PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Mandated Reporters' Experiences with Reporting Child Maltreatment: A Meta-Synthesis of Qualitative Studies
AUTHORS	McTavish, Jill; Kimber, Melissa; Devries, Karen; Colombini, Manuela; MacGregor, Jennifer; Wathen, Nadine; Agarwal, Arnav; MacMillan, Harriet

VERSION 1 - REVIEW

REVIEWER	Cathy Humphreys University of Melbourne Australia
REVIEW RETURNED	17-Sep-2016

GENERAL COMMENTS	I found this to be an excellent review of the literature in an important area of policy and practice. The methodology was complex and systematic. I initially found the reporting of results under the First order, second order and third order constructs 'unfriendly' to the reader, particularly when the results are themed and qualitative. However, the logic of this structure for reporting and the way in which it allowed for the contradictions and the concerning features of mandatory reporting to be discussed soon overcame my initial skepticism. My only criticism is that the discussion may not have clearly enough bought out the differences in different forms of maltreatment and the issues or rationale for mandatory reporting in the third-order construct and discussion. A mention is made of severe physical abuse and neglect that might be considered for MR. My understanding of the field is that child sexual abuse had some of the strongest arguments for mandatory reporting given the extensive secrecy and reluctance to report which has been demonstrated in inquiries and reviews. However domestic violence appears to be an unmitigated disaster area for mandatory reporting - simply overwhelming the child protection system. I make these last remarks by way of comment and my engagement with the field and with the paper. Unless these issues emerged clearly in the review of articles but were not reported, I leave it as a comment for the authors rather than an issue to be addressed. I look forward to this important paper being published.

REVIEWER	Donald Bross Kempe Center Pediatrics Department University of Colorado School of Medicine U.S. I am co-editor with Ben Mathews of "Mandatory Reporting of Severe Child Abuse and Neglect." (Springer, 2015). There are 25 chapters both favorable and critical of mandatory reporting, but we as editors believe that there a number of empirical and philosophical supports for mandatory reporting, including issues addressed in the edited volume
REVIEW RETURNED	04-Nov-2016

GENERAL COMMENTS	I'm writing Harriet because I've met her, respect her, and hope she will appreciate that my comments are not intended to disparage individuals but invite any response that anyone might with to make about my concerns. I recognize that earlier comments might already have been acted upon when I finally got to the point of completing the review. I chose this way of communicating because I'm not interested in a "twitter" discussion as much as communication between trusted colleagues about what sometimes seems to be a 'confirmation bias' the reporting of unsafe conditions must be more bad than good. At the beginning of your submission it is stated that: "(W)hile we began this project with the intent of doing a systematic review of studies of effectiveness about mandatory reporting, we were unable to find any studies that could be used for this purpose (i.e. no prospective controlled trials, cohort studies, or case-control studies) assessing the effectiveness of mandatory reporting in relation to child outcomes were retrieved from our systematic search)." If that is the test of the effectiveness of MR shouldn't the conclusion read more like: "The current research raises questions, especially about the experience of mandatory reporters, but didn't address the core question of the "effectiveness of MR"? I will be interested in your forthcoming review of 'qualitative research' on the experiences of children and parents found in the literature about child maltreatment. Will any of the research reviewed acknowledge the writing on adverse childhood experiences (ACE), Nurse Family Partnership, Safe Care, LONGSCAN, prospective studies of delinquency (Cathy Widom), or any of the innumerable controlled studies on treatment and mentoring effectiveness of maltreated children and their parents conducted just in the U.S. alone? This MR system made possible funding of these studies and when incident to the research, allowed for evaluations of reductions in reported child abuse based on the preventive intervention. The conclusion reached in this study is that: based on the impressions of reporters of child abuse, "child maltreatment has been variously implemented across jurisdictions and high-quality research on the effectiveness of this process is severely lacking. Along with focusing on approaches to improve mandatory reporting, the field needs to address whether or not mandatory reporting actually improves children's health outcomes through research that is sensitive to both severe and less overt forms of maltreatment." The study thus 'bootstraps' a study of impressions of individuals who might or might not be in favor of reporting, might or might not be in a

position to know actual impacts of reporting on children and parents, might or might not be representative of jurisdictions with more decades in the attempted development of a 'modern child protection system' than others, before reinforcing negative implications of MR as a whole. The position that "the field needs to address whether or not mandatory reporting actually improves children's health outcomes through research that is sensitive to both severe and less overt forms of maltreatment" could be made without the literature review of reporters. Is this study of reporters' experiences and attitudes a sufficient foundation for conclusions as to whether MR is worse for abused/neglected children and their families (or the reverse as was stated in the article)? Is it a conclusion in any way based on "studies of "high quality" suggesting that MR is either more harmful or more beneficial than no MR"?

I am interested that if the definition of "high quality" studies is "prospective controlled trials, cohort studies, or case-control studies" that somehow the work of Olds (who used MR data in Elmira and Denver to show how child abuse was prevented), Cathy Widom (who had a very large comparison population of children identified vs. not identified by MR) to follow into their 30's in terms of delinquency and crime risk), or the mentoring studies of Heather Taussig and her colleagues (who worked with children in foster care, many or most of them in care due to MR) are not relevant. Perhaps these are viewed as unrelated to MR. I will leave aside questions of whether the epidemiological writing of Runyan and others, sociological evaluations by Finkelhor and continuing updates on research related to ACE's in the CDC website as arguably being unrelated to MR and just as likely to have occurred without MR in the U.S. What I cannot help but point out, is that more research might be done on the attitudes of reporters in the four or five decades of MR of child abuse than more than a century of reports about communicable diseases. Fortunately, notwithstanding the high visibility of concerns when an STI such as HIV comes along, most don't question the value of MR of communicable diseases. If there are those that truly believe that MR is so destructive compared to allowing the issue to be addressed without MR, then it is possible to outline what is meant by "prospective controlled trials, cohort studies, or case-control studies" comparing MR and no MR. As a 25 year member of an IRB in the U.S. I will be interested to learn of proposals to carry out this type of high quality research on MR and the scientific and ethical discussions related to such submissions.

In any event, thanks for your time.

REVIEWER	Brad Donohue University of Nevada, Las Vegas; USA
REVIEW RETURNED	07-Dec-2016

| GENERAL COMMENTS | I enjoyed reading this study. The workload is, indeed, impressive. I only have a few minor suggestions.

1. Comment that in the USA child neglect far exceeds 16% in the Introduction, and is the leading maltreatment indicated in the States.
2. Specify how child abuse was broadly defined when discussing the search strategy on pg. 4.
3. Please comment more on the deplorable lack of outcome studies conducted to assist child maltreatment reporting. Along these lines, |

it might be important to put even more emphasis on strategies that have been found to be successful in reporting child maltreatment in outcome studies. In this regard, there are two additional studies that may be helpful:

Kenny, M. C., Lopez, A., & Donohue, B. (2016). Development and initial evaluation of a Cost-effective,
internet program to assist professionals in reporting child maltreatment. Journal of Child & Adolescent
Trauma

Donohue, B., Carpin, K., Alvarez, K., Ellwood, A., & Jones, R.W. (2002). A Standardized method of diplomatically and effectively reporting child abuse to State authorities: A controlled evaluation. Behavior Modification, 26, 684-699.

REVIEWER	William Turner PhD School for Policy Studies University of Bristol Bristol UK
REVIEW RETURNED	23-Jan-2017

GENERAL COMMENTS	Over the years I have reviewed a number of papers for publication but only in very few instances I have come across a manuscript (esp. one referring to the synthesis of qualitative research) that was of very good standards. Your paper falls in the latter category and I am especially thankful for giving the opportunity to review your work which ideally i would like to see it published in the BMJ. The research question (e.g. MRs experiences with reporting child maltreatment) is clearly stated and the methods used are especially appropriate for the focus of the study. Findings of the qualitative meta-synthesis are clearly described and discussed. Implications for policy and research follow logically and are offered in a balanced way. Overall, I would like to congratulate you on the thoroughness of your work and the high quality of reporting of your findings. I am sure the paper will make a significant contribution in the filed.

REVIEWER	Helen Buckley Trinity College, Dublin Ireland
REVIEW RETURNED	31-Jan-2017

GENERAL COMMENTS	This is a very interesting piece of work, its limitations are well explained. I am not an expert in systematic reviews but the methodology is explained sufficiently to convince me that the review was conducted within expected standards. Inclusion and exclusion criteria were clear, and the flow diagram was helpful in illustrating the process. While the first order constructs were all reported clearly, it would have been useful to know, in relation to the second order constructs, the proportion of staff that had undergone training as well as the nature of any training and whether there was a difference in responses in relation to that. This may not be possible to include but if it was available, would enhance the paper. The paper demonstrated the more nuanced aspects of reporting - in particular the conflict between ethics and legal requirements. This paper will

	be useful in terms of demonstrating empirically the difficulties associated with mandatory reporting, and the need for some debate on its utility. It will also be effective in illustrating the gaps in research about outcomes and effectiveness. Finally, the third order constructs discussed here will be useful for policy makers who want to introduce or improve reporting processes. I would recommend publication and I hope that it elicits a strong reaction.

REVIEWER	Andrew Shepherd University of Manchester, UK
REVIEW RETURNED	24-Feb-2017

| **GENERAL COMMENTS** | Many thanks for the opportunity to review this interesting manuscript relating to the experience of 'Mandated Reporters' in the process of disclosing incidents of suspected child abuse to child protection services across an international setting. As a whole, the paper is clearly presented and logically argued, beginning from a premise of addressing a topic of clear importance to a wide audience. The counter-intuitive finding of negative experiences of reporting and reports of negative outcomes for children in relation to such reporting is also of clear significance and raises, at the authors comment, the need for further research in this field.

The authors structure their report according to the criteria outlined in the ENTREQ statement (Tong, A., Flemming, K., McInnes, E., Oliver, S., & Craig, J. (2012). Enhancing transparency in reporting the synthesis of qualitative research: ENTREQ. BMC Medical Research Methodology, 12(1), 181. http://doi.org/10.1186/1471-2288-12-181) and this is also used to structure my review.

1. Aim: - The aim of the review is clearly stated within the abstract of the paper, but is less clearly represented within the manuscript proper. I am accustomed to treating the abstract and main article as separate items and would therefore have liked to see a clearer statement of the aims of the synthesis within the main manuscript, but accept that this is a stylistic comment.
2. Synthesis Methodology: - The authors adopt a 'meta-synthesis' approach and cite a precedent study (Feder et al) on which their methodology is based. I felt that a greater explanation as to the rationale for adopting such an approach, as opposed to other methodologies, was required, together with a description of the underpinning philosophy that guided the authors to the manner in which they conducted their search strategy (systematic and comprehensive versus theoretical) and synthesis (inductive construction of themes to approach the 'third order construct', or guidance statement).
3. Approach to Searching: - A 'comprehensive' search strategy was adopted by the authors, again I would have liked to see a brief argument with regard to the rationale for selecting this strategy versus, for example, a theoretically informed iterative strategy.
4. Inclusion Criteria: - The study selection criteria adopted are clearly demonstrated.
5. Data Sources: - The authors clearly lay out the databases reviewed within their search strategy; they also cite a 'forward and backward citation chaining' approach, although it is unclear whether this approach correlates directly with the 309 'additional records' identified in the PRISMA Flow Diagram representation of the search strategy (p7) |
|---|---|

6. Electronic Search Strategy: - Search terms are clearly stated in the report.

7. Study Screening Methods: - Two of the authors reviewed the Titles and Abstracts of identified studies, leading to their being put forward for 'full text review.' I could not tell precisely how the full-text review stage of the process was conducted and whether any discrepancies arose / were resolved at this stage.

8. Study Characteristics: - The participant and geographical characteristics of the identified studies are clearly laid out and described.

9. Study Selection Results: - The presented PRISMA diagram clearly represents this process; although, as described above, I was not entirely clear as to where the 'additional records' were identified from.

10. Rationale for Appraisal: - The authors adopt an appraisal process wherein an implicit 'value' is assigned to studies 'scoring' higher on their modified appraisal tool. This rationale for appraisal could perhaps have been more clearly stated as it is not always deemed appropriate in different qualitative approaches. Additionally, the representation of studies according to their appraisal score required, I felt, some further discussion. The authors place greater emphasis on studies that score higher according to their developed tool, but this could be seen as making an implicit assumption that all items within the appraisal tool are equally weighted whereas, arguably, some items (for example the appropriateness of a qualitative approach, or the ethical approval process for a study) could perhaps be seen as carrying greater weight than others. I felt therefore that this appraisal process required more exploration overall.

11. Appraisal Items: - The authors present their appraisal tool within the supplementary documents accompanying the manuscript.

12. Appraisal Process: - A consensus approach was adopted to the appraisal of identified studies with a final 'score' elaborated for each study.

13. Appraisal results: - The 'quality' of identified studies is shown through an included table, although (as indicated above) I would have some reservations about such a representation without additional comment.

14. Data extraction: - Two of the authors extracted the 'primary data from each study…' It is not clear what was included in this extraction however, for example was the whole text included, or just the results? Or results and discussion?

15. Software: - A simple spreadsheet was used in order to contain the extracted data. It is unclear whether this was also used to support the coding process undertaken.

16. Reviewers: - Two authors were identified as primarily undertaking the review and extraction of data from identified studies, with a third author arbitrating in disagreements in the data extraction process.

17. Coding: - The authors clearly describe the process whereby 'first and second order constructs' are identified from the identified substrate studies. Second order themes (those identified by original study analysis) that addressed concerns raised within first-order themes (raised by original study participants) were seemingly prioritised if they provided 'strategies' to address identified concerns. These were then 'reworded as recommendations' to generate the final 'third-order constructs.' A verification of the 'quality' of identified recommendations is undertaken by considering whether they are represented within those studies scoring in the 'top quartile' of the appraisal tool. It is not clear what roles were adopted by the varying

authors during this analysis process and how discussion / disagreement in the development of third-order themes was handled? Presumably all the authors were involved in this process in some way but I felt that the manner in which discussion and deliberation were conducted could have been more clearly set out.

18. Study Comparison: - The authors refer to identifying constructs occurring across studies, and in their Results section provide a numeric representation of the manner in which different themes were represented. Actual comparison between studies seems limited however, which is unfortunate as the differing settings (for example study participants, geographical location) could have provided an interesting point for comparison.

19. Derivation of themes: - The authors adopt an inductive approach discussed above under 'coding'.

20. Quotations: - The authors present 'illustrative quotations' for their various themes in the Results section.

21. Synthesis Output:- The primary output from the synthesis process are the 'third-order constructs' which represent a series of recommendations, grounded in the identified studies, with regard to the conduct of the referral process when concerns regarding potential abuse are made. This final descriptive output is of potential value in guiding future research in this area.

Overall, I fell this paper represents a potentially highly valuable contribution to this important area of research. My primary concerns, as outlined above, relate to the manner in which the methodology was selected, adopted and is represented in the Methodology section of the manuscript. I felt there was also a need for greater transparency with regard to the particular role adopted by Authors in relation to the analysis process, this is summarised in the 'Contributors' section at the end of the main manuscript but could, I believe, have been more accurately outlined in the methods section proper.

One further concern that I had, not reflected in the comments above, related to the use of numeric and percentile representations of the number of studies 'endorsing' each theme. It could be argued that this representation is appropriate given the comprehensive search strategy adopted. However, I have concerns about the manner in which such a representation reduces individual studies to an equal measure, with an implicit assumption of 'importance' for those themes ranked highest in terms of the number of studies endorsing the construct. For example, the second-order theme 'Cultural Competence' is represented in only 4 of the included studies but the illustrative example given is drawn from a study based in South Africa where this may adopt a particular resonance not represented in the relatively small number of papers addressing this topic. The authors do address this to an extent by commenting on the varying number of papers drawn from different geographical and cultural contexts.

This concern could perhaps be captured within a broader concern I would voice relating to the lack of transparency with regard to the manner in which comparison is made between different studies, taking account of the particular features / strengths of each identified study. In this way the views of varying participants (e.g clinicians versus police officers) and the impact of cultural context are lost to a degree through the semi-quantitative approach to appraisal and data representation. Again, this could be addressed through a more explicit description of the ontological / epistemological

	considerations taken by the Authors with regard to their methodology. This criticism is perhaps unjustified however since this paper represents a potentially valuable contribution in terms of its broad summary of a complex subject and its ability to highlight pertinent issues, while guiding potential future research.

VERSION 1 – AUTHOR RESPONSE

Cathy Humphreys, Reviewer 1

I found this to be an excellent review of the literature in an important area of policy and practice. The methodology was complex and systematic. I initially found the reporting of results under the First order, second order and third order constructs 'unfriendly' to the reader, particularly when the results are themed and qualitative. However, the logic of this structure for reporting and the way in which it allowed for the contradictions and the concerning features of mandatory reporting to be discussed soon overcame my initial skepticism.

We appreciate Dr. Humphreys' positive feedback about the manuscript. It is encouraging to know that the reasons for the structure of the manuscript became apparent and concerns about the 'unfriendly' aspects of the reporting diminished.

My only criticism is that the discussion may not have clearly enough bought out the differences in different forms of maltreatment and the issues or rationale for mandatory reporting in the third-order construct and discussion. A mention is made of severe physical abuse and neglect that might be considered for MR. My understanding of the field is that child sexual abuse had some of the strongest arguments for mandatory reporting given the extensive secrecy and reluctance to report which has been demonstrated in inquiries and reviews. However domestic violence appears to be an unmitigated disaster area for mandatory reporting - simply overwhelming the child protection system. I make these last remarks by way of comment and my engagement with the field and with the paper. Unless these issues emerged clearly in the review of articles but were not reported, I leave it as a comment for the authors rather than an issue to be addressed. I look forward to this important paper being published.

In our Discussion we suggest that mandatory reporting may be most appropriate "for cases of severe abuse and neglect" and we would consider many cases of sexual abuse to be severe abuse, for the reasons you have stated. We feel it is important to note, however, that in some cases of child sexual abuse of a less "severe" nature – for example, a single case of touching by an older child – children may not necessarily show signs of negative outcomes. Conversely, there are many cases of emotional abuse where children show signs of significant, deleterious outcomes. It is important from an assessment/intervention perspective (i.e., separate from considerations of mandatory reporting) not to assume that a certain exposure will correspond with a certain outcome. In terms of this meta-synthesis, there were only five papers where mandated reporters (MRs) discussed decisions to report in relation to sexual abuse and four of these papers were specifically about children with disabilities. MRs in these papers reported reluctance to report sexual abuse because they were "afraid of making it up"(1), they felt it would be "thrown out of the court" (in cases where the child had a disability) (2), or they had previous cases they reported that they felt did not benefit the child (3), but otherwise when sexual abuse was discussed the MRs seemed clear they needed to report it (1, 2, 4). Unfortunately, in most of the articles, information was not provided about what specific type of maltreatment the MRs were hesitant to report, only that they were especially uncertain when they lacked physical evidence of abuse (as noted in the manuscript). To clarify this issue in the manuscript, we added the following to the first-order constructs section: "MRs were reluctant to report their suspicions of abuse and

preferred to report only when they found physical evidence of abuse, such as physical injuries, bruises, broken bones, caries (and corresponding lack of treatment), or "total" changes in behaviour. Unfortunately, most MRs did not clarify their reporting decisions in relation to specific forms of maltreatment. For example, only five articles (1-5) discussed decisions to report (including hesitance to report) in relation to sexual abuse and four of these articles discussed maltreatment of children with disabilities (suggesting particular challenges they faced in reporting maltreatment of children with disabilities).

Donald Bross, Reviewer 2

I'm writing Harriet because I've met her, respect her, and hope she will appreciate that my comments are not intended to disparage individuals but invite any response that anyone might with to make about my concerns. I recognize that earlier comments might already have been acted upon when I finally got to the point of completing the review. I chose this way of communicating because I'm not interested in a "twitter" discussion as much as communication between trusted colleagues about what sometimes seems to be a 'confirmation bias' the reporting of unsafe conditions must be more bad than good.

All of us, including Harriet are grateful to Dr. Bross for his detailed and thought-provoking review in which he raises important questions for us and for the field. We have responded to each of the comments and will certainly consider these issues moving forward with the syntheses underway to examine the experiences of children and caregivers with mandatory reporting.

With respect to the point about 'confirmation bias', it should be noted that the main author (JRM) – expected that the review would address the harms of not reporting. It was only upon completing the analysis that it became clear the evidence was pointing to a different story altogether. Given the negative experiences identified by this meta-synthesis with respect to mandatory reporting, we made a concerted effort to make our methods clear so that readers could understand what led to these findings.

At the beginning of your submission it is stated that: "(W)hile we began this project with the intent of doing a systematic review of studies of effectiveness about mandatory reporting, we were unable to find any studies that could be used for this purpose (i.e. no prospective controlled trials, cohort studies, or case-control studies) assessing the effectiveness of mandatory reporting in relation to child outcomes were retrieved from our systematic search)." If that is the test of the effectiveness of MR shouldn't the conclusion read more like: "The current research raises questions, especially about the experience of mandatory reporters, but didn't address the core question of the "effectiveness of MR"?

We agree with Dr. Bross that the evidence does raise important questions about effectiveness of mandatory reporting and appreciate his emphasis of this point. We have therefore added the following statement for clarity in the Discussion: While our search retrieved no evidence about the effectiveness of mandatory reporting, and qualitative research cannot be mistaken for evaluation of effectiveness, findings from this review raise important questions about the effects of mandatory reporting by drawing on studies reporting the experiences of MRs across nine high-income and three middle-income countries.

Also added to the Conclusion: While our search retrieved no evidence about the effectiveness of mandatory reporting, through this meta-synthesis of MRs' experiences with reporting we have summarized many accounts of harm associated with reporting.

I will be interested in your forthcoming review of 'qualitative research' on the experiences of children and parents found in the literature about child maltreatment. Will any of the research reviewed

acknowledge the writing on adverse childhood experiences (ACE), Nurse Family Partnership, Safe Care, LONGSCAN, prospective studies of delinquency (Cathy Widom), or any of the innumerable controlled studies on treatment and mentoring effectiveness of maltreated children and their parents conducted just in the U.S. alone? This MR system made possible funding of these studies and when incident to the research, allowed for evaluations of reductions in reported child abuse based on the preventive intervention.

The issue raised by Dr. Bross of the mandatory reporting system's role historically in measurement of maltreatment as either an exposure or outcome variable in cross-sectional, longitudinal and intervention studies is an interesting one that we will certainly consider before completing the forthcoming review. A full discussion of this is likely beyond the scope of the review, however, it raises an important question of how mandatory reporting may support and or hinder research about outcomes associated with exposure to child maltreatment as well as effectiveness of interventions across the spectrum from prevention to reduction of recidivism and associated impairment.

The conclusion reached in this study is that: based on the impressions of reporters of child abuse, "child maltreatment has been variously implemented across jurisdictions and high-quality research on the effectiveness of this process is severely lacking. Along with focusing on approaches to improve mandatory reporting, the field needs to address whether or not mandatory reporting actually improves children's health outcomes through research that is sensitive to both severe and less overt forms of maltreatment." The study thus 'bootstraps' a study of impressions of individuals who might or might not be in favor of reporting, might or might not be in a position to know actual impacts of reporting on children and parents, might or might not be representative of jurisdictions with more decades in the attempted development of a 'modern child protection system' than others, before reinforcing negative implications of MR as a whole. The position that "the field needs to address whether or not mandatory reporting actually improves children's health outcomes through research that is sensitive to both severe and less overt forms of maltreatment" could be made without the literature review of reporters. Is this study of reporters' experiences and attitudes a sufficient foundation for conclusions as to whether MR is worse for abused/neglected children and their families (or the reverse as was stated in the article)? Is it a conclusion in any way based on "studies of "high quality" suggesting that MR is either more harmful or more beneficial than no MR"?

I am interested that if the definition of "high quality" studies is "prospective controlled trials, cohort studies, or case-control studies" that somehow the work of Olds (who used MR data in Elmira and Denver to show how child abuse was prevented), Cathy Widom (who had a very large comparison population of children identified vs. not identified by MR) to follow into their 30's in terms of delinquency and crime risk), or the mentoring studies of Heather Taussig and her colleagues (who worked with children in foster care, many or most of them in care due to MR) are not relevant. Perhaps these are viewed as unrelated to MR. I will leave aside questions of whether the epidemiological writing of Runyan and others, sociological evaluations by Finkelhor and continuing updates on research related to ACE's in the CDC website as arguably being unrelated to MR and just as likely to have occurred without MR in the U.S. What I cannot help but point out, is that more research might be done on the attitudes of reporters in the four or five decades of MR of child abuse than more than a century of reports about communicable diseases. Fortunately, notwithstanding the high visibility of concerns when an STI such as HIV comes along, most don't question the value of MR of communicable diseases. If there are those that truly believe that MR is so destructive compared to allowing the issue to be addressed without MR, then it is possible to outline what is meant by "prospective controlled trials, cohort studies, or case-control studies" comparing MR and no MR. As a 25 year member of an IRB in the U.S. I will be interested to learn of proposals to carry out this type of high quality research on MR and the scientific and ethical discussions related to such submissions.
In any event, thanks for your time.
With all best wishes,

Don

We agree with Dr. Bross that the position that "the field needs to address whether or not mandatory reporting actually improves children's health outcomes through research that is sensitive to both severe and less overt forms of maltreatment" could be made without the literature review of reporters. In fact this point has been made by others, but still the question remains unanswered. This synthesis has been carried out with the goal of moving the field forward by synthesizing what is available in the literature, despite its limitations. Our hope is that this research encourages clinicians, policy makers, researchers and other stakeholders to think about what the existing literature says with regard to experiences of MRs.

We have emphasized in our review that none of the studies addressed the research question of effectiveness. However, the systematic approach to reviewing the qualitative literature does raise important questions that need to be considered and addressed. One of our main points is about the importance of conducting research to determine the effectiveness of mandatory reporting. While we appreciate the important and high-quality research conducted by Drs. Olds, Widom, and Taussig, research by those authors did not set out to assess the effectiveness of mandatory reporting. Although it would not be ethical or feasible to conduct a controlled trial of mandatory reporting versus no mandatory reporting in areas where such reporting currently exists, it would be possible to address these questions in areas that currently do not have mandatory reporting. (For example, Ireland recently passed mandatory reporting legislation and is currently in the process of implementing it.) A clustered randomized controlled trial design could be used with jurisdictions randomly assigned to introduce mandatory reporting versus those without mandatory reporting (all other community and child protection system responses to a report would be the same across groups) to assess how the introduction of reporting requirements impacts child outcomes and recurrence of maltreatment. Since there are regions around the world where mandatory reporting does not exist, there is still the opportunity to determine through use of an experimental design whether mandatory reporting does more good than harm. We think that the types of issues our meta-synthesis have raised are the very questions that should be discussed and studied.

Added to the conclusion: Our findings in no way imply that the recognition and response to children exposed to maltreatment is not a significant public health concern that requires coordinated responses. Rather, it implies that we must work to ensure that all of our methods for recognizing and responding to children exposed to maltreatment demonstrate that they benefit children's safety and well-being and do no additional harm.

Brad Donohue, Reviewer 3

I enjoyed reading this study. The workload is, indeed, impressive. I only have a few minor suggestions.

We appreciate the positive comments from Dr. Donohue about our meta-synthesis.

1. Comment that in the USA child neglect far exceeds 16% in the Introduction, and is the leading maltreatment indicated in the States.

In response to this helpful suggestion, we have added the following statement to the Introduction: These estimates vary across countries. For example, according to 2015 United States (U.S.) child protective services (CPS) reports, 63.4% of reported children experienced neglect.(6)

2. Specify how child abuse was broadly defined when discussing the search strategy on pg. 4.

In response to this suggested revision, we have added the following phrase to the Methods section: child abuse (broadly defined, including, but not limited to terms for child welfare, physical abuse, emotional abuse, neglect, sexual abuse/exploitation, and children's exposure to IPV)

3. Please comment more on the deplorable lack of outcome studies conducted to assist child maltreatment reporting. Along these lines, it might be important to put even more emphasis on strategies that have been found to be successful in reporting child maltreatment in outcome studies. In this regard, there are two additional studies that may be helpful:
Kenny, M. C., Lopez, A., & Donohue, B. (2016). Development and initial evaluation of a Cost-effective, internet program to assist professionals in reporting child maltreatment. Journal of Child & Adolescent Trauma
Donohue, B., Carpin, K., Alvarez, K., Ellwood, A., & Jones, R.W. (2002). A Standardized method of diplomatically and effectively reporting child abuse to State authorities: A controlled evaluation. Behavior Modification, 26, 684-699.

In response to this suggested revision, we have added the following to the Discussion section: Emerging work is examining the methods by which health and social service providers can be trained to recognize and respond to child maltreatment disclosures and suspicions of child maltreatment (for example, see (7-9)). Given that the evaluation of these training programs falls outside the scope of this review, and that mandatory reporting is but one of many components of appropriate recognition of and response to children exposed to maltreatment, further work and evaluation is needed to understand the extent to which existing training programs are capable of improving MRs' recognition and response to children exposed to maltreatment or if further specialized training is needed.

William Turner, Reviewer 4

Dear Authors,
Over the years I have reviewed a number of papers for publication but only in very few instances I have come across a manuscript (esp. one referring to the synthesis of qualitative research) that was of very good standards. Your paper falls in the latter category and I am especially thankful for giving the opportunity to review your work which ideally i would like to see it published in the BMJ.
The research question (e.g. MRs experiences with reporting child maltreatment) is clearly stated and the methods used are especially appropriate for the focus of the study. Findings of the qualitative meta-synthesis are clearly described and discussed. Implications for policy and research follow logically and are offered in a balanced way. Overall, I would like to congratulate you on the thoroughness of your work and the high quality of reporting of your findings. I am sure the paper will make a significant contribution in the filed. All the best, William Turner PhD

We are very grateful to Dr. Turner for outlining the specific aspects of our manuscript that lead him to conclude that the paper will make a significant contribution to the field. We are particularly pleased by the feedback that the implications for policy and research are "offered in a balanced way." Thank you.

Helen Buckley, Reviewer 5

This is a very interesting piece of work, its limitations are well explained. I am not an expert in systematic reviews but the methodology is explained sufficiently to convince me that the review was conducted within expected standards. Inclusion and exclusion criteria were clear, and the flow diagram was helpful in illustrating the process.

We would like to thank Dr. Buckley for outlining her response to the manuscript and indicating that this is from the perspective of someone without specific expertise with systematic reviews. This is very helpful to know, given that we have intended the manuscript for a general audience.

While the first order constructs were all reported clearly, it would have been useful to know, in relation to the second order constructs, the proportion of staff that had undergone training as well as the nature of any training and whether there was a difference in responses in relation to that. This may not be possible to include but if it was available, would enhance the paper.

The training received by study participants is an empirically interesting question. However, after re-reviewing all of the included papers, we see that a discussion about participants' level of training was only mentioned in six papers. Further, aside from the Itzhaky 2014 paper which sought to examine "the impact of this training [on identification and treatment of children at risk] on interdisciplinary work with children at risk", all other references to training are vague (e.g., "Most of the teachers had minimal pre-service or on the job training about child abuse issues"). For this reason, it is unclear which participant quotes (and hence which themes) were given by those MRs with or without training. However, given that "training" is a significant first- and second-order theme, we can postulate (but not confirm) that many of the MRs in this study had not experienced training in relation to recognizing or responding to child maltreatment. We have added the following statement to the results for clarity: Only six articles (10-15) discussed any training that MRs received about recognizing and responding to child maltreatment; aside from one study (13) that was examining the impact of child maltreatment training, it is hard to determine if or how training (or lack of training) influenced MRs' responses.

The paper demonstrated the more nuanced aspects of reporting - in particular the conflict between ethics and legal requirements. This paper will be useful in terms of demonstrating empirically the difficulties associated with mandatory reporting, and the need for some debate on its utility. It will also be effective in illustrating the gaps in research about outcomes and effectiveness. Finally, the third order constructs discussed here will be useful for policy makers who want to introduce or improve reporting processes. I would recommend publication and I hope that it elicits a strong reaction.

We appreciate Dr. Buckley's detailed comments about how the manuscript will be useful. We hope that it will lead to debate and most importantly, additional research on the issue of mandatory reporting.

Andrew Shepherd, Reviewer 6

Many thanks for the opportunity to review this interesting manuscript relating to the experience of 'Mandated Reporters' in the process of disclosing incidents of suspected child abuse to child protection services across an international setting. As a whole, the paper is clearly presented and logically argued, beginning from a premise of addressing a topic of clear importance to a wide audience. The counter-intuitive finding of negative experiences of reporting and reports of negative outcomes for children in relation to such reporting is also of clear significance and raises, at the authors comment, the need for further research in this field.

We appreciate Dr. Shepherd's encouraging feedback about the manuscript. In responding to the feedback below, we have provided responses where needed.

The authors structure their report according to the criteria outlined in the ENTREQ statement (Tong, A., Flemming, K., McInnes, E., Oliver, S., & Craig, J. (2012). Enhancing transparency in reporting the synthesis of qualitative research: ENTREQ. BMC Medical Research Methodology, 12(1), 181. http://doi.org/10.1186/1471-2288-12-181) and this is also used to structure my review.

1. Aim: - The aim of the review is clearly stated within the abstract of the paper, but is less clearly represented within the manuscript proper. I am accustomed to treating the abstract and main article as separate items and would therefore have liked to see a clearer statement of the aims of the

synthesis within the main manuscript, but accept that this is a stylistic comment.

We are thankful to Dr. Shepherd for pointing out the need to improve how the aim is stated in the main text of the manuscript. We have modified a section of our paper to make the aim more clear: "Furthermore, while we began this project with the intent of doing a systematic review of studies of effectiveness about mandatory reporting, we were unable to find any studies that could be used for this…Given the paucity of data on effectiveness of mandatory reporting, the purpose of this meta-synthesis is to summarize qualitative research about MRs' experiences with reporting.

2. Synthesis Methodology: - The authors adopt a 'meta-synthesis' approach and cite a precedent study (Feder et al) on which their methodology is based. I felt that a greater explanation as to the rationale for adopting such an approach, as opposed to other methodologies, was required, together with a description of the underpinning philosophy that guided the authors to the manner in which they conducted their search strategy (systematic and comprehensive versus theoretical) and synthesis (inductive construction of themes to approach the 'third order construct', or guidance statement).

We have added the following to be more explicit about our methodology: Various methods for synthesizing qualitative literature exist depending on the purpose of the review (16) or the philosophical (17) or epistemological (18) stance of the researcher. As there is no standard way to summarize qualitative literature, for this meta-synthesis we follow the methods of Feder and colleagues (19), whose work builds on Noblit and Hare's (43) approach to meta-ethnography. Meta-ethnography does not offer suggestions for sampling or appraising articles and at times can be criticized for lack of transparency.(16) A benefit of Feder and colleagues' (19) method is that they conducted a systematic search of qualitative studies with clear inclusion and exclusion criteria, thus enhancing the transparency of their study selection process. While the benefit of appraising qualitative research is still debated (20), Feder and colleagues' approach to appraising qualitative literature prioritizes studies that are ranked as of higher quality, which supports increasing recommendations to consider study quality, but also does not inappropriately exclude so-called lower quality studies that make 'surface mistakes' that would not otherwise invalidate their study findings.(16) Finally, like Noblit and Hare's (43) work, Feder et al.'s (19) approach to synthesizing qualitative literature allows for the inductive creation of a set of higher-order constructs (third-order constructs, discussed below) that reflect concepts identified in individual studies, but also extends beyond them. While the quantification of qualitative work has been criticized, in this study individual concepts are "counted" in order to let the reader decide about the relative importance of the themes. We suggest that themes that appear at a lower frequency are not necessarily less important (e.g., one account of harm to a child is significant and must be considered), but rather that this theme was less of a focus for MRs and study authors. For example, the theme of "cultural competence" is not discussed by as many MRs as compared to all of the various factors that impact their decision to report, which is partially explained by the fact that 11 (25%) of included articles set out specifically to investigate factors that impact MRs' decision to report.

3. Approach to Searching: - A 'comprehensive' search strategy was adopted by the authors, again I would have liked to see a brief argument with regard to the rationale for selecting this strategy versus, for example, a theoretically informed iterative strategy.

The explanation above in #2 also addresses the question raised with regard to our approach to searching.

4. Inclusion Criteria: - The study selection criteria adopted are clearly demonstrated.

5. Data Sources: - The authors clearly lay out the databases reviewed within their search strategy; they also cite a 'forward and backward citation chaining' approach, although it is unclear whether this

approach correlates directly with the 309 'additional records' identified in the PRISMA Flow Diagram representation of the search strategy (p7)

To clarify this approach, we have changed the PRISMA diagram to say "Records identified through citation chaining."

6. Electronic Search Strategy: - Search terms are clearly stated in the report.

7. Study Screening Methods: - Two of the authors reviewed the Titles and Abstracts of identified studies, leading to their being put forward for 'full text review.' I could not tell precisely how the full-text review stage of the process was conducted and whether any discrepancies arose / were resolved at this stage.

We have added this to the paper: "Full-text articles were screened for relevance and put forward for consideration by one author (JRM); relevance for inclusion was confirmed by a second author (MK), with discrepancies being resolved by consensus."

8. Study Characteristics: - The participant and geographical characteristics of the identified studies are clearly laid out and described.

9. Study Selection Results: - The presented PRISMA diagram clearly represents this process; although, as described above, I was not entirely clear as to where the 'additional records' were identified from.

As noted above in response to #5, we have revised the PRISMA diagram to address this issue.

10. Rationale for Appraisal: - The authors adopt an appraisal process wherein an implicit 'value' is assigned to studies 'scoring' higher on their modified appraisal tool. This rationale for appraisal could perhaps have been more clearly stated as it is not always deemed appropriate in different qualitative approaches. Additionally, the representation of studies according to their appraisal score required, I felt, some further discussion. The authors place greater emphasis on studies that score higher according to their developed tool, but this could be seen as making an implicit assumption that all items within the appraisal tool are equally weighted whereas, arguably, some items (for example the appropriateness of a qualitative approach, or the ethical approval process for a study) could perhaps be seen as carrying greater weight than others. I felt therefore that this appraisal process required more exploration overall.

Please refer to response #2 above, where we address the rationale for appraisal as part of the overall issue of synthesis methodology. In addition, because we only included articles that had full quotes from participants (and these were our primary unit of analysis for first-order constructs), we felt that excluding any article with poor study design (or just poor reporting practices), would unfairly exclude statements from MRs that could impact the findings of our synthesis. Added to the methodology: Only the total CASP scores were considered and studies were not excluded for poor study design, as a) according to our inclusion criteria we only included articles with full quotes from MRs, b) we coded all MRs' quotes as first-order constructs, and c) we felt that the exclusion of any articles could exclude a valuable quote/perspective from an MR and that this exclusion could impact the meta-synthesis findings.

11. Appraisal Items: - The authors present their appraisal tool within the supplementary documents accompanying the manuscript.

12. Appraisal Process: - A consensus approach was adopted to the appraisal of identified studies with

a final 'score' elaborated for each study.

13. Appraisal results: - The 'quality' of identified studies is shown through an included table, although (as indicated above) I would have some reservations about such a representation without additional comment.

Please refer to our response above to #10.

14. Data extraction: - Two of the authors extracted the 'primary data from each study…' It is not clear what was included in this extraction however, for example was the whole text included, or just the results? Or results and discussion?

To clarify what was included in the extraction, we have added the following information to the manuscript: For first-order constructs, only direct quotes from participants (and any clarifying text provided by the study author), found in the Results sections of included articles, were considered for analysis. For second-order constructs, only study author recommendations (often worded as "should" or "ought" statements and found in the Discussion of the article) were considered for analysis.

15. Software: - A simple spreadsheet was used in order to contain the extracted data. It is unclear whether this was also used to support the coding process undertaken.

To address the above question, the following statement was added to paper: After reviewing discrepancies across excel files one author (JRM) developed a master list of codes and after discussion with a second author (MK) (where both authors reviewed all codes and corresponding data together) this list of codes was further modified. Any discrepancies identified by the two authors were resolved by a third researcher (HLM). After this point, one author (JRM) went back through and recoded all data in the excel file according to the master list of codes and a second author reviewed all recoding (MK). Readers are able to view this final excel file, which includes all extracted data, codes (including master list of codes), and overall quality rating of included articles. (Please note: this is the file that will be available in Dryad. We tried to submit with the original manuscript, but the BMJ Open submission system does not allow submission of excel files.)

16. Reviewers: - Two authors were identified as primarily undertaking the review and extraction of data from identified studies, with a third author arbitrating in disagreements in the data extraction process.

17. Coding: - The authors clearly describe the process whereby 'first and second order constructs' are identified from the identified substrate studies. Second order themes (those identified by original study analysis) that addressed concerns raised within first-order themes (raised by original study participants) were seemingly prioritised if they provided 'strategies' to address identified concerns. These were then 'reworded as recommendations' to generate the final 'third-order constructs.' A verification of the 'quality' of identified recommendations is undertaken by considering whether they are represented within those studies scoring in the 'top quartile' of the appraisal tool. It is not clear what roles were adopted by the varying authors during this analysis process and how discussion / disagreement in the development of third-order themes was handled? Presumably all the authors were involved in this process in some way but I felt that the manner in which discussion and deliberation were conducted could have been more clearly set out.

To address the issue of coding, the following information was added to the manuscript "Specifically, one author (JRM) identified third-order constructs that addressed strategies to improve MRs' experiences with the reporting process – especially when these themes were supported by strategies offered by MRs in first-order constructs – and these themes were, per Feder et al. (19), reworded as recommendations. These third-order constructs were first discussed with the two authors (MK, HLM)

involved in developing the first- and second-order constructs, in order to ensure they reflected their understanding of the data. Following this, a table that showed a "tally" of which first- and second-order constructs were combined to generate each third-order construct (and a brief rationale for combining them) was reviewed by all study authors and a discussion followed. Minor adjustments to the third-order constructs were made after this discussion. The biggest discrepancy across all authors of this meta-synthesis was whether or not we should offer recommendations specific to mandatory reporting at all, given that a) we didn't find any effectiveness data and b) the qualitative studies suggest many negative experiences with reporting. However, the third-order constructs represent what is found in the included studies that we synthesized (i.e., included studies did not recommend against mandatory reporting) and their presentation as recommendations is faithful to the approach used by Feder et al., which we set out to follow, and the experiences of MRs, as summarized in the included articles.

18. Study Comparison: - The authors refer to identifying constructs occurring across studies, and in their Results section provide a numeric representation of the manner in which different themes were represented. Actual comparison between studies seems limited however, which is unfortunate as the differing settings (for example study participants, geographical location) could have provided an interesting point for comparison.

Readers will have the excel file of data available to them and have the ability to do additional analysis specific to their interests (e.g., geography). Given the length of this submission, we thought it important to focus on the constructs. What the authors involved in coding found striking was how similar the themes were across time, geography and even different professions, as stated in the paper.

19. Derivation of themes: - The authors adopt an inductive approach discussed above under 'coding'.
20. Quotations: - The authors present 'illustrative quotations' for their various themes in the Results section.
21. Synthesis Output:- The primary output from the synthesis process are the 'third-order constructs' which represent a series of recommendations, grounded in the identified studies, with regard to the conduct of the referral process when concerns regarding potential abuse are made. This final descriptive output is of potential value in guiding future research in this area.
Overall, I fell this paper represents a potentially highly valuable contribution to this important area of research. My primary concerns, as outlined above, relate to the manner in which the methodology was selected, adopted and is represented in the Methodology section of the manuscript. I felt there was also a need for greater transparency with regard to the particular role adopted by Authors in relation to the analysis process, this is summarised in the 'Contributors' section at the end of the main manuscript but could, I believe, have been more accurately outlined in the methods section proper. One further concern that I had, not reflected in the comments above, related to the use of numeric and percentile representations of the number of studies 'endorsing' each theme. It could be argued that this representation is appropriate given the comprehensive search strategy adopted. However, I have concerns about the manner in which such a representation reduces individual studies to an equal measure, with an implicit assumption of 'importance' for those themes ranked highest in terms of the number of studies endorsing the construct. For example, the second-order theme 'Cultural Competence' is represented in only 4 of the included studies but the illustrative example given is drawn from a study based in South Africa where this may adopt a particular resonance not represented in the relatively small number of papers addressing this topic. The authors do address this to an extent by commenting on the varying number of papers drawn from different geographical and cultural contexts.
This concern could perhaps be captured within a broader concern I would voice relating to the lack of transparency with regard to the manner in which comparison is made between different studies, taking account of the particular features / strengths of each identified study. In this way the views of varying participants (e.g clinicians versus police officers) and the impact of cultural context are lost to

a degree through the semi-quantitative approach to appraisal and data representation. Again, this could be addressed through a more explicit description of the ontological / epistemological considerations taken by the Authors with regard to their methodology.

This criticism is perhaps unjustified however since this paper represents a potentially valuable contribution in terms of its broad summary of a complex subject and its ability to highlight pertinent issues, while guiding potential future research.

Regards

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VERSION 2 – REVIEW

REVIEWER	Cathy Humphreys University of Melbourne, Australia
REVIEW RETURNED	29-Apr-2017

GENERAL COMMENTS	This is a thoughtful example of an excellent and important review study. The authors have been attentive to the detailed requirements and recommendations from reviewers. I have no comments to make on any further amendments that are required.

REVIEWER	Donald Bross Professor Emeritus, Kempe Center, Department of Pediatrics, University of Colorado
REVIEW RETURNED	06-May-2017

GENERAL COMMENTS	As the W. I. Thomas once said, "Things perceived as real are real in their consequences." I still feel that the enterprise of discovering the degree to which MR has yielded overall benefits for children, or not, cannot be adequately addressed by a survey of individuals of whom we have no way to accurately weigh, much more understand, what their impressions represent of the real life experience of the children and parents subject to the MR systems of the world.

REVIEWER	Helen Buckley Fellow Emiritus Trinity College Dublin 2 Ireland
REVIEW RETURNED	14-May-2017

GENERAL COMMENTS	the clarification that I had requested was dealt with by the reviewers. I found this revised paper to be of a higher standard then previously and clearer to the reader. I recommend acceptance and look forward to follow up studies.

REVIEWER	Andrew Shepherd University of Manchester, UK
REVIEW RETURNED	12-May-2017

GENERAL COMMENTS	Many thanks once more for the opportunity to review a revised version of this manuscript. I am satisfied that the authors have satisfactorily addressed all of the points which I raised in my previous review and would recommend this article on that basis. One, extremely minor, typographical point that will no doubt be corrected at the next stage in the process. All references to Noblit

	and Hare's work on meta-ethnography is referenced through the use of round not square brackets () vs [] in this draft of the manuscript.