Author's response to reviews

Title: Nurses joining family doctors in primary care practices: perceptions of patients with multimorbidity

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Author's response to reviews: see over
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To the BMC-series Journals Scientific Editor,

On behalf of my colleagues and myself, I would like to thank you and the reviewers for the second review of our manuscript “Perceptions of Doctor-Nurse Collaborative Practices by Patients with Multimorbidity: A Qualitative Study” Version 1 (MS: 1611615641344555), submitted for publication to BMC Family Practice. Please note the manuscripts’ revised title: “Nurses joining family doctors in primary care practices: perceptions of patients with multimorbidity” to reflect the reframing of the paper according to one reviewer’s very pertinent suggestion.

Below, you will find a detailed response addressing each reviewer’s comment.

Reviewer: Judith G Baggs

Major Compulsory Revisions

Comment:
1) I do not think this paper is about collaboration. It seems to be about patients’ perceptions of nurses taking on some roles and work previously done by MDs. The definition provided simply indicates both nurses and physicians were “involved.” If the paper were re-framed in this fashion, it would be acceptable.

Response:
We thank the reviewer for this pertinent suggestion for a change that was indeed required. We completely reframed the paper as suggested. The research was more about the presence or involvement of nurses in primary care practices. The intent of the primary care reform is to promote collaborative practices but the arrival of nurses in primary care practices does not create collaborative practices per se as mentioned in our discussion.
Comment:

2) In addition, actual descriptions of what the MDs were doing and what the nurses were doing is minimal. It seems visits often were to one or the other. Were they collaborating? We don’t know, nor, apparently, did the patients.

Response:

In continuity with the previous comment and the complete reframing of the paper, we believe that this comment no longer applies.

Comment:

3) I would suggest that the way to get patients’ perspectives on RN/MD collaboration would be to collect baseline data, establish a genuine collaborative practice, and collect “after” data, or to compare perspectives in a site with and without clearly defined and described collaborative practice.

Response:

We agree with the reviewer. However, no change to our paper is required here. We agree that this study is about patient’s perceptions of the “presence” of nurses in primary care practices.

Minor Essential Revisions

Comment:

1) The authors indicate the sample was selected purposefully for maximum variation, but they need to explain what variables they used to find patients exhibiting maximum variation. Variation in what? Age, sex, amount of experience with nurses acting in roles formerly held by MDs, religion, country of origin, number of co-morbidities, types of co-morbidities, length of time in this practice, time since diagnoses? Any of these could be tapped, but the reader is not told which were.

Response:
The information was made more explicit. Variation was in the model of care in which the patients were involved. Page 4, end of 1st paragraph in “Methods”.

Reviewer: Linda S. Kahn

Comment:
I found in the authors' revisions responsive to my original critique. Based on the authors' responses to revisions suggested by all reviewers, I find the paper much improved.

Response:
We thank the reviewer for this appreciation.
Comment:

[One suggestion: I preferred the original title because I don't like "multimorbid" used as an adjective describing patients. But I will leave that to the authors’ discretion.]

Response:

The title was changed to better reflect the reframing of the paper.

Reviewer: Patricia Hill H Bailley

Major Compulsory Revisions:

1. Is the question well defined?

Comment:

a. The authors’ failure to ground their work in a theoretical context remains concerning (see Morse 2002, 12, p.295). The claim that the identification of a theoretical orientation was not possible leaves the assumptions upon which their work was based outside of the available body of literature and beyond examination by the reader. The values and assumptions of the researchers remain hidden.

Response:

Following the advice of one of the reviewers, we completely reframed the paper. We thank the reviewer for the reference to Morse’s editorial. We now provide further information on the analysis process which corresponds to Morse’s suggestion to “Develop a scaffold”. We indeed realized that we were not solely inductive in our analysis process. We used an analysis schema with a few codes inspired by the literature on Family Medicine Groups (FMGs). Then we added themes that emerged from the data. It is now made more explicit in the text.

Comment:
b. The added context data though helpful, does not adequately address the context of the nurses’ role in this research.

Response:

We believe that the complete reframing of the paper addresses this comment. The focus is no longer on collaboration that would have required more clarification for the context of the nurse’s role as mentioned by the reviewer.

2. Are the methods appropriate and well defined?

Comment:

a. The authors’ decision to limit the description of the analysis process and hence auditability of this work remains problematic.
Response:

Again we refer to the previous response on reframing the paper. The analysis process was described following the first comment by this reviewer. The only missing piece would be the interview guide that is not provided but remains fully described in “Methods” (Pages 5-6). We find that including the interview guide would not add to its own description that is already extensive. Other pieces are working documents not suitable for publication even as appendices. (The conceptual matrix is a ten-page legal format document with annotations).

We strongly believe that our process is transparent at least as much as the majority of qualitative papers and even more so following the first round of reviews.

3. Are the data sound?

Comment:

a) Although the authors have included more information about the participants, it is still unclear whether the data presented are representative of participants across the data base.

Response:

We could have decided to count the occurrences of the themes across the database to address this comment. There are differing views on this in the literature and some confusion in the paradigm involved (qualitative vs quantitative). However we can reassure the reviewer that all participants contributed to the results. Among the three co-authors with an expertise in qualitative research that we have on the team, it has never been a question. Doing something to circumvent the problem at this stage would require massive effort without adding to the results.

Comment:

b) The authors’ perception that the specific experience of nurses’ roles (actions) in the various settings is not central to their interpretation of the data is mystifying. I would argue that the context (specific experiences of nurse/physician interaction) is the essence of the question being asked and therefore an essential component of the context that requires description. I am not suggesting that the authors explain the collaborative realities that exist, but rather clearly define them.
Response:

We believe that the reframing of the whole paper addresses this comment also. It would eventually be interesting to conduct a case study on the perceptions of patients regarding collaborative practices in primary care settings.

Comment:

c) Description of the themes: My major concern with the manuscript as it is presented is the authors’ decision not to move the data from a descriptive to an interpretive level. Qualitative research is an interpretive process; seeing what we do not already know. With the utmost respect, I would suggest that the authors consider Thorne and Darbyshire’s (2005) perspective on “letting the data speak for themselves” (p.1109). These qualitative researchers contend that an approach that failure to analyze the data in this way is an unacceptable “abdication of interpretive responsibility.” As Morse argues: “the interpretive analysis is the creative contribution of our methods, identifying the meaning in what we all see, making the trivial profound and the obvious significant” (Morse, 2009, p. 579). Failure to interpret the data makes your work simply a description of your context. This has I’m sure been an important and useful undertaking for the planning of services in your region. Interpretation of the data, although time consuming and perhaps “risky,” however, is necessary for the transferability of your findings to other similar setting and add to the substantial body of literature in this area. The current thinking about sample size is also bound up in the development of theoretical interpretation. Transferability of knowledge in qualitative research occurs at a theoretical level, making interpretation essential.

Response:

We agree with the reviewer that qualitative research is an interpretive process. We thank the reviewer for the references. However, we appear to see our research as predominantly descriptive but we revised the categories to bring them to a higher degree of interpretation. Even if mostly descriptive, the results provide insight on patients’ perceptions of the fact that nurses are now part of primary healthcare teams. Our opinion is that we brought this research to the level of interpretation that was suitable according to the objective of the study. We leave it to the editors to decide if the level of interpretation is sufficient.
As requested, we have provided a written response for each point brought up by the reviewers. We hope that you now find our revised manuscript suitable for publication in your journal and look forward to hearing from you.

Sincerely,

Martin Fortin MD MSc CMFC