Cognitive-behavioural therapy for parents who have physically abused their children

Christoffersen M.N.
Corcoran J.
DePanfilis D.
Daining C.

PROTOCOL

The Campbell Collaboration Social Welfare Group

This review is co-registered within both the Cochrane and Campbell Collaborations.
A version of this review can also be found on Cochrane Library.
Cognitive-behavioural therapy for parents who have physically abused their children

Protocol information

Authors
Mogens N Christoffersen¹, Jacqueline Corcoran², Diane DePanfilis³, Claire Daining³

¹SFI / The Danish National Institute of Social Research, Copenhagen K, Denmark
²School of Social Work, Virginia Commonwealth University, Alexandria, USA
³School of Social Work, University of Maryland, Baltimore, USA


Contact person
Mogens N Christoffersen
SFI / The Danish National Institute of Social Research
Herluf Trolles Gade 11
DK-1052 Copenhagen K
Denmark

E-mail: MC@sfi.dk

Dates

<table>
<thead>
<tr>
<th>Assessed as Up-to-date:</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Date of Search:</td>
<td></td>
</tr>
<tr>
<td>Next Stage Expected:</td>
<td>12 February 2009</td>
</tr>
<tr>
<td>Protocol First Published:</td>
<td>Issue 3, 2008</td>
</tr>
<tr>
<td>Review First Published:</td>
<td>Not specified</td>
</tr>
<tr>
<td>Last Citation Issue:</td>
<td>Issue 3, 2008</td>
</tr>
</tbody>
</table>

Abstract

Background
Background

Description of the condition

Physical maltreatment constitutes the non-accidental physical injury, or risk of injury, of a child by a caretaker (Kolko 2002; Dubowitz 2000). In 2003, approximately 906,000 children were victims of child abuse or neglect in the United States (USDHHS 2005). Of these, over 171,000 (18.9%) were physically abused (USDHHS 2005). In Canada, 24% of the substantiated reports of child maltreatment in 2003 were cases of physical abuse (Trocme 2005). In the United Kingdom for the year ending March 2004, of the 25,900 children listed on the child protection registry, 18% were there due to physical abuse (GBONS / DFES 2005). Of Danish families receiving Social Assistance services in 2001, 12 percent suffered from physical abuse (Christoffersen 2002).

As well as injury, the short-term consequences of physical abuse include aggression, impaired social competence, reduced empathy, poor impulse control, academic and behavioural problems, and internalizing problems, such as depression and low self-esteem (Azar 1988; Conaway 1989; Graziano 1992; Malinosky 1993; Mueller 1989; Toth 1992). Long-term consequences such as posttraumatic stress disorder and other psychological sequelae of traumatic events are also associated with physical abuse suffered in childhood (Briere 2003). Adults who have experienced physical abuse as children are at increased risk for violence against others - their partners, children, and people outside the home (Malinosky 1993). Internalizing problems have also been reported in clinical populations of women physically abused as children (Malinosky 1993).

Description of the intervention

Cognitive-behavioural approaches derive philosophically, theoretically and empirically from four theories of learning: respondent conditioning (associative learning e.g., of sexual arousal and trauma), operant conditioning (the effect of the environment on patterns of behaviour, particularly reinforcement and punishment), observational learning (learning by imitation) and cognitive learning (the impact of thought patterns on feelings and behaviour). They combine to provide an integrated approach to assessment and intervention with careful attention to the developmental and social contexts in which
learning occurs (Macdonald 2004). This is a broad class of present-focused interventions with a shared focus on changing cognition (thoughts, beliefs, and assumptions about the world), changing behaviour, and building clients' coping skills.

**How the intervention might work**

Severe cognitive deficits in the parent have been associated with maltreatment of the child. The first deficit involves unrealistic expectations about children's abilities based on their developmental level (Azar 2006). A second deficit involves attributing to the child's behaviour negative intentions (i.e., "he did that to get on my nerves"). A lack of problem-solving skills exacerbates these distorted cognitive appraisals, the third area of deficit. This combination of interpretive processes result in skill deficits, including problems with social skills, an impaired ability to control anger and stress, a lack of problem solving skills that affect the acquisition of needed physical resources (housing, transportation, food, clothing), as well as coercive parenting methods. In this way, the cognitive and skill deficits not only increase the risk of child abuse through physical coercion, but also reduce protective influences that might lessen the risk of physical abuse in the home (Azar 2006).

**Why it is important to do this review**

Because the prevalence of physical abuse is high and the consequences of maltreatment potentially harmful, effective help is important. Chaffin and Schmidt (Chaffin 2006) in their review of treatment for physical abuse state that none of the interventions published in the literature have met the American Psychological Association (APA) standard for "well established" treatments. The definition of a well-established empirically supported treatment by the APA is that it has been compared in two or more group design manualized experiments to have statistically significant effects over another treatment or psychological placebo (Chambless 1998). In addition, two and more research teams have tested the treatment and have found similar positive results. Past non-systematic reviews of the literature have indicated that a variety of cognitive-behavioural approaches, including child management skills training, stress management, anger management, problem-solving, cognitive restructuring, and comprehensive packages of cognitive-behavioural treatment, have shown effectiveness with parents who physically punish their children (Runyon 2004). More recently, Lundahl, Nimer, and Parsons (Lundahl 2006) conducted a meta-analysis looking at a range of parent training programs on the prevention of child physical abuse on different outcomes, including documented abuse. The meta-analysis included studies in which participants had already abused their children and those who were at risk of abuse. Recidivism was only a focus of three studies with an overall Cohen's d of .45 at post-test. No systematic review has been performed on cognitive-behavioural treatment of child physical abuse, in this case, we now propose to undertake such a study.

**Objectives**

The aim of this review is to evaluate the effectiveness of cognitive behavioural therapy (CBT) with parents who have physically abused their children on preventing the recurrence of physical maltreatment.
Methods
Criteria for considering studies for this review

Types of studies
Experimental and quasi-experimental evaluations of cognitive-behavioural therapy with parents who have physically abused their children. The eligibility criteria for this review were that: the evaluation used an experimental or two-group quasi-experimental research design which included (1) a no-treatment (NT) comparison group or (2) routine community services-treatment (RCS) comparison group or (3) another intervention group. While experimental designs involve randomisation to these groups, a quasi-experimental design has non-randomised assignment of persons to treatment and control groups. Quasi-experimental designs in which treatment and comparison/control are held simultaneously will be included.

Types of participants
Families with substantiated physical abuse (e.g., registered in CPS-system or similar system), receiving cognitive-behavioural therapy on a voluntary basis or by court order will be the subject of studies. The perpetrator may be the father, the mother, step-parents, or paramours of the parents.

Exclude: Studies of sexually abused children and studies involving physical abuse in foster families or adoptive families. An exception is if children who are physically abused have also been maltreated in other ways (i.e., sexually abused), and undergo an intervention in which a significant component addresses the physical abuse. In addition, studies will be excluded if the perpetrator of the abuse is permanently removed from the family and/or the child victim has been placed in out-of-home care during the period of observation. Studies in which families are receiving CBT for neglect or psychological maltreatment as well as physical abuse will be eligible if data on the effects on physical abuse are separately available.

Types of interventions
Any cognitive-behavioural intervention that aims to educate a person about the interrelations between how he/she thinks, feels, and how he/she acts and which seek to change behaviour either by changing specific cognitions (e.g. changing negative automatic thoughts) or via altering the antecedents and consequences of the behaviours. This review will address cognitive-behavioural interventions with physically abused children and their families, where the interventions are described by the authors as cognitive-behavioural treatment (or therapy) or are recognisably so from the details of the study, contact with the authors, and/or acquisition of a manual or other materials (cf. Dalsbo 2006). Programs may be individual, couple, or group based and delivered in any setting.

Types of outcome measures
Primary outcomes
The recurrence of child physical abuse (CPA) in the family. Only studies that have reported an outcome measure involving potential recurrence of child maltreatment are included.

Recurrence is defined as a confirmed report of physical child abuse in a family after treatment had begun or during the follow-up period. Child physical abuse is generally defined as the presence of a non-accidental injury resulting from acts of commission by an adult or defined as acts of commission that involve either demonstrable harm or endangerment to the child. These acts are characterized by overt physical violence or excessive punishment (including poisoning and exposing to extreme heat or cold) (cf. Kelly 1983; Wolfe 1988; Malinosky 1993).

Standardized or commonly used measures of family functioning. Examples of standardized measures of family functioning that are used in child welfare settings are the McMaster Family Assessment Device (Epstein 2003), the Family Adaptability and Cohesion Scale (Olson 2003), the Family Assessment Measure III (Skinner 2000).

Outcome assessment may involve post testing (immediately after intervention) and, where available, at 3-6 months, 12 months, 24 months and 36 months follow-up. The overall analysis for this review will be based on the assessment closest to the 1-year follow-up. If two (or more) assessments are equally close to the 1-year follow-up, then the time point furthest from the end of the intervention will be chosen.

**Search methods for identification of studies**

Several strategies will be used to identify studies, published or otherwise, that meet the criteria, including a keyword search of computerized databases, and examination of study registers in journals.

An overview (log book) of the final strategies for each database will be enclosed in an appendix in the final review so that all search strategies are reproducible.

In order to minimize publication bias, unpublished studies (e.g., conference papers, unpublished dissertations, or government reports) will be identified by contacting authors of included studies and researchers working in this area, asking about their knowledge about ongoing research programmes, and unpublished and working papers.

**Electronic searches**

The following databases will be searched:

- Academic Search Premier
- Acompline
- Campbell Collaboration Social, Psychological and Educational & Criminological Trials Register (C2-SPECTR)*
- ClinicalTrials.gov
- Cochrane Controlled Trials Register*
- Community of Science
- Criminal Justice periodical index
- Cumulative Index to Nursing and Allied health (CINAHL)
- Current Contents
- Bibsys*
Danbib*  
Dissertation Abstracts online  
Education Full Text  
Education on-line (Leeds University database)  
ERIC (Education Resource Information Center) *  
Evidence Based Medicine Reviews  
GPO Monthly (Government Printing Office Monthly)  
Government publication reference file  
Head Start Bureau research database, USA*  
IBSS  
Inside Web  
Web of Knowledge  
Libris *  
MEDLINE  
National Clearinghouse on Child Abuse and Neglect *  
NCJRS (National Criminal Justice Reference Service)*  
Political Science Abstracts  
PAIS International (Public Affairs Information Service)  
Planex  
PsycINFO, (Psychological Abstracts) *  
Psychology and Behavioral Sciences Collection  
Sociological abstracts, *  
Social Science Citation Index*  
Social Services Abstracts  
Social Science Search (SciSearch), (same as Social Science Citation Index?)  
Sociofile (Sociological Abstracts and Social Planning And Development Abstracts)  
US Political Science Documents  
* - covered by the Nordic Campbell Center/ SFI (The Danish National Institute of Social Research).  
The remainder of databases will be searched at the University of Maryland and the Virginia Commonwealth University School of Social Work.  
The following terms will be used in the Medline search strategy. This search strategy will be adapted to the other databases, using the appropriate controlled vocabulary as applicable.  
1. child abuse/ (MeSH)  
2. ((infan$ or child$ or teen$ or adolscen$ or toddler$ or baby or babies) adj3 (maltreat$ or neglect$ or abus$))  
3. or/1-2  
4. behavior therapy/ or cognitive therapy/ (MeSH)  
5. (cognitive adj3 (therap$ or train$)) OR (behavior?r$ adj3 (therap$ or train$)) OR (behavior?r$ adj3 modif$) OR (family adj3 therap$) OR CBT  
6. or/4-5  
7. 3 and 6
Searching other resources

References from recent systematic or traditional reviews of child abuse and cognitive-behavioural treatment will be hand searched, as well as the journal Child Abuse Review.

All references identified as potentially eligible for this review will be entered into Reference Manager. A standardized form for evaluating the eligibility of studies will be used. Two independent reviewers will assess titles and abstracts, and screen potential studies for inclusion in this review. Divergences will be assessed.

Data collection and analysis

Selection of studies

Titles and abstracts of studies identified through searches of electronic databases will be independently reviewed by CD and DdeP to determine whether they meet the inclusion criteria. If there is uncertainty or disagreement, then JC will be approached for adjudication. Studies in Scandinavian languages will be reviewed by NJA and KRA. If there is any uncertainty, MC will be approached. Studies in other languages will be referred to the editorial base of the CDPLPG. Studies clearly not relevant to the topic will be excluded. Relevant and potentially relevant articles will be retrieved by CD and DdeP and assessed independently by JC and MC against the inclusion criteria. If there is uncertainty or disagreement, then DdeP will be approached for adjudication. Additional information will be sought from the authors of studies if this will resolve any disagreement.

Data extraction and management

A preliminary data extraction /coding protocol has been developed for this review. Information on study design and implementation, sample characteristics, intervention characteristics and characteristics of providers, control group characteristics and providers, implementation fidelity (in both arms of the study), and outcomes will be extracted from studies and coded on a data extraction form, as well as aspects of methodological quality. Two reviewers (CD and DdeP) will independently code all studies except studies, written in Scandinavian languages. These studies will be reviewed by NJA and KRA. Differences between coders will be resolved by discussion; if there is uncertainty or disagreement, then JC and MC will be approached for adjudication.

Citations and data will be entered and organized in RevMan 5.0. Authors of studies with missing data will be contacted.

Assessment of risk of bias in included studies

Each reviewer (JC and MC) will independently assign included studies to quality categories in accordance with the dimensions mentioned below (Higgins 2008). Reviewers will seek to resolve any differences, but where this is not possible DdeP will adjudicate. Studies written in Scandinavian languages will be reviewed by NJA and KRA. If there is any uncertainty, MC will be approached. Studies in other languages will be referred to the DPLPG editorial base for assistance with translation and data extraction.
Sequence generation
Description: the method used to generate the allocation sequence will be described in detail so as to assess whether it should have produced comparable groups; review authors' judgment: was the allocation concealment sequence adequately generated?
Ratings: 'Yes' (low risk of bias); 'No' (high risk of bias) and 'Unclear' (uncertain risk of bias)

Allocation concealment
Description: the method used to conceal allocation sequence will be described in sufficient detail to assess whether intervention schedules could have been foreseen in advance of, or during, recruitment; review authors' judgment: was allocation adequately concealed?
Ratings: 'Yes' (low risk of bias); 'No' (high risk of bias) and 'Unclear' (uncertain risk of bias)

Blinding
Description: any measures used to blind participants, personnel and outcome assessors will be described so as to assess knowledge of any group as to which intervention a given participant might have received; review authors' judgment: was knowledge of the allocated intervention adequately prevented during the study?
Ratings: 'Yes' (low risk of bias); 'No' (high risk of bias) and 'Unclear' (uncertain risk of bias)

Incomplete outcome data
Description: If studies do not report intention-to-treat analyses, attempts will be made to obtain missing data by contacting the study authors. Data on attrition and exclusions will be extracted and reported as well the numbers involved (compared with total), reasons for attrition/exclusion where reported or obtained from investigators, and any re-inclusions in analyses performed by review authors; review authors' judgment: were incomplete data dealt with adequately by the reviewers? (See also 'Dealing with missing data', below).
Ratings: 'Yes' (low risk of bias); 'No' (high risk of bias) and 'Unclear' (uncertain risk of bias)

Selective outcome reporting
Description: attempts will be made to assess the possibility of selective outcome reporting by investigators; review authors' judgment: are reports of the study free of suggestion of selective outcome reporting?
Ratings: 'Yes' (low risk of bias); 'No' (high risk of bias) and 'Unclear' (uncertain risk of bias)

Implementation integrity
Implementation integrity in the intervention group and in the control group (where the control group is given an active intervention) will be assessed in the following way.
Information on the frequency of supervision or other measures (e.g., reviewed videotapes) for quality assurance will be obtained. Treatment integrity ratings reflecting the percentage of correctly administered content for CBT will be analyzed.

Detection bias

Discovering reoccurrences of child physical abuse may be an effect of the frequency of contacts between caseworkers/therapists and families during the treatment process and the follow-up process. Information about the frequency of these contacts both in the treatment group and the control group will be obtained. Missing information will be provided from investigators, if possible. Study quality will not be scored on an additive basis. The impact of varying aspects of study quality mentioned above will be determined by sensitivity analysis, if data exist.

Measures of treatment effect

Continuous data

Continuous data will be analyzed if means and standard deviations are available or can be obtained from primary investigators or otherwise derived. Where scales measure the same outcomes (e.g., child physical abuse) in different ways, standardized mean differences (SMD) will be compared across studies. The RevMan formula for SMD is Hedges' $g$, which is like Cohen's $d$ but includes an adjustment for small sample bias. Inverse variance methods will be used to pool SMDs, so that each effect size is weighted by the inverse of its variance in an overall estimate of effect size. Confidence intervals of 95% will be used for individual study data and pooled estimates (Hasselblad 1995).

Binary data

Binary outcomes will be analyzed by calculating odds ratios with 95% confidence intervals (Lipsey 2001). RevMan uses Mantel-Haenszel methods for combining binary outcome data across studies. If some primary studies report an outcome (e.g., child physical abuse) as a dichotomous measure and others use a continuous measure of the same construct, a conversion between the two metrics will be performed (cf. Chinn 2000). Although the odds ratio provides an effect for use in meta-analysis, 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 (Lipsey 2001; Littell 2005).

Dealing with missing data

Systematic differences between the experimental and comparison/control groups because of loss of participants (e.g. withdrawals, dropouts, protocol deviations) will be examined in the studies. If information describing loss to follow-up is missing, the information will be obtained from investigators, if possible. Both overall and differential attrition rates will be coded and sensitivity analysis will be carried out to determine if these features are related to the effect size.

Intent-to-treat analysis will be given preference when available, and the authors will report whether or not studies analysed data on an intention to treat basis. For dichotomous outcomes, the authors will assume that those who were lost to follow up all
experienced the unsuccessful outcome. For continuous outcomes, the authors will assume that pre-test mean scores remained the same.

For exclusions, when data to compute effect sizes are unavailable from the primary studies, effect sizes will be estimated on the basis of other information, such as confidence intervals, P-values, risk ratios, hazard ratios, or odds ratios. If studies report "non significant" findings and provide no other data, the 'worst case' of effect size and variances will be estimated based on the boundaries of the significant tests (e.g., 95% boundaries). The smallest effect size and its variance will be imputed in the meta-analysis.

Assessment of heterogeneity

Heterogeneity will be evaluated in three ways:

1) with $I^2$, 2) the Chi-square test of heterogeneity test, and 3) by comparing results of fixed and random effects models (Higgins 2002). Possible sources of heterogeneity will be investigated, if there is evidence of heterogeneity (i.e. the statistical test of heterogeneity is statistically significant and $I^2 > 50\%$) (Deeks 2006).

If substantial heterogeneity is found in the primary study, the following factors will be considered as possible explanations: design quality, publication bias, voluntary or mandatory participation, intensity or length/period of the intervention, and differences in offending parents' characteristics, such as multiple problems/disorders. Studies also may be grouped according to characteristics of parents' or carers' substance abuse (e.g., crack cocaine, alcohol), history of violence, age groups of children, severity of the maltreatment (e.g. Maltreatment Classification System cf. Barnett 1993). If there are many primary studies, we will subgroup them according to these variables, and perform a moderator analysis (meta-regression) in order to identify whether these possible sources of heterogeneity appear to be important. If the primary studies are judged to be substantially heterogeneous even within these sub-groupings, only a descriptive analysis will be performed, particularly if there is variation in direction of effect.

Assessment of reporting biases

on or clinical diversity (differences in the population of families) between studies will be examined as possible explanations.

Data synthesis

Data synthesis will be conducted with RevMan 5.0, the latest version of the Cochrane Collaboration's meta-analysis software. When a primary outcome study reports multiple measures of the same construct (i.e., the recurrence of child physical abuse (CPA) defined as a confirmed report of child maltreatment in a family) at different points in time, we will use the single measure that is closest to 1-year follow-up as an overall measure. When a primary outcome study reports multiple measures of the same construct at the same points in time, we will use the average assessment as an overall measure.

Subgroup analysis and investigation of heterogeneity
The review will examine variation in the general recurrence odds-ratios by coded methodological moderator variables. Moderator analysis will involve multiple regression or categorical comparisons on the following three moderator variables:

The first moderator analysis will be performed on the type of design used in studies as follows:

(i) The lowest level of method quality (Farrington 2002) were weak quasi-experimental designs; such studies utilized a comparison that lacks comparability to the treatment group before the intervention. To determine group comparability, we will examine the statistical tests reported in the primary studies (t-tests, chi-squares or similar tests) to determine if there were significant differences between the initial parent and child characteristics between the treatment and comparison groups.

(ii) The next level of method quality, "standard quasi-experiment," was assigned to contrasts involving treatment and comparison groups that did not have statistically significant differences on important observed variables (e.g., age of the child; race); characteristics of parent or carers (aggression, hostility neuroticism, substance abuse), and environmental stressors (very low income; prior severity of the maltreatment). Experimental designs that were not well-implemented may also be downgraded to this category.

(iii) The highest level of method quality, "experimental designs," used random assignment of research participants to conditions and did not have large attrition problems. Quasi-random allocation (e.g., by day of week, case number or alphabetic order) is also included in this category.

Experimental designs and quasi-experimental designs will be meta-analyzed separately. The central weakness of all non-randomised controlled studies is their inability to control for unknown confounders.

In the second moderator analysis, studies will be grouped according to the characteristics of the control group. The control group may have no intervention (NI), routine community services (RCS), family therapy (FT), or another intervention. A separate analysis will be done for each group of studies.

In the third moderator analysis studies will be grouped according to characteristics of outcome assessment (e.g., immediately after intervention, and where available, at 3-6 month, 12 month, 24 month, and 36 month follow-up). Separate comparisons will be made between studies with outcomes of short term (e.g., within 3 months), medium term, and long term measures (e.g., 1-year or more)

**Sensitivity analysis**

Sensitivity analysis will be used to examine the robustness of the conclusions. Impact of differing methodological quality will be assessed by sensitivity analysis.

a) Intention to treat. We will do three separate analyses to see if differences in attrition are related to effect size. We will assume that those who were lost to follow up: (i) had proportionately the same outcomes as those who completed the control group; (ii) all experienced the successful outcome; (iii) all experienced the unsuccessful outcome. In our primary analyses we will take the reported data as given.
b) Differential drop-out. Studies with statistically significant (p<0.01) imbalance in terms of numbers of attrition will be excluded from the analysis to assess their influence on the overall result.

c) Other separate analyses might include, for example, randomised vs. quasi-randomised trials, and those studies in which the outcome assessors were blind to treatment allocation.

Use of data on program costs
We will code and summarize available data on the costs of experimental intervention and control groups' intervention within the studies under review.

Results
Description of studies
Risk of bias in included studies
Effects of interventions
Discussion
Authors' conclusions
Implications for practice
Implications for research

Acknowledgements
Contributions of authors
MC, DD, JC, and CD contributed to the drafting of the protocol. Anne-Marie Klint Jørgensen, Librarian with the Nordic Campbell Center assisted reviewers with developing the search strategy. CD will contribute to running the searches, will retrieve potentially relevant studies, and will conduct data extraction for selected studies. MC and JC will select studies and write the review. DD will provide topic expertise and contribute to writing and editing the review. JC will provide methodological advice, guidance and will help to write the review.

Declarations of interest
None known.

Differences between protocol and review
Published notes
Other references

Additional references

Azar 1988

Azar 2006

Barnett 1993

Briere 2003

Chaffin 1997

Chaffin 2004

Chaffin 2006

Chinn 2000

Christoffersen 2002
Christoffersen MN. Social support for children - A study of children who received preventative help for the first time in 1998 in relation to the social service legislation. The 5th evaluation report of preventive programs for children and youth. The Danish National Research Centre, Working paper 9. (Social støtte til børn - En undersøgelse af børn, der
modtog forebyggende hjælp i henhold til Serviceloven for første gang i 1998. 5. delrapport i evaluering af den forebyggende indsats over for børn og unge.
Socialforskningsinstituttets Arbejdspapir 9 [Danish]). Copenhagen: SFI, 2002.

**Conaway 1989**

**Corcoran 2000**

**Dalsbø 2006**

**Deeks 2006**

**Dubowitz 2000**

**Duvall 2000**

**Egger 2001**

**Epstein 2003**

**Eyberg 1994**

**Farrington 2002**
Farrington DP, Gottfredson DC, Sherman LW, Welsh BC. The Maryland Scientific Methods Scale. In: Lawrence W. Sherman, David P. Farrington, Brandon C. Welsh, and

**GBONS / DFES 2005**


**Graziano 1992**


**Hansen 1989**


**Hasselblad 1995**


**Higgins 2002**


**Higgins 2008**


**Jackson 2006**


**Kelly 1983**


**Kolko 1996**


**Kolko 2000**


**Kolko 2002**

**Kolko 2002a**


**Lipsey 2001**


**Littell 2005**


**Lundahl 2006**


**Macdonald 2004**


**Malinosky 1993**


**Milner 1986**


**Mitchell 2006**


**Mueller 1989**


**Olson 2003**

Runyon 2004

Skinner 2000

Toth 1992

Trocme 2005

USDHHS 2005

Wilson 2002

Wolfe 1988

Zuskin 2000

Data and analyses
Figures
Sources of support
Internal sources
• The Danish National Institute of Social Research, Copenhagen, Denmark
• University of Maryland School of Social Work, Baltimore, MD, USA
• Virginia Commonwealth University School of Social Work, Northern Virginia Campus, Alexandria, VA, USA

**External sources**

• The Danish Health Insurance Fund, Denmark
• The Nordic Campbell Centre, Denmark

Feedback

Appendices