Reviewer’s report

Title: Communication practices for delivering health behaviour change conversations in primary care: A systematic review and thematic synthesis

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Reviewer: Emma Ruth Miller

Reviewer's report:

GENERAL
Thank you for the opportunity to review this manuscript which is on an interesting topic of preventive health importance. While it is well written, there are some methodological and content issues that could be improved.

* The major issues:
1. There are insufficient details provided within the manuscript to repeat the systematic search and identify the selected studies. It is not until page 33 that the first mention of any quality assessment is made. The quality assessment process should be fully described in the method section. There is mention of following the process described by Parry and Land (2013) but this is really not sufficient explanation or rationale for using this particular process. Readers will want to know if a validated instrument used to assess quality, and if not why not? Who in the team was involved? What was the quality score (if any) cut off?

2. According to Figure 1 (currently incorrectly labelled 'table 1'), nearly 17,600 papers were identified with the search strategy once duplicates were removed. After screening, more than 17,200 of these were excluded. While this provides evidence of an oversensitive search strategy, there is a lack of transparency about the screening process here. How was the scanning of such a vast number of manuscripts accomplished? Given that 225 of the remaining 322 papers (70%) are described as 'irrelevant' in the next screening stage, it is hard to reconcile the 17,200 exclusions in the previous screening step.

3. The other serious issue with the review, and this is acknowledged by the authors, is the very small number of papers that were ultimately included. Focusing solely on how health providers initiate discussions about health behaviour change, the remaining 10 papers were spread across different settings, different behaviours and different health practitioner discipline. In such a small group, these categories become even smaller when one considers that the ten papers were from 8 studies. Unless I missed it, the authors do not discuss the validity of combining discussions about weight and smoking with those related to sexual behaviour, with the latter likely to be associated with greater stigma than the former.

4. The paper itself if over long, with multiple themes discussed. Perhaps the authors would consider restricting their findings and discussion to the more dominant themes arising from all papers.
The difficulty with the multiple sections is that none of the sections pertain to all 10 studies, with many only referring to one or two of the studies. Yet the authors present a level of confidence in this evidence that doesn't appear warranted.

Specific comments (some major some minor) per section are made below:

INTRODUCTION:
- While I absolutely do not argue that there isn't an issue here, a brief introduction that mainly states that clinicians want more support does not make a strong case for the gap this review is intended to address. A more comprehensive exploration of the issue and its consequences is required. If the results and discussion section were to be reworked as mentioned above, this could leave more room to provide a more compelling case for the review.

METHODS:
- There is not sufficient detail about the search strategy and quality assessment provided.
  - What was the rationale for the dates 1945-2016?
  - While the full search strategy is provided as an additional file, there needs to be at least some detail about the search terms provided here.
  - The description of 'appraisal' focused on what data were available rather than the rigour of the studies appraised. This needs some comment.
- In the last paragraph of page 8, the OSOP technique is referred to - this needs to be written out in full the first time and also a brief explanation is required.

RESULTS:
- 'Table' 1 is actually a figure, not a table.
  - An enormous number (>17,000) are 'screened out' but it not clear why? As mentioned, we need an understanding of the quality assessment.
- Paragraph 2, under subheading 'included studies', the behaviours discussed in the ten papers are listed. As mentioned above, is the range of issues a problem? STI and sexual behaviours carry with them an extra stigma.
- In Table 2 (should be Table 1), the Pilnick and Colemen study is clearly the one that has two papers, but which is the other? There is a need to clearly indicate here which of these studies had two further analyses that were included in your review.
- Page 22, under the Tapsell (1997) quotes, the excerpt is listed as number 6, but referred to in the text as excerpt number 7.
- Under excerpt 10 (page 25), the point is made that "the effectiveness of positive reinforcement was not explored in detail". The lack of any evidence for effectiveness, in terms of achieving behaviour change, is a recurring theme throughout and could reflect a serious limitation for the whole paper.
- Under excerpt 12 (page 28), there is a formatting issue.

DISCUSSION:
- Throughout the discussion it is claimed that evidence of various strength was presented in the review but it is not clear how the relative strength was evaluated. As discussed above, there were only 10 papers (on 8 studies) and multiple themes for which few included all 10 papers. For example, the claim in paragraph 2, page 32, "Strong evidence indicated that that presenting 'information, for people
in general', rather than 'advice for you' avoids potential problems..."
- Under subheading 'strengths and limitations', bottom page 33, is stated: "We assessed study quality and used that to describe the strength of evidence for conclusions." Given this assessment was used to weight the strength of evidence, the method should surely be described somewhere.
- At the end of 'strengths and limitations (page 34), is stated: "However, there is evidence that communication practices are relatively consistent." To what evidence does this refer?
- There are a number of places in the discussion in which the findings are overstated. Examples include the statement on line 74-5, page 35, that the evidence presented in the review has demonstrated the riskiness of linking health behaviours and specific conditions to initiate health behaviour change. And the following lines 76-8 in which it is stated: "...we have shown here that initiating HBCT opportunistically at the end of a consultation, independent of health concerns, could mitigate the delicacy of discussions, and had minimal potential for resistance" rather overstates what was found.
- On the top of page 36, the consistency of the review findings with clinical guidelines is discussed, in which context it is stated: "Having reviewed this variation in delivery we were able to identify ways that recommendations were implemented that seemed more effective, and make recommendations on ways to implement suggested HBC strategies." This is not consistent with the authors explicit recognition that none of the included studies evaluated effectiveness of the health practitioner discussions on health behaviour in patients.
- On page 37, second paragraph, the concept of 'action planning' is mentioned for the first time yet forms a major implication in the conclusion.

CONCLUSION:
- In the conclusion, it is stated: "However, one practice recommended by clinical guidelines; associating a patient's health concerns and their health behaviours, generally prompts patients to resist HBC." This doesn't really match your analysis. Of five papers looking at this only two by the same authors (Pilnick & Coleman 2003; Pilnick & Coleman 2010) discussed resistance. Further, these two papers were analyses of a single study.
- Next it is stated: "On the other hand, building conversations collaboratively through question-answer sequences may facilitate patient receptivity to changing their health behaviours." This also doesn't match your description of the four studies looking at this, which found varied responses to this strategy from patients.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Unable to assess

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.
Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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