Author’s response to reviews

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

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Author’s response to reviews:

PLEASE NOTE

OUR TRACKED CHANGES ON OUR PAPER ONLY SHOW PARTIALLY ON THIS SUBMISSION.

WE HAVE CONTACTED THE EDITOR ABOUT THIS ISSUE, AND ARE WAITING FOR A RESPONSE.

HOWEVER WE ARE SUBMITTING NOW AS WE ARE KEEN TO MEET OUR DEADLINE, WITH AN UNDERSTANDING THAT THESE CHANGES ARE NOT SHOWING

We would like to thank the reviewer and the editor for their constructive comments on this paper, and for taking the time to make carefully considered and helpful suggestions. We have made recommended changes to our paper, and have provided a written response to each reviewer below:
1. Editor Comments

Some general comments:

1. The reviewer has highlighted some issues regarding methodological reporting which it will be important to address.

We agree and have provided a detailed response of our changes to methodological reporting in response to the reviewer’s comments (below).

2. The 3 overarching thematic domains you developed are useful and provide a good framework for presentation of your results. I note there is little information in the closing HBCT domain derived from your review. This seems to be a finding in itself.

In response to this comment we have further highlighted this as a finding.

3. The discussion and conclusion did not flow clearly and did not persuasively argue what appears to be your key message, that linking HBCT with a patient’s health concern may be ‘risky’. This message appears overstated. The paper would benefit from editing and should be shorter and more concise, I agree with the reviewer it is overlong.

Following this advice from the editor and the reviewer we have edited this paper so it is shorter and more concise. We detail, below, the steps we took to condense our findings.

We have also reviewed the flow of our discussion to better argue our key findings.

Some specific comments:

Results

4. P 17 line 51, When you state that Pilnick and Coleman says displays of resistance are rarely seen in other medical consultations, I do not understand. I don't know what the excerpt refers to, and also whether they are talking about their own data or contrasting their findings to other literature in their discussion.

Pilnick and Coleman are referring to other work in the field of clinical advice-giving, and compare their results with this existing work to show how rare this type of resistance is. We have reworded to add clarity here and it now says:

“Pilnick and Coleman state that these displays of resistance in response to linking are ‘rarely seen in other medical consultations.”

5. P 24 line 200-208 This theme is not clear to me, including the title. What is the meaning of excerpt 10? A supportive quote or more information regarding resistance displays may be beneficial here, particularly as you are picking this up as an important theme.

In line with reviewer comments to enhance clarity and brevity (comment 18, below), we have only presented themes which were mentioned in more than one study. Therefore, this theme is no longer
presented.

6. P 28 line 224 I query if this is appropriate as a subtheme, given that you only have evidence from one paper and the data you provide to support that appears to be an example of poor consultation skills (aggressive, swearing clinician) rather than lack of patient response to evoking a worst-case scenario.

In line with the reviewer’s comments (comment 18, below), we have removed all references to conversational practices which were only reported in one paper. This included removing ‘worst case scenarios’ from this section.

7. P30 line 254 Can abandoning HBCT be considered in the category of pursuing HBCT? That seems to be a way of closing HBCT? You may have meant this to be (b) to above theme. There is no need for the subtheme ‘pursuing HBCT’ when it is the only theme in the ‘Managing resistance displays’

We agree, and had mis-numbered ‘initiating a change in topic’. It was indeed presented in the literature as a second option to deal with resistance displays (in contrast to pursuing HBCT). We have adjusted our reporting accordingly so that ‘pursuit’ and ‘topic change’ are presented as two ways of managing resistance displays.

8. Line 292 – I don't think you should refer to this as ‘pragmatic considerations’ or put it in your results given it is not a finding of your review. There is literature on brief interventions which you could draw on to determine if the action stage is important or not. I believe it is more interesting to note this seems to be an underexplored area in the literature you have examined.

We agree with this comment and have removed references to pragmatic considerations. Instead, following the editor’s advice here, we have stated a lack of evidence for this phase of the consultation, and highlighted that only two studies reported closing strategies. As an ‘affirmative action step’ was reported in only one paper, this has been removed from our reporting here, in line with reviewer comment (comment 18, below) to report only dominant themes in more than one paper.

9. Later in the discussion P 33 line 29-30 I am not sure if this is your interpretation, or the interpretation of the authors, and your presentation again of this in the implication section needs to be clearer.

We have adjusted this section to reflect this comment. This was the interpretation of the authors. The discussion section now says:

“This was reported to expedite closings, but was shown to be vague, and the authors hypothesised that the minimal responses that were received, a lack of providing an affirmative next step meant that it was unlikely to motivate behaviour change.”(lines 47-49)

Discussion:

10. Your ‘implications and recommendations’ and then ‘conclusion’ state that it may be ineffective to link behaviour change talk with the person’s own issues, and that dropping it in at the end may work
better. You go on to discuss that this is out of step with guidelines and other evidence but I think you need to make this more persuasive or be more cautious in your presentation of your argument. Specifically mentioning evidence on brief interventions and motivational interviewing strategies and acknowledging the strength or otherwise of the literature on this issue is needed if you are to be more persuasive.

Opportunistic health behaviour change at the end of a consultation was a theme which emerged from one study only. In line with comments from the reviewer this has now been removed from our reporting in this paper. Therefore, we have now not mentioned the evidence on brief interventions and motivational interviewing strategies which were associated with this strategy.

We elaborate in response to comment 35 how our conclusions about linking health behaviours and health were drawn. However, we have, in response to this, highlighted in the discussion that where linking occurs in the consultation may affect how it is received. We state that linking to initiate discussions carries risk of generating resistance, whilst linking to salient concerns following resistance displays can address resistance. However, we state that it is difficult to know what health conditions the patient will orient to as salient-for-them during these linking sequences.

11. Please review the flow of your discussion so your key messages are clear and well argued.

In response to this comment, and the wider changes to the discussion suggested by the reviewer, we have reviewed the clarity and flow of our discussion to better highlight and argue our key messages.

Minor comments
12. Error on figure 1 in numbers for full text review
Thank you for highlighting this, we have corrected the numbers for full text review

13. Typo p 15 line 5
Thank you for highlighting this. We have addressed this typo (p13, line 5), which now says: “To optimise the clinical relevance of the conversational strategies”

2. Reviewer comments

The major issues:

14. There are insufficient details provided within the manuscript to repeat the systematic search and identify the selected studies.

We agree that the search strategy is not detailed in this manuscript. This is because we used two strategies and they comprehensive and in-depth, and subsequently quite long. In line with the BMC instructions for authors we provided these as an additional file, and signpost this on page 8. However, in response to this comment (and the related comment below) we have provided more details and exemplify terms on page 7. However, if the reviewer would rather we report the full strategies in-text, and the editor would support this decision, we’d be happy to do so.
15. 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? We followed existing techniques to appraise these studies. In their guide to systematically reviewing and synthesizing evidence from conversation and discourse analytic research, Parry and Land state that “Unidimensional quality appraisal is not possible for this kind of evidence, instead record characteristics of data, settings, participants, analytic approach, and analytic depth in order to specify studies’ contribution to the review” (p.8 [1]). This is because “conversation analysis produces systematic and empirically grounded descriptions of concrete practices and their interactional consequences and functioning, it does not involve the kind of interpretation and theory generation that characterise in qualitative healthcare research. These distinctive features mean that no existing tools for quality appraisal of research are suitable.” ([1] p.8)

We agree strongly with this assessment by Parry and Land and therefore, quality score and cut off points were not relevant, as we did not use a validated instrument used to assess quality. This is because one does not exist, and would not be appropriate for this type of evidence. In line with best practice we considered (1) the type and amount of data available, and (2) the detail and depth of analysis. We relied on the team’s skill in conversation and discourse analysis to do this, and to decide of the strength of each study’s contribution to the review. We have stated this on page 8, and summarised the data available in table 2. We understand qualitative research such as this, which conducts turn-by-turn interactional analysis, does differ from other types of qualitative research in the field of healthcare, but can reassure the reviewer we have followed existing best-practices for how best to report this aspect of the review.

We agree with the reviewer that we had not clearly stated ‘quality assessment’, rather we called this section ‘appraisal’, on page 8. We have renamed this ‘quality appraisal’ and synthesis, to better signpost this section, and also highlighted this in the corresponding paragraph. We have also summarized our response above in text to signpost to readers that traditional methods of quality assessment were not appropriate for these data (lines 195-197), as the reviewer advised.

16. 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.

We have amended our reporting of screening to make this clearer. We followed standard procedures screening on titles, abstracts, then full texts. It was enormously labour intensive. This search strategy was highly sensitive but not specific because relevant studies are hard to find. We were advised by an librarian/information specialist in designing this strategy.

17. 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.

We agree that there were a small number of papers which met our inclusion criteria. We feel this is important to communicate to clinicians and researchers, because guidelines make recommendations about what doctors should say, but there is actually very little empirical evidence of how these conversations are carried out, and received. In response to this comment we have highlighted this further in our discussion and signpost this as an important area for further research.

To respond to the reviewer’s comment about the amount of studies included we are happy to clarify here, that frequency of reporting in studies is a key area where conversation analysis differs from both qualitative and quantitative approaches [2]. Typically, a larger amount of data is used than in other qualitative methods. These 10 papers, from 8 studies, represent results from naturally occurring conversations between clinicians and 1925 patients. Due to the highly organised nature of talk in interaction, we feel that this amount of data does represent the potential responses patients commonly displayed to the practices observed by the authors, and therefore can draw conclusions from the amount of data we have aggregated here.

We address the comment regarding the validity of combining discussions about weight and smoking with those related to sexual behaviour in our response to this expanded comment, 27, below.

18. 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.

We agree that presenting dominant themes provides more data from which to draw conclusions. We also agree that restricting our findings would enhance the key messages and readability of this paper. As no themes were presented in all 10 studies we have done the following to address this comment. Firstly, we have removed reference to any practice which was reported in only one study, and adjusted our reporting to reflect this. Secondly, we have reported higher level dominant themes, as the reviewer suggests, collapsing sub-themes into their higher-level categories where appropriate. This acted to enhance clarity and also to reduce the length of this review.

As we state above, these 10 papers, from 8 studies, represent results from naturally occurring conversations between clinicians and 1925 patients. We feel confident in our results of these aggregated data. This is because most ways of talking have a limited number of responses that can be produced afterwards [3-6]. In line with the aims of this qualitative review, we do not feel that frequency of a practice indicates its potential to be well received or not. Rather the reporting of a response shows it is a potential response to health behaviour change talk. In our response to comment 35 we discuss this concept of response relevance in detail.

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

19. 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.

We have provided additional background in this section to further justify this review. We show a specific example from guidelines, to highlight the lack of detail. We also discuss GP’s specific concerns about causing offence, and patient reports highlighting that how clinicians talk about health behaviours is viewed as important. We feel this better sets up the evidence gap which we aim to address, and adds additional context for this review.

METHODS

20. There is not sufficient detail about the search strategy and quality assessment provided.

We have detailed a response to this comment about search strategy in comment 14 above. Similarly, we have responded to this query about quality assessment in response to comment 15, above.

21. What was the rationale for the dates 1945-2016?
We used these dates as conversation analysis emerged as a discipline in 1960s, and discourse analysis in the 1950s, and we were keen to ensure foundational papers conducted in clinical settings could be included if eligible, as conversational patterns are consistent over time (we evidence this elsewhere in response to comment 34). We have provided this rationale on page 7.

22. 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.

We respond to this in comment 14 above.

23. The description of 'appraisal' focused on what data were available rather than the rigor of the studies appraised. This needs some comment.

We have responded to this comment in our response to comment 15, above.

24. 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.

Thank you for this comment, we agree, and this has been spelled out and a brief explanation provided. This now says:
“Similar themes across studies were then grouped hierarchically using the one sheet of paper (OSOP) technique [7], where conversational practices were summarised to produce top-level descriptive themes”
RESULTS:
25. 'Table' 1 is actually a figure, not a table.

Thank you for highlighting this, we have changed the legend and in-text referencing to ‘figure 1’ rather than ‘table 1’

26. An enormous number (>17,000) are 'screened out' but it not clear why? As mentioned, we need an understanding of the quality assessment.

We respond to this comment about quality assessment in comment 15, above. And in our response to comment 16 we state how we have added clarity to our reporting of the screening process. We included every DA or CA study in our review and none were screened out on quality criteria.

27. 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.

We acknowledge the reviewer's concerns about the inclusion of studies regarding differing health behaviours and we make related comments in the ‘limitations’ section. However, we found no empirical evidence in these conversational patterns indicating that conversations around these behaviours carry an extra stigma compared to those connected to obesity, for example, or smoking cessation. The closest available work to illuminate this topic is by Heath [8], and Bergmann [9]. Heath has examined how parties in clinical consultations organize and mark consultation items as interactionally ‘delicate’. He shows that doctors exercise interactional caution around these delicate topics. However, both Heath and Bergamnn show that we should not assume that particular topics are intrinsically ‘delicate’ (or, in the case of our work in this review carry more ‘stigma’ than others into a discussion). Bergman states that “viewed sociologically, there is not first an embarrassing, delicate morally dubious event…instead the delicate.. character of an event is constituted by the very act of talking about it cautiously and discreetly” p.154.

Conversations about health behaviour change have been under researched (as this paper highlights), and there has been no comparative interactional work which shows greater interactional delicacy employed when discussing sexual health when compared to other health behaviours, and we therefore did not make this assumption in this paper.

28. 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.

Thank you for highlighting this. Silverman et al’s was the second study which produced two papers. We have added this to the relabeled table 1, as “Silverman et al 1992a, and 1992b”.

29. Page 22, under the Tapsell (1997) quotes, the excerpt is listed as number 6, but referred to in the text as excerpt number 7.

Thank you for highlighting this, we have reviewed our in-text references so they match the excerpt
30. 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.

We agree that more research is needed on the relationships between conversational practices and action. However, whilst this is an area where future research may be needed, we do not see this dearth as a major limitation of this review. This is because it was not our aim to examine conversational practices associated with action. Rather we state in the introduction, “We aim to identify and synthesise evidence from conversation and discourse analytic studies regarding how clinicians communicate with their patients about health behaviour change (HBC), and the responses each practice is likely to generate from patients.”

This is because one reason that doctors report reluctance to engage in health behaviour change talk is out of concern for resistance in consultation. Therefore, the strategies highlighted here as more or less likely to generate resistance are helpful for doctors to be aware of, regardless of their association with action.

31. Under excerpt 12 (page 28), there is a formatting issue.

Thank you for highlighting this, we have addressed this issue.

DISCUSSION:
32. 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.

We have detailed our quality assessment process in response to comment 15, above. We used this process to provide a reasoning for our statement of strength of evidence.

33. 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.

We have addressed this comment in our response to comment 15 above. And have elaborated on our quality assessment process. We used this process to provide a reasoning for our statement of strength of evidence.

34. 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?

We have added references to support this statement.
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.

We do not feel this evidence is overstated. The unique positioning of conversation analysis as neither wholly qualitative nor quantitative, and grounded in interactional linguistics, may mean the way we have drawn our conclusions are not clear to someone who is not trained in this approach. We are happy to clarify for the reviewer how we drew conclusions, using the example of ‘linking’ (the other example mentioned in this comment, has been removed from the paper because it was reported in only one study).

Conversation analysis examines the patterns of conversation- for example a question followed by a response. The phrasing of a question one way or another way is likely invite different responses. This is called ‘response relevance’. Here the authors of five studies all mention ‘linking health behaviours and health’. Three studies explored this phenomenon and the responses it made relevant in detail [10-12]. (these therefore provide ‘higher quality’ evidence, due to detailed focused attention on this phenomenon of interest).

Resistance is not a relevant response in all circumstances. However, these authors show the conversational potential for resistance to be a relevant response following linking. Furthermore, this is not passive resistance (such as a minimal response or a no response) it is an active move by the patient to not align with the stance the GP is displaying (eg that smoking caused a bad cough) and to express outright rejection of the doctor’s words. Across these papers we see a number of relevant responses to linking (which is usually as all actions have a number of possible relevant responses), however one of these is clear and active resistance. Pilnick and Coleman examine their results in the context of other work in the field and state this active resistance is ‘rarely seen in other clinical consultations’. Therefore not only is resistance a possibly relevant response to linking, the type of resistance that is relevant is more marked than evident elsewhere in the literature on resistance following clinical advice giving.

Four of the authors of this review are trained conversation analysts who work extensively on clinical consultations. We are very familiar with this literature and agree strongly with Pilnick and Coleman’s statement – it is very rare for patient’s to overtly disagree with their doctor. We feel confident in our conclusion from the evidence reviewed here that linking is indeed risky. This is because one potential response this action makes conversationally relevant is resistance. Doctors state their wish to avoid upsetting patients, and are not sure how, this phenomenon can generate strong resistance displays and we think doctors should be aware of this risk which is not communicated in guidelines.

However, in response to this statement we have emphasised in the discussion that we found mixed evidence of effectiveness, and that there is a risk of resistance, which we think avoids overstatement our findings:

“However, we found mixed evidence of effectiveness. Our results here showed that linking health behaviours and health to initiate conversations may generate resistance displays. This is a potentially risky strategy to initiate HBC and may be best avoided or used cautiously. However, linking to a salient concern later in the discussion could be a helpful way to address resistance.” Lines 215-219, p29
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.

We are happy to clarify for the reviewer that we use the term ‘effective’ with relevance to our aim of identifying conversational strategies likely to be well received. Thank you for drawing this wording to our attention, we agree it was unclear, and have re-worded this to add clarity. This now says:

“Having reviewed this variation in delivery we were able to identify ways that recommendations were implemented that seemed more likely to be well received, and make the following recommendations on ways to implement guideline-recommended HBC strategies.”

On page 37, second paragraph, the concept of 'action planning' is mentioned for the first time yet forms a major implication in the conclusion.

We followed reviewer comment to present more prevalent themes, therefore action planning, presented in only one paper, is no longer reported in this review. Subsequent areas of the discussion relating to this have also been removed.

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.

We discuss in our detailed response to comment 35 how we drew conclusions about ‘linking’ weight and health. We are happy to further clarify the details of the specific papers. We state on p15, lines 43-45 that “five studies from primary care [10-14] reported that linking health concerns and health behaviours was commonly used to initiate HBCT, and three of these explored this phenomenon in detail [10-12]”. Of the 5 studies reporting its presence as a conversational phenomenon, two mentioned it, but not how it was received. So we could not draw any further detail for these studies. Of three reporting details, Pilnick and Coleman examine this practice in the greatest detail, build a definition, present the range of responses produced following this action, and provided the highest quality evidence. Returning to our description of response relevance (in our response to comment 35), these papers show a range of responses made relevant by ‘linking’: Minimal acceptance (shown in Freeman), minimal acknowledgement, and resistance. The potential for resistance to occur is not removed just because it was not identified in Freeman’s study, and Pilnick and Coleman, and Cohen, show that marked resistance it is indeed a potentially relevant action. Freeman accessed a corpus of 200 recordings in her analysis, whilst Pilnick and Coleman accessed a corpus of 538 for theirs, and Cohen 811. The larger corpora accessed by Pilnick and Coleman and Cohen likely provided greater opportunity to identify and categorise this phenomenon.
We have reworded our conclusion to add clarity that firstly, we are referring to linking as a way to initiate the topic of HBC, and we state it is ‘potentially’ risky, to accommodate the range of potentially relevant responses to linking:

“However, one practice recommended by clinical guidelines; initiating discussions by associating a patient’s health concerns and their health behaviours, is potentially risky and can prompt patients to resist HBC.”

39. 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.

This approach did indeed have some potential for variability. We state that these collaborative question answer sequences “usually led to clinicians inviting patients directly to comment on and agree with proposed HB changes that emerged from this joint enterprise, and patients responded with uptake displays.” Although most data showed uptake displays following this, we felt it was important to mention the conversational potential for response variability in this section; Showing the potential for problems if personalization occurred too early, and the spare evidence that this can be intrusive was important to demonstrate this is not a uniformly successful practice. In line with this comment we have further clarified this in the section “Collaborative HBCT”. We now conclude this section with the following statement:

“However, although this approach had potential for variability, collaborative HBCT was reported to most often result in displays of uptake from patients which likely indicate receptivity to HBC.”

1. Parry RH, Land V: Systematically reviewing and synthesizing evidence from conversation analytic and related discursive research to inform healthcare communication practice and policy: an illustrated guide. BMC Medical Research Methodology 2013, 13(69).