Abstract

Background: Parent depression is a well-established prospective risk factor for adverse offspring mental health. Multiple lines of evidence suggest that improvements in parent depression predicts improved offspring mental health. However, no systematic review has examined the impact on offspring of psychological treatment of purely parent depression after the postnatal period.

Aims: To systematically review the literature of randomised controlled trials examining the impact on offspring mental health outcomes of psychological interventions for parental depression after the postnatal period.

Method: We pre-registered our systematic review on PROSPERO (CRD42023408953), and searched the METAPSY database in April, 2023 and October, 2024, for randomised controlled trials of psychological interventions for adults with depression, which also included a child mental health or wellbeing outcome. We double screened 938 studies for inclusion using the ‘Paper in a Day’ approach. All included studies would be rated using the Cochrane Risk of Bias tool.

Results: We found no studies that met our inclusion criteria.

Conclusions: Robust research into psychological therapy for depression in adults outside the post-natal period has failed to consider the potential benefits for those adults’ children. This is a missed clinical opportunity to evaluate the potential preventive benefits for those children at risk of adverse psychological outcomes, and a missed scientific opportunity to test mechanisms of intergenerational transmission of risk for psychopathology. Seizing the clinical and scientific opportunities would require adult-focused mental health researchers to make inexpensive additions of child mental health outcomes measures to their evaluation projects. **Keywords:** parent depression, child mental health, intergenerational risk transmission,

**Key practitioner message:**

* Children of parents who have lived experience of an episode of depression, compared to children of parents without depression, are at a substantially greater risk of developing mental health problems. Meta-analytic evidence from randomized controlled trials shows that improvements in parent *post-natal* depression following psychological intervention predict positive child psychological outcomes.
* There is no systematic review of psychological interventions for adult depression outside the post-natal period that have assessed children’s psychological outcomes.
* In this pre-registered systematic literature review of randomized controlled trials examining the impact on offspring psychological outcomes following psychological interventions for (non-post-natal) depression in adults, we found no studies.
* This represents a missed clinical opportunity to evaluate the potential preventive benefits for those children at risk of adverse psychological outcomes, and a missed scientific opportunity to test mechanisms of intergenerational transmission of risk for psychopathology.
* Adult-focused mental health researchers can seize the clinical and scientific opportunities by making inexpensive additions of child mental health outcomes measures to their evaluation projects.

**Introduction**

Observational studies of parental depression demonstrate adverse associations with offspring development, including early risk markers for later psychopathology, such as disorganized attachment in the postnatal year (Hayes, Goodman and Carlson, 2013); dysregulated behaviour in early childhood (Conroy, Pariante, Marks, et al., 2012); less peer competence in middle childhood (Kersten-Alvarez, Hosman, Riksen-Walraven, et al., 2012); and poorer academic performance in adolescence (Brophy, Todd, Rahman, et al., 2021). Furthermore, offspring of parents who have lived experience of an episode of depression, compared to those of parents without this lived experience, have a two- to three-fold greater risk for major depressive disorder (Weissman, Warner, Wickramaratne, et al., 1997, Weissman, Wickramaratne, Nomura, et al. 2006, Weissman, Wickramaratne, Gameroff, et al. 2016), and a significantly greater risk of externalizing disorders (Brennan, Hammen, Katz, et al., 2002).

Clearly, having parents with lived experience of an episode of depression represents a serious risk to children’s wellbeing. But what happens if the parent’s depression improves? Multiple lines of evidence have begun to amass regarding the relationship between changes in parents’ depression symptoms and offspring psychopathology. One line of evidence pertains to the use of antidepressant medication. For example, in the Sequenced Treatment Alternatives to Relieve Depression (STAR\*D) trial, remission of maternal depression was associated with less severe offspring symptoms of internalizing disorders (Foster, Webster, Weissman, et al., 2008). A second line of evidence has focused on perinatal depression which is associated with negative effects on the mother-child relationship and poorer child cognitive, emotional and social developmental outcomes (Cooper & Murray, 1997; Goodman & Gotlib, 1999; Murray, Cooper, Wilson, et al., 2003; Thompson & Fox, 2010). In light of these risks, perinatal interventions have often focused on mothers’ parenting behaviours and / or the mother-infant relationship, as well as on mothers’ depression (Stein, Netsi, Lawrence, et al., 2018). Letourneau, Dennis, Cosic and Linder (2017) reported a systematic review of studies using an RCT or quasi-experimental design and found large effects at the end of treatment for both child development and mothers’ parenting behaviours in a systematic review of literature examining the impact of psychological (e.g., Cognitive Behavioural Therapy; Interpersonal Therapy) and parenting (e.g., home visiting, video feedback therapy) interventions for perinatal depression. Some studies had follow-up evaluations of children’s outcomes beyond 12-months, up to 5 years, post-treatment (e.g., Kersten-Alvarez et al., 2010; Murray et al., 2003), though treatment effects on children were evident only for those families who reported stressful life events (Kersten-Alvarez et al., 2010). Importantly, some earlier studies used non-randomized controlled trial designs (Clark et al., 2003; Fleming et al., 1992) and the findings from these studies are subject to biases introduced by their designs. A third line of evidence relates to examining the impact of treating depression in mothers whose children were experiencing mental health difficulties. For example, in a randomised trial, Swartz, Frank, Zuckoff, et al. (2008) examined the impact of brief interpersonal psychotherapy for mothers whose children were receiving psychiatric treatment, and found, compared to treatment as usual, that improvements in mothers’ depression were followed by improvement in offspring depression symptoms. This suggests that treating parental depression might have a positive impact on children’s mental health. However, it should be noted that in this line of research, the offspring were usually in receipt of treatment for their own difficulties meaning that it is difficult to determine whether improvements in their mental health should be attributed to the care that their depressed mother received. Similarly, the results of these studies are not generalisable to populations of unselected children who may not yet be reporting any symptoms themselves.

Despite multiple lines of intervention research, the literature is lacking a systematic review of the impact on children of psychological treatment of parental depression beyond the postnatal period or in unselected children (i.e., who were not selected on the basis of pre-identified mental health difficulties). Cuijpers et al. (2015) reported a meta-analysis of the effects of psychological treatments for mothers with lived experience of depression on maternal (in 8 studies) and child psychological outcomes (in 5 studies) within randomised controlled trials (RCTs), and found a medium positive effect (Hedge’s g = 0.40, 95%CI = 0.22 – 0.59) for child mental health. Crucially, however, of these five studies, three (Clark, Tluczeck and Brown, 2008; Forman, O’Hara, Stuart et al., 2007; Murray et al., 2003) focused exclusively on the postnatal period and the other two (Swartz et al., 2008; Verduyn, Barrowclough, Roberts, et al., 2003) only included mothers identified in light of existing child mental health problems. Furthermore, the interventions in four studies explicitly addressed children’s outcomes (Clark et al., 2008; Puckering, McIntosh, Hickey and Longford, 2010; Sheeber, Seeley, Feil, et al., 2012; Verduyn et al., 2003) by focusing on maternal parenting behaviours and / or mother-offspring relationships, as well as treating maternal depression.

Whilst clearly valuable, the reviews outlined above cast little light on the impact on offspring mental health outcomes of psychological therapy for a parent’s depression beyond the perinatal period, or on unselected children. This is an important gap. For both clinical and theoretical reasons, it is crucial that we understand the impact on children of improvements in parental depression. Randomized controlled trial (RCT) studies, which eliminate confounding biases, have offered the most useful clinical and theoretical evidence of the effects of a treatment on an outcome. Hence, we shall limit the focus of our review to studies that have used randomized controlled trial designs (for a recent discussion of innovations in RCT design, see Fernainy et al., 2024). This knowledge would open up new lines of research that help us to understand the intergenerational transmission of mental health, and would allow service providers to plan approaches that maximise the likelihood of good outcomes for the children of depressed parents.

Thus, the aim of the present study was to systematically review the literature of randomised controlled trials examining the impact on offspring mental health outcomes of psychological interventions for parental depression, after the postnatal period, and which focus entirely on treating parental depression (studies that also addressed parenting behaviours or gave any support directly to the child were excluded).

**Method**

We pre-registered our systematic review via the PROSPERO website: <https://www.crd.york.ac.uk/prospero/display_record.php?RecordID=408953>, and followed the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (Page et al., 2021).

Ethics

All authors abided by the Standards of Conduct, Performance and Ethics, and Code of Ethics and Conduct, as set out by the BABCP and BPS, respectively.

Eligibility Criteria

Studies were eligible for inclusion in the review if:

1. They recruited adults (aged 18 years +) who were a parent (including adoptive, biological, foster, grand, or other resident guardian)
2. Adult participants were recruited as the primary participant, and on the basis of their own (probable) depressive disorder (rather than being identified secondary to identification of a difficulty in their child). (We operationalized ‘probable depressive disorder’ as ascertained via diagnostic interview and / or scoring at or above a clinical cut-off on a standardised measure of depression.)
3. The index participant received a psychological treatment for adult depression. Any treatment delivery format was eligible (e.g., individual, group, face-to-face, online, etc.) and no restrictions were placed on the setting where the psychological treatment was conducted, the number of sessions, or the duration of follow-up.
4. They reported the results of an RCT evaluating the effects of the psychological treatment against a control group who were receiving a different intervention (e.g., any other psychological therapy) for comparison, or no intervention (for example, a waiting list control).
5. They assessed any child mental health outcome (diagnosis and / or symptoms) and / or child wellbeing after the index parent had received psychological treatment for depression.
6. They were primary research published in English in a peer-reviewed journal.

Studies were excluded if:

1. The intervention did not target adult depression as the primary focus, or was a pharmacological intervention, or focused specifically on perinatal depression, or focused on parenting or the parent-child relationship.
2. The children of the index participants received any psychological intervention within the context of the study.

Information Sources and Search Terms

The METAPSY depression psychotherapy database (Cuijpers, 2017) (www.metapsy.org) was used to identify all randomised controlled trials examining psychotherapy interventions for adults with depression. This database is updated every four months with systematic searches of four databases (PubMed, PsycINFO, Embase and the Cochrane Library). (This is reported in full here: <https://docs.metapsy.org/uploads/protocol.pdf>) For this project, on 26th April, 2023, we used the database that had been updated on 1st September, 2022. On 1st October, 2024, we re-ran our search using the most recent update of the METAPSY database (updated on 1st January, 2024). Full details of the search strategy are described in (Cuijpers et al., 2023) with the full search terms available at <https://protectlab.shinyapps.io/depressionShinyWebsite/search_strings.pdf>

The terms used to create and update the METAPSY database extend to all fields, and are reported in full here: https://raw.githubusercontent.com/metapsy-project/data-depression-psyctr/22.0.2/metadata/search\_string.txt

Study selection

This systematic review was conducted using the ‘paper in a day’ approach (Larsen, Hessinger, Larson, et al., 2023), with the aim of completing the study selection, data extraction, analysis, and composition of the first draft of the paper in a single day (28th April, 2023). Due to this, elements of the process were conducted in parallel to maximise efficiency. (We updated our search on 1st October, 2024.)

The full texts of studies (k = 935, plus the abstracts of k= 3 for which we were unable to obtain the full texts) from the ‘adult’ section of the METAPSY depression psychotherapy database were screened for inclusion. Studies from the ‘child and adolescent’ section of the METAPSY database were excluded from screening on the grounds that they were extremely unlikely to include any index participants who were themselves parents aged 18 years +. In the first stage of screening, the retrieved *full texts* of papers were independently screened twice by researchers, blind to others’ assessments (initials removed for blind review) for keywords relating to families using the following terms to identify papers relating to families: ‘offspring’, ‘baby’, ‘infant’, ‘child’, ‘adolescent’, ‘youth’, ‘parent’, ‘mother’, ‘father’. They were also checked for presence of any outcome measures relating to children or parenting. The first rater checked texts using computer software, and the second rater checked the texts by reading them. In the second stage of screening, all 215 papers identified at stage one as potentially meeting inclusion criteria were then screened against the full inclusion and exclusion criteria by two of (PJL, AD, SC-H) by reading the full texts. Furthermore, in February, 2025, we used artificial intelligence software (ChatGPT) to screen the available non-English language studies for terms relating to ‘child’ or ‘parent’. (This was not a feature of our pre-registered study protocol, but overcomes the limitation of excluding studies based on the language in which they were reported.) See Figure 1 for the PRISMA flowchart.

**INSERT FIGURE 1 HERE**

Data Extraction

An Excel spreadsheet was designed to record information on 28th April, 2023, from studies that met inclusion criteria. This recorded relevant information for each study including author, title, sample size, measures used to assess variables of interest, treatment and comparator characteristics, outcomes of interest, and effect sizes.

Quality Assessment

Quality assessment was conducted by the Cuijpers, Miguel, Harrer, et al. (2023) study team, who assessed the validity of all studies included in the METAPSY depression database using the Risk of Bias (RoB) assessment tool for randomised trials (Higgins & Green, 2011).

Planned Synthesis of Results

To facilitate comparison across studies, we planned to calculate effect sizes for the key analyses comparing child mental health and wellbeing outcomes after parents received an intervention for depression, with parents in a control group. To ensure consistency across studies, we would (re-) calculate effect sizes as Hedges’ g, and estimate our meta-analytic effect size using a two- or three-level meta-analysis model (depending on whether individual studies report more than one effect) with random effects. We would calculate heterogeneity using the *I*² statistic. (A value of 0% would indicate no heterogeneity and higher values, higher heterogeneity. Heterogeneity of 25% is defined as the threshold for low, 50% for moderate and 75% for high. To account for uncertainty, we would calculate 95% confidence intervals for *I*²). Furthermore, we planned to report the predictive intervals to estimate the range of the true effect. We planned to use visual inspection of funnel plots and QQ-plots to detect potential biasing effects. Furthermore, we planned to assess asymmetry using Egger’s test (Egger, Smith, Schneider and Minder, 1997) for two-level models, and a proxy Egger test for three-level models (Rodgers & Pustejovsky, 2021).

In the event that too few studies were retrieved for a meta-analytical synthesis, we planned to conduct a narrative synthesis of the results to summarise the results of available outcomes.

**Results**

We identified zero studies that met the inclusion criteria for this review.

**Discussion**

This study set out to determine whether purely treating a parent’s depression with psychological intervention beyond the perinatal period has an impact on the well-established risk of poor mental health in their children. We were able to find no studies that met our robust criteria. This result is disappointing, but not surprising: in recent systematic reviews seeking to understand the impact on children of treating parental anxiety (Chapman, Hutson, Dunn, et al., 2022) and parental bipolar disorder and schizophrenia (Can et al., 2024), we were also unable to find any robust evidence.

We aimed to find all randomised controlled trials that examined the simple impact on unselected offspring of treating their parent using a psychological intervention for depression. Studies that recruited participants on the basis of mental health difficulty in the child (rather than the parent) or focused on perinatal depression were excluded, as were studies where the intervention extended beyond treating the adult’s depression (e.g., intervened with the child or gave parent management training). No studies that met these criteria were found. As a result, it is impossible to draw any conclusions about the impact of simply treating a parent with depression on their children’s risk of poor outcomes.

This gap in our knowledge represents a considerable clinical and theoretical problem. Clinically, we do not know whether only treating a parent with depression reduces the risk of poor outcomes for their children, and, if it does, whether and to what extent the risk is remediated. If treating a parent’s depression were proved to be sufficient, this could mitigate the impacts of parent depression on children and, in turn, could remove some considerable demand on child mental health services. Similarly, some adult mental health services (such as the Improving Access to Psychological Therapies / NHS Talking Therapies for anxiety and depression system in England) expedite the treatment of clients who are known to be parents, on the grounds that this is likely to be protective to their children, but it is presently unclear whether this approach is empirically justified. Theoretically, the knowledge would support a clearer and deeper understanding of how and why mental health problems run in families: if successful treatment of depression in parents were shown to reduce risk of poor mental health in their children, this would open new avenues of research, examining, for example, whether treating parental depression has an impact on parenting attitudes and behaviours, and providing some insight into the mechanisms involved in the intergenerational transmission of poor mental health.

Although this systematic review returned no direct and robust evidence of the impact of simply treating parental depression on children’s risk of poor mental health, there are, as outlined in the Introduction above, clues from other sources: Although studies that focused purely on depression in the perinatal period were excluded from this review, there is evidence from a small number of RCTs focused on this period suggesting that psychological treatment of parents with depression is associated with improved functioning in offspring Cuijpers et al. (2015).

So, it appears plausible that treating parental depression will have positive impacts on children’s wellbeing. However, if this is, eventually, demonstrated to be the case, should this approach become the default? We would argue that although it might well be proven to be the first line response when a parent is depressed, it should not be the only option. High quality depression interventions may not always be available to, or wanted by, the parent, and, ultimately, are not always successful. In these situations, approaches that support the parent to be the best parent they can be, in the context of their mental health difficulty, will always be needed. Moreover, we know very little about the relative costs of treating purely the parent’s depression versus providing additional / alternative support for the parent and / or child, and these will need to be established.

Conducting standalone randomised controlled trials that evaluate the impact on children of treating parental depression will be costly and difficult. However, there is a more accessible alternative: inviting adult mental health researchers to include measures of child outcomes in their RCTs would be inexpensive and simple. Although issues of statistical power and of parental objectivity would likely need attention, these are surmountable, and we encourage adult- and child-focused researchers to collaborate in this manner. Key considerations will include i) whose report of children’s mental health should be sought, e.g., parents’, children’s, others’ (e.g., teachers); and ii) how to assess this, e.g., clinician interview, self-report, or both (Wolpert et al., 2016). Regarding i), a pragmatic approach for researchers could be to seek parental report only. This would, of course, be scientifically limiting, not least because depression appears itself to have an influence on parents’ judgement of their children’s symptoms (e.g., Clarke et al., 2001; Compas et al., 2015). Given that the essential nature of our suggestion is its practical simplicity - that adult mental health researchers seek information from trial participants about their children’s mental health - the practical simplicity might outweigh the scientific limitation.Regarding ii) the Common Measures in Mental Health Science Initiative (Farber et al., 2023) has endorsed the Revised Child Anxiety and Depression Scale (RCADS-25; Chorpita et al., 2005) for children aged at least 8 years, making this arguably the optimal measure to use. For studies examining mental health of children under 8 years, the widely used Strengths and Difficulties Questionnaire (SDQ; Goodman, 1997) for children aged at least 2 years could be optimal.

This review had a number of strengths, including tight inclusion criteria focusing on scientifically robust studies, and the use of the well-established and exhaustive METAPSY database, meaning that no eligible studies are likely to have been overlooked. We employed careful and highly reliable methods, meaning that we are unlikely to have failed to identify any appropriate studies. However, it also had a number of weaknesses, which should be taken into account: included studies were restricted to those that employed a psychological treatment modality. It is possible that studies employing a pharmacological treatment approach would have been eligible, although we are currently aware of only one (the STAR-D study, Foster et al. (2008)). Second, although the review used the highly robust METAPSY database, this database is, at the time of writing, up to date until January, 2024. It is possible that other relevant literature could have been published in the period since then.

We limited the focus of our review to studies using randomised controlled trial designs. Hence, we are unable to draw any conclusions about children’s outcomes in studies using other designs. Such a review could underpin the foundations laid by our review by identifying both a) whether children’s outcomes have been examined using designs less robust than an RCT and, if so, b) what impact intervention for depression in a parent might have for children’s mental health.

We limited the focus of our review to answer the simple question: *What happens to children’s mental health when we psychologically treat their parents’ depression?* Hence, we excluded studies in which interventions focused on parenting behaviours or parent-child relationships in addition to psychological treatment of parental depression. While this limited focus yielded our ‘empty review’, and thus the platform for our call to action to adult mental health researchers, it means that we are unable to comment on the value that such adjuncts might add for children’s mental health. Letourneau et al. (2017) did include such studies, as have more recent trials (e.g., Stein et al., 2018), which, clinically, can evaluate the benefits for children and, scientifically, help elucidate mechanisms operating in the intergenerational transmission of risk of adversity.

In summary, we know very little about the impact on children of treating parental depression beyond the perinatal period. This is a major clinical and theoretical knowledge gap which urgently needs addressing. Addressing this gap need not be costly or difficult, but will require adult mental health researchers, their funders, their sponsors and other stakeholders, to accept a role in answering this important question.

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**FIGURE 1 PRISMA Flowchart**

**Identification of studies via databases and registers**

Records removed *before screening*:

Duplicate records removed (k = 1)

Records marked as ineligible by automation tools (k = 0)

Records removed for other reasons (k = 0)

Records identified from\*:

Databases (k = 939)

Registers (k = 0)

**Identification**

Reports not retrieved excluded

(k = 0)

Reports sought for retrieval \*

(k = 938)

Reports screened

(k = 938)

Reports excluded (k =723)

**Screening**

Reports excluded:

No child outcome measure (k = 145)

Perinatal only (k = 58)

Non-English language (k = 9)

Parents recruited on basis of child characteristics (k = 3)

Reports assessed for eligibility

(k = 215)

Studies included in review

(k = 0)

Reports of included studies

(k = 0)

**Included**

\*All records were screened twice for ‘child’-related words, first by using software, second by human eyes.

*From:*  Page MJ, McKenzie JE, Bossuyt PM, Boutron I, Hoffmann TC, Mulrow CD, et al. The PRISMA 2020 statement: an updated guideline for reporting systematic reviews. BMJ 2021;372:n71. doi: 10.1136/bmj.n71

**Supplementary materials**

**Search Methods**

We searched the ‘adult’ section of the METAPSY depression psychotherapy database (version updated on 1st September, 2022) on 26th April, 2023, which contained k = 857 unique reports, and updated our search (version updated on 01st January, 2024) on 1st October, 2024, which contained k = 81 further reports.

Our screening was broken into two stages, both of which used the full text for each report (except for three studies, for which only the abstract was available).

First, two raters independently, and blind to each other’s assessments, searched for keywords relating to families, using terms associated with or derived from ‘offspring’, ‘baby’, ‘infant’, ‘child’, ‘adolescent’, ‘youth’, ‘parent’, ‘mother’, ‘father’. These raters also checked the Methods section of each report (and, where available, any online registration of the study) for any and all outcomes relating to children or parenting. Reports that potentially met inclusion criteria were assessed in stage two.

Second, two raters (from PJL, AD and SC-H) assessed the full text of k= 212 and abstracts of k= 3 papers identified at stage one as potentially eligible against all inclusion and exclusion criteria.

| **Section and Topic** | **Item #** | **Checklist item** | **Location where item is reported** |
| --- | --- | --- | --- |
| **TITLE** | | |  |
| Title | 1 | Identify the report as a systematic review. | title |
| **ABSTRACT** | | |  |
| Abstract | 2 | See the PRISMA 2020 for Abstracts checklist. | 3 |
| **INTRODUCTION** | | |  |
| Rationale | 3 | Describe the rationale for the review in the context of existing knowledge. | 5-7 |
| Objectives | 4 | Provide an explicit statement of the objective(s) or question(s) the review addresses. | 7 |
| **METHODS** | | |  |
| Eligibility criteria | 5 | Specify the inclusion and exclusion criteria for the review and how studies were grouped for the syntheses. | 7-8 |
| Information sources | 6 | Specify all databases, registers, websites, organisations, reference lists and other sources searched or consulted to identify studies. Specify the date when each source was last searched or consulted. | 7 |
| Search strategy | 7 | Present the full search strategies for all databases, registers and websites, including any filters and limits used. | 8 |
| Selection process | 8 | Specify the methods used to decide whether a study met the inclusion criteria of the review, including how many reviewers screened each record and each report retrieved, whether they worked independently, and if applicable, details of automation tools used in the process. | 9 |
| Data collection process | 9 | Specify the methods used to collect data from reports, including how many reviewers collected data from each report, whether they worked independently, any processes for obtaining or confirming data from study investigators, and if applicable, details of automation tools used in the process. | 9-10 |
| Data items | 10a | List and define all outcomes for which data were sought. Specify whether all results that were compatible with each outcome domain in each study were sought (e.g. for all measures, time points, analyses), and if not, the methods used to decide which results to collect. | 9-10 |
| 10b | List and define all other variables for which data were sought (e.g. participant and intervention characteristics, funding sources). Describe any assumptions made about any missing or unclear information. | 9-10 |
| Study risk of bias assessment | 11 | Specify the methods used to assess risk of bias in the included studies, including details of the tool(s) used, how many reviewers assessed each study and whether they worked independently, and if applicable, details of automation tools used in the process. | 10 |
| Effect measures | 12 | Specify for each outcome the effect measure(s) (e.g. risk ratio, mean difference) used in the synthesis or presentation of results. | 10 |
| Synthesis methods | 13a | Describe the processes used to decide which studies were eligible for each synthesis (e.g. tabulating the study intervention characteristics and comparing against the planned groups for each synthesis (item #5)). | N/A |
| 13b | Describe any methods required to prepare the data for presentation or synthesis, such as handling of missing summary statistics, or data conversions. | N/A |
| 13c | Describe any methods used to tabulate or visually display results of individual studies and syntheses. | N/A |
| 13d | Describe any methods used to synthesize results and provide a rationale for the choice(s). If meta-analysis was performed, describe the model(s), method(s) to identify the presence and extent of statistical heterogeneity, and software package(s) used. | N/A |
| 13e | Describe any methods used to explore possible causes of heterogeneity among study results (e.g. subgroup analysis, meta-regression). | N/A |
| 13f | Describe any sensitivity analyses conducted to assess robustness of the synthesized results. | N/A |
| Reporting bias assessment | 14 | Describe any methods used to assess risk of bias due to missing results in a synthesis (arising from reporting biases). | N/A |
| Certainty assessment | 15 | Describe any methods used to assess certainty (or confidence) in the body of evidence for an outcome. | N/A |
| **RESULTS** | | |  |
| Study selection | 16a | Describe the results of the search and selection process, from the number of records identified in the search to the number of studies included in the review, ideally using a flow diagram. | 10-11 |
| 16b | Cite studies that might appear to meet the inclusion criteria, but which were excluded, and explain why they were excluded. | N/A |
| Study characteristics | 17 | Cite each included study and present its characteristics. | N/A |
| Risk of bias in studies | 18 | Present assessments of risk of bias for each included study. | N/A |
| Results of individual studies | 19 | For all outcomes, present, for each study: (a) summary statistics for each group (where appropriate) and (b) an effect estimate and its precision (e.g. confidence/credible interval), ideally using structured tables or plots. | N/A |
| Results of syntheses | 20a | For each synthesis, briefly summarise the characteristics and risk of bias among contributing studies. | N/A |
| 20b | Present results of all statistical syntheses conducted. If meta-analysis was done, present for each the summary estimate and its precision (e.g. confidence/credible interval) and measures of statistical heterogeneity. If comparing groups, describe the direction of the effect. | N/A |
| 20c | Present results of all investigations of possible causes of heterogeneity among study results. | N/A |
| 20d | Present results of all sensitivity analyses conducted to assess the robustness of the synthesized results. | N/A |
| Reporting biases | 21 | Present assessments of risk of bias due to missing results (arising from reporting biases) for each synthesis assessed. | N/A |
| Certainty of evidence | 22 | Present assessments of certainty (or confidence) in the body of evidence for each outcome assessed. | N/A |
| **DISCUSSION** | | |  |
| Discussion | 23a | Provide a general interpretation of the results in the context of other evidence. | 11-13 |
| 23b | Discuss any limitations of the evidence included in the review. | 13 |
| 23c | Discuss any limitations of the review processes used. | 13 |
| 23d | Discuss implications of the results for practice, policy, and future research. | 13 |
| **OTHER INFORMATION** | | |  |
| Registration and protocol | 24a | Provide registration information for the review, including register name and registration number, or state that the review was not registered. | 7 |
| 24b | Indicate where the review protocol can be accessed, or state that a protocol was not prepared. | 7 |
| 24c | Describe and explain any amendments to information provided at registration or in the protocol. | N/A |
| Support | 25 | Describe sources of financial or non-financial support for the review, and the role of the funders or sponsors in the review. | 2 |
| Competing interests | 26 | Declare any competing interests of review authors. | 2 |
| Availability of data, code and other materials | 27 | Report which of the following are publicly available and where they can be found: template data collection forms; data extracted from included studies; data used for all analyses; analytic code; any other materials used in the review. | N/A |

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