Author's response to reviews

Title: Public Health Interventions in Midwifery: a systematic review of systematic reviews

Authors:

Jenny A McNeill (j.mcneill@qub.ac.uk)
Fiona Lynn (f.lynn@qub.ac.uk)
Fiona A Alderdice (f.a.alderdice@qub.ac.uk)

Version: 2 Date: 24 September 2012

Author's response to reviews: see over
Reviewer's report

Title: Public Health Interventions in Midwifery: a systematic review of systematic reviews

Version: 1 Date: 3 May 2012

Reviewer: Natalie Spearing

Reviewer's report:

Thank you for the opportunity to review this paper. The authors are to be congratulated on their synthesis of an enormous amount of information into a well-written, well-organised, and valuable summary of the literature in this field, which extends previous work in this area. The authors demonstrate good knowledge of the major issues to consider when reviewing systematic reviews (and of SR methods). The discussion and conclusions are supported by the data they present, and they clearly identify gaps in the review literature.

NS: Minor Essential Revisions

1. It is arguably just as important (if not more so) to highlight those interventions for which there is strong evidence, from well-conducted studies/reviews, of NO effect – as it is to showcase the interventions that are effective. I would recommend that the authors mention this in the paper – perhaps in the Discussion. [They might also consider another review of reviews in the future that highlights those interventions for which there is no evidence, based on “cream of the crop” reviews, as a means of educating clinicians and consumers about the “old wives’ tales”.] A paragraph has been added into the discussion and an additional table detailing those reviews of high quality which demonstrated inconclusive findings or no evidence of effect.

2. In the abstract, there is inconsistent use of tense in the Conclusion section.-changed
3. In the Introduction, the first item that is mentioned (the review of midwifery in the UK) should be referenced in this section. Reference has been added

4. In the Methods section, it is not clear what the purpose of the logic model is when it is first mentioned – although this becomes evident later on in the paper. Perhaps the end of the Methods section could be modified to clarify, briefly, why this was done? Further explanation has been added at the end of the methods section

5. It would be a useful addition to include a table that defines what exactly is meant by 1+, 1- or 2++ reviews etc. in the Quality Assessment and Effectiveness of Reviews section. An additional table has been included (Table 1)

6. “Kangaroo” has a typo. Corrected

7. Some minor inconsistencies were observed in the formatting of the references – would be worth double-checking the requirements of the journal. The references have been checked and corrected where necessary.

NS: Discretionary Revisions

8. The authors need to consider more use of paragraphs – these tend to be a bit long and unwieldy at present. For example, the authors could separate the 2nd paragraph under Methods into sections on the search/selection strategy and data extraction methods. Suggested subheading have been added

9. For clarity, would suggest grouping inclusion and exclusion criteria in the 2nd paragraph under Methods. Completed

10. There are a lot of acronyms, which are probably all very recognizable to the clinicians who will read this article, but it would be worthwhile reviewing the
Journal’s policy on defining acronyms. Would suggest adding a note to Table 1 with definitions of the acronyms used in the table. Definition of acronyms has been added to original Table 1 (now Table 3).

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.
Reviewer’s report

Title: Public Health Interventions in Midwifery: a systematic review of systematic reviews

Version: 1 Date: 21 August 2012

Reviewer: Jane Barlow

Reviewer’s report:

This is an important systematic review on a topic of considerable interest. The review has, on the whole been conducted with rigour and is well-written.

However, I have a number of concerns about the findings as follows:

1. I have found the failure to report the evidence about what does not work rather unsatisfactory. For example, one of the key public health interventions that midwives have used to date is antenatal education classes. Gagnon (2005) did a fantastic meta-analysis showing that this form of education is not effective.

Surely, midwives will be confused as to why the section on ANE only refers to PMFT, and they also need to know that ANE doesn’t work so that they do not waste further time and effort in that area. I spent rather a long time trying to work out why some topics and studies were just not mentioned at all.

This was an issue discussed at length by the authors when constructing the paper, however in order to manage and present the volume of information in a coherent manner we chose to focus only on effective reviews of high quality. However, the point raised is an important one and therefore we have included an additional table (Table 2) which details high quality reviews that reported on interventions which were not effective therefore providing some detail for readers regarding the subject areas of reviews that presented findings of no effect. In addition, reference has been made to this issue in the discussion to highlight the importance of evidence of no effect for readers. An updated version of the Gagnon review (2005) was included in this review (version 2007) however was categorised as inconclusive based on the authors conclusions and therefore not reported in this paper given the focus on ‘effective’ interventions. In this paper we have distinguished between those reviews which definitively reported interventions which were not effective (as detailed in Table 2) and those which reported inconclusive findings.

2. I am a bit confused about how the quality ratings were used in terms of
decisions about whether to include or exclude reviews:

a) The review states clearly that only reviews based on RCTs were included but what about reviews that included wide-ranging study designs and then reported the RCT findings separately or as part of a meta-analysis? This review appears to have excluded these. Reviews which reported RCT findings are only reported in this paper (SIGN Grading Score of 1- or above as detailed in the methods section). The original review included reviews that had wide ranging study designs however based on the SIGN grading of evidence system would have been allocated a lower score as the findings were not based solely on RCT/Meta analysis evidence.

b) Were all reviews meeting the above RCT criterion included or only reviews that were then rated also as high quality. Where is the quality rating reported… and how is that used to modify the assessment of the findings.

The quality rating is reported in the Methods section under ‘Quality Assessment and Effectiveness of Reviews’. An additional table has been included to provide clarity regarding the SIGN grading system. All reviews demonstrating effective interventions were initially graded based on SIGN and excluded if scored lower than 1-. A further quality assessment regarding the methodological conduct of the review was also undertaken, where reviews were scored as low, medium or high depending on the criteria outlined at the end of the first paragraph under ‘Quality Assessment and Effectiveness of Reviews’. In order to provide some further clarity in the paper regarding this process additional details are provided in this section.

3. I am unclear as to why some reviews have been included and not others. For example, in the section on postnatal mental health the authors appear to have included a systematic review of parenting programmes that is not focused on any aspect of the perinatal period and should undoubtedly not have been included in a the current review, while making no reference whatsoever to the NICE 2007 review, presumably because it included some studies that were not RCTs. I really don’t see how a review of mental health interventions in pregnancy cannot include the NICE 2007 guidance! Similarly, reviews of smoking cessation programmes in pregnancy which are included under ‘support’ rather than ‘education’ includes two reviews, but does not reference Melvin (2000) or Park (2004) or the Rubak (2005) meta-analysis of alcohol reduction during pregnancy.
Breastfeeding support during pregnancy isn't mentioned but Renfrew (2005) reviewed existing reviews (were none of these eligible) and the Dyson (2005) review must surely have been eligible. What about Kangaroo care (Moore 2007), parenting programmes for children 0-3 years (Barlow et al 2004); parental sensitivity (Bakermans-Kranenburg, 2003) (the results for RCTs are presented separately). I think that if the authors are intent on only publishing positive findings, they need to clearly state what evidence was not included in each section, and why, so that readers can be clear about why some reviews were not included, and indeed, why some topics were not included.

The systematic review of parenting programmes the reviewer refers to (Barlow & Coren 2003) was included in this review as it includes a parenting programme initiated in the antenatal and continuing into the early postnatal period. However based on the reviewers advice and further examination of the review itself we are happy to withdraw this systematic review from the paper.

The assumption of the reviewer is correct in that the NICE 2007 review was not included as it was not limited to RCT evidence. As this is an important and comprehensive review reference to it has now been included in the discussion section.

The reviews of smoking cessation have now been moved and are included under 'education'.

The reviews mentioned by the reviewer that were not included in this review were not eligible under the existing inclusion/exclusion criteria. (Melvin 2000 does not provide a search strategy or inclusion/exclusion criteria; Park 2004 and Rubak 2005 are is not specific to pregnancy and therefore not within the public health role of the midwife which was the original focus of this review; Dyson 2005 has been included however a typing error was present with regard to the date of publication (this has now been corrected); Moore 2007 has been included; Barlow et al 2004 would have been excluded as it extended beyond the role of the midwife). Bakermans-Kranenburg, 2003 was not identified with the search strategy used and we have reviewed the key terms of which none match with the search terms used and the focus is mainly on attachment as an outcome of parenting programmes extending up to children aged 54months and considered to be beyond the role of the midwife. We have acknowledged this as a limitation of our search strategy and have included reference to this in the limitations section.

Renfrew 2005 is a large review of health interventions to promote the duration of breast feeding for NICE but included B&A and case studies so would not have been eligible but actually the reviewers point is about the reviews within it. Renfrew 2005 included 3 reviews: Hodnett 2003; Mercer 2001 and Di Matteo 1996. Hodnett 2003 (now updated to 2007) has been included in the current review; Di Matteo 1996 does not meet the inclusion criteria (published from 1999 onwards) and Mercer 2001 was included in our original review but not eligible for reporting in this paper as it did not meet SIGN grading score required (1- or above). Mercer 2001 was graded 2++ as various types of studies
were included in the review-only one trial was included and Renfrew concluded the results have to be interpreted with caution.

4. A good starting point for this review would have been the Barlow et al (2008) review of health-led parenting interventions in pregnancy (DfE website) (also published in 2010 in Child and Adolescent Mental Health). The current review would then have needed to make clear what it was adding to what is already known on this topic. The publication from this report (Barlow et al 2010) was referenced in the introductory section originally. The report itself has (Barlow et al 2008) has now been incorporated into the paper.

4. The background section is rather thin on references particularly for the first paragraph and the references for second part are not well used. For example, refs 7 and 8 are really poor references for the importance of parenting in the early years. In fact reference 9 would have been better, and the NICE 2007 review used as reference instead for perintal mental health. Changes to the references as suggested have been completed.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests
Reviewer’s report

Title: Public Health Interventions in Midwifery: a systematic review of systematic reviews

Version: 1 Date: 14 August 2012

Reviewer: Denis Walsh

Reviewer’s report:

Major compulsory revisions: page 24 top of page:

There needs to be justification given for your assumptions about what has found its way into ‘routine practice’. What is currently written is to sketchy and fails to differentiate national guidelines and evidence that guidelines are applied well in practice. Knowledge transfer requires evidence that practice change has occurred through audit etc. It is not clear what your generalisations about what has been implemented (your example is folic acid supplementation) and what has not is based on. How would you measure this etc. You need a paragraph at least exploring this.

This is a very helpful comment from the reviewer and with reflection this section has been revised to provide clarity regarding the distinction between recommendation and implementation of guidelines and the importance of audit to evidence implementation into practice.

There is some inconsistency between a statement on page 25 about Sure start and family nurse partnership that you say has not been evaluation in the long term, though it is being rolled out and on page 26, middle paragraph about effective interventions for vulnerable women not being implemented. Isn’t sure start and family nurse partnership aimed at these groups?

The middle paragraph on p26 refers to the ‘current contribution of midwives to improving the health of women and families as core’ which is followed by using the logic model process (fig 2) to illustrate how an intervention (identification and support of vulnerable women) potentially might impact in the short, medium and long term. Further clarity regarding the example provided has been inserted.

Finally bottom of p27 and top of page 28 seems to say ‘don’t take much notice of
This section has been revised to reflect that the aim of reviews of reviews is to provide an important overview of the field, identifying current strengths and weaknesses in the research and to produce specific strategic information for moving forward with the role of the midwife in public health. It is also an important resource for those who want to explore specific areas in more depth.

**Level of interest:** An article of outstanding merit and interest in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests