Author's response to reviews

Title: Perceptions of Doctor-Nurse Collaborative Practices by Patients with Multimorbidity: A Qualitative Study

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Version: 3 Date: 26 May 2010

Author's response to reviews: see over
May 25, 2010

Nina Titmus
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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 comments made on 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: “Multimorbid patients’ perceptions of doctor-nurse collaborative practices in primary care.”

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

Reviewer: Carol E Fletcher

Minor essential revisions

Comment:
1.) In the first paragraph under main findings (p.6), the authors indicate that although the 18 subjects all had experience with some level of nurse-doctor collaboration, not all of the subjects had this experience in primary care. Technically the title does not indicate that the study was limited to doctor-nurse collaboration in primary care. However the thrust of the article focuses on primary care. In that case the authors would have been well advised to choose additional subjects whose experiences did reflect a primary care situation until they reached a likely saturation point (18 patients). The authors indicate that 13 of 18 subjects were open to collaboration, but they should also report how many of the 13 or 18 had experienced collaboration in primary care.

Response:
The manuscripts’ title was changed and the number of patients having experienced collaboration in primary care is noted in paragraph 1 of “Results”, page 7.
**Discretionary revisions**

**Comment:**
1.) Page 7, line 4, in the sentence "As the nurse was perceived" substitute because for as.

**Response:**
Done (Now appears on page 8)

**Comment:**
2.) Page 12, first sentence of last paragraph refers to the need for patients to properly understand the role of each provider. The authors might do well to state that, although it was beyond the scope of this paper, it is also important for providers to understand the role of each provider. That does not always seem to be the case as reflected in related literature.

**Response:**
The reviewer makes an interesting and valid point, however, if stated in the text, while recognizing that it goes beyond the scope of the paper, it would still require further development and justification in the text without adding to the issue of patient’s perceptions, therefore the comment is not added to the new version of the manuscript.

**Reviewer: Patricia Hill H Bailey**

**Major Compulsory Revisions**

**Comment:**
1. Is the question posed by the authors well defined?
   a. The authors are addressing a question that has received limited attention in the literature. More background information, however, regarding the specific nature of the collaborative practices between doctors and nurses within the settings included in their study population is necessary. For example, the authors do not describe the context of the three practice settings except to define “collaborative practice” as being “any intervention with a given patient involving at least one family doctor and one nurse in primary care.

**Response:**
The 3rd paragraph of “Background” elaborates more on the definition and question used in this study. In the first paragraph of “methods”, page 4, settings were further described as requested.

**Comment:**
   b. In addition, there is no information regarding the education or experience of any of the practitioners or the existing role norms and/or expectations in these settings. Perhaps Boon, Verhoef, O’Hara and Findlay’s (2004) work would be helpful in delineating the collaborative contexts and representing the nature of the health care provider working relationships in these various settings.
Response:

We thank the reviewers for the reference. We have reviewed it and find that we cannot use the models of team health care practice to delineate the collaborative contexts as the study of these contexts was beyond the scope of this study. However, in the “methods” section (paragraphs 1 and 2) we did further describe the settings, type of collaboration found and the education levels of the professionals involved.

Comment:
2. Are the methods appropriate and well defined?
   a. The use of a qualitative methodology is appropriate for the question being asked. The methods section, however, requires clarification to allow the reader to audit the analysis process. This expanded section should include reference to the questions asked of the data and a description of the initial data codes, subsequent themes and the decisions related to the development of the final themes.

Response:
The methods section was clarified and expanded as suggested.

Comment:
   b. Although the authors state that they developed case summaries and a ‘conceptual matrix’, these do not appear in the manuscript. The authors also do not discuss or illustrate how these processes informed their interpretation of the interview data.

Response:
Case summaries and conceptual matrices are part of the analysis process. They were not added to the text for the sake of conciseness. However, we made significant changes and additions to the analysis section to provide more information on the process. A detailed and in-depth description of each step of the analysis could eventually be the subject of an interesting paper focused on methods in order to illustrate the analysis process.

Comment:
3. Are the data sound?
   a. The initial information presented related to the “levels of experience of nurse-doctor collaboration” is unclear. What does “classify[ing] participants regarding their openness to collaboration” mean within the context of this qualitative study? Did the authors only use data from the 13 “receptive” participants?

Response:
Specific examples emerging from the data are now presented in the “Results” section (1st paragraph) to clarify this point.

Comment:
   b. A thick description of the context in which this study occurred is required. Given that the roles within these practice settings have not been presented, it was not possible to determine if the patients were simply describing traditional role conduct within the context of some institutionalized form of integrated practice.
Response:
The roles of providers as well as the context they operated in are described in the first paragraph of “Methods” for clarification. However the goal of the research was to explore perceptions. We agree that the contexts are factors that could influence experience. Our goal was not to explain the experience with this research.

Comment:
c. The themes identified by the authors are underdeveloped and overlapping. As presented, the limited data presented appear as patterns or categories rather than developed themes.

Response:
This is an interesting remark. We agree with the reviewer that the analysis process could have been more in-depth and could have brought a more developed list of themes but the price may have been an interpretation of the results that could have gone beyond the very ideas expressed by the patients encountered. An analysis with a different focus could have been done. We made a team decision to let the themes emerge in an inductive process and to reflect on the real and tangible content of the interviews. We are now at the end of the process with this submission. Re-doing the analysis at this stage, as suggested by the reviewer’s comment, is simply unfeasible. Therefore, we are resubmitting with humility in the hope that the overall responses to all the comments and the significant changes and re-writing made to the manuscript, contributed to the quality of this paper and that this improved version will be accepted for publication.

Comment:
4. Are the discussion and conclusions well balanced and adequately supported by the data?
   a. Integration of literature related to patient satisfaction and integrated practice models, precursors to this work, would strengthen both the background and discussion sections of this manuscript.

Response:
Nolte and Tremblay’s (2005) work was added in “Background” (Reference #29). These issues were also discussed in the 1st paragraph of P. 14 (4th paragraph of “Discussion”). The authors were consulted and judged that the literature on satisfaction would create expectations that this research could not meet. Therefore we decided not to include more.

Comment:
b. A critical review of the available literature related to collaborative practices is required in the background and/or discussion section of this manuscript.

Response:
The manuscript does refer to literature on patient's perception of collaborative practices which is the heart of our research (page 14, references #52 and #53). We agree that it is limited and that there is an abundance of literature on collaborative practices in general but the latter is beyond the scope of this paper.
Comment:
c. Although the authors did refer to Shaw’s 2006 publication to justify the need for their study, they are remiss in not addressing her subsequent work published in 2008.

Response:
We thank the reviewer for bringing this more recent work to our attention and have updated our references accordingly.

Comment:
5. Are limitations of the work clearly stated?
   a. The authors indicate that although their study was conducted in particular settings with a specific group of patients, they suggest that their finding could be transferable. Failure to provide a description of the context of the provider collaboration settings and practices calls this claim into question.

Response:
Further description of the context and setting were added to the text in the first paragraph of “Methods”.

Comment:
   b. In addition, the authors do not adequately address that the transferability to other jurisdictions in Canada would be confounded by the absence of primary care nurse practitioners in the Quebec.

Response:
Caution in transferability of study addressed in “limits”, paragraph 2, page 14.

Comment:
   c. The authors state that “various triangulation methods” have strengthened this research study. An explicit description of these triangulation methods, however, has not been provided in the manuscript.

Response:
Triangulation methods were described in various sections of the manuscript such as in “Interview guide”, where it states that 2 researchers developed the guide, in “Analysis” where it states that the data came from transcripts and field notes and where different team members examined the data and performed independent analysis.

Reviewer: Judith G Baggs

Major Compulsory Issues

Comment:
The definition of collaboration used in this study to orient participants, "any intervention with a given patient involving at least one family doctor and one nurse in primary care," is inadequate. As the authors note, there is a large literature on collaboration. This does not begin to tap into the work that has been done. What if the nurse simply put the patient in a room, is that being involved? Or if the nurse is getting materials ready for a
procedure to be performed by the physician that might be cooperation but it is not collaboration.

Response:
We recognize that the reviewer speaks to a major compulsory issue and not a revision. The definition that was provided to participants cannot be changed, however further information is provided to clarify the definition provided (Last paragraph of “Background”). The objective was to provide a very general definition to participants in order to elicit salient perceptions and expectations of collaborative practices to avoid suggestion of models or practices.

Essential Revisions

Comment:
The level of the nurses' educational preparation is never defined, except to say that "nurse practitioner in primary care was not an acknowledged profession" when the study was conducted. What was the level of education of these nurses? If baccalaureate or less, then the patient concerns are absolutely legitimate. Nurses at that level should not be prescribing medications. The term "practicalnursing procedures" on page 12 implies to me, as a nurse in the US, that these were practical nurses, as we call them licensed practical nurses (LPNs). They would have 1 year or very practical skills-based education, and would be even less qualified than baccalaureate prepared nurses.

Response:
Further detail on education preparation and roles of providers is given in the first paragraph of the methods section.

Comment:
The interview guide is not included. It is impossible to tell why patient responses went so strongly to the issue of nurse competence.

Response:

Comment:
What did nurses actually do in this setting?

Response:
Further detail on roles of providers is given in the first paragraph of the methods section.

Comment:
It is not clear when in the interview the definition was provided.

Response:
Comment addressed at the end of the first paragraph under “Process”, page 5. The definition was provided to all participants prior to starting the interview questions, when the interviewer presented the study and consent form.
The term "innovative" is used in the paper (p. 3), but depending on what nurses do in this practice, little that is innovative appears. If the nurses happened to actually be nurse practitioners, publications on the NP/primary care physician practice date back at least to 1984. (e.g., Physician-nurse practitioner interaction patterns in primary care practices. Lamb GS. Napodano RJ. American Journal of Public Health. 74(1):26-9, 1984 Jan.)

Response:
We acknowledge the opinion of the reviewer. The sentence was changed to innovative “in this context”, and a clarification of the settings of the study is provided in “Methods”

Minor issues.

Comment:
Page 6, paragraph 2. It is not clear what "The chronic conditions listed" means. Are they the patients' chronic diagnoses?
Response:
Comment addressed, changed for “The chronic conditions/diagnoses listed in the patients’ files…” (First paragraph in “Results”)

Reviewer: Linda S. Kahn

Minor Essential Revisions

Comment:
1. In the Methodology section of the manuscript, the recruitment of patients needs to be clarified. Was a chart review conducted in the various practices to identify patients with 5 or more chronic diseases? Or, did physicians provide the researchers with a list of patients that fit the inclusion criteria?
Response:
Comment addressed in paragraph 1 of “Process”, page 6.

Comment:
2. In the Main Findings section, the second sentence should read: Table 1 presents participant demographic characteristics.
Response:
DONE (Section now called “Results”, page 7)

Comment:
3. Figure 1 is confusing and should be revised/simplified or deleted altogether. Three suggestions are as follows: (1) The authors might want to simply list the major themes and specific themes in a one-column table. (2) Construct a simple two-column table with the themes down the left, with the major themes in bold and specific themes listed beneath (similar to current figure). The right column could contain illustrative quotes. (3) A third option is to make a similar two column table, with the number of times the specific theme was identified in the transcripts in the right-hand column.
Response:
Data from figure incorporated into a table (Table 2) as suggested in (1).

Discretionary revisions

Comment:
1. Flesh out the first paragraph of the Background -- with the assumption that not all readers are familiar with the multimorbidity literature. Specifically, the authors should consider providing brief examples corroborating the last two sentences. For example: "a number of problems in following up such patients have been identified, including ____________ and ______________." The same approach should be used with the last sentence in paragraph 1: "in the presence of multi-morbidity, providing care to patients becomes much more complicated, and may involve complex medication regimes, multiple laboratory tests, as well as referral and follow-up with specialists" [I am ad-libbing here.....] The point is to illustrate the demands of caring for patients with multimorbidities experienced by primary care providers who also are under time constraints.
Response:
DONE: “Background”, paragraph 1, page 3.

Comment:
2. The authors might consider adding a sentence to the background regarding the shortage of primary care physicians in the US and Canada. In the US, health service providers often mention "doctor extenders:" nurses or physicians assistants (PAs) that can be deployed in place of or as extensions of physicians for routine patient care, including follow-up care. [Do the Groupes de Medicine de Famille represent a Québec version of the "medical home model" currently being promoted in the US?]
Response:
Sentence added in paragraph 2 of “Background”.

Comment:
3. In the Methodology section, the researchers mention an interview guide that they developed. They might want to consider including this guide as an Appendix to the manuscript so that other researchers, conducting similar projects, might adapt it (or build on the present study).
Response:
The interview guide was not added in appendix; however, a summary of issues addressed with patients is presented in the “Interview guide” section of “Methods”.

Comment:
4. Consider adding the following to the limitations section:
   • Relatively small sample size
Response:
DONE (Paragraph 6 of “Discussion”)
• Emphasize that the study is limited to a province that is undergoing changes in the development/expansion of the nurse practitioners profession. I differ with the authors regarding the importance of the regional component here. As noted in the methodology section the data was collected before "nurse practitioner" became an established profession in Québec. This is a very important distinction between Québec and other parts of Canada and the US.

Response:
The emphasis was made in paragraphs 2 and 3, page 14.

Comment:
• By definition, all subjects in the study had multimorbidities and had "various experiences of collaborative practices." This is already noted in the limitations. However, I would also add that the limitations of the study did not allow for inclusion or comparison of non-complex patients, without multimorbidities: younger patients who might only see their health care provider once a year or for minor ailments.

Response:
We did not add this suggestion to our limits as it was not the aim of our study. Our objective was to “to explore the perceptions and expectations of patients with multimorbidity towards collaborative practices between doctors and nurses in primary care settings”. The study sampled patients with a high exposure to the health care system and chronic diseases, not for minor ailments. Page 5, paragraph 2.

Comment:
5. Consider adding to discussion: Future research might consider replicating this study in Ontario, parts of the US, and in Québec today to assess the acceptability of nurse practitioners as part of the primary care team.

Response:
Suggestion added to last paragraph of “Discussion”, page 15.

Comment:
6. Some suggested edits:
Page 4: "Combining perspectives of applied anthropology in primary care clinical practice, clinical research questions patients’ viewpoints to better inform practice." I would change the sentence to read: Combining perspectives of applied anthropology in primary care clinical practice, clinical research elicits patients’ viewpoints to better inform practice.

Response:
Done

Page 6: "Nevertheless, we were able to classify participants regarding their openness to collaboration and found them receptive for the majority (13/18).” I would change the sense to read: Nevertheless, we were able to classify participants regarding their openness to collaboration and found the majority to be receptive (13/18).
Done

**Comment:**
Page 11: "Among other conditions for optimal doctor-nurse collaboration that emerged in the analyses, we should mention the nurse’s proper use of health care protocols and the employment stability of healthcare professionals." Do the authors mean "employment stability" or continuity of care provided by healthcare professionals?

**Response:**
The authors mean “employment stability”.

As requested, we have provided a written response for each point made 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