

Author's response to reviews

Title: The Use of Telehealth for Diabetes Management: A Qualitative Study of Tele-Health Provider Perceptions

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Author's response to reviews: see over

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Dr. Robbie Foy
Associate Editor
Implementation Science

Dear Dr. Foy,

Attached please find the revised manuscript for our manuscript entitled “The Use of Telehealth for Diabetes Management: A Qualitative Study of Tele-Health Provider Perceptions” (MS 1578033387114102). Abstracts of editor and reviewer comments are noted below in bold, followed by our comments and appropriate references to the revised manuscript.

We would like to call particular attention to item #11 (under responses to Marie-Pierre Gagnon) where we suggest a modified title to the originally submitted manuscript, which is noted in quotes in the first sentence of this letter.

Editor Comments

1. Firstly, like the reviewers, I enjoyed reading this well presented and clearly written manuscript. The reviewers suggest adding some detail in some places and reframing some sections, e.g. focusing on issues around the implementation of a relatively immature intervention. I'd be grateful if you consider their suggestions and modify the paper or justify not following their suggestions.

We appreciate both the positive feedback and suggestions for revision. With regards to focusing on issues related to implementation, we have more clearly articulated our suggestions for steps needed for successful implementation, as shown in the discussion section (pp. 16-19).

2. Secondly, we always try to strike a balance between ensuring that our articles report methods and findings in sufficient detail (an advantage of an electronic journal being that we have more space) whilst keeping the content relatively concise. We also appreciate that qualitative research articles often need to be longer than quantitative reports. However, we usually suggest a limit of around 5000 words to manuscripts and I think yours is currently around the 5500 mark. I'd be grateful if you could consider how to cut out some words without losing meaning. For example, I thought the description of previous research in the introduction could be more concisely written.

We think you will be pleased to see that the manuscript word count is now 4,744 words (background through conclusion sections, not including abstract or references), and so now is well within your guidelines. As you suggested, we were able to make the description of previous research more concise without losing meaning.

3. I also thought, like one of the reviewers, that the telehealth intervention and how it is meant to work could be described in the methods section - thereby allowing more economy of words later on in the results section. These are only suggestions and you may come up with a better way of tackling this.

We carefully considered whether or not to move the description of the intervention to the methods section. However, we realized that 1) One of our research questions concerns how the system is used for diabetes management; hence, details on the intervention, including enrollment and use of the system, are most appropriate for the results section; and 2) including all of the details on system use would make for a very long methods section. Therefore, we have added a few more details on the MMD system in the methods section (under “setting” page 6). However, for the reasons cited above, most of the details on MMD processes, including referral, enrollment, and the alert system, remain in the first section of the results (pp 8-9).

Reviewer Comments: Marie-Pierre Gagnon

1. I am not convinced that the study offers a comprehensive analysis of the barriers and facilitators of using the MMD system...the discussion should be reframed in order to emphasize the challenges of implementing an innovative technology for the self-management of diabetes patients and to identify emerging solutions to overcome these challenges...

We appreciate the helpful comments from Ms. Gagnon on these issues, and have carefully considered them in our revisions. For details, see our response to editor review comment #1

**2. The authors indicate that 10 nurses, from two facilities, were identified through a purposive sampling method. This deserves more precision:
-On what purpose were those nurses selected?
-What is the total population of homecare nurses working in these facilities?
-What was the proportion of nurses from each site?
-What was the proportion of nurse practitioners vs. other nurses (what type of nurses?)**

We have provided more detail on our selection of the study sample and the non-probabilistic sampling methodology employed for this study (p. 6). However, as this study is specifically focused on persons who were currently providing tele-health services, and was not specifically designed to be a study of nurses per se, we do not think that it is necessary or helpful to provide details on nurse staffing configurations at the two sites.

3. I have difficulty understanding why the authors talk about “patients use of the MMD” while they report nurses’ perceptions of patient use. To reflect patient use, it would have been necessary to interview patients directly! The question #3 and the

corresponding results section should thus be presented as: nurses perceptions on patient use of MMD.

We have chosen to use a slightly different heading that we think emphasizes that the results are based on provider, not patient, perceptions (see heading entitled “Telehealth Provider Observations of Patient Use of the MMD System” on p. 13)

4. In qualitative research, a generally accepted method to ensure internal validity is the saturation of data. The authors do not question the appropriateness of their sample size to represent the various points of views in the study population. Other methods to increase validity, such as participants checking of the analyses, could have been explored. This should be discussed as study limitations.

We have noted the small sample size and non-representative nature of the facilities under study limitations, pp. 18-19.

5. In the first section of the Results, there is no need to specify that the three first questions are discussed separately, since the results associated to the fourth question are presented in the same manner than for the previous three questions.

These sentences have been modified (see p. 8)

6. A paragraph on the study’s strengths and limitations should be placed separately at the end of the Discussion section and emphasize some considerations in ensuring rigor in qualitative research.

See revised limitations section, pp. 18-19.

7. Also, if the authors decide to discuss the specific challenges and issues surrounding the implementation of the MMD, the Discussion section should be reframed according to emerging themes, such as “Patient selection and participation”, “Increased nurse workload”, “New nurse roles and responsibilities” “Technical issues”... Thus, the first three paragraphs of the Discussion should be integrated to the subsequent sections and the limitations moved at the end.

The discussion section (pp. 16-18) has been re-framed according to these recommendations.

8. The last two paragraphs of the Discussion also appear unnecessary given that the study focused on one tool, the MMD, and that the findings do not mention any of the other tools discussed there.

We have removed mention of other tele-health tools.

9. Also, a comparison of the study findings with the international literature on home telecare tools for diabetes self-management is missing.

We were not sure exactly what literature or tools were being referenced in this comment, and discussion of tools other than MMD seems to be beyond the scope of the present study.

10. The authors say they used “structured interviews” (p. 6) and then, “semi-structured interviews” (p. 7). Since the “interviewers varied the interview format as needed”, they should use the wording “semi-structured interviews”.

This has been corrected.

11. Also, it is necessary to uniform the naming of the participants. Sometimes, the authors employ “nurses” and other times “care managers”. [Related comment] To offer a more representative title and ensure greater uniformity in the naming of participants, I would suggest the authors to change “clinician” for “nurse” in the title.

Both of these comments refer to the appropriate terminology to use for our respondents. Although all of our respondents were nurses, they were selected not on the basis of their nursing background, but because they were persons currently providing tele-health services using the MMD system. The findings are relevant not just for nurses, but for others (social workers, physicians, and other health care professionals) who may wish to use these systems in the future. Therefore, so as to not unnecessarily restrict the reading audience to persons with an interest in providing nursing services, we have chosen to use the term “tele-health provider” in the title and in text, except when specifically referring to the respondents’ nursing background.

12. The description of how the system is introduced to patient home presented on p.11 is not clear: patient takes unit home, then nurse brings the device... What do the unit and the device refer to?

Both of these terms refer to the MMD device, and so we have clarified by using the term “device” consistently in the manuscript.

13. Last but not least: What type of diabetes patients was targeted? I suppose that they are non-insulin dependant (Type II) diabetics, but I have not seen it clearly indicated in the text.

We have clarified that the population in the VHA consists primarily of persons with type II diabetes (see last paragraph of background section, page 5).

Reviewer Comments (Tracy Finch)

(note: no major compulsory revisions were requested by this reviewer—all comments below refer to “minor essential revisions”)

1. The introduction provides a good account of the relevant literature and makes clear the purpose of the paper. This could however, be enhanced further with

reference to emerging research relating broadly to the field of telehealth on barriers and facilitators to implementation and ‘normalisation’, particularly since this broader research resonates well with the findings the authors report in the paper.

We agree that mention of this literature is very appropriate, and so have referenced literature related to the normalization of telemedicine in the introduction (see pp. 4-5).

2. The description of the MMD Programme appears insufficient for the reader to interpret the findings that follow, since telehealth services such as this can be configured in vastly different ways. I would advise that further details be included here about exactly what the system requires the nurse to do (make judgements about appropriateness, configure profiles for particular patients?, frequency of checking the system? Etc) and what it requires the patients to do (i.e. do they initiate contact or respond to prompts, how often, what measures do they have to take and send electronically?). Some of these details become apparent through the reporting of the results but additional detail here would make the rest of the paper clearer for the reader.

See our response to editor comment #3

3. Given the small number of participants, I would suggest including some additional detail under ‘sampling’ concerning how many facilities were included (later on in the paper referred to as two facilities), and whether the ten participants were all the nurses who may have potentially used the service (i.e. were there other nurses not identified for the study because they didn’t use the service?).

The sampling section (p. 6) has been clarified to provide some of these details, with limitations of the sampling method noted under study limitations, p. 18

We appreciate the helpful reviews and the opportunity to further revise the manuscript. We look forward to hearing from you.

Sincerely,

Faith Pratt Hopp, PhD