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# **Effects of parental imprisonment on child antisocial behaviour and mental health: a systematic review**

Joseph Murray, David P. Farrington, Ivana Sekol,  
Rikke F. Olsen



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## Plain language summary

Parental imprisonment can cause many problems for the family left behind, including difficulty organising childcare, loss of family income, trouble maintaining contact with the imprisoned parent, stigma, and home, school and neighbourhood moves. Children and parents can be distressed by the separation. Children may respond by acting out or becoming withdrawn, anxious or depressed. We conducted an exhaustive search for studies that examined children's antisocial behaviour and mental health after parental imprisonment. We found 16 studies with appropriate evidence. These studies all showed that children of prisoners are more likely than other children to show antisocial and mental health problems. However, it was unclear whether parental imprisonment actually caused these problems. They might have been caused by other disadvantages in children's lives that existed before parental imprisonment occurred. Children of prisoners are a vulnerable group. More research is required to determine whether or not parental imprisonment causes an increase in child antisocial behaviour and mental health problems.

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# Executive summary/Abstract

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## BACKGROUND

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The number of children with parents in prison is increasing in many countries worldwide. Theory and qualitative research suggest that parental imprisonment might contribute to child antisocial behaviour and mental health problems, because of the trauma of separation, strained child-care arrangements during parental imprisonment, loss of family income, other stressful life events such as moving home and school, and the stigma of parental imprisonment.

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## OBJECTIVES

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The first aim of this review is to assess evidence on parental imprisonment as a predictor of child antisocial behaviour (including criminal behaviour) and poor mental health. The second aim is to assess evidence on the possible causal effects of parental imprisonment on these outcomes. A third aim is to investigate whether characteristics of children, parents, prisons, and wider social and penal settings might moderate the effects of parental imprisonment on children.

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## SEARCH STRATEGY

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We searched for studies of children of prisoners by contacting experts in the field, examining the bibliographies of prior reviews, and searching electronic databases of references for the years 1960 to 2008. We searched to identify both published and unpublished literature. The searches were international in scope. Over 10,500 references were screened, 319 full text reports were retrieved, and 165 reports of studies of children of prisoners were identified.

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## SELECTION CRITERIA

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Studies that compared children of prisoners with children whose parents were not imprisoned on antisocial or mental health outcomes were first identified as studies that might be eligible for the review. Studies were included in the review if the



comparison group of children was either selected to represent the general population of children (to estimate the strength of prediction of child outcomes following parental imprisonment) or to be similar to children of prisoners on confounding variables (to estimate the causal effects of parental imprisonment on children). Sixteen studies were eligible for the review.

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## **DATA COLLECTION AND ANALYSIS**

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The results of 16 studies are described in a narrative review and in a meta-analysis. Weighted mean effect sizes are reported for the associations between parental imprisonment and child outcomes. Moderator analyses were used to investigate possible explanations for variations in the study results.

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## **MAIN RESULTS**

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Children of prisoners have about twice the risk of antisocial behaviour and poor mental health outcomes compared to children without imprisoned parents. All except one of the studies suggested that parental imprisonment might cause an increase in these outcomes for children (i.e., had positive effect sizes even after controlling for covariates). However, these tests of causal effects might be systematically biased because studies often did not control for prior child behaviour, parental criminality, and other important confounds associated with parental imprisonment. There were not enough studies to conduct more than exploratory analyses of moderators of the relationship between parental imprisonment and child outcomes.

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## **REVIEWERS' CONCLUSIONS**

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We conclude that children of prisoners are at greater risk of undesirable outcomes than their peers. However, it is not known whether parental imprisonment causes an increase in risk for children or whether other disadvantage in children's lives accounts for this association. There is increasing research interest in the possible effects of parental imprisonment on children. It is important to conduct new research that can estimate the causal effects of parental imprisonment on children more accurately, and investigate mediators and moderators of its effects.

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# 1 Introduction

With rates of imprisonment growing rapidly in many countries worldwide (Walmsley, 2005), the possible effects of parental imprisonment on children is an issue of increasing social concern. Children of prisoners have been called the "forgotten victims" of crime (Matthews, 1983), the "orphans of justice" (Shaw, 1992), the "hidden victims of imprisonment" (Cunningham & Baker, 2003), "the Cinderella of penology" (Shaw, 1987, p. 3), and the "unseen victims of the prison boom" (Petersilia, 2005, p. 34). This review examines the possible effects of parental imprisonment on child antisocial behaviour and mental health.

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## 1.1 THE PREVALENCE OF PARENTAL IMPRISONMENT

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In many countries, there is little information about how many children have parents in prison. National inmate surveys in the United States show that the number of children under age 18 with an imprisoned parent increased from 945,600 in 1990 to 1,706,600 in 2007, reaching 2.3% of the nation's children (Glaze & Maruschak, 2008). Although the number of mothers in prison has recently been increasing more rapidly than the number of fathers in prison, still the vast majority of children with a parent in prison have a father in prison (91% in the United States, Glaze & Maruschak, 2008). Black children (6.7%) in the United States are seven and a half times more likely than white children (0.9%) to have a parent in prison, and Hispanic children (2.4%) are more than two and a half times more likely than white children to have a parent in prison (Glaze & Maruschak, 2008).

Provisional estimates suggest that around 125,000 (about 1%) of children under age 18 have a parent in prison in England and Wales (Murray, 2007). Ayre, Philbrick, and Reiss (2006), estimated that the number of children with parents in prison was 4,400 in Ireland, 68,800 in France, 73,500 in Italy, 8,500 in Sweden, 17,100 in Portugal, 79,500 in Spain, and 26,100 in the Netherlands (based on the assumption that each prisoner has an average of 1.3 children). Even less is known about the cumulative number of children who experience parental imprisonment any time during childhood. However, Wildeman (2009) estimated that one in forty white children and a staggering one in five black children born in the United States in 1990 had one of their parents imprisoned before their ninth birthday. Quilty (2005)

calculated that about 5% of all children under 16 have ever had a parent imprisoned in Australia.

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## **1.2 DEFINITIONS OF KEY TERMS**

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### **Parental imprisonment and criminality**

In this review, we use the term parental imprisonment to refer to any kind of custodial confinement of a parent by the criminal justice system, except being held overnight in police cells. We do not examine the issue of parents being held as a prisoner of war (e.g., McCubbin, Dahl, Lester, & Ross, 1977; Najafi, Akochkian, & Nikyar, 2007). Imprisonment can refer to confinement in jails or prisons (state or federal, in the United States) and open or closed prisons (local or training, in the United Kingdom). This review concerns the possible environmental effects of parental imprisonment on children, and focuses on parental imprisonment that occurs during childhood, as opposed to parental imprisonment occurring before children's births. Parental criminality refers to parental criminal behaviour (i.e., committing acts that are against the law and could be grounds for criminal conviction) and parental propensity to engage in criminal behaviour.

### **Child outcomes**

We review two types of undesirable outcome for children that might follow parental imprisonment: antisocial behaviour and mental health problems. These two outcomes were chosen because theory suggests that parental imprisonment might contribute to these problems and prior reviews suggested that they have been studied quite frequently as outcomes for children of prisoners. Antisocial behaviour refers to a wide variety of behaviours that violate societal norms or laws (Rutter, Giller, & Hagell, 1998). We examine antisocial behaviour (also called externalising behaviour) that does not necessarily involve criminal activities, for example persistent lying and deceit, as well as criminal behaviour as measured by self-reports, arrests, convictions or imprisonment of the child. We restrict our review of mental health problems to internalising problems. Internalising problems primarily refer to anxiety and depression (Goldberg & Goodyer, 2005). Substance abuse in the absence of other antisocial or mental health problems is not examined as an outcome. Child outcomes can occur any time following parental imprisonment: while parents are in prison or after release, in childhood or in adulthood. Thus, by child outcomes we mean outcomes for children of prisoners, not outcomes that necessarily happen in childhood.

To assess the relationship between parental imprisonment and child outcomes, we find it useful to consider whether or not parental imprisonment is a risk factor or a causal risk factor, using the definitions of these terms provided by Helena Kraemer et al. (Kazdin, Kraemer, Kessler, Kupfer, & Offord, 1997; Kraemer et al., 1997; Kraemer, Lowe, & Kupfer, 2005). These terms are defined below.

## **Risk factors**

Risk factors are variables that are shown to predict an outcome. Prediction requires association and precedence. Association should be tested by comparing outcomes for children of prisoners and children in the general population without imprisoned parents (this is called the bivariate association). In addition, it should be demonstrated that the risk factor precedes the outcome. Hence, longitudinal data are required. "That key distinction between a correlate and a risk factor, the temporal precedence of the factor, relates to what is perhaps the most common mistake in research: calling a factor, shown only to be a correlate, a risk factor" (Kraemer et al., 2005, p. 16). Thus, to investigate whether parental imprisonment is a risk factor, studies should examine the bivariate association between parental imprisonment and a later child outcome. Causal risk factors are risk factors that can change and, when changed, cause a change in risk for the outcome. To establish that something is a causal risk factor, association and precedence need to be demonstrated, and exposure to the risk factor must be shown to cause an increase in the outcome.

## **Causal risk factors**

Causal risk factors are the 'gold' of risk estimation - they can be used both to identify those of high risk of the outcome and to provide the bases for interventions to prevent the outcome" (Kraemer et al., 2005, pp. 32-33). The term causal risk factor is used instead of cause, because the term cause can suggest deterministic effects, and causal relations in social science are probabilistic (Farrington, 1988; Kraemer et al., 2005): changes in X are followed by changes in Y with a certain probability. Causal risk factors should be tested by investigating changes in the outcome following changes in the risk factor while controlling for confounding variables, in an experimental or quasi-experimental study, or using statistical controls.

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## **1.3 THE POSSIBLE EFFECTS OF PARENTAL IMPRISONMENT ON CHILDREN**

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Given that both parental criminality (Farrington, Coid, & Murray, 2009; Farrington, Jolliffe, Loeber, Stouthamer-Loeber, & Kalb, 2001; Rakt, Nieuwbeerta, & Graaf, 2008) and "broken homes" (Amato, 2001; Amato & Keith, 1991; Bowlby, 1946; Juby & Farrington, 2001) are established risk factors for child antisocial behaviour and mental health problems, it seems likely that parental imprisonment also predicts these outcomes. It is important to test whether parental imprisonment does indeed predict undesirable child outcomes, because this would suggest that children of prisoners need extra support. If it were found that parental imprisonment does not predict child outcomes, it would be unlikely that it is a causal risk factor.

An important body of in-depth, qualitative research on families and children of prisoners suggests that parental imprisonment might be a causal risk factor. This

research has described the many difficulties for families and children that can follow parental imprisonment, including psychological distress, confused explanations given to children, changes in child care arrangements, difficulties in maintaining contact with imprisoned parents, loss of family income, stigma associated with the imprisonment, and home and school moves (see, e.g., Boswell, 2002; Braman, 2004; Henriques, 1982; Kampfner, 1995; Pellegrini, 1997; Poehlmann, 2005; Richards et al., 1994; Sack, 1977; Sack, Seidler, & Thomas, 1976; Skinner & Swartz, 1989). These studies suggest that parental imprisonment can cause multiple life changes and psychological difficulty for children, and it is possible that this contributes to children's antisocial behaviour and mental health problems.

Four key criminological theories suggest that parental imprisonment might cause an increase in child antisocial and criminal behaviour (for detailed discussions see Hagan & Dinovitzer, 1999; Murray & Farrington, 2008a). First, social bonding theory suggests that parental imprisonment might harm children because parent-child separation disrupts children's attachment relations (for a detailed discussion of this theory, see Murray & Murray, in press).

Second, strain theory (Agnew, 1992; Agnew, Brezina, Wright, & Cullen, 2002; Merton, 1938) suggests that the loss of family income and other negative life events after parental imprisonment might cause an increase in offending behaviour. According to strain theory, life stresses tend to increase negative affect and cause children to attack or try to escape the source of adversity, use illegitimate means to achieve their goals, or manage the negative affect through use of illicit drugs (Agnew, 1992).

Third, social control theory suggests that parental imprisonment might cause delinquency via reduced quality of care and supervision of children. Fourth, labeling theory suggests that social stigma and official bias following parental imprisonment might cause an increased probability of the child being charged or convicted for criminal behaviour. These processes of attachment disruption, strain, poor quality childcare, and stigma are also associated with mental health problems for children (Garber, 2000; Harrington, 2002; Hinshaw & Cicchetti, 2000; Klein & Pine, 2002). Hence, parental imprisonment might contribute to both antisocial behaviour and mental health problems for children.

These theories suggest that parental imprisonment is most likely to affect children who directly experience the event, although parental imprisonment may also indirectly affect children via increased economic strain or stigma. Therefore, we hypothesise that parental imprisonment experienced during childhood is likely to have stronger effects than parental imprisonment occurring before birth or in cases where children are not living with their parent.

Two alternative theories suggest that parental imprisonment does not contribute to undesirable outcomes for children. First, even if parental imprisonment predicts undesirable outcomes for children, this might be because of parental criminality and disadvantage before the imprisonment, not because parental imprisonment itself causes these problems. Second, imprisonment of an abusive or antisocial parent might actually decrease children's likelihood of developing behaviour problems because it removes a disruptive and antisocial influence from their lives (see, e.g., Jaffee, Moffitt, Caspi, & Taylor, 2003). Existing evidence needs to be synthesized to evaluate these competing hypotheses.

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## **1.4 OBJECTIVES OF THE REVIEW**

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The two main aims for the review are to assess evidence on whether parental imprisonment is a risk factor for undesirable child outcomes and to assess evidence on whether parental imprisonment is a causal risk factor. A third aim of the review is to investigate whether associations between parental imprisonment and child outcomes differ according to child, parent, and environmental characteristics (moderators). The main moderators we hoped to investigate were child sex, child age at parental imprisonment, maternal versus paternal imprisonment, length of parental imprisonment, and country of study (categorised by length of prison sentences and rates of imprisonment). Ideally, if enough studies reported relevant information, we hoped to investigate other moderators, such as quality of parenting, frequency of child-parent contact before and during imprisonment, social support, family income, and type of prison. We also aimed to analyse whether results varied in relation to the methodological characteristics of studies (e.g., by type of study design and publication type).

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## **1.5 SYSTEMATIC REVIEWS ON RISK FACTORS**

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Parental imprisonment is not an intervention as typically studied in Campbell systematic reviews. It is not a deliberately implemented programme aimed to reduce or prevent undesirable outcomes. Instead it is a type of criminal justice treatment of adults that might have unintended consequences for children. Moreover, effects of parental imprisonment have not been evaluated in randomized experiments, as have other criminal justice interventions. In principle, the effects of parental imprisonment on children could be studied in a randomised experiment, by including child outcome measures in a study similar to the one conducted by Killias, Aebi, and Ribeaud (2000a; 2000b), which randomly assigned people who had been convicted for a crime (and volunteered for the study) to prison (the usual sentence) or community service. However, studies of parental imprisonment have not used this experimental approach. They have been observational, using matched comparison groups and statistical balancing techniques to investigate possible effects on children. According to existing reviews, most studies have been of poor

methodological quality, with low internal validity (Hagan & Dinovitzer, 1999; Murray, 2005; Murray & Farrington, 2008a).

An important issue is whether it is worth conducting a systematic review when it appears that there are few high quality studies from which to draw confident causal conclusions. We think that it is worthwhile for the following reasons. First, a systematic review might uncover high quality studies that were not found using less thorough searching methods. Second, more high quality studies may take a long time to appear, and policy-makers need interim evidence with which to consider their decisions. Third, if a systematic review demonstrates that high quality studies are lacking, this could encourage a new generation of higher quality primary research. Hence, even though existing reviews suggest that there are few high quality studies of parental imprisonment, we believe it is still worth conducting a systematic review on this topic.

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## 2 Methods

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### 2.1 CRITERIA FOR INCLUSION OF STUDIES IN THE REVIEW

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The scope of this review is studies that compare antisocial behaviour or mental health between a group of children with imprisoned parents and a group of children whose parents have not been imprisoned. The first set of seven eligibility criteria that were used to identify studies for the review is shown below. Studies had to meet all seven criteria to be eligible:

1. The study must include children of prisoners and at least one group of children without imprisoned parents.
2. The study must include a measure of child antisocial behaviour or mental health.
3. The child outcome must have been measured after parental imprisonment first occurred. (Note, some eligible studies were still ambiguous regarding the timing of parental imprisonment and the child outcome. This was because the reference period of the child outcome measure overlapped with when parental imprisonment first occurred. Rather than excluding such studies from the review, we point out this problem where it is relevant, and treat it as a methodological quality issue for consideration in the review.)
4. The study must use the same measure of child outcome for children of prisoners and the comparison group.
5. Numerical information: At least one effect size must be reported, or there must be enough numerical information to calculate at least one effect size.
6. Publication: Studies may be published or unpublished.
7. Location and language: Studies may be conducted in any country and may be reported in English, German, Dutch, French, Spanish, Portuguese, Swedish, Danish, or Norwegian.

After initial screening of studies using the seven eligibility criteria above, it was clear that additional criteria were required to exclude other studies that were not relevant to the review's objectives. The following three criteria were added to select studies for inclusion in the review. Thus, studies had to meet a total of 10 criteria to be eligible for the review.



8. Studies were excluded if all children were recruited from courts or mental health clinics. We did not exclude studies that recruited parents from courts (e.g., to compare children of imprisoned parents with children of parents on probation). Rather, we excluded studies in which children themselves were all recruited from courts or mental health clinics. In these studies, comparison children are clearly not representative of the general population of children. As such, they are not suitable for assessing the bivariate association between parental imprisonment and child outcomes, and testing whether parental imprisonment is a risk factor. In addition, the causal effects of parental imprisonment cannot be estimated when all children in the study have a delinquent or mental health outcome (as indicated by being at a court or clinic).
9. Studies were excluded if the only comparison group of children was separated from a parent for other reasons, or were the best friends of children of prisoners. Comparing children of prisoners with these children does not provide estimates of the bivariate associations between parental imprisonment and child outcomes. Also, because separation of children from parents for other reasons may also cause undesirable outcomes, specifying this as the comparison condition may underestimate the causal effects of parental imprisonment on children. Children who are the best friends of children of prisoners may be influenced by the behaviour of children of prisoners, and so are not a suitable comparison group for estimating the effects of parental imprisonment on children.
10. One adoption study was excluded from the review because its design could only be used to estimate the genetic association between maternal imprisonment and child outcomes, not the environmental effects of parental imprisonment on children.

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## **2.2 THE SEARCH STRATEGY**

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Between June and September 2008, we searched for eligible studies. Several strategies were used to conduct an exhaustive search for eligible studies. We started with an existing set of documents collected by Joseph Murray during his previous research on the effects of parental imprisonment on children (Murray, 2005, 2006, 2007; Murray & Farrington, 2005, 2006, 2008a, 2008b; Murray, Janson, & Farrington, 2007). We then used three methods to search for additional studies. First, we searched electronic databases using keywords, as described below. Second, we examined bibliographies of prior reviews (Dallaire, 2007; S. Gabel, 2003; Hagan & Dinovitzer, 1999; Johnston, 1995; Murray, 2005; Murray & Farrington, 2008a; Myers, Smarsh, Amlund-Hagen, & Kennon, 1999; Nijnatten, 1998). Third, we contacted experts in the field. Using these search methods, we compiled a list of 10,727 references of reports that might be relevant to our review.

The keywords that were used to search electronic databases for relevant studies are shown in Figure 1.

*Figure 1. Keywords used to search electronic databases*

Prison\* OR Jail\* OR Penitentiary OR Imprison\* OR Incarcerat\* OR Detention  
AND  
Child\* OR Son\* OR Daughter\* OR Parent\* OR Mother\* OR Father\*  
AND  
Antisocial\* OR Delinquen\* OR Crim\* OR Offend\* OR Violen\* OR Aggressi\* OR  
Mental health OR Mental Illness OR Internaliz\* OR Depress\* OR Anxiety OR  
Anxious OR Psychological\*

The 23 electronic databases that were searched for the years 1960-2008 are shown in Figure 2 below (numbers in parentheses show the number of non-duplicated hits retrieved from each database).

*Figure 2. Electronic databases searched for the review*

- Bibliography of Nordic Criminology (16)
- Blackwell/Wiley (0)
- C2-SPECTR (3)
- Cochrane (13)
- Criminal Justice Abstracts (1,689)
- Dissertation Abstracts (728)
- Education-Line (2)
- Embase (409)
- ERIC (357)
- Google (26)
- Google Scholar (9,140, of which the first 1,000 could be examined)
- Ingenta (217)
- JSTOR (779)
- Medline (408)
- National Institute Of Corrections Information Centre (73)
- National Criminal Justice Reference Service (1,079)
- Newton: University Of Cambridge Library Catalogue (99)
- PsychInfo (1,517)
- Science Direct (658)
- System for Information on Grey Literature in Europe (17)

- Sociological Abstracts (571)
- Springer (0)
- Web of Science (1,003)

Two groups of researchers and practitioners were emailed and asked to bring to our attention any studies that they thought might be eligible for the review. The first group consisted of about 65 researchers and practitioners who we knew had a professional interest in children of prisoners. The second group consisted of about 30 directors of major longitudinal studies in criminology (for a list of these studies see Farrington & Welsh, 2007, pp. 29-36). We thought that these longitudinal researchers might have important results that were eligible for our review but were not published or were hidden in articles that did not mention parental imprisonment in titles or abstracts. From all these sources, 10,727 references were retrieved for further screening.

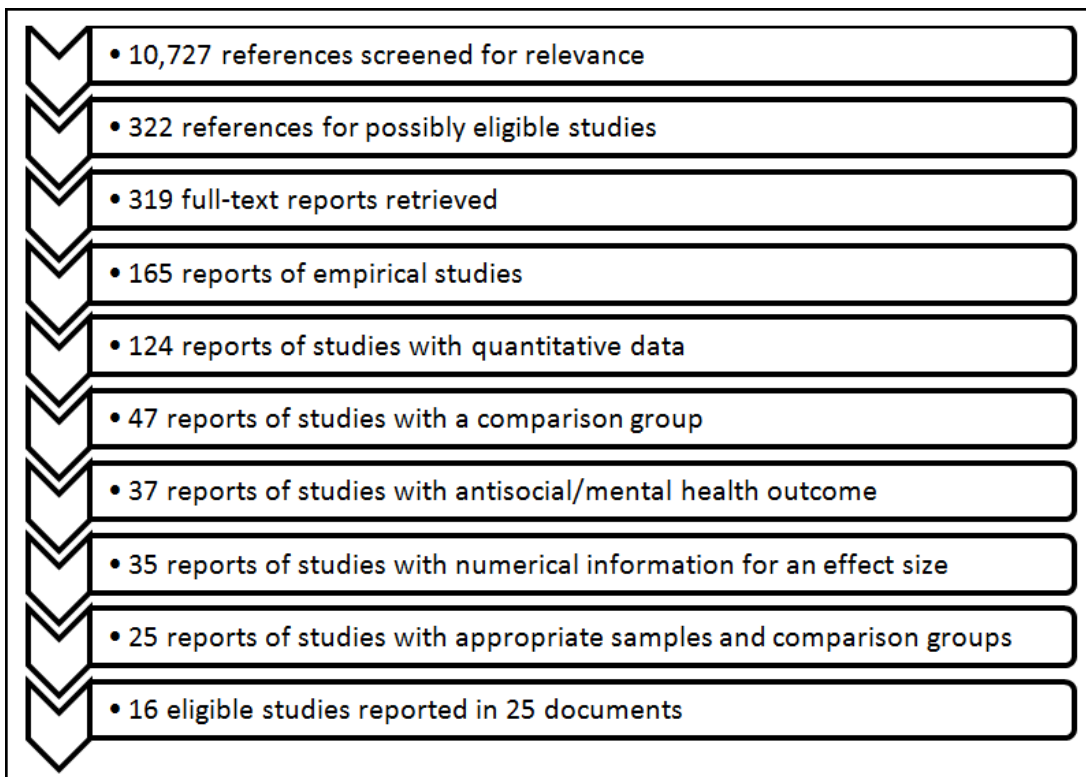
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### 2.3 SCREENING FOR ELIGIBLE STUDIES

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A flow chart of the screening process is shown in Figure 3 below.

*Figure 3. Screening for eligible studies*



We screened the titles (and abstracts if titles looked possibly relevant) of the 10,727 reports identified in our searches. Reports that were obviously not relevant to the review were discarded by Ivana Sekol, leaving 322 reports that looked possibly

eligible for the review. 319 were retrieved as full-text reports for further inspection. 165 of these reports described an empirical study of children of prisoners (and were not review articles). From these 165 reports, we excluded studies that did not meet one or more of the ten eligibility criteria described above. This process was conducted by Rikke Olsen and Ivana Sekol, with reference to Joseph Murray in cases of doubt. Joseph Murray made the final inclusion/exclusion decision in cases of doubt. Forty-one empirical studies were qualitative, which were not eligible for the review.<sup>1</sup> Out of the remaining 124 reports, 99 were not eligible for the review for the following reasons. (a) Seventy-seven reported on studies that did not include a comparison group of children without imprisoned parents. (b) Ten other studies did not include a measure of antisocial behaviour or mental health as a child outcome. (c) Two studies (Guo, Roettger, & Cai, 2008; Kampfner, 1995) did not have numerical information with which to calculate an effect size. (c) Six reports (Bryant & Rivard, 1995; Dannerbeck, 2001, 2005; Evens & Stoep, 1997; Stewart Gabel & Shindlecker, 1993; Phillips, Burns, Wagner, Kramer, & Robbins, 2002) were of studies which recruited all children from health clinics, courts or social services. (d) Two studies used comparison groups of children who were separated from their parents for other reasons (Moerk, 1973) or were best friends of the prisoners' children (Trice & Brewster, 2004). (e) Two reports (Crowe, 1972, 1974) were based on an adoption study, which was designed to measure the genetic effects of parental imprisonment on children. After eliminating studies that did not meet all ten eligibility criteria, 16 studies (reported in 25 documents) were identified as eligible for the review. Appendix A lists the 140 references to empirical studies that were excluded from the review.

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## **2.4 INCREASING RESEARCH ON CHILDREN OF PRISONERS**

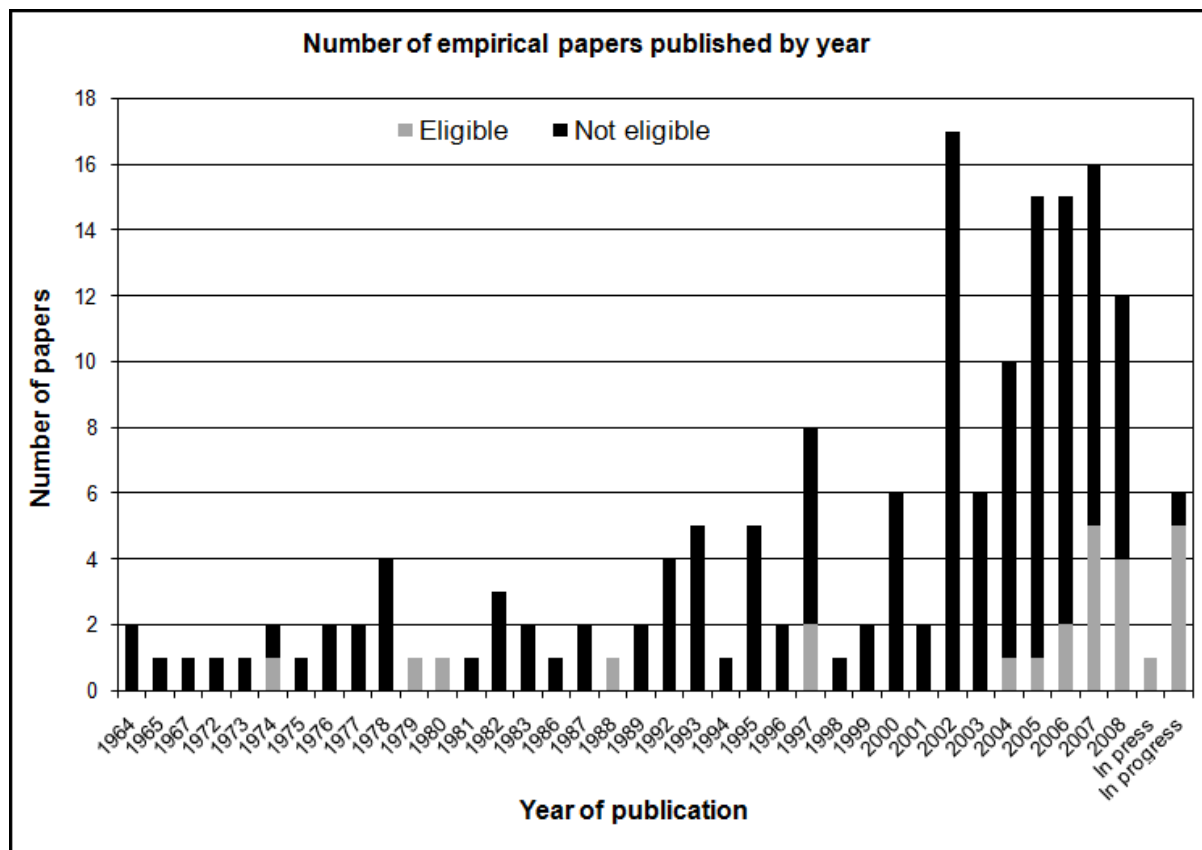
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Figure 4 shows the number of studies of children of prisoners that were published each year. Although few studies of children of prisoners were conducted between 1960 and 2000, there has been a surge of research interest in this topic since 2000. Indeed, most of the studies that were eligible for this review were conducted in the last five years.

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<sup>1</sup> Having retrieved many qualitative studies about children of prisoners, we would be delighted if colleagues with good qualitative research skills would like to collaborate in reviewing these studies.

Figure 4. Increasing research interest in children of prisoners



## 2.5 CODING OF STUDIES

Studies included in the review were coded for the following key features by Joseph Murray. A copy of the full coding sheets is included in Appendix B.

- Reference information (title, authors, publication year, etc.)
- Sample characteristics (age, gender, ethnicity, response rates, etc.)
- Details about the measure of parental imprisonment
- Details of sub-samples, and multiple comparisons made in the study
- Details of the comparison group(s) used to derive effect sizes
- Types of outcome measured, and measurement details
- Methods used to control for confounding variables to estimate causal effects
- Methodological quality of the study for drawing conclusions about risk factors and causal risk factors (see section below on methodological quality assessment)
- Statistical information used to derive an effect size

If some statistical information was missing that was needed to calculate an effect size, study authors were contacted to try to obtain the relevant information. If other

information was not available in a study (e.g., details about the measurement of parental imprisonment), these variables were coded as missing.

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## 2.6 METHODOLOGICAL QUALITY ASSESSMENT

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An important aspect of a Campbell Collaboration review is the careful examination of the quality of the evidence. This review aims to integrate evidence about whether parental imprisonment is a risk factor for undesirable child outcomes and whether it is a causal risk factor. We assessed the methodological quality of studies for drawing conclusions about risk factors and causal risk factors using eight criteria. These criteria were adapted from a set of checklists for evaluating risk factor and causal risk factor research in systematic reviews (see Murray, Farrington, & Eisner, 2009). Joseph Murray coded the studies in this review using these eight criteria. Given that the studies were coded by one person, data on reliability of the scores are not available.

### Criteria for assessing if parental imprisonment is a risk factor

As defined in the introduction, a risk factor is a variable that is both associated with and precedes an outcome in a population. To assess whether parental imprisonment is a risk factor, studies need to use representative sampling methods, include a reasonable number of study participants, use good measures of parental imprisonment and the child outcome, and clearly establish that parental imprisonment came before the outcome (Kraemer et al., 2005; Murray et al., 2009). To evaluate study quality for drawing conclusions about whether parental imprisonment is a risk factor, we used six criteria described below. On each item, studies were coded ‘1’ (study feature present) or ‘0’ (study feature not present, or not able to determine). A score of ‘1’ indicates high quality and ‘0’ indicates low quality. If it was not possible to determine whether a study feature was present (because of a lack of information), the study was scored ‘0’ for that item because, without positive information about study quality, confident conclusions cannot be drawn.

#### 1. Adequate sampling method

1 Total population sampling <i>OR</i> random sampling.
0 Convenience sampling <i>OR</i> case-control sampling.

Representative samples are needed so that results can be generalised to study populations. Two issues about generalisability need to be distinguished here. The first is whether the study population (the universe from which the study sample is drawn) is part of the wider population of interest in the review. This issue concerns the eligibility of the study for the review, not the quality of the study. If the study population is not part of the wider population of interest (for example, if the study

used a sample of children of prisoners of war) the study was not included in the review at all.

The second issue, which is relevant to study quality, is whether or not the study sampling method was likely to produce a sample representative of the study population. Some sampling methods produce samples that are more representative of the study population than other methods, and can be used to draw more confident conclusions about risk factors. If an entire population is included in a study, clearly findings are representative of that population. Random sampling, in which every member of the study population has an equal chance of being included in the sample, is the best alternative method to achieve representative results (Kraemer et al. 2005: 77). Stratified random sampling (in which particular groups are over-sampled with a known probability and weighted in analyses) can also produce generalisable results.

However, when convenience (non-randomized) samples are used, findings can rarely be generalised with confidence. For example, a study that recruits a volunteer sample of families of prisoners through newspaper adverts would not be reliable for drawing conclusions about whether parental imprisonment was a risk factor, because there is non-random variation in newspaper readership and willingness to respond to newspaper adverts. Retrospective case-control studies, which separately sample children with the outcome (cases) and children without the outcome (controls) and compare them on previous exposure to parental imprisonment, are also unreliable for making inferences about association in the original population (those exposed and unexposed to the risk factor). This is because populations can change in composition from the time of risk exposure to the time of sampling, and because of other sampling artefacts (Kraemer et al. 2005: 85; Shadish et al. 2002).

## ***2. Adequate response rates***

1 Response and retention rates  $\geq 70\%$  AND Differential attrition  $\leq 10\%$ .

0 Response rate  $< 70\%$  OR Retention rate  $< 70\%$  OR Differential attrition  $> 10\%$ .

Poor response rates can bias results and reduce the generalisability of findings. Therefore, adequate response rates are needed to draw confident conclusions about risk factors. Response rates (relative to the target baseline sample) should be high for measures of both the risk factor and the outcome. In longitudinal studies, retention rates also need to be high, and attrition should not differ too much between groups. Like in experimental studies, differential attrition between children of prisoners and comparison children can cause bias in the estimated relationship between parental imprisonment and the outcome.

Because evidence is lacking on how different levels of non-response affect study bias, the cut-offs chosen here are quite arbitrary. Cut-offs were chosen by erring towards inclusion of studies (setting the criterion for response rates at 70% not 75%), in order to identify at least some studies as higher quality than others in this regard (Murray et al., 2009).

**3. Adequate sample size**

- 1 Sample size  $\geq$  400.
- o Sample size  $<$  400.

Larger samples produce more precise estimates of association, and allow more confident conclusions to be drawn about risk factors. Sometimes studies with small samples fail to detect an association just because they do not have enough statistical power. Although this is less of an issue in meta-analysis, in which results can be weighted in relation to sample size, random effects analyses sometimes gives almost equal weight to smaller and larger studies. Therefore, it is still important to assess whether studies used adequately sized samples. Clearly, more confident conclusions can be drawn based on a sample of 1,000 participants than a sample of 100 participants (all other things being equal). We think that an adequately sized sample ought to be able to detect small effect sizes ( $d = 0.2$ ). In a 2 by 2 table, about 400 participants are required to detect such a small effect size (in a 2 tailed test, with  $p = .05$  as the cut off for significance). Therefore, we define an adequate sample size as 400 or more participants. Note, it is the size of the achieved sample (used in analyses) that is important here, not the size of the target sample. The achieved sample can be considerably smaller than the sample targeted for inclusion in a study, because of poor response rates or high attrition.

**4. Good measure of parental imprisonment**

- 1 Children of prisoners were identified by sampling parents in a prison OR Official criminal records were used to determine whether parents were imprisoned OR Parents themselves were asked about their own history of imprisonment.
- o People other than the imprisoned parents reported about parental imprisonment and the measure was not validated.

Inadequate measurement can have a major impact on results. Studies need to use reliable and valid measures of parental imprisonment (and the child outcome) to draw confident conclusions about whether parental imprisonment is a risk factor.



Reliability refers to consistency in measurement; validity refers to how well a test measures what it is supposed to measure.

Because of the stigma of parental imprisonment, we are concerned about considerable under-reporting of parental imprisonment by other people (even other family members). On average, men self-report more antisocial behaviours than their partners do about them (Caspi et al., 2001), and the same seems true of men's imprisonment (Bendheim-Thoman Center for Research on Child Wellbeing, 2002). Thus, studies were only coded as having a good measure of parental imprisonment if parents themselves provided the information about their imprisonment, or if official or prison records were used to identify imprisoned parents.

### **5. Good measure of child outcome**

- 1 Reliability coefficient  $\geq .75$  AND Reasonable face validity, OR Criterion or convergent validity coefficient  $\geq .3$  OR More than one instrument or information source used to assess correlate OR Official records of arrest, conviction, or imprisonment of the child were used to measure the outcome.
  - o None of the above.

As defined above, reliability refers to consistency in measurement; validity refers to how well a test measures what it is supposed to measure. Reliability can be assessed by comparing scores for different items on a scale (internal consistency), test scores over time (test-retest reliability), and test scores produced by different observers (inter-rater reliability). As a rule of thumb, reliability coefficients should be at least .75 (Fleiss, 1981).

Validity can be assessed by evaluating whether test-scores correlate with other measures of the same construct (criterion validity), and whether test-scores correlate with other variables they are supposed to correlate with (convergent validity). As a rule of thumb, validity coefficients should be at least 0.3. Sometimes researchers report that a measure has good "face validity", also called content validity, which means that the items on the scale appear to measure what they are supposed to measure (e.g., questions about stealing behaviour have reasonable face validity for measuring crime). This is a much weaker test of validity than tests of criterion or convergent validity. Hence, face validity should at least be combined with high reliability.

To increase the quality of measurement, multiple measurement methods can also be used. Using multiple instruments or multiple informants allows researchers to distinguish between information relevant to the theoretical construct and bias

attributable to the method of measurement. Confidence in results is generally increased when multiple instruments or informants are used and scores are combined. Hence, more confident conclusions about risk factors can be drawn by using reliable and valid measures and multiple methods of measurement.

### ***6. Temporal precedence of parental imprisonment before the outcome***

1 Parental imprisonment was measured before the child outcome in a prospective longitudinal design, OR Parental imprisonment was measured retrospectively using official records that were compiled before the child outcome was measured OR The child outcome was measured while the parent was held in prison AND The child outcome did not refer back to a period before parental imprisonment.

o The study used retrospective self-reports of parental imprisonment and the child outcome OR The outcome measure referred to a period before parental imprisonment first occurred. For example, the child outcome was measured three months after parental imprisonment first occurred, but the measure referred to child behaviours over the previous six months, i.e. referred to behaviours that might have occurred three months before parental imprisonment.

### **Criteria for assessing if parental imprisonment is a causal risk factor**

Causal risk factors are risk factors that cause an increase in risk for the outcome. Two quality criteria were used to assess whether studies adequately tested for causation. The best type of study for drawing conclusions about causation is a randomised experiment. However, all studies in this review were non-randomised, observational studies, which generally have lower internal validity than randomised experiments. Two design issues are critical for investigating causal effects in non-randomized, observational studies: analysis of within-individual change in outcome, and control for confounding extraneous variables (Murray et al., 2009).

## **7. Analysis of within-individual change**

- 1 The study investigated within-individual change in child outcome from before to after parental imprisonment, for example using change scores, regression analyses controlling for pre-prison child outcome scores, or matching on pre-prison child outcome scores.
- o The study did not investigate within-individual change in child outcome from before to after parental imprisonment.

Studies need to investigate within-individual change in child outcome to identify whether child outcomes change from before to after parental imprisonment. Although the usual approach in social science is to examine between-individual differences in outcomes (Farrington, 1988; Labouvie, 1986, p. 145; Rutter, 1981, p. 525), causal conclusions are far more compelling when based on analyses of within-individual change, because the concept of cause involves the concept of change within individual units (Farrington, 1988, p. 158). By investigating changes in outcomes from before to after parental imprisonment, essentially individuals act as their own controls, which holds constant many individual factors that might otherwise bias study results (Farrington, 1988; McCartney, Bub, & Burchinal, 2006; Winship & Morgan, 1999). Thus, studies were coded on whether or not they assessed within-individual change in child outcome from before to after parental imprisonment.

## **8. Control for confounding variables**

- 1 Adequately controlled: The study controls for at least three important covariates that occurred before parental imprisonment.
- o Inadequately controlled: The study does not control for at least three important covariates OR Some of the variables that were controlled for were measured after parental imprisonment.

A fundamental issue when investigating causal risk factors is controlling for confounding variables (Farrington, 1988; Rutter, 1981, 1988, 2003). Confounding occurs because events like parental imprisonment are not randomly distributed in the population. Parental imprisonment is associated with multiple other risk factors that might contribute to undesirable child outcomes. For example, parental imprisonment is associated with poor educational attainment, low IQ, parental

antisocial/criminal behaviour, poor parental supervision, poor marital relations, large family size, low family socioeconomic status, and low family income (Murray & Farrington, 2005), which all predict child delinquency (Farrington, 2003). Therefore, even if delinquency increases after exposure to parental imprisonment, confounding variables might explain this relationship.

Possible confounds such as age, race, sex, and social class are often controlled for in risk research, but other covariates should also be controlled for to estimate causal effects. This can be done using a variety of research designs and statistical adjustment methods (Academy of Medical Sciences, 2007; McCartney et al., 2006; Murray et al., 2009; Winship & Morgan, 1999). We use a list of critical covariates to evaluate whether studies were “adequately controlled” or “inadequately controlled” (see Figure 5). The list specifies correlates of parental imprisonment found in previous research (for a review, see Murray & Farrington, 2008a) and well-known predictors of child antisocial behaviour and mental health problems.

We coded studies as “adequately controlled” if they controlled for at least three critical covariates in the list *and* if all the covariates occurred before parental imprisonment. Otherwise studies were coded as “inadequately controlled”. Studies were coded “inadequately controlled” if covariates were measured after parental imprisonment because such covariates might represent mediating mechanisms (i.e. links in the causal chain between parental imprisonment and child outcomes), not confounds. Controlling for mediating mechanisms can bias estimates of the overall effects of parental imprisonment on children. For example, if parental imprisonment affects children through a reduction in family income, family income is a mediating mechanism. If family income is measured after parental imprisonment, and controlled for in analyses, this could result in underestimating the overall effects of parental imprisonment on children.

Ideally, studies should control for many more than three critical covariates. However, from existing reviews, our impression was that studies on this topic have generally included very few controls. Therefore, we chose a low number of controlled covariates (three) as a cut-off for identifying “adequately controlled” studies, in the hope that some studies might be distinguished as better controlled than others.

*Figure 5. List of critical covariates (that should be measured before parental imprisonment)*

- Child covariates

Impulsivity, attention deficits, IQ, school attainment

- Parent covariates

Parental antisocial/criminal behaviour, parental age, parental education, parental mental health, parental substance abuse

- Parenting covariates

Low parental supervision, harsh parental discipline, abuse of child, neglect of child, parent-child conflict, inter-parental conflict

- Family covariates

Family size, socio-economic status, family income

- Wider environmental covariates

Peer delinquency, neighbourhood deprivation, neighbourhood crime, school crime

It is particularly important to control for parental criminality when estimating effects of parental imprisonment on children, and we give particular attention to whether or not studies did this. Parental criminality can be controlled, for example, by comparing children of prisoners with children whose parents are serving a different type of criminal justice sentence (such as a community service order), or by statistically controlling for a measure of parental criminality (e.g., the number times the parent has been arrested). However, even with these kinds of controls, studies might overestimate the effects of parental imprisonment on children, because imprisoned parents are likely to have more serious criminal histories than parents receiving other types of criminal sanctions.

We are very clear that fully convincing and defensible causal conclusions cannot be drawn from the kind of observational studies included in this review, even if they do control for important covariates and investigate within-individual change in child outcome from before to after parental imprisonment. Unmeasured confounding variables might account for any difference observed between children of prisoners and comparison children. However, observational studies that control for important confounding variables and analyse change in outcome from before to after parental imprisonment provide some evidence for considering possible causal effects. While

conclusions about causal effects must be very tentative based on such observational evidence, it is important to extract and summarize the best evidence available.

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## **2.7 EFFECT SIZES USED IN THE REVIEW**

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Effect sizes were calculated by Joseph Murray in Microsoft Excel (2007) and then copied into Comprehensive Meta-Analysis (Version 2.2.046) for analysis. Data input and calculations were double-checked.

### **The odds ratio**

The odds ratio is used as the common effect size to measure the association between parental imprisonment and child outcomes. The odds ratio was chosen for the following reasons. First, many primary studies reported results using odds ratios. Second, many measures of parental imprisonment and the child outcome were dichotomous (e.g., imprisoned or not, convicted or not). Third, the odds ratio is easily used as an effect size in meta-analysis. Fourth, the odds ratio is easily interpretable.

The odds ratio indicates the increase (or decrease) in odds for an outcome associated with parental imprisonment. The odds of an outcome are equal to the number of children with the outcome divided by the number of children without the outcome. For example, in a sample of 60 children of prisoners, if 20 children are arrested and 40 children are not arrested, the odds of arrest for children of prisoners are  $20/40 = 0.5$ . In a comparison group of 60 children, if 10 children are arrested and 50 children are not arrested, the odds for comparison children are  $10/50 = 0.2$ . The odds ratio is the number of times greater (or smaller) the odds of the outcome is for children of prisoners versus comparison children. Thus, in this example the odds ratio is  $0.5/0.2 = 2.5$ , and children of prisoners have 2.5 times greater odds of arrest than comparison children.

In this report, we always express results so that an odds ratio above one indicates a greater probability of the outcome for children of prisoners, and an odds ratio below one indicates a reduced probability of the outcome for children of prisoners. An odds ratio of one indicates zero association between parental imprisonment and the child outcome. Because we report undesirable child outcomes (antisocial behaviour and mental health problems), we refer to odds ratios above one as showing “harmful” effects and odds ratios below one as showing “beneficial” effects of parental imprisonment on children.

The confidence interval for an odds ratio is calculated from the number of children of prisoners with the outcome (A), the number of children of prisoners without the outcome (B), the number of comparison children with the outcome (C), and the number of comparison children without the outcome (D).

The confidence interval for the odds ratio is calculated from the following:

$$\text{Odds ratio} = \text{OR} = (A/B)/(C/D)$$

$$\text{Natural logarithm of OR} = \text{LOR} = \text{LN}(\text{OR})$$

$$\text{Variance of LOR} = \text{VLOR} = (1/A) + (1/B) + (1/C) + (1/D)$$

$$\text{Standard error of LOR} = \text{SELOR} = \text{Square root (SQRT) of VLOR}$$

$$\text{Confidence interval of LOR} = \text{LOR} \pm 1.96 * \text{SELOR}$$

$$\text{Confidence interval of OR} = \text{Exponent of the confidence interval of LOR.}$$

### **The standardised mean difference**

Another effect size we extracted from some studies is the standardised mean difference (d), which we convert into the odds ratio for this review. Where studies report means and standard deviations for children of prisoners and comparison children, d was calculated in the following way (Lipsey & Wilson, 2001, pp. 173, 198):

$$d = (\text{MP} - \text{MC}) / \text{SDP}, \text{ where}$$

MP = mean of the outcome score for children of prisoners

MC = mean of the outcome score for comparison children

SDP = the pooled standard deviation = SQRT of the pooled variance (VP)

The pooled variance (VP) is calculated as follows:

$$\text{VP} = [(N_X - 1) * V_X + (N_Y - 1) * V_Y] / (N_X + N_Y - 2), \text{ where}$$

N<sub>X</sub> = number of children of prisoners

V<sub>X</sub> = variance of children of prisoners' scores = squared standard deviation of their scores

N<sub>Y</sub> = number of children in comparison group

V<sub>Y</sub> = variance of comparison group's scores = squared standard deviation of their scores

From d, an odds ratio is estimated as follows (Lipsey & Wilson, 2001, p. 198):

$$\text{OR} = \text{EXP} [\pi * d / \text{SQRT}(3)]$$

or, more simply,

$$\text{LOR} = 1.8138 * d.$$

An odds ratio based on d is interpretable like any other odds ratio: the increase (or decrease) in odds associated with parental imprisonment. However, it is necessary to interpret the underlying continuous variable, which is used to calculate d, as dichotomous. For example, Stroble (1997) compared mean depression scores between children of prisoners and children without imprisoned parents. From means and standard deviations, we calculated that d = 0.3, and converted this into an OR = 1.8. This shows that parental imprisonment was associated with 1.8 times the odds of high depression scores compared with no parental imprisonment.

The confidence interval for d is calculated from the following (Lipsey & Wilson, 2001, p. 72):

$$\text{Confidence interval of } d = \text{CI}_d = d \pm 1.96 * \text{SE}_d$$

SEd = Standard error of d =  $\text{SQRT } Vd$ , where  
 $Vd = \text{Variance of } d = (NX + NY)/(NX * NY) + d^2/(2 * (NX + NY))$ , where  
 NX = number of children of prisoners  
 NY = number of children in comparison group.  
 The confidence interval for an OR based on d is calculated from the following:  
 Lower confidence limit (OR) =  $\text{EXP} [\pi * LLd / \text{SQRT}(3)]$   
 Upper confidence limit (OR) =  $\text{EXP} [\pi * ULd / \text{SQRT}(3)]$ , where  
 LLd = lower confidence limit for d  
 ULd = upper confidence limit for d.

### **Covariate-adjusted effect sizes**

To estimate causal effects of parental imprisonment on children, studies controlled for confounding variables in several different ways. Some matched children of prisoners and comparison children on confounding variables (e.g., parental conviction). Some compared children whose parents were imprisoned during childhood with children whose parents were imprisoned only before birth. This comparison controls for any confounding variables that are similar between the two groups. Other studies statistically controlled for confounding variables, for example, in logistic regression analyses. Results from these studies are expressed as “covariate-adjusted” odds ratios in this review.

Covariate-adjusted odds ratios indicate the number of times greater (or smaller) the odds of the outcome are for children of prisoners versus comparison children, while taking into account confounding variables. For example, by comparing children of prisoners and children of parents receiving another criminal justice sentence, the “covariate-adjusted” odds shows the difference in odds of an outcome associated with parental imprisonment, while taking into account parental conviction (and any other characteristics that are similar between the groups).

Covariate-adjusted odds ratios can be calculated directly from 2 X 2 tables of matched treatment and comparison groups (as in the calculations above for any other odds ratio), extracted directly from logistic regression results, or converted from a d-type effect size (as above), where covariates were controlled in calculating d.

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## **2.8 CRITERIA FOR DETERMINATION OF INDEPENDENCE OF FINDINGS**

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One issue that must be dealt with in research synthesis is the assumption of statistical independence of results. Studies sometimes report multiple measures for the same outcome or multiple comparisons for single samples, and different authors can report multiple findings for the same study. Using more than one result from the same sample in a meta-analysis can lead to underestimating error variance and



inflating significance tests. To determine independent findings for each meta-analysis, first we identified independent samples by doing the following:

1. Separate meta-analyses were conducted for antisocial behaviour and for mental health, and separate analyses were conducted for bivariate effect sizes and covariate-adjusted effect sizes. Thus, only if multiple results from a study were reported in any one of these four categories would we need to determine independence of findings further.
2. Independent samples of boys and girls were coded separately and used as the unit of analysis. (This was done even if combined results, for boys and girls together, were also reported.) Thus, only if a study reported multiple results either for boys or for girls for any particular outcome, would we need to determine independence of findings further. Although there might be some dependence between effect sizes derived from boys and girls in the same study, we assumed that they were independent in this review.
3. Within a study, when more than one sample of children of prisoners was compared with a single comparison group, the results were averaged, and the average effect size was used in analysis.<sup>2</sup> For example, if a study included one group of children who experienced parental imprisonment early in childhood and one group of children who experienced parental imprisonment in late childhood, and compared each of them with a single comparison group, we used the mean odds ratio (and mean variance) from these two comparisons.
4. In some studies, one group of children of prisoners was compared with multiple comparison groups. In these cases, we selected or combined comparison groups into a single comparison group for analysis. For analyses of bivariate effect sizes, we selected (or combined groups to create) a comparison group of children whose parents were not imprisoned and were *most similar to the general population of children*. For covariate-adjusted effect sizes, comparison groups were selected or combined to produce a single comparison group *most similar to the children of prisoners* with respect to variables before parental imprisonment.

Sometimes, multiple measures of the same outcome were reported for a single sample. When this occurred, we selected a single effect size for outcomes in childhood (0-17) and a single effect size for outcomes in adulthood (18+), so that these could be compared in analyses of moderator effects. We then took the mean of these two effect sizes to use in all other analyses. For childhood and adulthood outcomes separately, we did the following, in order, until we identified a single effect size.

5. If an outcome was measured at multiple time points (during childhood or during adulthood), the measure longest after parental imprisonment was

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<sup>2</sup> It was not possible to pool the groups of children of prisoners before calculating an effect size in these studies.

selected for analysis, unless attrition since the previous measure was over 10%. For example, a measure of conviction at ages 30-40 would be selected instead of a measure of conviction at ages 20-30, so long as the later measure did not have over 10% attrition since the previous measure.

6. If there were multiple covariate-adjusted effect sizes, the effect size reflecting maximum control of pre-prison covariates was selected for analysis. For example, if one effect size estimated the effects of parental imprisonment while controlling for family income and another effect size controlled for family income and parental criminality, the latter effect size was selected.
7. Measures of outcome with higher reliability or validity were selected in preference to measures with lower reliability or validity.
8. Measures of child outcome based on children's own reports were chosen in preference to effect sizes based on other people's reports (e.g., carers or teachers' reports).
9. For mental health outcomes, measures of general internalising problems were selected in preference to measures of depression or anxiety specifically. If results for general internalising problems were not reported and results for more than one specific internalising problem (e.g., both depression and anxiety) were reported, these results were combined into one effect size.
10. For antisocial behaviour, a measure of criminal behaviour was selected in preference to a measure of antisocial behaviour that does not necessarily break the law. A measure of antisocial behaviour that is closer to official delinquency (e.g., the "delinquency" sub-scale on the Child Behavior Checklist) was selected instead of general antisocial behaviour. Measures of more general crime (e.g., conviction for any offence) were selected in preference to measures of specific types of crime (e.g., conviction for violence). Measures of self-reported criminal behaviour, or conviction, or imprisonment were selected in preference to measures of arrest.
11. If there were still multiple measures of child mental health or antisocial behaviour, results were combined to produce one effect size.

Using these procedures for handling multiple comparisons and multiple measures of outcomes, each sample counted only once in each meta-analysis in this review.

### 3 Description of Sixteen Eligible Studies

In this section, we describe the key characteristics and results of the sixteen studies eligible for the review. References for the studies are shown in Table 1, below. Of these 16 studies, nine investigated the effects of parental imprisonment that clearly occurred during childhood (0-18 years), and seven studies investigated parental imprisonment that might have occurred before or after children were born. We describe these two sets of studies separately because we hypothesise that parental imprisonment during childhood has stronger effects on children than parental imprisonment before birth. A detailed description of all 16 studies, and how effect sizes were derived from them, is given in Appendix C.

*Table 1. Study references used in the review*

Reference used in the review	Study Name	Documents results retrieved from	Timing parental imprisonment
Huebner	National Longitudinal Survey of Youth	Huebner & Gustafson (2007)	Before birth/ childhood
Johanson	-	Johanson (1974)	Before birth/ childhood
Johnson	Panel Study of Income Dynamics	Johnson (2009)	Childhood
Kandel	Danish Cohort Study	Kandel et al. (1988)	Before birth/ childhood
Kinner	Mater University Study of Pregnancy	Kinner, Alati, Najman, & Williams (2007) (see also Bor, McGee, & Fagan, 2004)	Before birth/ childhood
Murray CSDD	Cambridge Study in Delinquent Development	New calculations (see also Murray, 2006; Murray & Farrington, 2005, 2008a, 2008b; Osborn & West, 1979)	Childhood
Murray PM	Project Metropolitan	New calculations (see also Murray et al., 2007)	Childhood
Pakiz	Simmons Longitudinal Study	Pakiz et al. (1997)	Before birth/ childhood

Reference used in the review	Study Name	Documents results retrieved from	Timing parental imprisonment
Peniston	Children at Risk	Peniston (2006)	Childhood
Rakt	Criminal Careers and Life Course Study	New calculations (see also Rakt, Murray, & Nieuwbeerta, in progress)	Childhood
Roettger	National Longitudinal Study of Adolescent Health	Roettger (2008) (see also Roettger & Swisher, in progress)	Before birth/ childhood
Stanton	-	Stanton (1980)	Childhood
Stroble	-	Stroble (1997)	Before birth/ childhood
Wakefield	Project on Human Development in Chicago Neighborhoods	Wakefield (2007) (see also Wakefield, in progress)	Childhood
Wilbur	-	Wilbur et al. (2007)	Childhood
Wildeman/ Geller	Fragile Families and Child Well-being Study	Wildeman (2008) Geller, Garfinkel, Cooper, & Mincy (in progress) (see also Garfinkel, Geller, & Cooper, in progress)	Childhood

Note. Studies are identified by the first author's last name and, in the case of multiple studies by the same author, an abbreviation of the study name. Some studies did not have a name.

### 3.1 NINE STUDIES OF PARENTAL IMPRISONMENT DURING CHILDHOOD

Nine studies were eligible for this review and measured parental imprisonment that clearly occurred during childhood. Only one (Stanton) of these nine studies was originally designed to study the effects of parental imprisonment on children. All others represent re-analyses of longitudinal data that were originally collected for other purposes. Key characteristics of the studies are summarised in Table 2 (see also Appendix C). Effect sizes are shown separately for the bivariate association and the covariate-adjusted association between parental imprisonment and child outcomes.

All of the studies that assessed bivariate associations between parental imprisonment and child outcomes showed that children of prisoners are at higher risk for antisocial-criminal behaviour and mental health problems compared with their peers. No study randomly assigned parents to prison or an alternative (e.g., community) sentence to test the causal effects of parental imprisonment on children. Studies used several different methods of controlling for confounding variables to estimate causal effects. One study compared children of prisoners and

children of parents on probation to try to disentangle effects of parental imprisonment from confounding variables. Other studies compared children whose parents were imprisoned during childhood and children whose parents were imprisoned only before birth. The logic of this comparison is that, if children whose parents were imprisoned only before birth have similar backgrounds to children whose parents are imprisoned during childhood, any differences between the two groups should reflect effects of the *experience* of parental imprisonment on children.

Many studies statistically controlled for covariates to try to isolate the effects of parental imprisonment on children. A variety of different covariates was measured in the studies. Unfortunately, many of the studies measured covariates after parental imprisonment, which could result in underestimating the causal effects of parental imprisonment, if the covariates acted as mediating mechanisms.

All studies found that parental imprisonment predicted undesirable child outcomes (i.e. had odds ratios greater than 1.0), even after controlling for confounding variables. However, few results were statistically significant. All except four studies (MurrayCSDD, MurrayPM, Rakt, Stanton) did not control for the criminality of children's parents. Therefore, many studies in this review might have systematically overestimated the effects of parental imprisonment on children.

We bring particular attention to two studies (Wakefield, Wildeman) that controlled for measures of child behaviour before parental imprisonment, and thus used "analysis of within-individual change" to examine whether child problem behaviours increased from before to after parental imprisonment. The study by Wakefield showed strong and significant increases in child antisocial and mental health problems following parental imprisonment. The study by Wildeman showed only moderate effects of parental imprisonment for boys and virtually no effect for girls. These different results might be explained by the different covariates that were controlled for in each study, or the different ages of the children (9-18 years at the time of outcome measurement in Wakefield's study and five years in Wildeman's study). In both studies, covariates were not clearly measured before parental imprisonment, which might have caused an underestimation of the effects of parental imprisonment on children. However, parental criminality was not controlled for, which might have caused an overestimation of prison effects.

There was considerable heterogeneity in the study populations, the nature of parental imprisonment that was investigated, and the child outcomes that were measured. The nine studies were conducted in four different countries (the United States, the United Kingdom, the Netherlands, and Sweden). Parental imprisonment was measured in early childhood in some studies and only during adolescence in other studies. Parental imprisonment normally referred to the father's imprisonment. Only one study specifically investigated maternal imprisonment (Stanton). In two studies (MurrayCSDD, Stanton) children had not been

permanently separated from their parent before parental imprisonment while, in other studies, children might not have been living with their parent before the imprisonment. These different situations might result in very different effects of parental imprisonment on children.

Table 2. Nine studies of parental imprisonment during childhood

Study	Study location	Sample size: Children of prisoners - CP Comparison children - CC	Child sex (Age at start of study)	Parents imprisoned (Age of child at parental imprisonment)	Comparison children	Child outcome (Age at outcome)	Bivariate OR	Covariate adjusted OR	Covariates controlled for
Johnson	National - USA	CP + CC = 3,540	Boys/girls (3-17)	Mother/father (0-5, 6-11, 11-16)	General population	Antisocial Behaviour (3-17)  Internalising (3-17)	-  -	3.1*  3.1	Parental imprisonment at other ages, neighbourhood quality, neighbour policing for drugs, family member alcohol problems, religiosity, parental education, mother married, child sex, age & race
Murray CSDD	London - UK	CP <sup>1/2</sup> = 23 CC <sup>1</sup> = 382 CC <sup>2</sup> = 17	Boys (8)	Mother/father (0-10)	CC <sup>1</sup> General population (in working-class neighbourhoods) CC <sup>2</sup> Parental imprisonment before birth only	Conviction (10-18) (19-50)  Neuroticism (16) Internalising (48)	5.3*  2.7*	1.4  1.8	Number parental convictions, boy's IQ, daring, family size
Murray PM	Stockholm - Sweden	CP <sup>1</sup> = 221 CC <sup>1</sup> = 14,834 CP <sup>2</sup> = 283 CC <sup>2</sup> = 245	Boys/girls (10)	Mother/father (0-6, 7-19)	CC <sup>1</sup> General population CC <sup>2</sup> Parental imprisonment before birth only	Conviction (19-30)	2.4* boys 2.8* girls	1.6 boys 1.4 girls	Number parental convictions, family social class
Peniston	Texas, Connecticut, Tennessee, Georgia, Washington - USA	CP = 27 CC = 622	Boys/girls (11-13)	Caregiver (following two years)	General population (in at risk neighbourhoods)	Incarceration (following two years)	2.7*	-	-

Study	Study location	Sample size: Children of prisoners - CP Comparison children - CC	Child sex (Age at start of study)	Parents imprisoned (Age of child at parental imprisonment)	Comparison children	Child outcome (Age at outcome)	Bivariate OR	Covariate adjusted OR	Covariates controlled for
Rakt	National - Netherlands	CP = 1,254 CC = 569	Boys/girls (18+)	Father (0-12, 12-18)	Father imprisoned before birth only	Conviction (18-30)	-	1.1 boys 1.6 girls	Number offences of father, criminal trajectory group father, father born abroad, alcohol/drug abuse by father, parental separation, family size, teen-pregnancy mother, child age and sex
Stanton	California - USA	CP = 22 CC = 18	Boys/girls (4-18)	Mother (4-18)	Mother on probation	Antisocial behaviour (4-18)	-	2.3	Criminal justice involvement of mothers (comparison group = children with mothers on probation)
		CP = 24 CC = 17				Low self-esteem (4-18)		5.1*	
Wakefield	Chicago - USA	CP = 69 CC = 2,313	Boys/girls (6-15)	Father (following 3 years)	General population	Antisocial behaviour (9-18)	2.0*	1.9*	Prior child behaviour, primary caregiver employment, household income, parental divorce, primary caregiver = mother, child age, sex, & race
						Internalising (9-18)	1.9*	2.4*	
Wilbur	Boston - USA	CP = 31 CC = 71	Boys/girls (0)	Father (6-11)	50% exposed to cocaine in utero (also true for CP)	Antisocial behaviour (6-11)	-	2.3	Exposure to cocaine in utero, age and & sex of child
						Internalising (6-11)	-	1.1	



Study	Study location	Sample size: Children of prisoners - CP Comparison children - CC	Child sex (Age at start of study)	Parents imprisoned (Age of child at parental imprisonment)	Comparison children	Child outcome (Age at outcome)	Bivariate OR	Covariate adjusted OR	Covariates controlled for
Wildeman	20 cities - USA	CP = 306 CC = 2,080	Boys/girls (0)	Father (30-60 months)	General population (with oversample of unmarried mothers)	Antisocial behaviour (60 months)	2.2* boys 1.7* girls	1.4* boys 0.9 girls	Prior child behaviour, child race, parental age, education, number of children, in utero nicotine exposure, birth weight, parental self-control, days with father, poverty, maternal mastery, domestic abuse, parental relationship quality, social father, prior relationships, corporal punishment, erratic punishment, low collective efficacy, neighbourhood social disorder

Notes. Age = in years. CP<sup>1</sup>/CC<sup>1</sup> = comparison used to calculate bivariate effect size. CP<sup>2</sup>/CC<sup>2</sup> = comparison used to calculate covariate-adjusted effect size. Results are shown separately for boys and girls where available. If studies have multiple measures of the outcome or parental imprisonment, average effect sizes are shown. \* Confidence interval for odds ratio does not include 1.

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### **3.2 SEVEN STUDIES OF PARENTAL IMPRISONMENT DURING CHILDHOOD AND BEFORE BIRTH**

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Seven studies examined the association between parental imprisonment and child antisocial behaviour or mental health but measured parental imprisonment in such a way that it might have occurred before children's births. These studies are summarised in Table 3 (see also Appendix C).

We hypothesised that parental imprisonment during childhood has stronger effects on children than parental imprisonment that occurs before children are born. Thus, we expected that the studies of parental imprisonment that occurred before or after children's births to have relatively weak effect sizes. In fact, the seven studies of this nature showed quite strong associations between parental imprisonment and children's antisocial behaviour. Effect sizes were smaller for mental health outcomes.

Only one study (Huebner) investigated maternal imprisonment specifically. In this study, although maternal imprisonment might have occurred before children's births, there were significant effects on offspring adult convictions, even after controlling for maternal and offspring characteristics. Apart from Huebner's study, no study controlled for parental criminality, for example by comparing children of prisoners with children of probationers. No study investigated change in child outcome from before to after parental imprisonment (which would not be possible in these studies, given that parental imprisonment could occur before children were born). Thus, these studies might systematically overestimate the causal effects on children, because important covariates were not controlled for and within-individual change in outcome was not analysed. Several studies measured covariates after parental imprisonment, which might result in an underestimation of prison effects, if the covariates acted as mediating mechanisms between parental imprisonment and the child outcomes. In some studies, parental imprisonment might have occurred after the child outcome, and results from these studies are very difficult to interpret.

It is noticeable that, unlike the studies of parental imprisonment during childhood, the majority of this set of studies only included boys. We explore possible differences in the effects of parental imprisonment on boys and girls in meta-analyses, in the next section.

Table 3. Seven studies of parental imprisonment during childhood and before birth

Study	Study location	Sample size: Children of prisoners - CP Comparison children - CC	Child sex (Age at start of study)	Parents imprisoned (Age of child at parental imprisonment)	Comparison children	Child outcome (Age at outcome)	Bivariate OR	Covariate adjusted OR	Covariates controlled for
Huebner	National - USA	CP = 31 CC = 1,666	Boys/girls (0)	Mother (Up to 18-24)	General population	Conviction (Up to 18-24)	3.1*	3.0*	Child age, sex, race, delinquency, education; maternal absence, delinquency, education, smoking during pregnancy, age; parental supervision, home environment, peer pressure
Johanson	National - Sweden	CP = 35 CC = 189 (CP = 4 for mother)	Boys (19-23)	Father/mother (Unknown)	General population (with oversample of youth prisoners)	Imprisonment (19-23)	6.2*	-	-
Kandel	Copenhagen - Denmark	CP = 92 CC = 513	Boys (0)	Father (Unknown)	General population (excluding those whose fathers had other criminal sanctions)	Imprisonment (up to 34-36)	8.5*	-	-
Kinner	Brisbane - Australia	CP = 137 CC = 2,262	Boys/girls (0)	Father (Up to 14)	General population	Antisocial behaviour (14)  Internalising (14)	1.7 boys 1.5 girls  1.2 boys 2.0* girls	boys 1.2 girls  1.1 boys 1.9 girls	Maternal age and education, family income, maternal anxiety/depression, maternal substance use, dyadic adjustment, domestic violence, parenting style
Pakiz	North East USA	CP + CC = 375	Boys (5)	Father/mother (Up to 18)	General population	Antisocial behaviour (21)	-	5.4*	Childhood behaviour problems, family disadvantage, school grades, physical abuse in family, marijuana use.

Study	Study location	Sample size: Children of prisoners - CP Comparison children - CC	Child sex (Age at start of study)	Parents imprisoned (Age of child at parental imprisonment)	Comparison children	Child outcome (Age at outcome)	Bivariate OR	Covariate adjusted OR	Covariates controlled for
Roettger	National - USA	CP = 784 CC = 5,344	Boys (12-18)	Father (Up to 18-24)	General population	Crime (Up to 18-24)	1.8*	1.6*	Race, drink/substance abuse, family structure, parental strictness, father involvement, physical abuse, social service care, school attachment, high school dropout, employment, marriage, cohabitation, poverty, race/education of census tract
Stroble	Richmond - USA	CP = 15 CC = 30	Boys/girls (14-18)	Father/mother (Up to 14-18)	50% in single parent families; 50% in families with both parents	Depression (14-18)	1.8	-	-

Notes. Age = in years. Results are shown separately for boys and girls where available. If studies have multiple measures of parental imprisonment, average effect sizes are shown. \* Confidence interval for odds ratio does not include 1.

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## 4 Findings from Meta-analyses

To synthesise the results from the studies eligible for this review, we conducted meta-analyses of their results. First, we synthesised bivariate effect sizes to assess whether parental imprisonment is a risk factor. Second, we synthesised covariate-adjusted effect sizes to assess whether parental imprisonment might be a causal risk factor. Third, we investigated possible moderating factors that account for variability in child outcomes after parental imprisonment. Fourth, we examined whether methodological characteristics of the studies were related to their findings. Fifth, we examined the possibility of publication bias in this review.

Some studies were not included in some of the analyses because they lacked relevant results. For example, some studies only provided results on antisocial behaviour and not on mental health problems and so were not included in analyses of mental health problems. Thus, different numbers of studies are included in different analyses.

When a single study included separate results for boys and girls, two effect sizes (based on these two independent samples) were included in analyses.

The meta-analyses were conducted using the inverse variance-weight approach recommended by Lipsey and Wilson (2001), and were performed in the computer package Comprehensive Meta Analysis (Version 2.2.046).

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### 4.1 ANALYSES OF BIVARIATE EFFECT SIZES

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#### **Parental imprisonment and antisocial behaviour**

Figure 6 shows the bivariate associations between parental imprisonment and child antisocial outcomes for 13 samples. (These results are taken from ten studies, three of which reported results separately for boys and girls). All 13 effect sizes showed that parental imprisonment was associated with higher rates of child antisocial outcomes (although only ten effect sizes were significant,  $p < .05$ ). We pooled the results from these 13 samples using both fixed and random effects models.<sup>3</sup> The

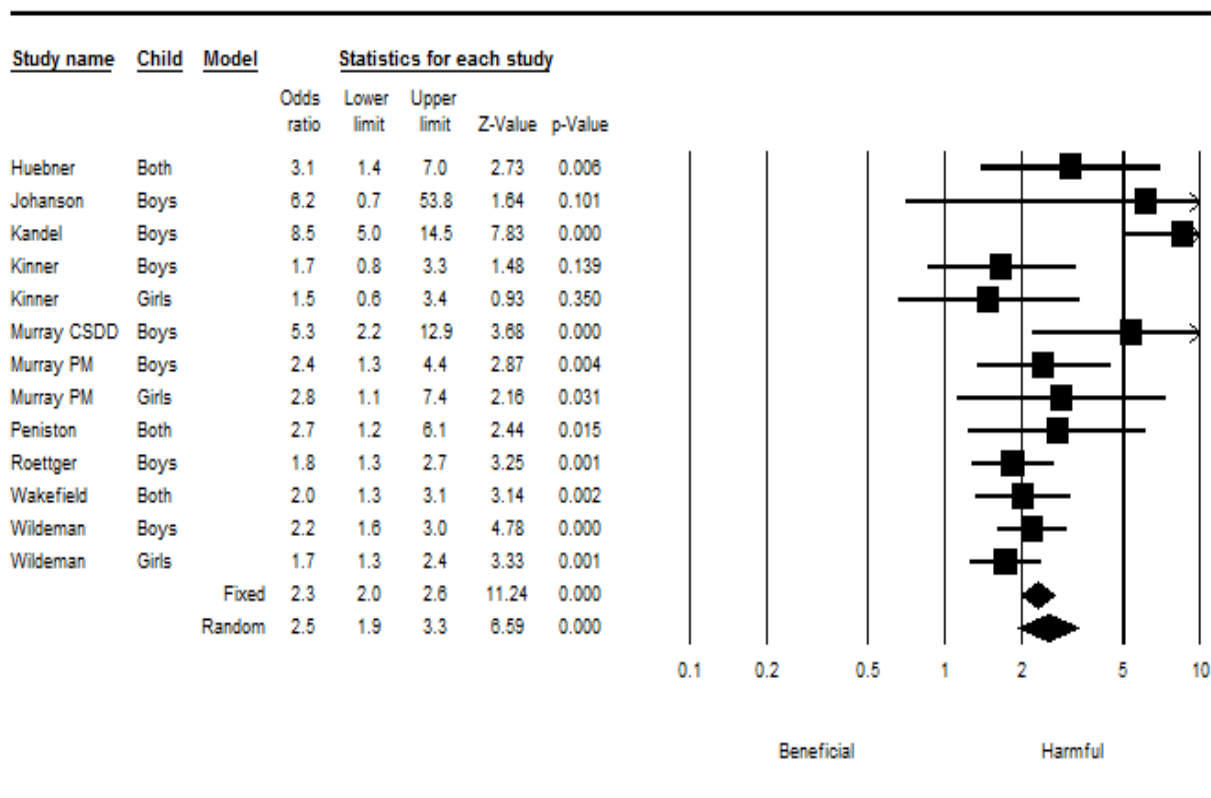
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<sup>3</sup> There are advantages and disadvantages of both fixed and random effects models. The main disadvantage of the fixed effects model is that it may not fit the data if there is significant heterogeneity in the study results. The main disadvantage of the random effects model is that it sometimes gives

pooled odds ratio was 2.3 (CI = 2.0-2.6) in fixed effects analysis and 2.5 (CI = 1.9-3.3) in random effects analysis. Thus, the average association between parental imprisonment and child antisocial outcomes was large and significant. To examine variability in these effect sizes we calculated the Q statistic, which is the weighted sum-of-squares of individual effect sizes around the mean. The Q statistic was significant ( $Q = 35.04$ ;  $df = 12$ ;  $p < 0.001$ ), indicating heterogeneity in these results that could not be accounted for by sampling error alone.

The largest effect size came from a study (Kandel) that probably overestimates the association between parental imprisonment and child delinquency (by excluding from the comparison group children whose fathers were arrested but not imprisoned). Excluding this result from the analyses produced the following pooled odds ratios: 2.1 (CI = 1.8-2.4) in fixed effects analysis and 2.1 (CI = 1.8-2.4) in random effects analysis.

Figure 6. Antisocial behaviour: Bivariate associations



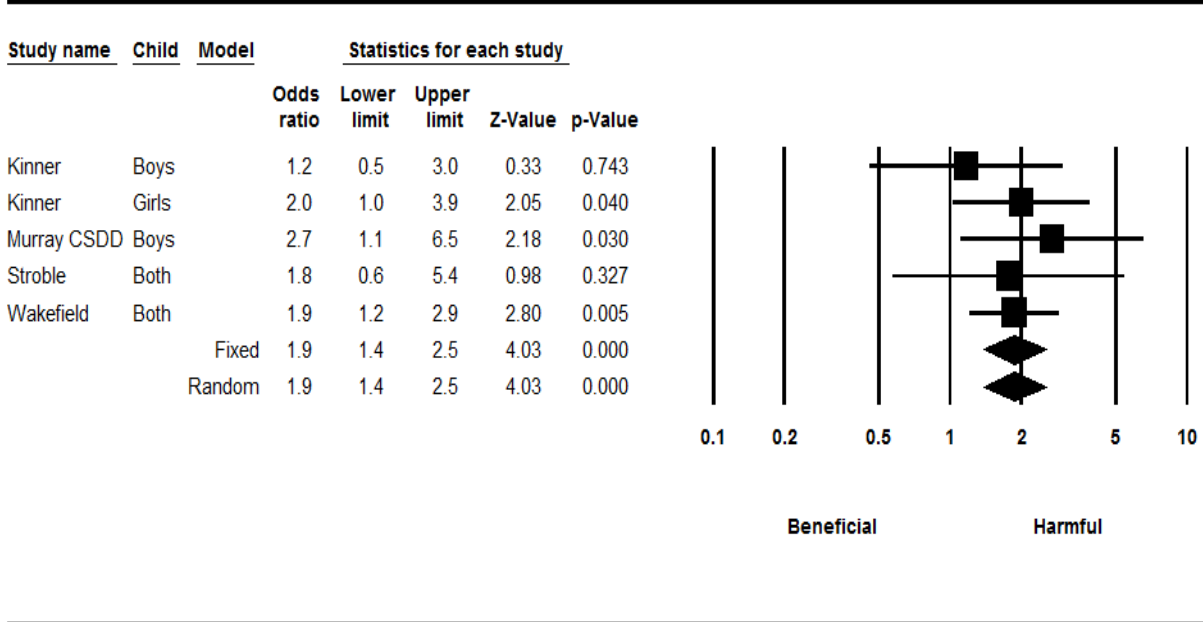
### Parental imprisonment and poor mental health

Figure 7 shows the bivariate associations between parental imprisonment and child mental health outcomes in five samples (taken from four studies). All five effect sizes showed an association between parental imprisonment and poor mental health (although only three effect sizes were significant,  $p < .05$ ). The average odds ratio

almost equal weight to each study (depending on the size of  $Q$ ), rather than giving greater weight to larger studies.

was quite large and significant in both fixed and random effects analyses (both: OR = 1.9; CI = 1.4-2.5). The Q statistic was not significant (Q = 1.65; df = 4; p = 0.800), although this may have been because of low power (insufficient number of studies).

Figure 7. Mental health: Bivariate associations



**4.2 ANALYSES OF COVARIATE-ADJUSTED EFFECT SIZES**

**Parental imprisonment and antisocial behaviour**

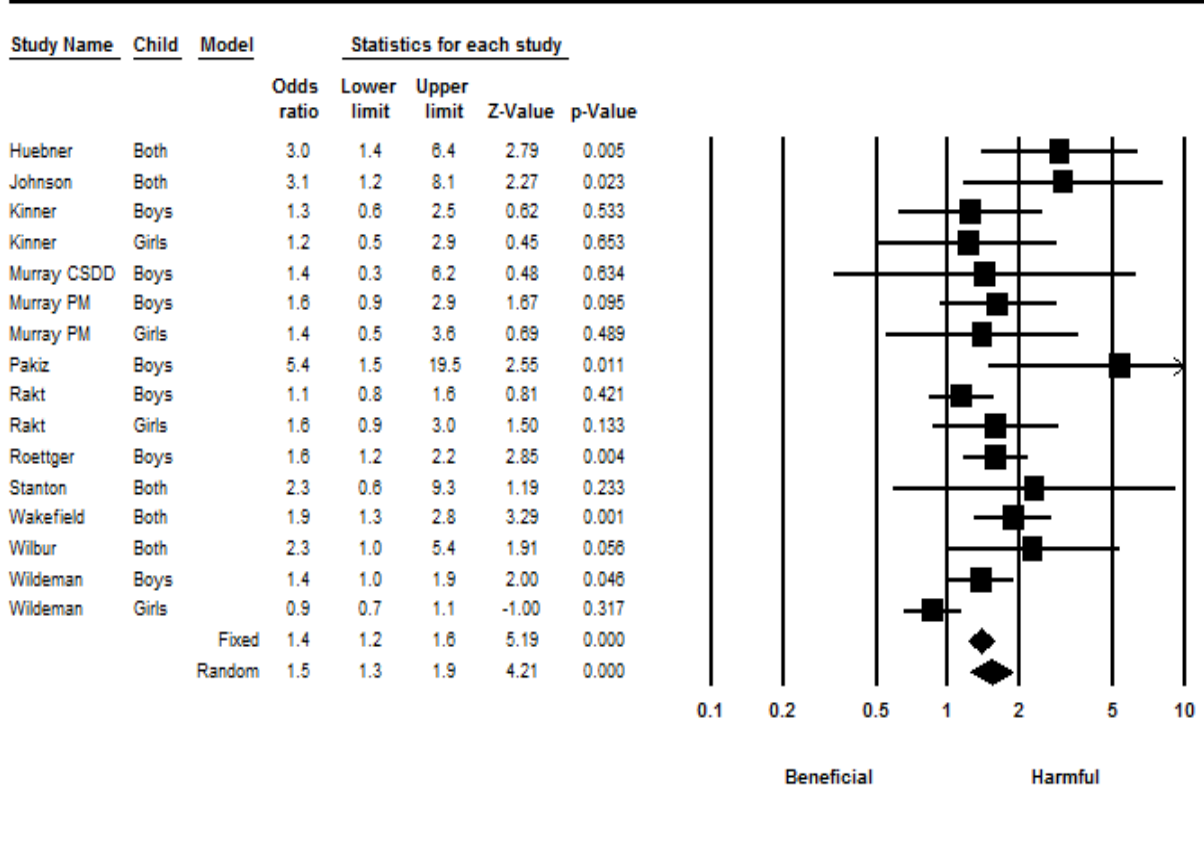
Figure 8 shows the covariate-adjusted effect sizes for the relationship between parental imprisonment and child antisocial outcomes for 16 samples (in 12 studies). Apart from one, all effect sizes were larger than 1.0, indicating possible harmful effects of parental imprisonment on child antisocial outcomes. However, only six individual results were significant (p < .05). There was significant heterogeneity in the effect sizes that could not be accounted for by sampling error alone (Q = 28.49; df = 15, p = 0.019). The average odds ratio across the 16 samples was 1.4 (CI = 1.2-1.6) in fixed effects analyses, and 1.5 (CI = 1.3-1.9) in random effects analyses, suggesting moderate and significant effects of parental imprisonment on child antisocial outcomes.<sup>4</sup>

Heterogeneity in the study results might be explained by the different covariates that were controlled for in each study. Only two studies, by Wakefield and Wildeman, controlled for prior child behaviour and analysed within-individual change in antisocial outcomes from before to after parental imprisonment. Other studies might have overestimated causal effects because of this omission. The study by

<sup>4</sup> Excluding the largest effect size (from Pakiz) from the analyses did not change the average odds ratio, which was 1.4 (CI = 1.2-1.6) in fixed effects analysis and 1.5 (CI = 1.2-1.8) in random effects analyses.

Wakefield, including both boys and girls, showed quite a large increase in child antisocial behaviour from before to after parental imprisonment. The study by Wildeman showed a slight increase for boys but not for girls. Only five studies (Huebner, Murray CSDD, Murray PM, Rakt, and Stanton) controlled for parental criminality. Other studies might have overestimated causal effects because of this omission.

Figure 8. Antisocial behaviour: Covariate-adjusted effect sizes



Only two studies (Stanton and Wilbur) measured covariates that occurred before parental imprisonment. All other studies, that measured covariates after parental imprisonment, might have underestimated causal effects of parental imprisonment on children, because the covariates might represent mediating mechanisms.

The studies included in this analysis have different strengths and weaknesses. Overall, they suggest a trend towards increased antisocial outcomes after parental imprisonment, even after controlling for a variety of covariates. However, these observational studies, often lacking control of critical covariates, may be systematically biased. Firm causal conclusions cannot be drawn from these studies.

**Parental imprisonment and poor mental health**

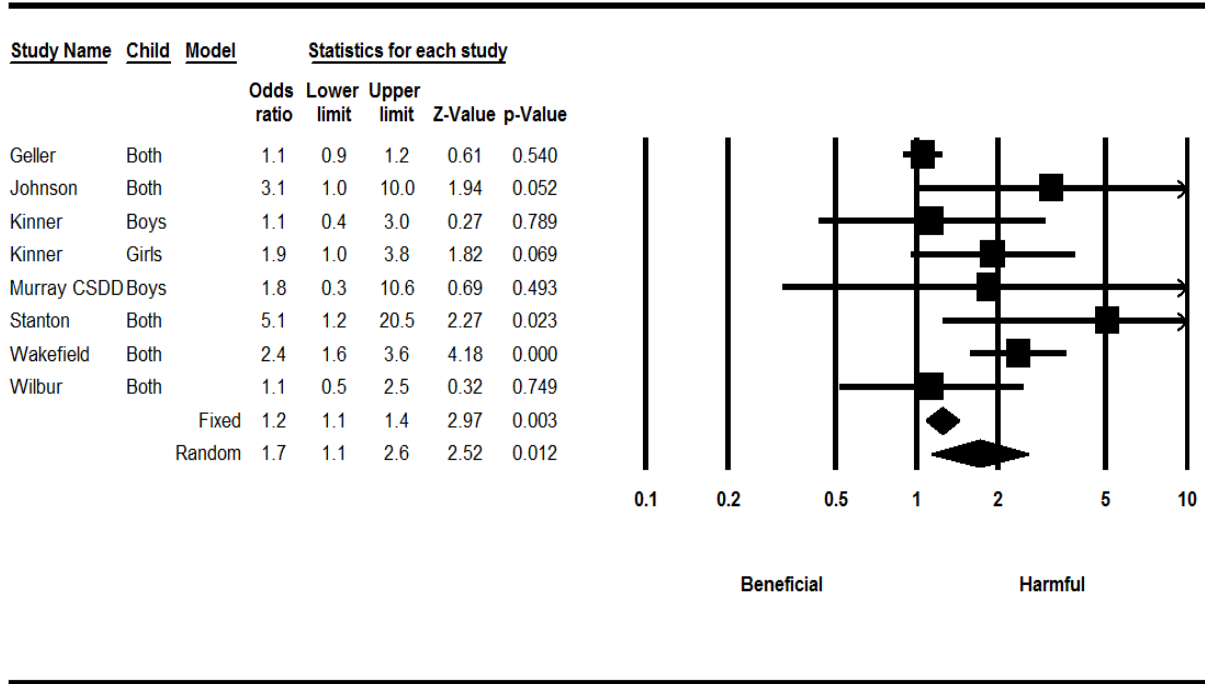
Figure 9 shows eight covariate-adjusted effect sizes (from seven studies) for the association between parental imprisonment and child mental health outcomes.



Although only three effect sizes are significant ( $p < 0.05$ ), all odds ratios are larger than 1.0, indicating possible harmful effects of parental imprisonment on child mental health outcomes. There was significant variation in these effect sizes ( $Q = 21.90$ ;  $df = 7$ ;  $p = 0.003$ ). The average odds ratios across the eight samples was 1.2 (CI = 1.1-1.4) in fixed effects analyses, and 1.7 (CI = 1.1-2.6) in random effects analyses, showing only a moderate overall association between parental imprisonment and child mental health outcomes, once covariates were controlled for.<sup>5</sup>

Only one study (Wakefield) analysed change in child mental health from before to after parental imprisonment. This study showed a significant increase in risk for mental health associated with parental imprisonment. However, parental criminality was not controlled for, which may have caused an overestimation of the effects of parental imprisonment on children. In all studies except Stanton’s, covariates were measured after parental imprisonment, which might have caused an underestimation of the causal effects of parental imprisonment on children.

Figure 9. Mental health: Covariate-adjusted effect sizes



### 4.3 VARIATION IN EFFECTS BY PARTICIPANT AND STUDY CHARACTERISTICS

Originally, we hoped to investigate whether associations between parental imprisonment and child outcomes varied by: child sex, which parent was imprisoned (mother or father), age at which parental imprisonment occurred, length

<sup>5</sup> If the particularly large effect from Stanton is excluded, pooled odds ratios remain very similar in fixed effects analysis (OR = 1.2, CI = 1.1-1.4) and in random effects analysis (OR = 1.6, CI 1.0-2.4)

of parental imprisonment, country of research (categorised by length of prison sentences and incarceration rates), and other sample characteristics that were frequently reported. To investigate moderators requires a sufficient number of studies with different values on each moderator variable. We did not conduct moderator analyses of the association between parental imprisonment and child mental health outcomes, because there were so few studies that provided relevant results. We conducted an exploratory investigation of moderators of the covariate-adjusted association between parental imprisonment and child antisocial outcomes in 16 samples (the samples used in these analyses are shown in Figure 8).

First, we tested whether the association between parental imprisonment and child antisocial outcomes varied by the following variables: child sex (boys vs. girls); parent sex (maternal vs. paternal imprisonment); timing of parental imprisonment (parental imprisonment 0-18 vs. also before birth); child age at parental imprisonment (childhood 0-10 vs. adolescence 11-17); and whether the study was conducted inside or outside of the United States. Next, we explored whether the association between parental imprisonment and child outcomes differed according to the outcome measure (antisocial behaviour vs. criminal behaviour) and the child's age at time of the outcome (juvenile 0-17 vs. adult 18+).

Results of these exploratory moderator analyses are shown in Table 4. No moderator variable was statistically significant. Slightly larger odds ratios were found for boys (compared with girls), maternal imprisonment (compared with paternal imprisonment), parental imprisonment occurring any time up to when children were 18 (compared with parental imprisonment occurring 0-18), parental imprisonment occurring during adolescence (compared with during childhood), antisocial behaviour outcomes (compared with crime outcomes), outcomes in juvenile years (compared with adult years) and parental imprisonment occurring in the United States (compared with outside the United States). Whether or not these differences (some of which were extremely small and based on few samples) arose by chance would need to be assessed in a future review including more primary studies.

*Table 4. Exploratory analyses of moderator variables*

Moderator	Category 1 (n)	OR (1)	LCL (1)	UCL (1)	Category 2 (n)	OR (2)	LCL (2)	UCL (2)	QB	p
Child sex	Boys (7)	1.4	1.2	1.7	Girls (4)	1.1	0.8	1.5	1.88	0.171
Parent sex	Maternal imprisonment (3)	1.7	0.7	4.1	Paternal imprisonment (7)	1.5	1.2	1.8	0.11	0.735
Timing parental imprisonment	Child 0-18 (9)	1.5	1.3	1.9	Child 0-18 or before birth (6)	1.6	1.1	2.4	0.02	0.883
Age at parental imprisonment	Childhood 0-10 (3)	1.2	0.8	1.9	Adolescence 11-17 (4)	1.8	0.9	3.9	0.73	0.391
Outcome measure	Antisocial (9)	1.6	1.1	2.2	Crime (7)	1.5	1.2	1.8	0.17	0.683
Age at outcome	Juvenile (9)	1.6	1.2	2.2	Adult (7)	1.5	1.2	1.8	0.18	0.675
In USA	Yes (9)	1.8	1.3	2.4	No (7)	1.3	1.0	1.6	2.52	0.113

Notes. Results from mixed-models. OR = Odds ratio; LCL = Lower Confidence Limit; UCL = Upper Confidence limit; QB = Q statistic for heterogeneity between categories; p = significance value for QB.

Table 5 shows the strength of association (phi correlation) between each of the moderator variables analysed above. Some moderators, for example “type of outcome” and “age at outcome”, were quite highly correlated. To take account of such confounding, and to investigate the effects of multiple moderators simultaneously, weighted regression analyses can be used (Lipsey & Wilson, 2001, pp. 138-140). However, the small number of samples, and missing values for some moderators, meant that there were too few effect sizes to conduct such analyses.

Ideally, other possible moderators would also have been investigated, such as whether children in the study were living with their parent before the imprisonment, what children were told about the event, children’s caregiving arrangements, length of parental imprisonment, prison practices regarding prisoner-family contact, local support services for prisoners’ families, and the social and penal culture in which parental imprisonment occurred. However, the small number of studies, and the lack of information on these variables, did not permit this. Future analyses, with a larger database of primary studies, should examine these moderators in weighted regression analyses.

Table 5. Correlations between moderators

	Child sex	Parent sex	Timing parental imprisonment	Age at parental imprisonment	Outcome measure	Age at outcome	In USA
Child sex	1						
Parent sex	*	1					
Timing parental imprisonment	-0.18	-0.13	1				
Age at parental imprisonment	-0.17	*	*	1			
Outcome measure	0.07	0.13	0.05	0.42	1		
Age at outcome	0.21	-0.29	0.05	0.42	0.75	1	
In USA	0.18	0.36	-0.05	-0.42	-0.49	-0.49	1

Notes. \* The correlation could not be calculated because there was no variation on one variable.

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#### 4.4 VARIATION IN EFFECTS BY STUDY METHODOLOGICAL QUALITY

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We explored whether methodological study features might explain variation in study results. First, we assessed the methodological quality of each study using eight criteria (described from page 21). These methodological assessments are shown below in Table 6. All studies were rated “inadequately controlled” because either they did not control for many ( $\geq 3$ ) important covariates, or the covariates did not clearly precede parental imprisonment. Thus, this variable could not be analysed as a possible moderator. Instead, we coded whether or not studies controlled for a measure of parental criminality (e.g., through matching or statistical control), and examined this as a possible moderator of study results.

*Table 6. Methodological quality assessment of eligible studies*

<b>Study</b>	<b>Good sampling</b>	<b>Good response rate (≥ 70%)</b>	<b>Adequate sample size (≥ 400)</b>	<b>Good measure parental imprisonment</b>	<b>Good measure outcome</b>	<b>Clear precedence of parental imprisonment before outcome</b>	<b>Analysis of change</b>	<b>Adequately controlled</b>	<b>Controlled for parental criminality</b>
Huebner	Y	Y	Y	Y	N	Y	N	N	Y
Johanson	N	Y	N	Y	Y	N	-	-	-
Johnson	Y	Y	Y	Y	Y	Y	N	N	N
Kandel	Y	N	Y	Y	Y	N	-	-	-
Kinner	Y	N	Y	N	Y	N	N	N	N
Murray CSDD	Y	Y	N	Y	Y	Y	N	N	Y
Murray PM	Y	Y	N	Y	Y	Y	N	N	Y
Pakiz	Y	N	N	N	N	Y	N	N	N
Peniston	Y	Y	Y	Y	N	N	-	-	-
Rakt	Y	Y	Y	Y	Y	Y	N	N	Y
Roettger	Y	Y	Y	N	Y	N	N	N	N
Stanton	N	N	N	Y	N	Y	N	N	Y
Stroble	Y	Y	N	N	Y	N	-	-	-
Wakefield	Y	N	Y	N	Y	N	Y	N	N
Wilbur	Y	N	N	N	Y	Y	N	N	N
Wildeman	Y	N	Y	N	N	N	Y	N	N

Notes. If a study had different quality evaluations for antisocial behaviour and mental health problems (e.g., Stanton, regarding quality of the outcome measure) the code for the antisocial outcome evaluation is reported in this table. Some studies reported bivariate effect sizes but not covariate-adjusted effect sizes and so were not evaluated on the last three items.

We explored whether methodological quality features explained variation in covariate-adjusted effect sizes for antisocial behaviour in 16 samples (the samples used in these analyses are shown in Figure 8). Table 7 shows results from these analyses. There was no significant moderator among the eight methodological quality variables. Larger effect sizes were found among studies with poorer sampling methods, smaller samples, better quality measures of parental imprisonment, poorer quality measures of outcome, lack of clear precedence of parental imprisonment before the outcome, no analysis of change, and without control for parental criminality. Whether or not these differences (many of which are very small) arose by chance would need to be assessed in a future review using a larger number of primary studies.

*Table 7. Variation in effects by methodological quality features*

Moderator	Category 1 (n)	OR (1)	LCL (1)	UCL (1)	Category 2 (n)	OR (2)	LCL (2)	UCL (2)	QB	p
Good sampling	Yes (15)	1.5	1.2	1.9	No (1)	2.3	0.6	9.3	0.35	0.552
Good response rate	Yes (8)	1.5	1.2	1.9	No (8)	1.5	1.1	2.1	0.03	0.874
Adequate sample size	Yes (10)	1.4	1.1	1.8	No (6)	1.9	1.3	2.8	1.60	0.206
Good measure parental imprisonment	Yes (8)	1.6	1.2	2.1	No (8)	1.5	1.1	2.0	0.15	0.700
Good measure outcome	Yes (11)	1.5	1.3	1.8	No (5)	1.7	1.0	2.9	0.19	0.662
Clear precedence	Yes (10)	1.3	1.0	1.8	No (6)	1.4	1.0	2.0	2.11	0.146
Analysed change	Yes (3)	1.3	0.8	2.0	No (13)	1.6	1.3	1.9	0.68	0.410
Controlled for parental criminality	Yes (7)	1.4	1.1	1.8	No (9)	1.5	1.2	2.1	0.12	0.733

Notes. Results from mixed-models. OR = Odds ratio; LCL = Lower Confidence Limit; UCL = Upper Confidence limit; QB = Q statistic for heterogeneity between categories; p = significance value for QB.

Table 8 shows the strength of association (phi correlation) between each of the methodological quality features analysed above. Some quality features, for example having a good response rate and good measure of parental imprisonment, were quite highly correlated. Unfortunately, weighted regression analyses of multiple quality features could not be conducted because of the small number of samples.

Table 8. Correlations between methodological quality features

	Good sampling	Good response rate	Adequate sample size	Good measure parental imprisonment	Good measure outcome	Clear precedence	Analysed change	Controlled for parental criminality
Good sampling	1.00							
Good response rate	0.26	1.00						
Adequate sample size	0.33	0.00	1.00					
Good measure parental imprisonment	-0.26	0.75	-0.26	1.00				
Good measure outcome	0.38	0.41	0.04	0.14	1.00			
Clear precedence	-0.20	0.52	-0.60	0.78	0.04	1.00		
Analysed change	0.12	-0.48	0.37	-0.48	-0.37	-0.62	1.00	
Controlled for parental criminality	-0.29	0.63	-0.36	0.88	0.05	0.68	-0.42	1.00

#### 4.5 ANALYSIS OF PUBLICATION BIAS

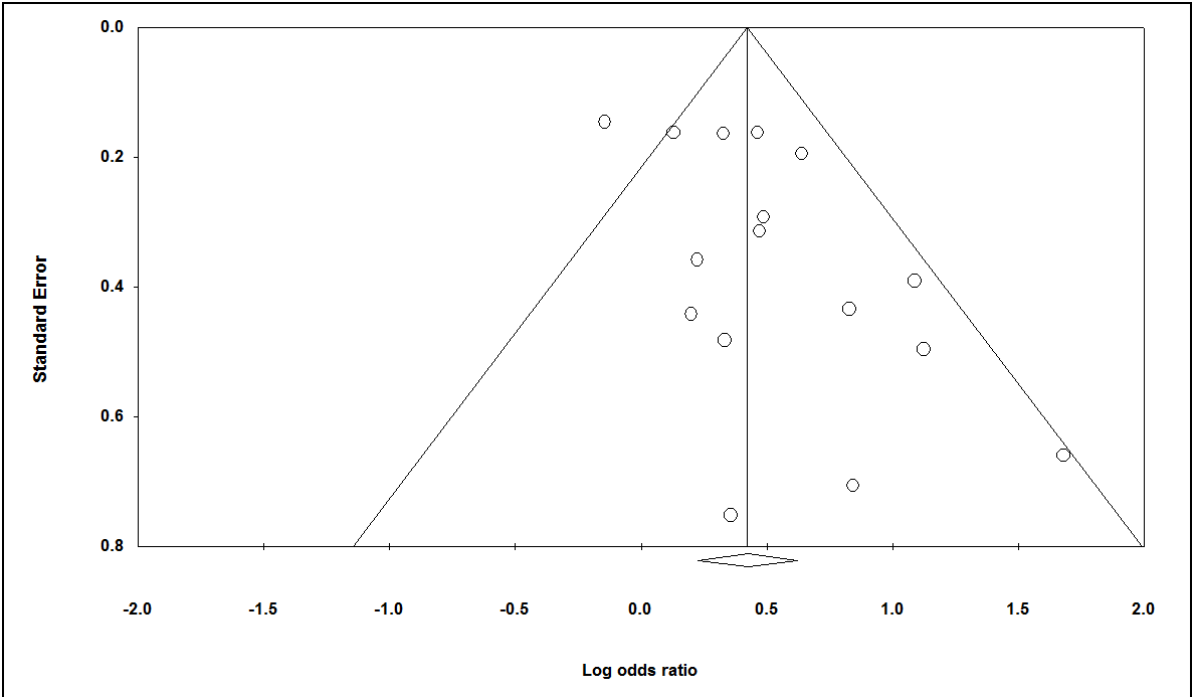
Unpublished studies might be underrepresented in a review if they are harder to locate or retrieve. If unpublished studies have different effect sizes compared with published studies, this can bias meta-analytic results. Missing unpublished studies might have smaller nonsignificant findings than published studies included in a review, because smaller and nonsignificant findings are harder to publish.

The possibility of publication bias can be investigated by comparing results from published and unpublished studies in a review. In the current review, six studies (Peniston, Rakt, Roettger, Stroble, Wakefield, Wildeman) were unpublished. We investigated publication bias using results for the covariate-adjusted association between parental imprisonment and child antisocial behaviour (shown in Figure 8). Consistent with the possibility of publication bias, unpublished studies had smaller effect sizes (pooled OR = 1.3) than published studies (pooled OR = 1.9), and this difference was almost significant (QB = 3.37, p = 0.066).

To investigate publication bias further, we examined a funnel plot of effect sizes versus their standard errors. In a funnel plot, larger studies are shown nearer the top, while smaller studies are shown nearer the bottom. The funnel plot in Figure 10 shows that, in this review, larger studies tended to have smaller effect sizes than

smaller studies. This asymmetry might reflect publication bias, if unpublished studies with small samples and small effect sizes are missing.

Figure 10. Funnel plot to examine publication bias



To consider what effects publication bias might have on meta-analytic results, missing studies were imputed using the Trim and Fill method. By including imputed missing studies in the analyses, the average odds ratio for the covariate-adjusted association between parental imprisonment and child antisocial behaviour reduces from 1.5 (CI = 1.3-1.9) to 1.2 (CI = 1.0, 1.5). Thus, publication bias might be causing a slight overestimation of the effects of parental imprisonment on child antisocial behaviour in this review.



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## 5 Discussion

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### 5.1 SUMMARY OF FINDINGS

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Sixteen studies of parental imprisonment were eligible for inclusion in this review. A meta-analysis of these studies showed that children of prisoners have about twice the risk for antisocial outcomes and poor mental health problems compared with their peers. We therefore conclude that parental imprisonment is quite a strong risk factor for these outcomes.

After doing this review, we cannot draw firm conclusions about whether or not parental imprisonment causes an increase in child antisocial behaviour or mental health problems. No randomised experiment has been conducted on this topic. Twelve observational studies used matched comparison groups or statistically controlled for covariates to try to isolate the effects of parental imprisonment on children. Despite the different covariates that were controlled for and the different populations that were studied, all but one study suggested that children of prisoners have higher rates of antisocial and mental health outcomes than their peers, even after controlling for covariates. Thus, the evidence points towards the possibility that parental imprisonment has harmful effects on children. However, these results might be systematically biased because of the poor quality of the studies.

It is important to test whether child outcomes change from before to after parental imprisonment when investigating causal effects. However, only two studies conducted such tests, and these studies showed quite different results. One (Wildeman, 2008) found only weak association between parental imprisonment and child outcomes in a sample of young children. The other study (Wakefield, 2007) found strong effects of parental imprisonment in a sample of adolescents.

Only five studies controlled for a measure of parental criminality to estimate the effects of parental imprisonment on children. Thus, many studies might have overestimated the effects of parental imprisonment on children because they did not control for prior child behaviour or parental criminality. A variety of other covariates was measured and statistically controlled for in the studies. However, nearly all studies measured covariates after parental imprisonment. Studies that measure and control for covariates after parental imprisonment might underestimate the overall effects of parental imprisonment on children.

We did not identify significant moderators of the effects of parental imprisonment on children, but there were very few studies with which to investigate this issue.

We conclude that parental imprisonment is quite a strong risk factor for both child antisocial behaviour and mental health problems, but that it is not known whether parental imprisonment is a causal risk factor, and more studies are needed to identify possible moderators.

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## **5.2 IMPLICATIONS FOR POLICY AND PRACTICE**

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Increasing numbers of children are experiencing parental imprisonment in many countries worldwide. These children are at greater risk for undesirable outcomes than their peers, and the experience of parental imprisonment itself might contribute to these outcomes. We discuss the policy and practice implications of these findings here. However, it is very important to bear in mind that it has not demonstrated that parental imprisonment causes an increase in child problem behaviour.

### **Parental imprisonment as a risk factor**

Because parental imprisonment predicts undesirable outcomes for children, it could be used to indicate that children might be in need of extra support. Even if parental imprisonment does not itself contribute to children's antisocial behaviour or mental health problems, the fact that it predicts these outcomes shows that it can be associated with other causes of child problem behaviour. For example, Murray and Farrington (2005) calculated the number of individual and family risk factors among boys in the Cambridge Study, according to the boy's history of parental imprisonment until age 10. Boys whose parents were imprisoned from birth to age 10 had, on average, significantly more (5.4) risk factors than boys who had no history of parental imprisonment or separation (2.3). The risk factors examined were high daring, low IQ, and low junior school attainment of the boy, poor parental supervision, poor parenting attitudes of mothers and fathers, poor parental relations, neuroticism of mothers and fathers, low family income, low family social class, and large family size. Using data from the Great Smoky Mountains Study, which is a longitudinal survey of over 1,400 children in North Carolina, Phillips, Erkanli, Keeler, Costello, and Angold (2006) found that parental imprisonment is associated with economic strain and instability in children's care and living arrangements.

Thus, parental imprisonment indicates deprivation of various kinds as well as an increased probability for antisocial behaviour and mental health problems. As Kemper and Rivara (1993) suggest, it might be appropriate for professionals, such as child health workers, to include questions about parental imprisonment as part of a comprehensive biosocial assessment of children. If a history of parental

imprisonment is apparent, children may be screened for antisocial behaviours or mental health problems, and offered appropriate treatment.

Other studies show that, among children in courts and clinics, children with a history of parental imprisonment tend to have more disadvantaged backgrounds and problem behaviours than other children in these settings. For example, Phillips et al. (2002) found that, among youth at a mental health clinic, those with a history of parental imprisonment were more likely than others to have been exposed to parental substance abuse, extreme poverty, and abuse or neglect. Dannerbeck (2005) found that delinquent youth with a history of parental imprisonment were more likely than other delinquent youth to have experienced severely ineffective parenting, child abuse or neglect, and to have parents who abuse drink or drugs or have a mental illness. Thus, when children do show antisocial behaviour or mental health problems, professionals should be aware of a possible history of parental imprisonment and its associated problems.

Although professionals need to be aware of the risks associated with parental imprisonment, it is important to note that parental imprisonment is far from deterministic in predicting undesirable outcomes. Many children of prisoners do not develop antisocial or mental health problems. More research should be conducted to identify why some children develop problematic behaviours following parental imprisonment while other others do not.

### **Parental imprisonment as a possible causal risk factor**

We cannot be sure that parental imprisonment is a causal risk factor for child antisocial behaviour or mental health problems. Existing evidence is inconclusive. Thus, we do not discuss in detail the policy implications of this possibility. For a more detailed discussion of policy and practice options that might be used to mitigate undesirable effects of parental imprisonment on children, see Murray and Farrington (2006; 2008a).

An obvious option for preventing harmful effects of parental imprisonment on children is to imprison fewer parents. This could be achieved by increasing the use of alternative forms of criminal punishment, such as probation, intensive supervision, house arrest, electronic monitoring, community service, and day fines. However, the obstacles to such criminal justice reforms are complex (Tonry, 1996, Chapter 4) and often political (Tonry, 2004). Therefore, it is also important to consider programmes that might reduce undesirable effects of parental imprisonment when it does occur.

Programmes for children of prisoners should be developed based on what is known about how parental imprisonment affects children. Depending on the mechanisms linking parental imprisonment and undesirable child outcomes, different interventions will be needed to protect children. For example, several interventions are suggested by the possibility that parental imprisonment harms children because

of traumatic separation and threats to children's attachment relations (Murray & Murray, in press). These include providing children with more stable care arrangements, giving children's caregivers advice about how to provide honest and clear explanations about parental imprisonment to children, offering counselling and therapeutic services to children and families of prisoners, and increasing children's opportunities to maintain good-quality contact with their imprisoned parent. Different kinds of intervention would be needed if other mechanisms were important, such as family economic strain, strained caregiving, or stigma and labelling (see Murray & Farrington, 2006, 2008a). The effectiveness of any programme designed to mitigate undesirable effects of parental imprisonment on children should be carefully evaluated in demonstration projects using randomized controlled trials and in systematic reviews. For example, see the ongoing experimental evaluation of a prison parenting programme in Oregon (Eddy et al., 2008).

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### **5.3 IMPLICATIONS FOR RESEARCH**

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Hagan and Dinovitzer (1999, p. 152) rightly argued that “the implication of not having better and more systematic research on the collateral effects of imprisonment is that we are making penal policy in a less than fully, indeed poorly, informed fashion”, and laid out a useful framework for future research. We describe key research needs on the effects of parental imprisonment on children here (see also, Murray & Farrington, 2008a).

There is a need for replication studies that test the strength of the association between parental imprisonment and adverse child outcomes. We found only ten studies eligible for this review that tested the bivariate association between parental imprisonment and child antisocial behaviour, and only four studies that tested the association for child mental health outcomes. It is important to note that none of the studies used diagnostic measures of antisocial behaviour (e.g., measures of conduct disorder) or mental health (e.g., measures of clinical depression or anxiety). It would be an important advance to estimate accurately the risk for psychiatric diagnoses as well as symptoms of problems associated with parental imprisonment. Other child outcomes after parental imprisonment, such as alcohol and drug use, educational and employment outcomes, and relationship success, should also be investigated (see Murray & Farrington, 2008a, for some results on this).

Almost half of the studies in this review measured parental imprisonment that might have occurred either during childhood or before birth. Theoretically, parental imprisonment during childhood might have stronger effects on children (as any effect of parental imprisonment before birth can only affect children indirectly). We suggest that future studies should focus on the effects of parental imprisonment occurring during childhood.

New prospective longitudinal studies should be conducted with representative samples, suitable comparison groups, and reliable and valid measures of key constructs. Studies must make sure that parental imprisonment clearly precedes the child outcome being measured. If the child outcome measure overlaps with the period before parental imprisonment, the precedence of parental imprisonment before the child outcome is ambiguous.

There is a great need for more research on the causal effects of parental imprisonment on children. It is critical that future research tries to disentangle the causal effects of parental imprisonment from the effects of pre-existing disadvantage more effectively. Randomized experiments that might rigorously investigate this issue are ethically and practically possible (Killias et al., 2000a, 2000b; Villettaz, Killias, & Zoder, 2006). If child outcomes are measured in experiments that randomly assign convicted parents to prison (the usual treatment) or other (e.g., community) sentences, the causal effects of parental imprisonment on children could be estimated with greater validity than has been possible to date.

Future observational studies should make several methodological improvements to draw more confident conclusions about the causal effects of parental imprisonment on children. First, wherever possible, studies should investigate change in child behaviour within-individuals from before to after parental imprisonment. Second, it is critical that studies measure and control for important covariates that might confound the relationship between parental imprisonment and child outcomes. Most notably, studies must control for the criminality of parents, as this is such an important risk factor for child outcomes and is so highly associated with parental imprisonment. It is important that these covariates are measured before parental imprisonment. This is because controlling for covariates measured after parental imprisonment might “control away” some of the prison effects. These research requirements suggest that new longitudinal studies are required that measure multiple influences on children’s lives before, during, and after parental imprisonment.

New research should also investigate the mechanisms linking parental imprisonment and child outcomes. Theory and qualitative research suggest many possible mechanisms, but there is still a lack of systematic tests of these mechanisms. Longitudinal studies should test whether variables representing hypothesised mechanisms change from before to after parental imprisonment, and whether they mediate the effects of parental imprisonment on child outcomes.

Finally, factors that alter the impact of parental imprisonment on children (moderators) need more research attention. These can be examined in longitudinal studies that include enough children of prisoners and comparison children to test for interaction effects between parental imprisonment and possible moderators in

predicting child outcomes. Some possible moderators we propose for investigation are:

- Child characteristics, such as the child's sex, race, temperament, and IQ
- Maternal versus paternal imprisonment
- The extent and quality of involvement of parents with their children before parental imprisonment
- The parent's antisocial influence in the home
- What children are told about their parent's imprisonment
- Children's caregiving arrangements
- Parent-child contact during parental imprisonment
- Family social support and use of prisoner-family support groups
- Neighbourhood environments
- Wider social and penal contexts

Following this research, there is also a need to know about effective intervention programmes to reduce undesirable effects of parental imprisonment on children. Knowledge could be drawn from other areas of child development (e.g., research on reducing the effects of parental mental illness and the effects of parental divorce on children). Qualitative and quantitative research should be used to investigate additional support needs of prisoners' families, and systematic evaluation of intervention programs should be conducted to test how effectively they reduce undesirable outcomes for children of prisoners.

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## 7 Appendix A. References to Studies not Included in the Review

References to studies of children of prisoners excluded from the review are shown below. A number next to each reference shows the first criterion by which the study was judged ineligible for the review:

1. Qualitative study.
2. Study does not include results for both children of prisoners and a comparison group of children without imprisoned parents.
3. Study does not include a measure of child antisocial behaviour or mental health as an outcome.
4. Study does not include information from which it was possible to calculate an effect size.
5. Study sample is inappropriate for the review (e.g., all children were recruited at mental health clinics or courts, or the only comparison group consisted of children separated from parents for other reasons, or best friends of the children of prisoners).

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## 8 Appendix B. Coding Sheets

### FORM 1: STUDY ELIGIBILITY

1. Initials of coder \_\_\_\_\_
2. Date of coding \_\_\_\_\_
3. Study Identifier Number (ID)= \_\_\_\_\_ (000 to 999)
4. Author(s) name \_\_\_\_\_
5. Year of publication \_\_\_\_\_
6. Title of report \_\_\_\_\_  
\_\_\_\_\_
7. Publication type: Journal article\_\_\_ Technical report\_\_\_ Unpublished ms\_\_\_  
Book\_\_\_ Dissertation\_\_\_ Other \_\_\_\_\_
8. Name of study \_\_\_\_\_
9. If other reports held on same study record other report id(s), and first author/year  
\_\_\_\_\_

### Eligibility criteria

- |     | Yes | No  |   |
|-----|-----|-----|---|
| 10. | ___ | ___ | The report describes outcomes for children of prisoners (CP) and a comparison group of children without imprisoned parents (CC).      |
| 11. | ___ | ___ | CP and CC were compared on a measure of antisocial behavior or mental health.   |
| 12. | ___ | ___ | The child outcome was measured after parental imprisonment first occurred (after the first day the parent entered custody).           |
| 13. | ___ | ___ | The same measure of child outcome was used for both CP and CC.  |
| 14. | ___ | ___ | Enough numeric information is reported to calculate an effect size.   |
| 15. | ___ | ___ | The study did NOT recruit all children from clinics or courts.  |
| 16. | ___ | ___ | The comparison group does NOT represent children separated from parents for another reason, or best friends of children of prisoners. |
| 17. | ___ | ___ | The study is NOT an adoption study.   |



## FORM 2: STUDY LEVEL VARIABLES

18. Year(s) of research (data collection) \_\_\_\_\_

19. Place of research \_\_\_\_\_

20. Country of research \_\_\_\_\_

21. Sampling method:

1. Total population sampling \_\_\_\_\_

2. Random sampling \_\_\_\_\_

3. Convenience sampling \_\_\_\_\_

4. Case-control sampling \_\_\_\_\_

5. Other ( \_\_\_\_\_ )

6. Not known \_\_\_\_\_

22. Age of children at start of study (years):

1. Children of prisoners mean \_\_\_\_\_ range \_\_\_\_\_ / not known \_\_\_\_\_

2. Comparison children mean \_\_\_\_\_ range \_\_\_\_\_ / not known \_\_\_\_\_

3. OR Combined (CP & CC) mean \_\_\_\_\_ range \_\_\_\_\_ / not known \_\_\_\_\_

23. Gender composition:

1. Children of prisoners % female \_\_\_\_\_ % male \_\_\_\_\_ / not known \_\_\_\_\_

2. Comparison children % female \_\_\_\_\_ % male \_\_\_\_\_ / not known \_\_\_\_\_

3. OR Combined (CP & CC) % female \_\_\_\_\_ % male \_\_\_\_\_ / not known \_\_\_\_\_

24. Ethnic composition:

1. Children of prisoners \_\_\_\_\_ / not known \_\_\_\_\_

2. Comparison children \_\_\_\_\_ / not known \_\_\_\_\_

3. OR Combined (CP & CC) \_\_\_\_\_ / not known \_\_\_\_\_

25. Eligible sample size:

1. Children of prisoners \_\_\_\_\_ / not known \_\_\_\_\_

2. Comparison children \_\_\_\_\_ / not known \_\_\_\_\_

3. OR Combined (CP & CC) \_\_\_\_\_ / not known \_\_\_\_\_

26. Initial sample size:

1. Children of prisoners \_\_\_\_\_ / not known \_\_\_\_\_

2. Comparison children \_\_\_\_\_ / not known \_\_\_\_\_

3. OR Combined (CP & CC) \_\_\_\_\_ / not known \_\_\_\_\_

27. % Initial Response

1. Children of prisoners \_\_\_\_\_ / not known \_\_\_\_\_

2. Comparison children \_\_\_\_\_ / not known \_\_\_\_\_

3. OR Combined (CP & CC) \_\_\_\_\_ / not known \_\_\_\_\_

**Longitudinal studies only**

28. Final sample size:

(at last outcome measure)

1. Children of prisoners \_\_\_\_\_ / not known \_\_\_\_\_

2. Comparison children \_\_\_\_\_ / not known \_\_\_\_\_

3. OR Combined (CP & CC) \_\_\_\_\_ / not known \_\_\_\_\_

29. % Retention

(re. initial sample size)

1. Children of prisoners \_\_\_\_\_ / not known \_\_\_\_\_

2. Comparison children \_\_\_\_\_ / not known \_\_\_\_\_

3. OR Combined (CP & CC) \_\_\_\_\_ / not known \_\_\_\_\_

**Measure of parental imprisonment**

30. How were children of prisoners (CP) identified? What measures were used?

1. Parents sampled at a prison \_\_\_\_
2. Official records of parental imprisonment \_\_\_\_
3. Self-report of imprisonment by parent \_\_\_\_
4. Child-report of parental imprisonment \_\_\_\_
5. Other-report (e.g., mother reporting about father's imprisonment) \_\_\_\_
6. Other ( \_\_\_\_\_ )
7. Not known \_\_\_\_

31. In the comparison group (CC), for which parents was it verified that they had not been imprisoned?

1. Not verified for either parent \_\_\_\_
2. Verified for both father and mother \_\_\_\_
3. Verified for father only (not mother) \_\_\_\_
4. Verified for mother only (not father) \_\_\_\_
5. Other ( \_\_\_\_\_ )
6. Not known \_\_\_\_

32. How was it determined that parents of the comparison group (CC) had not been imprisoned?

1. Search of official records of parents \_\_\_\_
2. Self-report \_\_\_\_
3. Child-report \_\_\_\_
4. Other-report (e.g., mother about father) \_\_\_\_
5. Other ( \_\_\_\_\_ )
6. Parental imprisonment status of control group not verified \_\_\_\_
7. Not known \_\_\_\_

**Sub-samples and multiple effect sizes**

33. Which sub-samples and multiple comparisons are coded separately in this study?

1. No breakdown: only one sample coded \_\_\_\_
2. Child gender \_\_\_\_
3. Age of children at time of parental imprisonment \_\_\_\_
4. Mother/father imprisoned \_\_\_\_

34. Which effect sizes are coded from this study?

1. Delinquency, juvenile, bivariate association \_\_\_\_
2. Delinquency, adult, bivariate association \_\_\_\_
3. Delinquency, juvenile, partial association \_\_\_\_
4. Delinquency, adult, partial association \_\_\_\_
5. Mental health, juvenile, bivariate association \_\_\_\_
6. Mental health, adult, bivariate association \_\_\_\_
7. Mental health, juvenile, partial association \_\_\_\_
8. Mental health, adult, partial association \_\_\_\_

35. How many effect sizes in total are coded from this study? \_\_\_\_\_

36. Concerns about methodological quality not captured in coding forms (*turn over if necessary*)

---

### FORM 3: SUB-SAMPLE LEVEL VARIABLES (REPEAT FOR EACH SUB-SAMPLE)

#### Sub-sample ID

37. Sub-sample identification number (SID) = \_\_\_\_ (ID). \_\_\_\_ (00 to 99)

38. Which breakdown does this sub-sample SID refer to?

1. No breakdown (total sample) \_\_\_\_
2. Child gender \_\_\_\_
3. Age of children at time of parental imprisonment \_\_\_\_
4. Mother/father imprisoned \_\_\_\_

39. Description of sub-sample coded here (e.g. boys) \_\_\_\_\_

40. How many effect sizes are coded for this sub-sample? \_\_\_\_

#### Gender

41. Gender distribution of children:

1. All boys (100%) \_\_\_\_
2. Mostly boys ( $\geq 80\%$ ) \_\_\_\_
3. All girls (100%) \_\_\_\_
4. Mostly girls ( $\geq 80\%$ ) \_\_\_\_
5. Mixed (boys  $< 80\%$  & girls  $< 80\%$ ) \_\_\_\_

#### Children of prisoners (CP)

42. How old were the children when parental imprisonment occurred? \_\_\_\_\_

1. Preschool (0-4) \_\_\_\_
2. Primary school (5-9) \_\_\_\_
3. Secondary school (10-17) \_\_\_\_
4. Preschool-primary school mix (0-9) \_\_\_\_
5. Primary-secondary school mix (5-17) \_\_\_\_
6. Any age (0-17) \_\_\_\_
7. Parent "ever imprisoned" (up to 18 & including parental imprisonment before birth) \_\_\_\_
8. Not known \_\_\_\_

43. Which parents were imprisoned?

1. Biological mothers \_\_\_\_
2. Biological fathers \_\_\_\_
3. Biological mothers/fathers \_\_\_\_
4. Biological/ step mothers \_\_\_\_
5. Biological/ step fathers \_\_\_\_
6. Any parent \_\_\_\_
7. Not known \_\_\_\_

44. What was the length of parental imprisonment (years)?

1. Mean \_\_\_\_\_
2. SD \_\_\_\_\_
3. Minimum length \_\_\_\_\_
4. Maximum length \_\_\_\_\_
5. Not known \_\_\_\_

**FORM 4: EFFECT SIZE LEVEL VARIABLES (REPEAT FOR EACH EFFECT SIZE)**

**Effect size ID**

45. Effect size identification number (EID) = \_\_\_\_ (ID). \_\_\_\_ (SID). \_\_\_\_ (00 to 99)

46. Which effect size does this EID refer to?

1. Delinquency, juvenile, bivariate association \_\_\_\_
2. Delinquency, adult, bivariate association \_\_\_\_
3. Delinquency, juvenile, partial association \_\_\_\_
4. Delinquency, adult, partial association \_\_\_\_
5. Mental health, juvenile, bivariate association \_\_\_\_
6. Mental health, adult, bivariate association \_\_\_\_
7. Mental health, juvenile, partial association \_\_\_\_
8. Mental health, adult, partial association \_\_\_\_

**Comparison children (CC)**

47. What comparison group were children of prisoners compared to (to produce this effect size)?

1. General population of children (including school population) \_\_\_\_
2. Children of parents receiving other criminal sanction \_\_\_\_
3. Children of parents who were imprisoned before the child's birth but not afterwards \_\_\_\_
4. Other ( \_\_\_\_\_ )
5. No comparison group \_\_\_\_
6. Not known \_\_\_\_

**Outcome measures**

48. What type of delinquent outcome was measured?

1. Antisocial/ externalizing behavior \_\_\_\_
2. Criminal behavior \_\_\_\_
3. Not known \_\_\_\_

49. What type of mental health outcome was measured?

1. Internalizing problems (general) \_\_\_\_
2. Depression \_\_\_\_
3. Anxiety \_\_\_\_
4. Other internalizing problems ( \_\_\_\_\_ )
5. Not known \_\_\_\_

50. Did the outcome refer to the period during parental imprisonment or after release?

1. During parental imprisonment \_\_\_\_
2. After release \_\_\_\_
3. Not known \_\_\_\_

51. When was the measure taken?

- Years after the parent was first imprisoned: \_\_\_\_
1. Short-term (< 2 years) \_\_\_\_
  2. Long-term (≥ 2 years) \_\_\_\_
  3. Not known \_\_\_\_

52. Informant: Who reported the outcome?

1. Self report \_\_\_\_\_
2. Outside caregiver report \_\_\_\_\_
3. Imprisoned parent report \_\_\_\_\_
4. Teacher rating \_\_\_\_\_
5. Criminal record (arrest, conviction, other \_\_\_\_\_) \_\_\_\_\_
6. Psychiatric record \_\_\_\_\_
7. Other \_\_\_\_\_
8. Not known \_\_\_\_\_

53. What instrument was used to measure the outcome? \_\_\_\_\_  
Not known \_\_\_\_\_

54. What reference period did the instrument have? \_\_\_\_\_  
Not known \_\_\_\_\_

### Methodological quality

(See Cambridge Quality Checklists: Murray, Farrington, & Eisner, 2009, for scoring details)

55. Assessment of association

	Yes (1)	No (0)	Not known (0)
1. Adequate sampling	_____	_____	_____
2. Adequate responder rates	_____	_____	_____
3. Adequate sample size	_____	_____	_____
4. Good measure of correlate	_____	_____	_____
5. Good measure outcome	_____	_____	_____

*Topic specific criteria for scoring studies:*

Good measures of correlate (parental imprisonment) include:

- a) If parents were sampled in a prison
- b) Official records were used to determine whether or not parents were imprisoned
- c) Parents self-report whether or not they themselves were imprisoned

Good measures of official crime include:

- a) Official measures of arrest, conviction or imprisonment

56. Did parental imprisonment clearly precede the child outcome?

1. Yes \_\_\_\_\_
2. No \_\_\_\_\_

(Note even if parental imprisonment occurred before the child outcome was measured, if the child outcome measure refers to a period before parental imprisonment occurred, it is not clear that parental imprisonment occurred before the child outcome.)

57. Did the study analyse change in child outcome from before to after parental imprisonment?

1. Yes \_\_\_
2. No \_\_\_

58. What methods were used to analyse change in child outcome?

1. Tested whether CP and CC significantly different on pretests (& no differences found) \_\_\_
2. CP and CC matched on pretest scores \_\_\_
3. Pretest scores included in propensity score \_\_\_
4. Pretests used to calculate change scores (from before to after parental imprisonment), which were used in analyses of prison effects \_\_\_
5. Statistical control of pretests scores in analysis of outcome (e.g., in regression analysis) \_\_\_
6. CP and CC not balanced on pretest of outcome \_\_\_
7. Not known \_\_\_

59. Were other covariates controlled for in analyses of the outcome?

1. Yes \_\_\_
2. No \_\_\_

60. What methods were used to control for other covariates between CP and CC?

1. Tested whether CP and CC significantly different on covariates \_\_\_
2. CP and CC matched on specific covariates \_\_\_
3. CP and CC matched on propensity scores \_\_\_
4. Statistical control of covariates (e.g., regression/covariance) in analysis of outcome \_\_\_
5. Other ( \_\_\_\_\_ )
6. No balancing of background covariates \_\_\_
7. Not known \_\_\_

61. Which covariates were balanced between CP and CC, and when were they measured?

After parental imprisonment (a)

Before parental imprisonment (b)

Concurrently with parental imprisonment (c) = around first day of imprisonment

1. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)
2. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)
3. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)
4. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)
5. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)
6. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)
7. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)
8. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)
9. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)
10. \_\_\_\_\_ (a \_\_\_ b \_\_\_ c \_\_\_)

62. Based on the covariates controlled for (above), should this study be described as "adequately controlled" or "inadequately controlled"?

Note. At least three critical covariates must be controlled for the study to be rated as "adequately controlled". Also all covariates must be measured before or concurrently with parental imprisonment for the study to be rated as adequately controlled. If a study controls for any covariates that are measured after parental imprisonment, the study should be rated "inadequately controlled".

1. Adequately controlled ( $\geq 3$  critical covariates are controlled) \_\_\_\_\_
2. Inadequately controlled ( $< 3$  critical covariates are controlled) \_\_\_\_\_

*List of critical covariates*

- Child covariates:  
Impulsivity, attention deficits, IQ, school attainment
- Parent covariates:  
Parental antisocial behavior/ criminality, parental age, parental education, parental mental health, parental substance abuse
- Parenting covariates:  
Low parental supervision, harsh parental discipline, abuse of child, neglect of child, parent-child conflict, inter-parent conflict
- Family covariates:  
Family size, socio-economic status, family income
- Wider environmental covariates:  
Peer delinquency, neighborhood deprivation, neighborhood crime, school crime
- Other critical covariates not listed, but agreed by coding team:  
\_\_\_\_\_  
\_\_\_\_\_

**NOW CHOOSE ONE FORM TO RECORD THE EFFECT SIZE:**

- FORM 5: BIVARIATE EFFECT SIZES
- FORM 6: PRE-TEST ADJUSTED EFFECT SIZES
- FORM 7: COVARIATE ADJUSTED EFFECT SIZES (USING MATCHING)
- FORM 8: COVARIATE ADJUSTED EFFECT SIZES (USING STATISTICAL CONTROLS)

**FORM 5: BIVARIATE (ZERO-ORDER) EFFECT SIZES**

63. Prevalence of child outcomes (cases with outcome vs. noncases)

1. Page number(s) \_\_\_\_\_

	Children of Prisoners	Comparison Children
Number of noncases		
Number of cases		

2. Odds Ratio \_\_\_\_\_

3. Confidence Interval \_\_\_\_\_

64. Child outcome scores

1. Page number(s) \_\_\_\_\_

	Children of Prisoners	Comparison Children
Mean		
SD		
n		

2. d \_\_\_\_\_

3. SE \_\_\_\_\_

65. Other numerical information used to calculate effect size

1. Page number(s) \_\_\_\_\_

- r (point biserial) \_\_\_\_\_ proportion of children of prisoners in sample \_\_\_\_\_
- t value \_\_\_\_\_ n (CP) \_\_\_\_\_ n (CC) \_\_\_\_\_
- F ratio \_\_\_\_\_ n (CP) \_\_\_\_\_ n (CC) \_\_\_\_\_
- X<sup>2</sup> value \_\_\_\_\_ Total N \_\_\_\_\_
- B (unadjusted for covariates) \_\_\_\_\_ SE \_\_\_\_\_
- SE of mean \_\_\_\_\_ N \_\_\_\_\_ (for any one group: \_\_\_\_\_)
- p value (for test: \_\_\_\_\_) \_\_\_\_\_
- Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
- Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
- Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
- Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_

2. Estimated d \_\_\_\_\_

3. SE \_\_\_\_\_



## FORM 6: PRETEST-ADJUSTED EFFECT SIZES

### 66. Gain scores

1. Page number(s) \_\_\_\_\_

	Children of Prisoners	Comparison Children
<b>Pretest</b>		
Mean		
SD		
N		
<b>Posttest</b>		
Mean		
SD		
n		

2. Pooled SD at posttest \_\_\_\_\_

3. Pretest adjusted d \_\_\_\_\_

4. SE \_\_\_\_\_

5. Was this pretest adjusted effect size also adjusted for covariates?

1. Yes \_\_\_\_\_

2. No \_\_\_\_\_

### 67. Logistic regression-adjusted effect sizes (controlling for pretest)

1. Page number(s) \_\_\_\_\_

2. Adjusted OR (from logistic regression analyses) \_\_\_\_\_

3. CI \_\_\_\_\_

4. Was this pretest adjusted effect size also adjusted for covariates?

1. Yes \_\_\_\_\_

2. No \_\_\_\_\_

### 68. Linear regression-adjusted effect sizes (controlling for pretest)

1. Page number(s) \_\_\_\_\_

2. Adjusted B (from regression analyses) \_\_\_\_\_

3. SE \_\_\_\_\_

4. Was this pretest adjusted effect size also adjusted for covariates?

1. Yes \_\_\_\_\_

2. No \_\_\_\_\_

### 69. Other pretest-adjusted effect size

1. Page number(s) \_\_\_\_\_

o Other statistic (\_\_\_\_\_) \_\_\_\_\_

o Other statistic (\_\_\_\_\_) \_\_\_\_\_

o Other statistic (\_\_\_\_\_) \_\_\_\_\_

o Other statistic (\_\_\_\_\_) \_\_\_\_\_

2. Estimated d \_\_\_\_\_

3. SE \_\_\_\_\_

**FORM 7: COVARIATE-ADJUSTED EFFECT SIZES USING MATCHING  
(NOT BALANCED ON PRE-TEST SCORES)**

70. Prevalence of child outcomes (cases with outcome vs. noncases)

1. Page number(s) \_\_\_\_\_

	Children of Prisoners	Comparison Children
Number of noncases		
Number of cases		

2. Odds Ratio \_\_\_\_\_

3. Confidence Interval \_\_\_\_\_

71. Child outcome scores

1. Page number(s) \_\_\_\_\_

	Children of Prisoners	Comparison Children
Mean		
SD		
n		

2. d \_\_\_\_\_

3. SE \_\_\_\_\_

72. Other numerical information used to calculate effect size

1. Page number(s) \_\_\_\_\_

- r (point biserial) \_\_\_\_\_ proportion of children of prisoners in sample \_\_\_\_\_
- t value \_\_\_\_\_ n (CP) \_\_\_\_\_ n (CC) \_\_\_\_\_
- F ratio \_\_\_\_\_ n (CP) \_\_\_\_\_ n (CC) \_\_\_\_\_
- $X^2$  value \_\_\_\_\_ Total N \_\_\_\_\_
- B \_\_\_\_\_ SE \_\_\_\_\_
- SE of mean \_\_\_\_\_ N \_\_\_\_\_ (for any one group: \_\_\_\_\_)
- p value (for test: \_\_\_\_\_) \_\_\_\_\_
- Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
- Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
- Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
- Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_

2. Estimated d \_\_\_\_\_

3. SE \_\_\_\_\_

**FORM 8: COVARIATE-ADJUSTED EFFECT SIZES USING STATISTICAL CONTROLS  
(NOT BALANCED ON PRE-TEST SCORES)**

73. Logistic regression-adjusted effect sizes

1. Page number(s) \_\_\_\_\_
2. Adjusted OR (from logistic regression analyses) \_\_\_\_\_
3. CI \_\_\_\_\_

74. Linear regression -adjusted effect sizes

1. Page number(s) \_\_\_\_\_
2. Adjusted B (from regression analyses) \_\_\_\_\_
3. SE (B) \_\_\_\_\_
4. sd \_\_\_\_\_
5. Estimated d \_\_\_\_\_
6. SE (d) \_\_\_\_\_

75. Effect size adjusted for covariates in analysis of covariance

1. Page number(s) \_\_\_\_\_

	Children of Prisoners	Comparison Children
Mean		
SD		
n		

2. F \_\_\_\_\_
3. Estimated d \_\_\_\_\_
4. SE \_\_\_\_\_

76. Other covariate-adjusted effect size

4. Page number(s) \_\_\_\_\_
  - o Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
  - o Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
  - o Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
  - o Other statistic ( \_\_\_\_\_ ) \_\_\_\_\_
5. Estimated d \_\_\_\_\_
6. SE \_\_\_\_\_

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## 9 Appendix C. Detailed Description of Sixteen Studies Included in the Review

### **Huebner: National Longitudinal Survey of Youth, United States**

Huebner and Gustafson (2007) compared adult offending behaviour of 31 children whose mothers had been imprisoned and 1,666 children whose mothers had not been imprisoned, in the National Longitudinal Survey of Youth. This is a nationally representative longitudinal study of males and females who were aged 14-22 in 1979 (Center for Human Resource Research, 2006), and the females' children, who were the subjects of the study by Huebner and Gustafson. Maternal imprisonment was measured in annual interviews with mothers from 1979-1994, and in biannual interviews from 1996-2000. This measure is likely to exclude occasions of short-term imprisonment (under three months) and occasions of imprisonment occurring between interviews (Huebner & Gustafson, 2007). Fathers' imprisonment was not measured.

In 2000, the children ranged between 18-24 years of age. Thus, for younger children, it is possible that their mothers were imprisoned before they were born (from 1979-1982). Adult convictions of the children were measured using self-reports from 1994-2000. No adult conviction occurred before maternal imprisonment. However, some children in the study were too young (under eighteen) to have been at risk when adult convictions were measured. The following covariates were measured: child delinquency and education, maternal absence, maternal delinquency, maternal education, maternal smoking during pregnancy, adolescent mother, parental supervision, home environment, peer pressure, and the age, sex, and race of the child. Many covariates (including child delinquency) referred to periods after maternal imprisonment might have occurred.

Huebner and Gustafson reported that 26% of children with imprisoned mothers were convicted as an adult compared with 10% of comparison children. This translates into a bivariate odds ratio of 3.1 (CI = 1.4-7.1).<sup>6</sup> Huebner and Gustafson

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<sup>6</sup> Note that some odds ratios in this Appendix have slightly different confidence intervals than those reported in the meta-analyses (Figures 6-9). This is because of rounding during transformations of confidence intervals in the programme used for the meta-analyses (Comprehensive Meta Analysis).

(2007) also reported significant effects of maternal incarceration on adult conviction (OR = 3.0; CI = 1.4-6.4), even after controlling for the covariates listed above. However, controlling for covariates measured after maternal imprisonment (especially child delinquency) might underestimate the overall effects of maternal imprisonment on children. Change in child outcome from before to after parental imprisonment was not analysed, which might mean that effects of parental imprisonment were overestimated in this study.

### **Johanson: Sweden**

Johanson (1974) used a case-control design and compared rates of paternal and maternal imprisonment between 128 male youth prison inmates released in 1951 (cases), and 128 males who were born at the same time and place in Sweden (the controls). It was determined whether participants had histories of parental imprisonment for 107 cases and 117 controls, using data from the central penal register and court ordered psychiatric reports. Maternal imprisonment data were obtained for 127 participants in both groups. Data on parental imprisonment were collected between the years 1964-67, but the timing of parental imprisonment is not known. Therefore, parental imprisonment might have occurred before the participant's birth, during childhood, or even after the outcome (youths' own imprisonment). Twenty-seven cases had fathers who had been imprisoned, compared with eight controls. This translates into an odds ratio of 4.6 (CI = 2.0-10.7). Four cases had mothers imprisoned compared with zero controls. This translates into an odds ratio of 8.2<sup>7</sup> (CI = 0.4-157.3).

### **Johnson: Panel Study of Income Dynamics, United States**

Johnson (2009) compared outcomes for children whose parents were imprisoned during three different stages of childhood (0-5, 6-10, and 11-16) and children who did not have a parent imprisoned, in the Panel Study of Income Dynamics. This is a longitudinal study of a nationally representative sample of families recruited in the United States in 1968. From 1997, data were collected on over 3,500 of the participants' children (Manieri, 2005). Parental imprisonment was measured by identifying whether parents were imprisoned at each interview wave until 2005, and by asking them, in 1995, whether and when they had previously served time in jail or prison. Imprisonment of both mothers and fathers was measured in this study. 584 children had a father who had ever been imprisoned, but it was not reported how many children had a mother imprisoned, or the number of children who had a parent imprisoned at different times during childhood.

Caregivers reported child internalising and externalising behaviour in 1997 and in 2002-03. Data were available for 3,540 children aged 3-17. Internalising behaviour referred to the following items: "child has felt loved, been fearful or anxious, easily confused, felt worthless, is disliked by other children, obsessed with thoughts, sad or depressed, withdrawn, clinging to adults, cried too much, and has felt others were

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<sup>7</sup> Using 0.5 for the cell with zero count.

out to get him or her”. No details of the externalising scale were reported, and the psychometric properties of both scales were not reported.

Johnson analysed the relationship between parental imprisonment and child behaviour, while controlling for a number of covariates in OLS regression models. Only occasions of parental imprisonment occurring before the child outcome were used in analyses (R. Johnson, personal communication, 2008). The covariates that were included in the regression models were: parental imprisonment at other times in childhood and before the child’s birth, neighbourhood quality, neighbour policing for drugs, family member with alcohol problems, religiosity, parental education, whether or not the mother was married, and the child’s sex, age, and race.

Johnson reported regression  $\beta$ -weights (and standard errors) for child behaviours associated with parental imprisonment, and (in personal communication) provided standard deviations for child behaviours for the whole sample. Using these statistics, we computed the standardised mean difference (d)<sup>8</sup> for each outcome, and translated these into odds ratios. Adjusted odds ratios for externalising behaviour were 2.1 (CI = 1.1-4.1) for parental imprisonment 0-5; 1.6 (CI = 0.8-3.1) for parental imprisonment 6-10; and 5.2 (CI = 1.6-17.2) for parental imprisonment 11-16. Odds ratios for internalising problems were: 2.6 (CI = 1.1-6.1) for parental imprisonment 0-5; 1.8 (CI = 0.8-4.0) for parental imprisonment 6-10; and 4.7 (CI = 1.1-19.1) for parental imprisonment 11-16. Some covariates were measured after parental imprisonment, which may have resulted in underestimation of the effects on children. Change in child outcome was not analysed, and parental criminality was not controlled for, which might have resulted in overestimating the effects of parental imprisonment on children.

### **Kandel: Danish Cohort Study, Denmark**

Kandel et al. (1988) compared the criminal outcomes of 92 sons with fathers who had at least one prison sentence and 513 sons of fathers who had never been registered with the police, in a birth cohort of 1,944 males born between 1936-1938 in Copenhagen, Denmark. 1,400 males were targeted for follow-up in this study. The study report suggests that 795 participants were excluded from analyses because their father had been arrested and not imprisoned. Details were not provided, but it appears that paternal imprisonment might have occurred any time until 1972, when sons’ records were searched. Therefore, paternal imprisonment might have occurred before birth, during childhood, or even after the son’s criminal outcome. Maternal incarceration was not measured.

Of sons with imprisoned fathers, 39% received at least one prison sentence themselves by ages 34-36. Of sons in the comparison group, 7% received at least one prison sentence. This translates into an odds ratio of 8.5 (CI = 5.0-14.6). This might overestimate the bivariate association between paternal imprisonment and son’s

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<sup>8</sup> Confidence intervals for  $\beta$ -weights were calculated using the standard errors, then converted into confidence intervals for d’s.

imprisonment because sons whose fathers had other kinds of criminal record (e.g., an arrest record) were excluded from the comparison group.

### **Kinner: Mater University Study of Pregnancy, Australia**

Kinner Alati, Najman, and Williams (2007) compared the behaviours of 137 children of imprisoned fathers and 2,262 controls in the Mater University Study of Pregnancy.<sup>9</sup> This is a longitudinal study of 8,458 women who were pregnant in Australia in 1981 and children arising from the pregnancy (Najman et al., 2005). When the children were aged 14 years, mothers were asked whether their current partner had ever been detained in prison. Therefore, paternal imprisonment does not necessarily refer to the child's biological father. Maternal imprisonment was not measured. At age 14, child externalising and internalising problems were measured using the Child Behavior Checklist and the Youth Self Report (Achenbach, 1991a, 1991c). Because these measures refer to the prior six months, there is some overlap between parental imprisonment and the child outcome in this study. The following covariates were measured between birth and age five: maternal age and education, family income, maternal anxiety/depression, maternal substance use, dyadic adjustment, domestic violence and parenting style. Kinner et al. (2007) analysed data on 2,399 adolescents for whom complete data were available.

In bivariate analyses, odds ratios relating paternal imprisonment and youth reported externalising problems were 1.7 (CI = 0.9-3.3) for boys and 1.5 (CI = 0.7-3.4) for girls. Odds ratios for internalising problems were 1.2 (CI = 0.5-3.0) for boys and 2.0 (CI = 1.0-3.9) for girls. In multivariate analyses, adjusting for the covariates listed above, odds ratios relating paternal imprisonment and youth reported externalising problems were 1.3 (CI = 0.6-2.5) for boys and 1.2 (CI = 0.5-2.9) for girls. Adjusted odds ratios for internalising problems were 1.1 (CI = 0.4-3.0) for boys and 1.9 (CI = 1.0-3.8) for girls. Because covariates were measured after paternal imprisonment might have occurred, these multivariate analyses might underestimate the effects of paternal imprisonment on children. Change in child outcome was not analysed, and parental criminality was not controlled for, which might have resulted in overestimating the effects of parental imprisonment.

### **MurrayCSDD: Cambridge Study in Delinquent Development, United Kingdom**

Murray and Farrington (2005; 2008a; 2008b) compared 23 boys whose parents were imprisoned in the boys' first ten years of life and 382 boys who did not experience parental imprisonment, in the Cambridge Study in Delinquent Development.<sup>10</sup> This is a longitudinal study of 411 boys born in 1953 and living in South London in 1963 (Farrington, 2003; Farrington et al., 2006). Data were collected through interviews with the study males, their parents, their teachers, and

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<sup>9</sup> Bor, McGee, and Fagan (2004) also briefly reported the association between parental imprisonment and child delinquency in this study.

<sup>10</sup> See also Murray (2006) and Osborn and West (1979).

through searches of criminal records until age 50 years. The criminal records of the boys' mothers and fathers were repeatedly searched until 1994.

To estimate the bivariate association between parental imprisonment and boys' outcomes for this review,<sup>11</sup> we compared boys whose parents were imprisoned in the boy's first ten years of life ( $n = 23$ ) with all boys whose parents were not imprisoned until age 18 ( $n = 382$ ). None of the 23 boys who had a parent imprisoned had been permanently separated from their parent before the imprisonment. The outcomes examined were convicted between ages 10-18, convicted between ages 18-50, neuroticism at age 16 (which reflects vulnerability to internalising problems, Eysenck & Eysenck, 1964),<sup>12</sup> and internalising problems at age 48 (using the General Health Questionnaire, Goldberg & Williams, 1988). In bivariate analyses, parental imprisonment predicted all four outcomes with the following odds ratios: 6.0 (CI = 2.4-14.5) for conviction at ages 10-18; 4.7 (CI = 2.0-11.5) for conviction at ages 18-50; 2.3 (CI = 1.0-5.3) for neuroticism at age 16; and 3.2 (CI = 1.3, 8.0) for internalising at age 48.

To estimate covariate-adjusted effect sizes for this review, we compared the same boys whose parents were imprisoned during childhood ( $n = 23$ ) with boys whose parents were imprisoned only before the boy's birth ( $n = 17$ ). The logic of this comparison is that children whose parents are imprisoned only before their birth do not experience parental imprisonment but might have similar family backgrounds to children whose parents are imprisoned during childhood. We also statistically controlled for covariates that predicted boys' outcomes. Previous analyses identified which child, parent, and family covariates, measured at age 10, predicted boys' outcomes (Murray & Farrington, 2005, 2008b). The most important predictors were the number of convictions of the boy's parents, the boy's IQ, whether or not the boy was "daring", and family size. Thus, we controlled for these covariates in logistic regression analyses to estimate the effects of parental imprisonment on children.

Comparing boys whose parents were imprisoned during childhood (between birth and age 10) with boys whose parents were imprisoned only before birth, while adjusting for age 10 covariates, adjusted odds ratios were 1.3 (CI = 0.3-5.4) for conviction at ages 10-18; 1.5 (CI = 0.3-7.2) for conviction at ages 19-50; 1.2 (CI =

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<sup>11</sup> We calculated new results from the Cambridge Study for this review for the following three reasons. First, we wanted to combine several comparison groups that were analysed separately in previous investigations. This was easier to do from the raw data than from the results previously published. Second, we previously analysed internalising problems mainly as continuous variables (Murray & Farrington, 2008b). We preferred to calculate odds ratios directly from the data, instead of converting results from continuous measures into odds ratios (see Murray & Farrington, 2008a, regarding the dichotomous variables that we use for this review). A third reason for calculating new results was that two outcome variables that were most suited to this review (convicted ages 10-17 and convicted ages 18-50) had not been previously investigated regarding the causal effects of parental imprisonment on children.

<sup>12</sup> Although neuroticism is a measure of vulnerability to internalising problems, rather than internalising problems themselves, we include this measure in our review, because we found so few other eligible results on mental health.



0.2-5.8) for neuroticism at age 16; and 2.9 (CI = 0.4-19.0) for internalising at age 48. The fact that covariates were measured after parental imprisonment, at about age 10, might have caused an underestimation of the effects of parental imprisonment on children in this study. Change in child outcome was not analysed, which might have caused an over-estimate of prison effects.

### **MurrayPM: Project Metropolitan, Sweden**

Murray, Janson, and Farrington (2007) compared criminal convictions of children whose parents were imprisoned between birth and age 18 and children whose parents were not imprisoned in Project Metropolitan. This is a longitudinal study of all 15,117 children born in 1953 who lived in Stockholm in 1963 (Hodgins & Janson, 2002; Janson, 2000). Parental imprisonment was measured by searching the criminal records of the father (or the mother, if information was not available about the father) until 1972, when children were aged 19. It is not known exactly how many mothers' records were searched, but parental imprisonment refers primarily to the fathers' imprisonment. Children's criminal records were searched for the years 1972-1983, corresponding to when they were aged 19-30.

To estimate the bivariate association between parental imprisonment and children's convictions for this review, we compared children whose parents were imprisoned between birth and age 6 (early childhood,  $n = 75$ ) or between ages 7-18 (late childhood-adolescence,  $n = 146$ ) with children whose parents were not imprisoned at all between birth and age 18 ( $n = 14,834$ ).<sup>13</sup> Children whose parents were imprisoned in both early childhood and late childhood-adolescence were excluded from the analysis.

Parental imprisonment in early childhood predicted conviction in adulthood (ages 18-30) with odds ratios of 1.7 (CI = 0.8-3.5) for boys and 5.2 (CI = 2.1-12.4) for girls. Parental imprisonment in late childhood-adolescence predicted conviction in adulthood with odds ratios of 3.5 (CI = 2.2-5.5) for boys and 1.6 (CI = 0.6-4.3) for girls.

To calculate covariate-adjusted effect sizes for this review, we compared children whose parents were imprisoned any time between birth and age 18 ( $n = 283$ ) and children whose parents were imprisoned only before the child's birth ( $n = 245$ ). When making this comparison, we also controlled for the number of criminal convictions the parents received (until the child was age 18) and the social class of the family at age 10 in logistic regression models. The adjusted odds ratios comparing parental imprisonment in childhood with parental imprisonment only before birth were 1.6 (CI = 0.9-2.9) for boys and 1.4 (CI = 0.5-3.6) for girls. However, covariates were measured after parental imprisonment, which might have

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<sup>13</sup> Results reported previously by Murray et al. (2007) used a slightly different comparison group for bivariate analyses, and did not investigate the possible causal effects of parental imprisonment on boys and girls separately. Therefore, we calculated new results from Project Metropolitan for this review.

biased the results, and change in child outcome was not analysed, which might have caused an over-estimation of prison effects.

### **Pakiz: United States**

Pakiz, Reinherz, and Giaconia (1997) compared children whose parents had been imprisoned up to age 18 with children whose parents had not been imprisoned in The Simmons Longitudinal Study. This is a longitudinal study of 777 children who were aged five years in 1977 (see, e.g., Reinherz, Giaconia, & Paradis, 2007). At age 18, parental imprisonment was measured as part of a structured interview on family environment. It appears that parental imprisonment might have occurred any time until children were aged 18 (although this is not completely clear in the study report). It was not reported how many children had had a parent imprisoned. Three hundred and seventy-five participants remained in the study at age 21 when antisocial behaviour was measured in interviews, using items from DSM-III-R (Robins, Helzer, Cottler, & Goldring, 1989). The following covariates were measured for males between ages 5-18: family disadvantage, childhood behaviour problems, school grades, physical abuse in the family, participant marijuana use/dependency. The following covariates were measured for females between ages 5-18: childhood hostility, self-esteem, school suspension, attention problems, parental divorce, antisocial behaviour, sexual abuse in family, and need for social support. Analyses were based on 188 males and 187 females with complete data at age 21.

Regression models predicting age 21 antisocial behaviour were computed, separately for males and females, controlling for the covariates listed above. For males, having an imprisoned parent by age 18 was significantly associated with age 21 antisocial behaviour ( $r_{pb} = .20, p < .001$ ), but for females it was not. We estimated that, for males, the odds ratio for antisocial behaviour associated with parental imprisonment was 5.4 (CI = 1.6-20.7).<sup>14</sup> It was not possible to estimate an effect size for females, because only a “non-significant” finding was reported. This study might have underestimated the effects of parental imprisonment on children because covariates (including child antisocial behaviour) were measured after parental imprisonment. Change in child outcome was not analysed, and parental criminality was not controlled for, which might have resulted in overestimating the effects of parental imprisonment.

### **Peniston: Children at Risk, United States**

Peniston (2006) compared rates of delinquency between 27 children whose caregivers had been incarcerated and 622 children whose caregivers had not been incarcerated, in the Children at Risk study. Children at Risk is a longitudinal-experimental study of 11-13 year old children and their caregivers living in high risk

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<sup>14</sup> We did this by estimating  $d$  from  $r_{pb}$  (using Fischer's  $Z_r$  transformation, and the assumption that 5% of boys in the study had a parent imprisoned), and then estimating OR from  $d$ . The assumption of 5% prevalence of parental imprisonment was based on the estimate that, of lower-class white children born in the U.S. in 1978, 2.9% had a parent imprisoned between birth and ages 11-14 (Wildeman, 2009).

neighbourhoods in Texas, Connecticut, Tennessee, Georgia, and Washington (Harrell, Cavanagh, & Sridharan, 1999, 2000). Baseline data were collected in 1993 from children and their primary caregivers. Adolescents were randomly assigned to receive drug and delinquency prevention services ( $n = 338$ ) or no extra services ( $n = 333$ ), or were selected to form a quasi-experimental group of children from similar high risk neighbourhoods ( $n = 203$ ). At follow-up in 1995, caregivers were asked whether they had been in jail any time during the previous two years. Incarceration of other parents was not measured. Also at follow-up, youth were asked nine questions about their delinquent behaviour in the previous two years. Thus, there is complete overlap in the reference period regarding caregiver imprisonment and youth delinquency in this study.

Peniston reported that, of 27 youth whose caregivers had been imprisoned, 37% had been incarcerated themselves. Of 622 youth whose caregivers had not been imprisoned, 18% had been incarcerated. This translates into a bivariate odds ratio of 2.7 (CI = 1.2-6.1).

### **Rakt: Criminal Careers and Life-Course Study, The Netherlands**

Rakt, Murray, and Nieuwbeerta (in progress) compared criminal convictions of 1,858 children whose fathers were imprisoned and 4,123 children whose fathers were not imprisoned during childhood in the Criminal Careers and Life-Course Study. This is a longitudinal, record-based study of a random sample of 4% of men convicted of crimes in the Netherlands in 1977, and their children (Nieuwbeerta & Blokland, 2003). Paternal imprisonment was measured by searching fathers' criminal records until 2003. Maternal imprisonment was not measured in this study. Rakt et al. selected 5,981 children (of 3,590 fathers) who were over age 18 in 2003. Because all children had fathers with at least one criminal conviction, the study is not suited to estimate the bivariate association between paternal imprisonment and child outcomes.

To estimate covariate-adjusted effect sizes for this review, children whose fathers were imprisoned between ages 0-12 ( $n = 935$ ) or between ages 12-18 ( $n = 319$ ), were compared with children whose fathers were imprisoned only before the child's birth ( $n = 569$ ). Children whose fathers were imprisoned during both periods (0-12 and 12-18) were excluded from the analysis. The following covariates were measured using fathers' criminal records and national population registers: the total number of offences that fathers committed until children were aged 18, the criminal trajectory group of the father (out of four trajectories measured until 2003), whether or not the father was born abroad, alcohol and drug abuse by the father, parental separation, total number of siblings, teen-pregnancy of the mother, and child age and sex.

Effects of paternal imprisonment on boys and girls were estimated in logistic regression models first for imprisonment occurring between birth and age 12

(childhood) and second for imprisonment occurring between ages 12-18 (adolescence). The outcome was the average chance of conviction per year between ages 18-30. This outcome was used because some children were younger than 30 in 2003. After controlling for the covariates listed above, adjusted odds ratios comparing paternal imprisonment in childhood with paternal imprisonment before birth were 1.2 (CI = 0.9-1.5) for boys and 1.5 (CI = 1.0-2.2) for girls. Adjusted odds ratios for paternal imprisonment in adolescence were 1.1 (CI = 0.7-1.6) for boys and 1.7 (0.8-3.7) for girls. Because the covariates controlled for were measured until 2003, they might have occurred after paternal incarceration. Thus, these results might underestimate the effects of paternal incarceration on children. However, change in child outcome was not analysed, which might have resulted in overestimating the effects of parental imprisonment.

### **Roettger: National Longitudinal Study of Adolescent Health, United States**

Roettger (2008) compared levels of serious and violent delinquency between 784 males whose fathers had ever been imprisoned and 5,344 males whose fathers had never been imprisoned in the National Longitudinal Study of Adolescent Health.<sup>15</sup> This is a longitudinal study of about 20,000 adolescents who were in grades 7-12 in 1994-95. A sub-sample of about 7,500 male participants was eligible for follow-up interviews in 2001-02, when participants were between ages 18-24. In these interviews, they were asked, “Has your biological father ever served time in jail or prison?” Maternal imprisonment was not measured. Self-reported serious and violent delinquency was also measured at this time using 15 questionnaire items referring to the previous twelve months (which were converted into a 5-point scale). Thus, the outcome measure of delinquency refers to a period that overlaps with when paternal incarceration might have occurred.

The bivariate odds ratio for serious and violent delinquency associated with paternal imprisonment was 1.8 (CI = 1.3-2.7). Logistic regression was used to estimate an adjusted odds ratio after controlling for covariates. The covariates that were measured were the participant’s race, drink/substance abuse, family structure, parental strictness, father involvement, physical abuse, care by social services, school attachment, high school dropout, employment, marriage, cohabitation, poverty, and the racial and educational characteristics of the census tract in which the participant lived. The covariate-adjusted odds ratio associated with paternal imprisonment was 1.6 (CI = 1.2-2.2). Covariates in this study were measured after paternal imprisonment so this might underestimate the causal effects of paternal

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<sup>15</sup> Roettger and Swisher (in progress) also estimated the effects of paternal imprisonment on youth delinquency in this sample separately according to youth race. Roettger (2008) and Guo, Roettger, and Cai (2008) estimated the effects of parental imprisonment on youth delinquency in a separate twin sample of the study, using hierarchical linear modelling. These analyses showed that paternal incarceration was significantly associated with both serious and violent delinquency both before and after controlling for individual, family and community covariates (Roettger, 2008, Table 4.3). However, we were unable to convert these results into effect sizes for inclusion in this review.

imprisonment on delinquency. However, change in child outcome was not analysed, and parental criminality was not controlled for, so this result might overestimate the effects of parental imprisonment.

### **Stanton: United States**

In a pioneering study on the effects of maternal imprisonment on children, Stanton (1980) compared children of fifty-four mothers in jail and twenty-one children with mothers on probation. The mothers had a total of 166 children, aged four to eighteen. Children had been living with their mother before her arrest. Stanton first collected data from the children's mothers, children's outside caregivers, and children's teachers during the mother's imprisonment. Because the comparison group consisted of children with mothers on probation, this study is not suitable for estimating bivariate associations between maternal imprisonment and child outcomes, but it can be used to estimate causal effects.

Low self-esteem of the child was rated by teachers or counsellors. For teachers, the Coopersmith Behavior Rating Form (Coopersmith, 1967) was used. Although low self-esteem indicates vulnerability to internalising problems rather than internalising problems themselves, we include these results because there were so few eligible studies with results for children's mental health problems. Of 22 children with jailed mothers, 13 were rated as having low self-esteem, compared with 4 out of 18 children whose mothers were on probation. This translates into an odds ratio of 5.1 (CI = 1.2, 20.5).

Stanton also re-interviewed the mothers one month after their release from jail. At that time, the mothers reported whether or not their children had been in trouble with the police, the school, or neighbours (the reference period was not specified). Of 24 children of jailed mothers, 10 had been in trouble, compared with 4 out of 17 children with mothers on probation. This translates into an odds ratio of 2.3 (CI = 0.6, 9.3).

Comparison of imprisoned mothers and mothers on probation showed that the groups differed in their prior criminal history, marital history, socioeconomic status, unemployment rates, and educational levels. Because these differences were not controlled for in the analyses, the results are likely to be biased. Moreover, four of the probation mothers in the study had previously been imprisoned, confounding the comparison between their children and children of jailed mothers. This study also did not analyse change in child outcome, and so the effects of maternal imprisonment might have been over-estimated.

### **Stroble: United States**

Stroble (1997) compared levels of depression between 15 children who had a history of parental imprisonment, 15 children living in single-parent families for reasons other than parental imprisonment, and 15 children living with both parents. All

children in the study were in grades 9-12 at a high school in Richmond, Virginia, and all were African-American. Eighty-percent of children were female. Children self-reported if their parent had been imprisoned, or if they lived in a single-parent family for other reasons, or neither. It appears that parental imprisonment might have occurred at any time in the past (including before the child's birth), although this is not entirely clear in the report. Child depression was measured using the Children's Depression Inventory (Kovacs, 1983). Originally, 20 students in each group were eligible for the study, but full data were only available for 15 in each group. Mean (sd) depression scores were 54.6 (14.8) for children of imprisoned parents, 55.0 (13.2) for children living in single-parent families for reasons other than parental imprisonment, and 46.3 (9.6) for children living with both parents. We estimated<sup>16</sup> that the odds ratio for depression comparing children of imprisoned parents with all other children in the study was 1.8 (CI = 0.6-5.5).

### **Wakefield: Project on Human Development in Chicago Neighborhoods, United States**

Wakefield (2007) compared the behaviours of 69 children whose fathers were imprisoned and 2,313 children whose fathers were not imprisoned, in the Project on Human Development in Chicago Neighborhoods.<sup>17</sup> This is a longitudinal study of 6,000 children over six years old in 80 Chicago neighbourhoods (Earls, Brooks-Gunn, Raudenbush, & Sampson, 2002). Wakefield (2007) selected children who were aged 6-15 at baseline. Data on father incarceration were collected at baseline and at follow-up, three years later, apparently in interviews with children's caregivers (although this is not completely clear in the study report). Sixty-nine children had fathers who were incarcerated between baseline and follow-up. Data on maternal incarceration were not used in this study. At baseline and at follow-up, the Child Behavior Checklist (Achenbach, 1991a) was used to measure children's internalising and externalising problems. The Child Behavior Checklist refers to child behaviours in the previous six months, so there is some overlap in the reference period of the outcome measure and the period that paternal imprisonment might have occurred.

Wakefield (2007) reported means and standard deviations for child behaviour scores at follow-up. Using these results, we calculated standardised mean differences (d) between children whose fathers were incarcerated and children whose fathers were not incarcerated (we combined two comparison groups to make this comparison). We then converted these results into odds ratios. Odds ratios were 2.0 (CI = 1.3-3.1) for externalising problems (based on the delinquency sub-scale of the Child Behavior Checklist) and 1.9 (CI = 1.2-2.9) for internalising problems.

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<sup>16</sup> The scores from the two comparison groups were pooled. Then d was calculated, and the odds ratio was estimated from d.

<sup>17</sup> See also Wakefield (in progress).

To estimate the causal effects of parental imprisonment on children, Wakefield (2007) calculated child behaviour scores at follow-up controlling for baseline child behaviour scores and other covariates, in OLS regression models. Covariates included the age, race, and sex of the child, whether the primary caregiver was employed, the household income, parental divorce between baseline and follow-up, and whether the primary caregiver was the mother or father of the child. We transformed the  $\beta$ -weights from the OLS regression models into d-type effect sizes and then into odds ratios. Adjusted odds ratios were 1.9 (CI = 1.3-2.8) for externalising problems (using the total externalising score on the Child Behavior Checklist) and 2.4 (CI = 1.6-3.6) for internalising problems. Because covariates were measured after paternal imprisonment occurred, these results might underestimate the effects of paternal imprisonment on children. Although change in child outcome was analysed (by controlling for baseline child behaviour), parental criminality was not controlled for, which might have caused an overestimate of the effects of parental imprisonment.

### **Wilbur: United States**

Wilbur et al. (2007) compared the behaviours of 31 children whose fathers were incarcerated and 71 children whose fathers were not incarcerated, in a cohort of 252 children born in Boston between 1990-93. Infants were originally selected for the study to investigate the effects of in utero cocaine exposure on children, and approximately one half of the original sample (n = 123) had been exposed to cocaine in utero (Frank et al., 2002; Frank et al., 1999). At each follow-up interview, when children were aged 6, 8, 9, and 11 years, children's caregivers were asked whether the child's father had been imprisoned in the previous two years or since the last interview. Wilbur et al. compared children whose fathers had been imprisoned when the children were aged 6-11 (n = 31) with children whose fathers had not been imprisoned during this period (n = 71). Children who had an imprisoned mother (n = 5) were excluded from the analyses. The Child Behavior Checklist (Achenbach, 1991a) was administered in each interview between 6-11 years.<sup>18</sup> The last measure of child behaviour available after the first report of the father's imprisonment was used as the outcome. Because half of the cohort of children had been exposed to cocaine in utero, this study is not suitable for calculating the bivariate association between paternal imprisonment and child outcomes.

Wilbur et al. estimated the causal effects of paternal imprisonment on children by comparing children of imprisoned fathers with children whose fathers were not imprisoned, while controlling for covariates in OLS regression models. The covariates that were measured included child age and sex; in utero exposure to cocaine, alcohol, tobacco and marijuana; the mother's perception of the father's

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<sup>18</sup> The Teacher Report Form (Achenbach, 1991b), and the Children's Depression Inventory (Kovacs, 1983) were also administered in this study, but we selected results based on the Child Behavior Checklist for this review because the authors provided additional information about these results to calculate an effect size.

drug/alcohol problems at birth; the current caregiver (birth mother versus other); distress of current caregiver; and child's exposure to violence. However, only significant ( $p < .05$ ) covariates were retained in the final models (child age, child gender, and cocaine exposure during pregnancy).

Wilbur et al. reported the differences in child behaviour T-scores (population mean = 50, standard deviation = 10) between children of imprisoned fathers and comparison children while controlling for covariates (these differences were reported as  $\beta$ -weights with standard errors). We divided these differences by 10 to produce standardised mean differences (d), and then converted d's into odds ratios.<sup>19</sup> Adjusted odds ratios associated with a father's imprisonment were 2.3 (CI = 1.0-5.4) for externalising problems, and 1.1 (CI = 0.5-2.5) for internalising problems. However, change in child outcome was not analysed, and parental criminality was not controlled for, so these results might overestimate the effects of parental imprisonment.

### **Wildeman/ Geller: Fragile Families and Child Wellbeing Study, United States**

Wildman (2008) analysed the effects of parental imprisonment on children in the Fragile Families and Child Wellbeing Study. This is a birth cohort study of 4,898 children born in 20 cities in the United States between 1998 and 2000 (Reichman, Teitler, Garfinkel, & McLanaghan, 2001). Nonmarital births were oversampled in the study. Parental imprisonment was measured in interviews with children's mothers at 30 months and at 60 months.

Wildeman (2008) compared the aggressive behaviours of 306 children whose fathers were imprisoned between 30-60 months and 2,080 children whose fathers had not been imprisoned. Children whose mothers were imprisoned were excluded from the analyses (even if their father was imprisoned). Wildeman analysed mothers' responses to three questions about their children's physically aggressive behaviours at 36 and 60 months. Thus, there is some overlap in the reference period of the outcome measure at 60 months and the period in which parental imprisonment might have occurred (30-60 months).

Wildeman reported the bivariate association between a father's imprisonment and child aggressive behaviours at 60 months as a standardised mean difference (represented as a  $\beta$ -weight for a standardised behaviour score). From these results, we calculated that odds ratios for childhood aggression following father imprisonment were 2.2 (CI = 1.6-3.0) for boys and 1.7 (CI = 1.3-2.4) for girls.

To estimate the causal effects of paternal imprisonment on child aggression, Wildeman calculated the standardised mean difference in aggressive behaviours at

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<sup>19</sup> We calculated the confidence interval for differences in T scores and then divided these by 10 to obtain confidence intervals for d's.



60 months, controlling for child aggressive behaviours at 36 months, and other covariates. The covariates that were controlled for were measured up to 36 months (C. Wildeman, personal communication, 2008) and included: parental age and education, child race, number of children, in utero nicotine exposure, low birth weight, parental self-control, days with the father, poverty, “maternal mastery”, domestic abuse, parental relationship quality, “social father”, prior relationships, corporal punishment, erratic punishment, low collective efficacy, and neighbourhood social disorder. Adjusted odds ratios for aggressive behaviour controlling for prior child behaviour and covariates were 1.4 (CI = 1.0-1.9) for boys and 0.9 (CI = 0.7-1.1) for girls. Because covariates did not clearly occur before parental imprisonment, controlling for them might have caused an underestimation of prison effects. Although change in child outcome was analysed (by controlling for prior child behaviour), parental criminality was not controlled for, which might have caused an overestimate of the effects of parental imprisonment.

Geller, Garfinkel, Cooper, and Mincy (in progress) also used data from the Fragile Families and Child Wellbeing Study to investigate the effects of parental imprisonment on children.<sup>20</sup> Unlike Wildeman, Geller et al. investigated parental imprisonment that might have occurred at any time (including before children’s births) until children were aged three years. Because we are most interested in effects of parental imprisonment during childhood, in this review we mainly report results from Wildeman’s analyses. However, Geller et al. analysed the effects of maternal as well as paternal imprisonment and analysed internalising outcomes as well as antisocial outcomes, which were not reported by Wildeman. Thus, we also report results on maternal imprisonment and on internalising outcomes from Geller et al.’ analyses.

Data on parental imprisonment were missing for about 10% of the sample in the analyses by Geller et al.. They imputed the missing data to produce 4,789 cases for analyses. Of these, 2,641 children had parents who had never been imprisoned, 117 had a mother (only) who had been imprisoned, 1,794 had a father (only) who had been imprisoned, and 237 children had both a mother and a father who had been imprisoned. Children’s aggression and anxious and depressive symptoms were measured using the Child Behavior Checklist (Achenbach & Rescorla, 2000) at age three years. Because the Child Behavior Checklist refers to behaviours in the previous six months, there is some overlap between when parental imprisonment was measured and children’s reported behaviours.

Geller et al. reported rates of child problem behaviour adjusting for parents’ race, age, education, and impulsivity. The odds ratio for anxiety/depression following imprisonment of either parent was 1.1 (CI = 0.9-1.2), indicating almost zero effect. The odds ratios for outcomes following maternal imprisonment (only) were 0.9 (CI

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<sup>20</sup> See also Garfinkel, Geller, Cooper (in progress).

= 0.5-1.7) for aggression and 0.5 (CI = 0.3-1.1) for anxiety/depression. Covariates might have occurred after parental imprisonment, which could result in underestimating the effects of parental imprisonment. However, change in child outcome was not analysed, and parental criminality was not controlled for, which might cause an overestimation of causal effects.