Author's response to reviews

Title: Functions of behavior change interventions when implementing multi-professional teamwork at an emergency department: a comparative case study

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**Functions of behavior change interventions when implementing teamwork at an emergency department: a comparative case study**

*Dear Editor,*

We are delighted for the opportunity to resubmit our manuscript originally submitted December 13th 2013. Thank you to the reviewers for the valuable comments which we believe have been very helpful in improving the clarity of the manuscript. Below is a brief point-by-point response to the reviewers’ comments.

Sincerely yours

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**Reviewer 1**

Overall: Thanks for the kind words about the study; they were much appreciated!

**Major compulsory revisions:**

1. **Description of the Intervention.** After reading the methods section of the manuscript I struggled to understand what the Behavior Change Intervention was for the two groups. As replicability is an important component of intervention design it would be helpful for the authors to provide a more detailed description of the two interventions the groups developed, either as a table or a protocol in the Additional Files. The authors clearly report the deliverability of the intervention via observations and the participants’ opinions of the intervention in the results section but the article could be improved with a more clarity (detailed description) of the teamwork intervention itself.

Authors’ response: Thanks for pointing out the unclarities in the description of the intervention. Following the suggestions from both reviewers, we have made substantial changes to the description of the intervention (p 9), and now refer the reader to a study that goes into substantial detail on the development of the intervention.

2. While the authors are detailed in reporting observation and interview findings, it is bit unclear at this point what was included in the intervention documentation. The manuscripts
would benefit from reporting the authors’ evaluation of the intervention documentation, even if the Documentation was poor.

Authors’ response: The content of the intervention documentation has been clarified (p 12), and all included documents are now described in an additional file (Additional file 3). Also, we have highlighted where the documentation adds to the understanding in the Results section.

3. In the Introduction the authors mention that the D-COM is a model often used to evaluate the Applied Behavioural Analysis that is grounded in Skinner’s Theory of Operant Learning / Operant Conditioning. In the Discussion it is unclear how the findings, as coded onto DCOM, map back to ABA. As someone who is familiar with Operant Learning Theory but not necessarily DCOM, it would be helpful to how antecedents and consequences are reflected in the findings (as described by Capabilities, Motivation, Opportunity, Direction) and what that means for behaviour change. This was partially done in the Discussion, but the authors reflected more about Social Cognitive Theory (anticipated consequences, Self-efficacy and Intention (motivation)) may explain their results and less about antecedents or consequences.

Authors’ response: Thank you for this important comment. We have added the theoretical perspective of ABA and operant psychology throughout the discussion (pp 21-23). This includes detailed descriptions of how operant conditioning, antecedents and consequences can be used to explain behavior change.

4. Have the authors consider the applicability of this type of intervention in other complex clinical settings?

Authors’ response: We have now clarified that we believe the most important application of the results of this study is not necessarily the intervention itself, but rather the process by which it was developed and implemented. We have added a section on this (p 26).

Minor essential revisions:

1. The text formatting of the behaviours measured varies throughout the manuscript. In the Data Collection section of the Methods they are just listed. However in the results they vary from having quotations around them to being italicized, with some having been numbered. This inconsistency makes it difficult to follow the evaluation of the behaviour throughout the manuscript.

Authors’ response: We have changed the text formatting so that the behaviors measured are now italicized and capitalized (pp 11, 14). The staff and management description of important elements of teamwork (p 14) is written as running text and not capitalized or italicized. The purpose of this is to avoid confusing them with the five key team behaviors measured.

2. In the data collection of the Methods the authors refer to ‘degree of performed teamwork behavior’ as ‘implementation fidelity’. I would suggest that what they really mean is ‘delivery fidelity’ as implementation fidelity is a higher-level concept that incorporates delivery as well and other factors (acceptability, feasibility, etc). I also wonder if the ‘degree of performed teamwork behavior’ how the intervention was implemented or if it is an outcome measure or the intervention. This question may be clarified by a more thorough description of the intervention.
Authors’ response: We agree with the reviewer that the terminology in implementation science can be confusing, with different scholars using different definitions. We used the term implementation fidelity in line with Carrol et al. [1], but to avoid confusion we have chosen to omit the term as it is not used elsewhere in the manuscript.

3. In Figure 2: I am assuming that the 1-5 numbering system reflects the behaviour measured. Due to lack of clarity as described in the previous point, this was not initially clear. Either a legend or indication in the text would be helpful.

Authors’ response: Unfortunately, our figure legends were placed before the references rather than after, making the interpretation of the figures difficult if not impossible. We have moved them to after the References and also made further clarification in the legend that the number refers to the number of team behaviors observed in each patient case.

Discretionary revision:

1. The Authors mention the COM-B by Michie et al. in their Introduction and their rationale for using the DCOM for theorizing the effective components of the intervention as the intervention is targeting the organizational level. I wonder if the authors discussed using the BCT Taxonomy as a method of coding/desccribing the intervention, to allow for the intervention to be applied in other complex clinical team environments as well as making it clearer to the reader what the interventions entailed.

Authors’ response: The BCT taxonomy was not discussed as a method of describing the intervention. However, we realize that the description of the interventions was somewhat unclear and have tried to correct this.

Quality of written English: Needs some language corrections before being published

Authors’ response: The manuscript has been edited by a professional, native English-speaking language editor.

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

Reviewer 2

Background # Authors provide a nice introduction and describe existing theoretical behavior change models. However, they missed the “theoretical domains framework”, which is an important, validated framework that should also be included in the discussion; here’s the reference: # French SD, et al. Developing theory-informed behavior change interventions to implement evidence into practice: a systematic approach using Theoretical Domains Framework. Implementation Sci 2012;7:38.

Authors’ response: Thanks for pointing out this omission. The Theoretical Domains Framework is now acknowledged in the introduction (p 5).
Methods: Authors indicate using a comparative case study design, but they should explain what this is and why it was selected as their methodology to answer their questions. They should also explain how the current study is related to the larger project in the ED setting. What is the study period?

Authors’ response: The reasons for choosing a comparative study design are now described in more detail, as is how this study relates to the larger project, and the study period (2010-2012) has been added to the Study design section. All changes appear on p 8.

Teamwork intervention: How was the teamwork intervention developed? Who was involved in this? Was there a pilot? How did they operationalize “changes in work processes that enhanced inter-professional collaboration”?

Authors’ response: Thanks for giving us the opportunity to be much clearer about the intervention. The development of the intervention is now described in more detail (p 9).

Implementation of the teamwork: It is not clear what authors mean by “free to choose behavior change interventions”Authors should explain why teamwork was abandoned after a summer break. This section needs further clarity in terms of what the a priori implementation plan was, what was actually done, and where and when; and what worked/didn’t work at each setting.

Authors’ response: We have made several additions and clarifications in the text, including clarifying that the teamwork was abandoned during the summer due to summer vacation (rather than after; our mistake) (p 10). We also now describe that the implementation plan evolved, and that the details of this process can be found in the Results section, as these are factors that help address the aim of the study.

Data collection: Figures and diagrams are not numbered so it’s not clear which figure authors are referring to. What determined how many observations should be undertaken? (i.e., a total of 76 observations took place at general surgery and 68 at internal medicine) How did they select participants for the semi-structured interviews? (i.e., purposive, etc?) Was the interview guide pilot tested? It appears that they tested a further 7 people after the first 4, which sounds like snowball sampling – this should be indicated.

Authors’ response: The numbering of figures and tables has been revised. We added information on the selection of staff for interviews (in fact, it was exactly as you assumed: purposive and snowball sampling combined), and clarified that the number of observations were stopped after saturation had been reached at each section. (p 11). The interview guide was not formally pilot tested. On the other hand, the interviews were performed by experienced interviewers using an interview guide with broad categories, and there was an openness to adapting and changing the guide if necessary. In this case, no formal changes were made.

Data analysis: It’s not really clear how the data was analyzed: What is a “hybrid thematic analysis”? How was data organized and managed? Was any qualitative software used to assist in the analysis of data (e.g., NVivo)? Who are these “external consultants” who were approached to corroborate the results of the analysis? What documents were used to triangulate the interview and observational data? How was this done?
Authors’ response: We have added information on the analytic method and clarified what software we used (and yes, it was NVivo) (p 12). The external consultants, as described earlier in the text, are engaged in supporting the development and implementation of teamwork. We have added information on the documentation in an additional file (3), as well as information on how the documentation was used, and refer to it explicitly when appropriate in the Results section (p 13).

Results # The results are a bit confusing, and if this work was aimed at decision makers, I don’t see how this inform them – it may help to organize findings according to the observational piece and then the interviews and then focus on the important pieces (presumably from the triangulation) # Results are compared between ED sections, but I didn’t think this was part of the research question

Authors’ response: We have slightly reworded the aim to reflect the comparative nature of the research question (p 7). Concerning the presentation of the results, we have chosen not to rearrange it according to data sources, as we believe this will create some other issues with clarity as there is a large overlap in the information from different sources. Also, we hope that using a truly mixed method approach by combining information from different sources in the same text has the potential to increase the richness of the results. We have clarified how the documentation contributes to the findings, and we hope that with the clarification of the aim, method and numbering of figures, this will be sufficient for the readability of the results. We have also added a section on implications for practice, which we hope will help inform decision-makers (p 26).

Discussion # The discussion refers to the methods as qualitative, but this is not how it is described in the methods – in fact, much of the description here could be moved to the methods # What are the next steps? # What are the implications to practice? How can decision makers apply this knowledge?

Authors’ response: We have chosen to keep the methodological considerations in the discussion, as we believe it is very important to highlight both the advantages and limitations of the methods chosen. With that said, as the reviewers pointed out, the descriptions of the methods needed to be more detailed. Our belief is that the changes following the reviewers’ recommendations have made it clearer that the section in the Discussion section is precisely a discussion rather than a presentation of the methods.

References