notes**

This protocol is co-registered in both the Campbell and Cochrane Collaborations.

# Treatment foster care for improving outcomes in children and young people

## Background

Treatment foster care (TFC) is a foster family-based placement that aims to provide young people (and, where appropriate, their families) with an individually tailored programme designed to help bring about positive changes in their lives ([Bereika 1992](#), [Clark 1993](#)). Because it is individually tailored programme, TFC is sufficiently flexible to accommodate different client populations with a wide range of clinical problems and shifting community needs ([Clark 1993](#)). TFC was designed specifically to cater for the needs of children whose difficulties or circumstances place them at risk of multiple placements and/or more restrictive placements such as hospital or secure residential or youth justice settings ([Webb 1988](#)). These groups include children who have experienced trauma, neglect or abandonment; children and adolescents with mental health problems, children with problems of antisocial behaviour and offending, and children with serious medical conditions ([Davis 1984](#); [Foster 1982](#)).

Multiple placements deprive children of the opportunities to establish strong attachments with carers, to establish and maintain friendship networks and to realise their potential in education. The long-term adverse consequences for children are formidable. Mainstream foster care or residential care placements do not typically provide interventions designed specifically to address the needs of young people placed; nor do they provide carers with the skills and support services needed to implement them. Externally, the paucity of professional services available to children, compounded by long waiting lists and the serendipitous nature of services that are available, often mean that appropriate help is not there when it is needed. This is the vacuum which treatment foster care seeks to fill.

### *Treatment foster care defined*

The term 'Treatment Foster Care' is not the only term used to describe placements with carers that are specifically designed and delivered to provide tailored support to young people, their carers and, where appropriate, their families. 'Specialised foster care' was a term used early on to describe what later became called treatment foster care ([Chamberlain 1990](#)). Other terms include 'wraparound' foster care ([Clark 1996](#)) and multidimensional treatment foster care ([Chamberlain 2007](#)). Some reviewers define the term even more widely, to include supportive arrangements such as Foster Extended Family ([Barsh 1983](#), see [Clark 1993](#) for a review). Others attempt to distinguish categories such as specialised foster care and treatment foster care (see Reddy and Pfeiffer 1997 ([Reddy 1997](#))).

The definition of treatment foster care used in this systematic review draws on the nine 'basic ingredients' identified by Snodgrass as characterising treatment foster care programmes ([Snodgrass 1989](#)):

1. The stated goal or objective of the programme is to serve children and youths who would otherwise be in more restrictive nonfamily settings (usually institutions), or at

risk of admission to those settings.

2. There is a clearly articulated philosophy with strong community links and individually designed treatment and education plans that include "a stated, measurable goal, a written set of procedures for achieving the goal, a written set of procedures for achieving the goal, and a process for regularly assessing the result" ([Snodgrass 1989](#), p. 77)
3. Foster carers are selected and trained to provide therapeutic care to children and youths who have special needs that may result from emotional disturbance, developmental disabilities, behavioural difficulties, or special medical needs.
4. Care is provided within a family setting, in a home owned or under the control of the foster carers who are responsible for the implementation of the young person's treatment plan
5. The number of children placed in the home is limited to no more than two.
6. Foster carers receive support, consultation, and supervision from professionals who carry a small caseload with crisis intervention services available around the clock.
7. Foster carers are regarded as a professional member of the service and treated as such.
8. Foster carers receive payments above those provided for regular foster care and may also receive a special stipend based on each child's treatment needs.
9. The programme is administered by specialist agencies or, if part of a host agency, by a unit specifically identified as a treatment foster care programme, even if using a different name.

### *Early findings*

Results from initial programme evaluations and experimental studies of treatment foster care programmes in Canada, United Kingdom and the United States suggest that:

- TFC programmes can serve as alternatives to residential treatment ([Almeida 1989](#); [Colton 1988](#); [Hazel 1981](#)), to correctional institutions ([Chamberlain 1988](#), [Chamberlain 1990a](#)), and to psychiatric hospitalisation ([Chamberlain 1991](#)).
- Children and adolescents served in TFC experience positive in-programme changes in self-esteem, sense of identity, and personal growth ([Bogart 1988](#); [Clark 1993](#); [Colton 1988](#), [Colton 1990](#); [Hazel 1981](#); [Fanshel 1989](#); [Larson 1977](#); [Rubenstein 1978](#); [Yelloly 1979](#)).
- Most TFC placements were completed as outlined in the child's or adolescent's case plan ([Chamberlain 1988](#), [Chamberlain 1990](#); [Fanshel 1989](#); [Fenyo 1989](#); [Hazel 1981](#), [Hazel 1990](#); [Smith 1986](#); [Yelloly 1979](#)).
- Compared to youth discharged from institutional care, those discharged from TFC are more likely to go to less restrictive settings and are less likely to be institutionalised subsequently ([Almeida 1989](#); [Chamberlain 1988](#), [Chamberlain 1990](#); [Colton 1990](#); [Fanshel 1989](#); [Hawkins 1989](#); [Larson 1977](#); [Stroul 1989](#)).
- Finally, TFC programmes appear to be less costly than the institutional and group-home alternatives to which they were compared ([Almeida 1989](#); [Chamberlain 1990](#); [Rubenstein 1978](#)).

These papers have been included in one or more of three literature reviews ([Galaway 1995](#), [Hudson 1994b](#) and [Meadowcroft 1994](#)). Although tentatively supportive of

TFC, these reviews are limited in two respects. First of all, all reviewers use different, and generally very broad definitions of treatment foster care. Secondly, the methodological rigour of included studies varies ([Hudson 1994b](#); [Meadowcroft 1994](#)). Only two involved random assignment ([Chamberlain 1988](#); [Chamberlain 1991](#)) and four involved no control or comparison group ([Larson 1978](#); [Hazel 1981](#); [Smith 1986](#); [Fanshel 1989](#)). Studies with control groups compared TFC with other treatment settings, including regular foster care ([Bogart 1988](#)), treatment in the community ([Chamberlain 1988](#); [Chamberlain 1990](#)), residential children's homes ([Colton 1988](#); [Colton 1990](#)), and other residential treatment centres ([Rubenstein 1978](#)). One study comprised an experimental, retrospective longitudinal design ([Fanshel 1989](#)), six studies involved a post-test measure only (e.g., [Almeida 1989](#); [Chamberlain 1988](#); [Chamberlain 1990](#); [Hazel 1981](#); [Smith 1986](#); [Yelloly 1979](#)) and seven were described as quasi-experimental with repeated measures ([Bogart 1988](#); [Larson 1978](#); [Colton 1988](#); [Hawkins 1989](#); [Thomlison 1992](#)). Meadowcroft ([Meadowcroft 1994](#)) concluded that 'the positive results demonstrated for the few treatment foster care programs that have been studied cannot be extended to all programmes. Treatment foster care programs in general vary greatly regarding children served; treatment parent selection, training, and supervision; staff expertise; involvement of children's families; and frequency and types of interventions used to help children adjust. Nor it is clear what aspects of these successful treatment foster care programs contribute to the positive results. The research to date has not isolated the critical components of successful treatment foster care programs' (p. 575).

In a meta-analytic research synthesis of 40 outcome studies conducted between 1974 and 1996, Reddy and Pfeiffer ([Reddy 1997](#)) reported overall positive social-psychological changes in children and adolescents placed in TFC. The largest effects were in the domains of children's social skills and placement permanency with medium effects observed in behaviour problem reduction, level of restrictiveness at post-discharge, and psychological adjustment. The researchers caution against drawing definitive conclusions about the overall effectiveness of TFC due to the limited number of rigorous outcome studies and, as a result, the pooling of data relating to children of different age groups and clinical populations, and different lengths of follow-up periods.

In a review of five studies specifically designed to assess the impact of TFC on violence prevention, Hahn and colleagues ([Hahn 2004](#)) concluded there was i) insufficient evidence to determine the effectiveness of TFC programmes in which clusters of foster-parent families cooperated in the care of children aged 5-13 with severe emotional disturbance, but that ii) data from three studies in which programme personnel 'collaborated closely and daily with foster families caring for adolescents (aged 12-18 year) with a history of chronic delinquency' suggested that this form of TFC might be effective.

Given the proliferation of TFC programmes in countries (including England, the Netherlands, Denmark, Germany and Israel) facilitated in part by increased economic and political support for alternative placement services, a systematic review of the effectiveness of these interventions is timely.

## **Objectives**

To assess the impact of Treatment Foster Care on psychosocial and behavioural outcomes, delinquency, placement stability, and discharge status for children and adolescents who require out-of-home placement.

## **Criteria for considering studies for this review**

### **Types of studies**

Studies in which allocation of study participants to groups was by random allocation or quasi-random allocation, i.e., by day of the week, alternate numbers, case number or alphabetical order. Studies comparing a TFC intervention versus control will be included. The control group can be a no-treatment, wait-list control, or regular foster care. There will be no language restrictions.

### **Types of participants**

Children and adolescents up to the age of 18 who, for reasons of severe medical, social, psychological and behavioural problems, are placed out of home. This can include:

- children and adolescents with mental health problems who may require psychiatric hospitalisation;
- drug and substance dependent children and youth who may be in need of out-of-home placements in group child welfare and/or hospital settings;
- delinquent youth at risk of incarceration or placement in highly restrictive group/residential settings.
- children placed in out of home care as a result of abuse or neglect and who have, or are deemed at risk of developing, one or more of the problems identified above.

### **Types of interventions**

Any Treatment Foster Care programme (as defined above) providing individualised, therapeutic, community- and foster family-based intensive services to children and adolescents (and their biological or adoptive families), designed to prevent multiple-placements, and/or as an alternative to restrictive institutional placement options.

### **Types of outcome measures**

A range of outcome measures will be identified and analysed where appropriate. These will include:

#### **A. Looked-after child outcomes:**

- Psychological functioning (including psychiatric diagnosis and symptoms) as measured by (preferably) standardised scales (e.g., Child Behavior Checklist

[CBCL] [Achenbach 1983](#)) and reports provided by therapeutic foster carers and other mental health professionals;

- Educational outcomes (i.e. level of school attendance and educational achievement) as indicated by grades obtained and teacher reports; where data will be available (and applicable to the population examined) training and employment outcomes (as indicated, for example, by employers' reports and agency records) will be considered alongside educational outcomes;
- Interpersonal functioning will include **1.** community participation and involvement as measured, for example, by foster carer reports on extent of community services and resources accessed, and **2.** peer relationships as indicated by either self-reports and/or treatment foster carers and/or teacher reports; and
- Behavioural outcomes; These would cover: **1.** behavioural problems within the treatment foster home as measured by treatment carers reports and any (standardised) measures of externalising behaviour (i.e., physical and verbal aggression, self-harming, rule-breaking behaviour, defiance, truancy); **2.** anti-social behaviour as measured by rates of delinquency, rates of arrest, rates of conviction, and rates of incarceration, **3.** drug and substance abuse (based on self reports and biological measures); **4.** prescribed medication for behavioural symptom reduction/management (as indicated by agency records and carers reports).
- Psychological functioning including confidence, resilience, and adaptability as measured by self-reports and/or agency assessment(s) and/or standardised scales (e.g., Rosenberg Self-Esteem scale ([Rosenberg 1965](#)));
- Mental health status. This might include **1.** measures of well-being and self-esteem; **2.** psychiatric status (e.g. DSM-IV diagnosis), or **3.** prescription and adherence to antipsychotic medication
- Physical health

**Note:** Interpreting the use of medication, particularly in the field of mental ill health, is complex. First, where associated with maintenance or improvement of health then starting or maintaining a pharmacotherapeutic regimen could be a good outcome (e.g. schizophrenia). In some cases, however, medication might mean that participants are not receiving the psychological therapy that would be more beneficial/have less harmful side effects. Secondly, TFC foster carers may be more likely to advocate for psychiatric assessment and treatment, on the basis of children's needs, or because it is seen as an answer to difficult child management problems. Finally, it could be indicative of harm if the need for medication precipitates stress and associated mental health problems. Given the challenges of disinterring these processes we propose reporting these outcomes in the review and only synthesising and/or interpreting them if the meaning of the data are unambiguous i.e. clearly beneficial, clearly indicative of appropriate advocacy, clearly an unintended harm.

#### **B.** Treatment foster carer(s)/family outcomes:

- Measures of skills (e.g., behaviour management skills, problem solving skills), knowledge and attitudes as measured by agency personnel and/or standardised scales;
- Interpersonal functioning including cohesion and communication patterns within the treatment home.

### C. TFC agency outcomes:

- Placement stability (e.g., number of requests for removal, number of unrequested removals) and/or completion of allocated stay;
- Attainment of treatment goals;
- Level of restrictiveness at programme completion;
- Level of independent living skills attainment at post-discharge (if applicable).

### D. Costs

It is generally accepted that TFC is less expensive than residential care, although there is wide variation in costs among programmes ([Curtis 2001](#)). Where possible, data on programme cost will be reported. In addition, outcome data will be sought for the post-treatment, short-term (up to one year post-intervention) and long-term (over one year post-intervention).

## Search strategy for identification of studies

We will search the following databases: the Cochrane Controlled Trials Register (CENTRAL), MEDLINE, CINAHL, PsycINFO, ASSIA, LILACS, ERIC, Sociological Abstracts, and the National Research Register. The following search strategy will be used for MEDLINE, to be searched via OVID:

- 1 Child/
- 2 INFANT/
- 3 ADOLESCENT/
- 4 (child\$ or adolescen\$ or boy\$ or girl\$ or teen\$ or schoolchild\$ or preschool\$ or pre-school\$ or infant\$ or baby or babies or young person\$ or young people).tw.
- 5 or/1-4
- 6 family based residential treatment.tw.
- 7 (foster adj6 treatment).tw.
- 8 (foster adj6 special\$).tw.
- 9 (foster adj6 therapeutic).tw.
- 10 (foster adj6 care).tw.
- 11 (foster adj6 medical).tw.
- 12 (foster adj6 family based).tw.
- 13 or/6-12
- 14 5 and 13
- 15 randomized controlled trial.pt.
- 16 controlled clinical trial.pt.
- 17 randomized controlled trials.sh.
- 18 random allocation.sh.
- 19 double blind method.sh.
- 20 single-blind method.sh.
- 21 or/15-20
- 22 (animals not humans).sh.
- 23 21 not 22
- 24 clinical trial.pt.
- 25 exp clinical trials/
- 26 (clin\$ adj25 trial\$).ti,ab.

27 ((singl\$ or doubl\$ or trebl\$ or tripl\$) adj25 (blind\$ or mask\$)).ti,ab.  
28 Placebos.sh.  
29 placebo\$.ti,ab.  
30 random\$.ti,ab.  
31 research design.sh.  
32 or/24-31  
33 32 not 22  
34 33 not 23  
35 comparative study.sh.  
36 exp evaluation studies/  
37 follow up studies.sh.  
38 prospective studies.sh.  
39 (control\$ or prospectiv\$ or volunteer\$).ti,ab.  
40 or/35-39  
41 40 not 22  
42 41 not (23 or 34)  
43 23 or 34 or 42  
44 14 and 43

Search terms will be modified to suit other databases and trials filters applied where relevant. All search strategies will be recorded within the completed review.

Other sources of information may include the bibliographies of systematic and non-systematic reviews and reference lists of articles identified through electronic searches above. Authors and known experts will be contacted to identify any additional or unpublished data and no language restrictions will be applied.

## **Methods of the review**

### **1. Trial selection strategy**

Both reviewers will independently screen titles and abstracts identified in the search and indicate which reports should be retrieved. If there is not enough information in the title and abstract to make such decisions, the full text will be retrieved. Both reviewers will independently read full reports and determine whether these studies meet the inclusion criteria. Selection decisions will be reviewed and any disagreements will be resolved by the review team; in the event this is not possible, the editorial base of the CDPLPG will be approached. Specific reasons for exclusion will be documented for each study that does not meet inclusion criteria.

### **2. Data extraction and management**

Both reviewers will independently code all studies and extract data. Differences between coders will be resolved in order to refine coding schemes and establish inter-rater reliability. Citations and data will be entered and organized in RevMan 4.2.9. Authors will be contacted to supply missing data from included studies. Information on study design and implementation (including data on programme differentiation [[Dane 1998](#); [MRC 2000](#)]), sample characteristics, intervention characteristics (including theoretical underpinning of services, delivery, duration, and within-intervention variability) will be extracted and coded on a data extraction form. Outcome data as listed above will also be extracted.

When more than two treatment arms are included in the same trial, all arms will be described.

### **3. Methodological quality**

Existing scales for measuring the quality of controlled trials have not been properly developed, are not well-validated and are known to give differing (even opposing) ratings of trial quality in systematic reviews ([Moher 1995](#)). At present, evidence indicates that 'scales should generally not be used to identify trials of apparent low quality or high quality in a given systematic review. Rather, the relevant methodological aspects should be identified a priori and assessed individually' ([Juni 2001](#)).

#### **Allocation concealment**

Both reviewers will independently assign each included study to a quality category described in the Cochrane Handbook ([Higgins 2005](#)) where:

- (A) indicates adequate concealment of the allocation (for example, by telephone randomisation, or use of consecutively numbered, sealed, opaque envelopes);
- (B) indicates uncertainty about whether the allocation was adequately concealed (for example, where the method of concealment is not known);
- (C) indicates that the allocation was definitely not adequately concealed (for example, open random number lists or quasi-randomisation such as alternate days, odd/even date of birth, or hospital number)

For the purposes of this review, only trials meeting categories (A), (B) and (C) will be included. Included studies will also be assessed on: adequate implementation of random assignment (where relevant); standardization and blinding of assessments; attrition, and intent-to-treat analysis.

Studies will be rank-ordered in terms of their ability to support intent-to-treat analysis and use of standardized or objective outcome measures. In studies classified as 'B' (unclear), 'C' (inadequate).

### **4. Missing data**

When necessary, the corresponding author will be contacted to supply any unreported data (e.g., group means and standard deviations (SDs), details of dropouts, and details of interventions received by the control group). Other authors will be contacted if necessary. If a study reports outcomes only for participants completing the trial or only for participants who followed the protocol, authors will be contacted and asked to provide additional information to permit an intention-to-treat analyses.

### **5. Data synthesis**

#### **Outcome data**

RevMan 4.2 will be used to perform the following calculations.

#### ***Binary data***

Binary outcomes will be analyzed by calculating odds ratios with 95% confidence intervals. Although the odds ratio provides an effect for use in meta-analysis ([Lipsey 2001](#)), attempts will be made to preserve information about base rates (actual

proportions) and differences in proportions, since this information is of interest to policy makers. RevMan 4.2 uses Mantel-Haenszel methods for combining binary outcome data across studies.

### ***Continuous data***

Mean differences, standardised mean differences (SMDs) and 95% CIs will be calculated for comparisons of continuous outcome measures. If necessary, other information from studies (e.g., test statistics and degrees of freedom from a focused comparison) will be used to compute the SMD. Where different scales measure the same clinical outcomes (e.g. psychiatric symptoms), an overall SMD and 95% CI will be calculated ([Higgins 2005](#)). SMDs will be calculated using Hedge's *g*.

### ***Multiple measures***

When a single study uses multiple measures of the same outcome, we will report all measures. For example, if a study includes two measures of satisfaction (either measures completed by the same respondent or measures completed by different respondents), we will report both of them (Table of outcomes).

If measures of an outcome are combined for meta-analysis, we will conduct multiple meta-analyses if multiple studies report multiple measures that can be combined in this way. If a study includes multiple measures where only one effect estimate can be used for meta-analysis, we will calculate the average effect for this purpose (e.g. the average SMD or RR weighted by variance).

### ***Multiple arms***

All eligible outcome measures for all trial arms will be reported in the review. If two or more eligible intervention groups are compared to an eligible control, thus requiring that the reviewers choose a single intervention group for comparison or inclusion in a meta-analysis, that which most closely matches the definition of TFC above will be included in the meta-analysis, and the decision process by which authors selected arm will be clearly documented.

If a single eligible intervention group is compared to multiple eligible control groups, the least intensive control condition (e.g., 'no-treatment') will be chosen over other groups for comparison and inclusion in meta-analyses.

### ***Meta-analysis***

Meta-analyses may be conducted to combine comparable outcome measures across studies. All overall effects will be calculated using inverse variance methods. Random-effects models will be used because studies may include somewhat different treatments or populations. If some primary studies report an outcome (e.g., recidivism) as a binary measure and others use a continuous measure of the same construct, two separate meta-analyses will be used (one for odds ratios and another for SMDs) if we are unable to convert data odds ratios to SMD.

When a primary outcome study provides multiple measures of the same construct (e.g., foster carer and youth reports on post-discharge functioning) at the same point in time, an average effect size will be used to avoid dependence problems.

When a primary outcome study reports data obtained at different time points we will organise our analyses by length of follow up as follows: post treatment; short-term

(up to one year) and long term (over one year). In these analyses we will take the measure that is closest to each of these points.

## **6. Types of analyses**

Studies in which participants are analysed as members of the groups to which they were originally assigned (intention-to-treat analysis), studies that include only those participants who were willing or able to provide data (available-case analysis), and studies that analyse participants who adhered to the study's design (per-protocol analysis; [Higgins 2005](#)) will be analysed separately. Studies in which the reasons for excluding participants from analyses can not be determined from relevant reports or through contact with the authors will be considered with per-protocol analyses.

## **7. Assessment of heterogeneity**

The consistency of results will be assessed visually and using the  $I^2$  statistic ([Higgins 2002](#), [Higgins 2003](#)), a quantity which describes approximately the proportion of variation in point estimates that is due to heterogeneity rather than sampling error. We will supplement this with a test of homogeneity to determine the strength of evidence that the heterogeneity is genuine.

## **8. Subgroup analyses and investigation of heterogeneity**

Large numbers of subgroup analyses may lead to misleading conclusions ([Oxman 1992](#); [Yusuf 1991](#)). These analyses will be exploratory as they involve non-experimental (cross-study) comparisons and any conclusions will be treated with caution.

Subgroup analyses will focus on i) population served (e.g. delinquents, children with social and emotional problems, children with medical or mental health problems), ii) structure of the intervention, iii) characteristics of participants (children versus adolescent; males versus females, and iv) foster carer characteristics. Depending on the number of primary studies in the analysis, attempts will be made to study variations in effect sizes between studies, using weighted multiple regression or categorical comparisons.

## **9. Assessment of bias and sensitivity analyses**

Sensitivity analysis will be used to examine the robustness of conclusions. Separate analyses will examine studies that support intent-to-treat analysis and those in which the outcome assessment was blind to treatment allocation.

To investigate the possibility of bias, including publication bias, funnel plots will be drawn ([Deeks 2005](#), [Egger 1997](#), [Sterne 2001](#)). In the event of asymmetry, the reviewers will seek input from methodologists, including the Cochrane and Campbell Collaboration Methods Groups, on appropriate analyses.

## **10. Use of qualitative research**

The narrative review will draw on available qualitative data within included studies to discuss programme processes and implementation issues.

## **11. Use of data on programme costs**

We will summarize available data on the costs of programs within the studies under review.

## **Potential conflict of interest**

None known.

## **Additional references**

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### **Almeida 1989**

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Barsh ET, Moore JA, Hamerlynck LA. The foster extended family: A support network for handicapped foster children. *Child Welfare* 1983;62(4):349-359.

### **Bereika 1992**

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### **Chamberlain 2007**

Chamberlain P, Leve LD, DeGarmo DS. Multi-dimensional Treatment Foster Care for girls in the juvenile justice system: 2-year follow-up of a randomized clinical trial. *Journal of Consulting and Clinical Psychology* 2007;75(1):187-93.

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### **Clark 1996**

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### **Curtis 2001**

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**Dane 1998**

Dane A, Schneider B. Program integrity in primary and early secondary prevention: Are implementation effects out of control? *Clinical Psychology Review* 1998;18:23-45.

**Davis 1984**

Davis AB, Foster PH, Whitworth JM. Medical foster family care: A cost effective solution to a community problem. *Child Welfare* 1984;63(4):341-349.

**Deeks 2005**

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**Egger 1997**

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**Fanshel 1989**

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**Galaway 1995**

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### **Hawkins 1989**

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