Author's response to reviews

Title: What's wrong with meta-ethnography reporting? A methodological systematic review of published studies

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Author's response to reviews: see over
Dear Dr Megan Morris and co-editors,

Subject: Manuscript 1760641054136432

We would like to thank the reviewers, Nicky Britten and Zachary Munn, for their considered and very helpful reviews of our paper. We have revised our paper on the basis of their feedback and we believe this has resulted in an improved and stronger paper. We have described in the table below how we have addressed each of the issues they raised. We have indicated changes and additions to the manuscript text in red font.

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<td>In the background, you mention on line 95 that meta-ethnography uses as data the interpretations (themes, concepts or metaphors) in primary qualitative study reports which is in contrast to other methods of QES. From my experience the meta-aggregative approach is not in contrast to this and also considers interpretations as data.</td>
<td>We have clarified our meaning here. Meta-ethnography re-interprets the conceptual data of primary study authors. Meta-aggregation is an aggregative rather than interpretive approach- so although it may use interpretations as data, it does not re-interpret them. We have amended the wording on pages 5-6:</td>
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<td>‘In contrast to other QES approaches, the aim of a meta-ethnography is to produce new interpretations of the primary study author’s interpretations (e.g., themes, concepts or metaphors) of the research participants’ experiences in published primary qualitative studies [6, 15, 17]. Many other QES approaches only attempt to report on or aggregate identified themes/concepts.’</td>
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| **Minor Essential Revisions** | |
| • Minor copy-editing revisions. I have not provided an extensive list but for example: o Line 55: e.g. what? o Line 129: ‘to synthesise a very large numbers of studies’ o Include reference to Campbell et al’s report in last line of background | We have made these amendments and further minor changes throughout to typographical or grammatical errors after a thorough proof-read of the entire manuscript. |
| I am surprised by the author’s limited mentioning of the ENTREQ statement. The authors develop their tool based on ‘meta-ethnography publications providing rich methodological detail’ and their own experience, but also state they drew on other publications as well (Dixon-Woods, Hannes). I understand that the ENTREQ is a generic tool but I do not understand the decision to completely ignore it in the development of the author’s checklist considering their use of other publications (Dixon-Woods, Hannes). | Our main focus was on the analytic and synthesis phases of meta-ethnography - phases 4-6. The 2 review papers (Dixon-Woods, Hannes) were sufficient to inform data extraction for Noblit and Hare’s meta-ethnography phases 1-3 & 7. We have added further explanation to pages 11-12 to explain why we did not draw on ENTREQ: |
| ‘We were also informed by Dixon-Woods et al’s [10] and Hannes and Macalitis’ [8] reviews of qualitative syntheses for questions relating to other aspects of reviews (corresponding to Noblit and Hare’s phases 1 to 3), such as literature search and selection methods. We did not draw on ENTREQ [26] because it does not contain detailed guidance specific to the analysis and synthesis methods for meta-ethnography (phases 4 to 6) on which we intended to focus’ |
| Furthermore, we have provided additional |
information on what we regard as the limitations of the ENTREQ guidelines on page 18: ‘ENTREQ was not developed using methods recommended as good practice in health research reporting guideline development, such as expert consensus techniques [31], and it is unlikely to improve reporting quality of meta-ethnographies because it provides no detailed guidance on how to report the distinct analytic synthesis process.’

**Major Compulsory Revisions**

1. The search limits appear quite constrained, and there is no real justification for only searching during 2013. There should be additional information on why these limits were chosen and why the publication of Campbell’s report was deemed the cut-off point.

   We searched for meta-ethnographies published during a 2-year period from January 2012 – December 2013 and for clarity we now specify that we searched during a 2-year time period in the methods pages 9-10. We were interested in the most recent trends in meta-ethnography reporting subsequent to the publication of Campbell et al’s report in December 2011. We consider this a seminal publication which could be influential on meta-ethnography conduct and reporting. On pages 7-8 of the manuscript we have provided further detail on the content and importance of Campbell et al’s report:

   ‘More recently in 2011 Campbell et al [19] in their ‘Evaluating meta-ethnography’ health technology assessment (HTA) report gave a detailed account of how they operationalised and conducted each of Noblit and Hare’s seven phases in two meta-ethnographies carried out as part of their evaluation of the meta-ethnography approach for synthesising health-related qualitative studies. In this report they drew together substantive methodological research from several of their earlier published meta-ethnographies [5, 16, 17]. They gave detailed worked examples of their analysis and synthesis from two separate meta-ethnographies of 38 and 25 primary studies respectively including explicit descriptions of the translation (phase 5) and synthesis of translation (phase 6) processes. We consider the report to represent the most comprehensive methodological research on meta-ethnography (in health research) since the publication of Noblit and Hare’s [15] book.’

2. I question the author’s decision to call this paper a systematic review in the paper title. A more appropriate classification for this paper would be either a ‘scoping review’ or a ‘methodological systematic review.’

   We have amended the title to refer to the review as a ‘methodological systematic review.’

3. In general, I would like to see more justification and detail regarding how each of the items to evaluate meta-ethnography reports were decided upon.

   The justifications for why we felt that the items/data we analysed were important appear throughout the analysis and discussion section alongside the findings, e.g. page 14: ‘When searching for papers, the title and abstract are the only information available to the searchers unless the full paper is retrieved so the title and abstract should enable searchers to
judge the paper’s relevance. In terms of how the reviewers in this review described their studies, the titles of only 10 papers included the term ‘meta-ethnography’.

Page 17:
'The existence of syntheses that some reviewers claim are meta-ethnographies but that do not follow the approach could potentially damage the reputation of meta-ethnography making it less likely that users of evidence syntheses, e.g., researchers, clinical guideline developers, and policy makers, will have confidence in meta-ethnography findings.'

Page 17:
'Citing key methodological texts is not a guarantee of high quality reporting, neither is absence of these in citation lists an indicator of poor reporting. However for methodological progress and to avoid reviewers ‘reinventing the wheel,’ it could be advantageous for them to build on seminal meta-ethnography methodological texts.'

These are just a few examples.

We wanted to avoid repeating why items were important so we did not give this detail in the methods section as well as in the analysis/discussion section. We have now added a sentence to explain this in the methods section on page 12: 'In the analysis and discussion section we have explained the importance of each of the items analysed.'

4. Campbell’s report states in its conclusion that meta-ethnography is ‘still evolving and cannot, at present, be regarded as a standardized approach capable of application in a routinized way.’ (Campbell 2011). You mention this in your document. As this is the case, is it possible to create a standard set of reporting guidelines for meta-ethnography? Some discussion on this may be useful. Additionally, this work has raised a number of questions regarding the conduct and reporting of meta-ethnography studies. It is clear from this paper that authors of meta-ethnography papers struggle to report accurately the conduct of their research. I would like to see some information regarding what a potential method-specific reporting guideline “could” look like – even if this is only mentioned briefly in the discussion. We have now included a rationale for why we believe it is possible to create meta-ethnography reporting guidelines in a new section ‘raising reporting standards’ on pages 33-34. A precedent has been set by the RAMESES reporting guidelines for two other types of qualitative synthesis approaches. We give some detail as to what the RAMESES guidelines contain to give a flavour of what meta-ethnography guidelines could look like.

Reviewer 2: Nicky Britten

Minor essential revisions

The papers included in the synthesis (the data set for this review) should have a different numbering system from the papers in the...

Our response

For clarity we have renamed the synthesis papers which form the data set for the review as suggested S1, S2 etc. in the manuscript and
A source of ambiguity is the terminology used to refer to the authors of this paper and the authors of the synthesis papers. For example in paragraph 2 of data extraction and analysis, the term ‘reviewer’ is used to refer to both of these sets of authors.

We now refer to ourselves as ‘current authors’ and to the authors of papers included in the data set for our review as ‘reviewers’ to clearly differentiate between the two. We have also created separate endnotes on page 40 to explain our use of these and other terms. We have also amended some other similar terminology throughout for greater clarity.

There are some stylistic points, all minor essential revisions not for publication.

- a. The first sentence of the second paragraph on page 13 is unclear – who or what does ‘they’ and ‘them’ refer to?
- b. In the first sentence of the third paragraph of page 13, the author of paper 13 is referred to as an individual when the paper was co-authored (both papers 13 were co-authored).
- c. In the first paragraph on page 16, line 4, it would be better to say that “7 papers cited Atkins et al” rather than “a quarter cited Atkins et al”. In the same paragraph, it is worth emphasising that all 32 papers cited Noblit and Hare (1988).
- d. At the end of the second paragraph on page 16, there needs to be some signposting to explain that the authors will go on to discuss each of the 7 phases of metaethnography in turn.

- e. Duplicates of references in the reference section e.g. references 5 and 8 are the same paper. Give the full HTA reference for reference 23

- j. In table 7 it would be better to inserts ticks rather than leave blank spaces to indicate citation.

Table 7 does contain ‘ticks’ to show which papers cited seminal texts, so perhaps there was a problem in the way this was displayed in the pdf file.

**Discretionary revisions**

- iv) Noblit and Hare gave no guidance on how to appraise study reports. Although they gave no guidance, they did state that ‘The worth of studies, in their view and in ours, is determined in the process of achieving a synthesis’ (Noblit and Hare 1988, p 16). This suggests that Noblit and Hare did not see the need for the prior appraisal of study reports

- v) In the sixth paragraph of the background the authors state there are no examples of refutational syntheses, but there are examples of papers which do not fit into emerging syntheses. For example in the meta-ethnography reported by Campbell et al (2003), there was one paper which could not be synthesised with the other papers the background

- vi) Another issue which could be discussed at the end of the paper is the lack of refutational syntheses. Do the current authors think that the reporting of meta-ethnographies should include

- vii) Another issue which could be discussed at the end of the paper is the lack of refutational syntheses. Do the current authors think that the reporting of meta-ethnographies should include

We did refer to Noblit and Hare’s viewpoint on quality appraisal later in the article on page 21 but we have now added a sentence to this effect in the background on page 6: ‘Noblit and Hare’s [15] seminal book gave no guidance on how to sample or appraise study reports for inclusion in a meta-ethnography (although they did not advocate formal appraisal of studies prior to synthesis)...’

We have now included this point in the background on page 8:

‘There are no examples of refutational syntheses of which we are aware, aside from Noblit and Hare’s example [15], although in some meta-ethnographies reviewers have identified and excluded a paper(s) which could not be synthesised with the other papers [16] or identified multiple lines of argument that were difficult to synthesise into a single line of argument [20].’

This is an interesting and important issue you have raised. We have expanded on this point in the analysis & discussion section on page 27: ‘No reviewers carried out a refutational synthesis,'
whether synthesis authors sought disconfirming studies or concepts? [discretionary revision]  

all were reciprocal or line of argument syntheses. ‘However, it could be argued that reviewers should seek out primary studies containing contradictory concepts as part of the translation process, in line with the principles of the constant comparative analytic method in which disconfirming or contradictory cases are used to test emerging understandings of the data [14].’

We have now included a question for further research related to refutational synthesis in our discussion of ‘Raising Reporting Standards’ on page 35: ‘Is refutational synthesis (or searching for disconfirming cases) a necessary aspect of a good quality meta-ethnography and its reporting?’

We believe that this is one of a number of important issues that should be debated by a wider community of experts. We have clarified this on page 35: ‘While we have offered our views and presented current thinking on these questions in the analysis and discussion section, we consider that they can only be addressed satisfactorily through further methodological work and/or debate amongst a wider international community of qualitative synthesis and meta-ethnography experts.’

<table>
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<td>Given that there was low agreement between 3 of the current authors’ judgements about the synthesis authors’ descriptions of the analysis process, it is not clear why only 2 of 6 current authors of this paper discussed this issue. Surely the whole team needed to contribute to this discussion and reach an agreement? If not, how can this be justified? [major compulsory revision]</td>
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<td>We have added further detail to our methods on page 13 to clarify the involvement of all authors: ‘The rationale for the final agreement reached by EF and NR was presented to RT, JN, MM and RJ who found it acceptable. All current authors had access to the papers included in the review, had opportunities to challenge or agree with NR and EF during several conference calls and email discussions, and were involved in data interpretation.’</td>
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vi) The authors end their paper with a useful list of questions for further research, but I think the paper would be strengthened by a deeper consideration of some of the issues raised. The current brief concluding paragraph does not do the topic justice. […]. In the fifth paragraph of the results they pose the question ‘at what point does an adaptation of the meta-ethnography approach become something other than a metaethnography?’

We intended this list of further research questions to summarise all the issues we had raised and considered throughout the analysis and discussion section. We have now more clearly indicated throughout the analysis and discussion section where we gave deeper consideration to each of the questions raised and expanded on some of the issues, e.g. Page 21: ‘However, the conditions under which exhaustive searches might be desirable or necessary when conducting a meta-ethnography require further exploration.’ Pages 21-22: ‘The most appropriate role, if any, for QA of primary qualitative studies for inclusion in a meta-ethnography still needs further debate and investigation.’ Page 23: ‘Thus, how many primary studies are too many to
Page 26:
To our knowledge the two contrasting methods have not been systematically compared, so it is not clear how the order of reading and synthesising papers affects the output of a synthesis, or whether one method is better than another. However, it is reasonable to suppose that the concepts presented in the paper with which a reviewer starts a meta-ethnography might have a disproportionate influence on the final conceptual output of the synthesis.

(These are just a few examples).

In addition, we have revised two paragraphs on page 16 to more clearly show how we considered the question of when is a meta-ethnography no longer a meta-ethnography:

'Meta-ethnography, like any research approach, should be open to novel use as long as the methods are transparent, but we debated at what point does an adaptation of the meta-ethnography approach become something other than a meta-ethnography? We identified two key issues here: (1) reviewers adapting ('evolving') the approach in response to methodological or practical challenges they encountered in ways that appeared to enhance the method and that seemed to be in keeping with Noblit and Hare's original philosophy, e.g., by separately synthesising two or more groups of papers with a like focus before attempting to do an over-arching synthesis of all studies; (2) reviewers claiming they were doing a meta-ethnography but not following the key principles of the approach (applying it inappropriately or in ways at odds with Noblit and Hare's philosophy), for example, they had not synthesised the interpretations (conceptual data) of the primary study authors (paper S15) or they appeared to have aggregated primary study findings rather than performed a translation and synthesis of translations (e.g. paper S12).'

We believe that these questions are important issues that should be addressed and debated by a wider community of experts and through further research (we have clarified this on page 35). For this reason we have not always given definitive answers to these questions.

What is the difference between an ‘evolving’ method and one which is ‘inconsistently applied’ (abstract)? They do not really reflect on these questions, which are important.

We have deleted reference to inconsistent application of meta-ethnography because it was ambiguous - we meant it was applied in diverse ways, sometimes inappropriately. The word ‘evolving’ refers to a quote from Campbell et al (2011) and we have extended the quote on page 16 to include this term: ‘supporting Campbell et al’s statement that,
despite the potential of meta-ethnography as a qualitative synthesis approach, ‘it is still evolving and cannot be regarded as a standardised approach to be applied in a routinised way’ ([19]: p. 125).’

We have amended page 16 to clarify what we meant by ‘evolving’:
‘reviewers adapting the approach in response to methodological or practical challenges they encountered in ways that appeared to enhance the method and that seemed to be in keeping with Noblit and Hare’s original philosophy, e.g., by separately synthesising two or more groups of papers with a like focus before attempting to do an over-arching synthesis of all studies’ (page 16).

Nor do they explore the question about whether or not it is desirable or realistic to have a standardised approach, apart from saying that reporting guidelines would be more useful than synthesis guidelines (in the concluding paragraph). The authors need to discuss the question of standardisation of methods.

I think we probably need a balance between sufficient agreement about key principles (see sixth paragraph of the results), can the authors state what they think these key principles are?) and the necessary flexibility which allows qualitative researchers to be creative in what is essentially an inductive process. The current authors may have different views, but the reader needs to know what they are [major compulsory revision]

We have added a new section ‘raising reporting standards’ on pages 33-34 which explains why we think that a standardised approach to meta-ethnography reporting is desirable and provides a rationale for why we believe it is possible to create meta-ethnography reporting guidelines.

We did not intend to imply that reporting guidelines would be more useful than synthesis guidelines- we believe both are important so we have added a sentence on Page 30:
‘Reviewers could benefit from a comprehensive guide about how to conduct a meta-ethnography that pulls together and extends learning from seminal publications.’

and to our conclusions on page 37:
‘We propose that meta-ethnography’s unique, complex analysis methods would benefit from comprehensive guidance on its conduct and a method-specific reporting guideline.’

On page 15 we have now specified a list of key principles for judging whether or not a paper was reporting a meta-ethnography:
‘We specified these as key principles of a meta-ethnography:

- the reviewers had synthesised published primary qualitative studies
- they had used an interpretive not an aggregative approach
- reviewers had re-interpreted and synthesised conceptual data (even if they also synthesised some participant quotes or other descriptive data from primary studies)
- reviewers had conducted a process which resembled translation (phase 5) as described by Noblit and Hare reviewers had intended to reach a new interpretation even if it was not possible for them to achieve this.’

- [15]
We posed several questions for further research on pages 34-35, and now give more detail to specify that we believe they can only be satisfactorily addressed through further methodological work and/or debate amongst experts:

‘While we have offered our views and presented current evidence on many of these questions in the analysis and discussion section, we consider that they can only be addressed satisfactorily through further methodological work and/or debate amongst a wider international community of qualitative synthesis and meta-ethnography experts.’

The need for expert consensus applies equally to refining the key principles:

‘what are the defining characteristics (key principles) of a meta-ethnography compared to other synthesis approaches?’

We also say this on p16:

‘we developed our own criteria for judging whether a study was a meta-ethnography, however there is need for wider debate among experts about this issue.’

We have made some further amendments to the paper throughout in line with the author guidelines e.g. to the formatting, or for clarity of expression.

We thank you for considering our article and look forward to your response.

Yours sincerely,

Emma France (on behalf of Nicola Ring, Rebecca Thomas, Jane Noyes, Ruth Jepson and Margaret Maxwell)