Author’s response to reviews

Title: Feasibility of referral to a therapist for assessment of psychiatric problems in primary care – an interview study

Authors:

Agneta Pettersson (agneta.pettersson@gmail.com; agneta.pettersson@sbu.se)
Sonja Modin (sonja.modin@gmail.com)
Henna Hasson (henna.hasson@ki.se)
Ingvar Krakau (ingvar.krakau@ki.se)

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Author’s response to reviews:

Dear Editor,

Thank you for considering our manuscript for publication in BMC Family Practice and for the helpful suggestions for improvement. We have revised the manuscript and hope that it now is acceptable for publication.

We realized that an underlying problem with the manuscript was that we did not clarify that there is a second, recently published part of the study [11]. That publication focused on experiences of the MINI itself. This manuscript describes experiences of the diagnostic process. The manuscript now is cleaned from information that governs MINI as such. At the same time as we have added some information from [1] in order to increase the understanding.

Below, we have responded in detail to the comments of the two reviewers.

Reviewer 1

Q1. In the Table 1, it is not clear for me if it concerns "patient" or "FP". Can you precise and verify the appropriateness between the text and the table?

Response: We apologize, the wrong numbers were used. It is now corrected.
Q2. The difference between the PHCC 1 and 2 are not so clear. What were exactly the difference and similarity of the two centers? I think that a table/schema will be necessary.

Response: Thank you for the idea, we have added a table (Table 1) describing the two PHCCs.

Q3. It exist big difference concerning the profiles of the FP (some are young doctors and some with a lot of year of experience). That is an important limitation.

Response: Yes, FPs with different levels of experience participated. What is important is that the profile of participating doctors does not differ from those that did not participate. We perceive that the profile is representative for Swedish PHCCs. We have stressed this in the manuscript (Highlighted line 128).

Reviewer 2:

Q1. The study aims to assess the feasibility of an alternative diagnostic pathway for depression and anxiety in primary care. The authors state that accurate diagnosis can improve the chances of adequate treatment and it would be useful to see them link this to outcome data?

Response: Yes, this is a crucial aspect. However, we have considered that it was more linked to the MINI itself than to the diagnostic process. Thus, it was dealt with in the previous publication [1].

Q2a. It would also be useful to see the authors draw on some international literature that discusses the use of patient centred measures for depression and anxiety in primary care.

Response: This was discussed in the previously mentioned publication.

Q2b. Also, in other international primary care systems where similar systems already exist, for example within the Improving access to psychological therapies programme (IAPT) in the UK.

Response: Yes, we agree, this gives a broader picture. We have added this in the Discussion (highlighted, lines 398 and 442) and we feel that this also covers comment 16.
Q3. At the end of the background section the aim is stated as identifying the barriers and enablers of task shifting diagnosis to therapists in FP. However, elsewhere the aim is explained using different terminology. I would like to see consistency and for the title to better reflect this aim.

Response: We agree, the title is changed, and we have clarified that the study had two aims (highlighted line 94).

Q4. Use of the COM-B model is appropriate. Reference to some other literature in this area where the model has been used would be useful.

Response: The number of publications that have used the COM-B model has accelerated during 2018. We have added three references, which could be of interest for primary care, in the section Strengths and limitations (highlighted line 464).

Q5. Line 130 - table number is incorrect. Table 2 should be referenced here.

Response: We apologize, the numbering is corrected.

Q6. I have some concerns as Table 2, which outlines the fidelity of each site to the intended process clearly describes how the process was not clearly followed by PHCC 2. I wonder then whether this system is feasible if indeed it cannot be standardised across two sites...

Response: Thank you for the comment! We have not described the prerequisites for the study sufficiently. The overarching aim of the full study was to investigate whether the MINI was feasible in primary care. As we knew that time pressure could be a barrier, FPs could choose to refer to a therapist or make the assessment themselves. At PHCC1 there was a common decision to try the referral route only. At PHCC2 the FPs were supposed to conduct the MINI themselves, but they had the opportunity to refer to a therapist. We have revised the text, highlighted lines 146-150, as well as Table 3 (highlighted parts).

Q7. Why was the training process different at each site? Did the lack of one of the training sessions at PHCC impact on their lack of fidelity to the system?

Response: Here the manuscript needed cleaning. The first training session dealt with the MINI, and was not related to the referral process, which is the subject of the present article. We have shortened and clarified the text (highlighted lines 152-156).
Q8. Line 155-158 - Rigour and commitment to the process would have been improved had all therapists received the same one-day training on the MINI, was there an expectation that this should happen?

Response: As above, the manuscript needed cleaning. The training was strictly on how to work practically with the MINI and how to interpret the result. The study plan with the diagnostic process was discussed at meetings where the therapists were present. The paragraph about therapist training has therefore been omitted.

Q9. Given the list sizes of the practice and the high prevalence of depression and anxiety it seems a small number of eligible patients over a 1-year period.

Response: We agree. However, there are several reasons for the low number. First, not all FPs participated. Second, the FP should consider which patients would benefit from an assessment. Here, the focus groups indicated that many patients were not considered for various reasons. It was not possible to gather data on reasons for non-referral in a systematic way, which we now have addressed as a limitation (highlighted line 476).

Q10. It would be useful to see the wording used for the VAS satisfaction measure.

Response: This has been added (highlighted line 204).

Q11. Were interviews and focus groups transcribed verbatim?

Response: Yes, and this has been added (highlighted line 180).

Q12. What were the inclusion criteria and who applied it? It is not clearly stated anywhere.

Response: The inclusion criteria for the patients are added (highlighted lines 130-134).

Q13. It would be useful to see more information on the patient sample, perhaps a descriptive table including diagnoses, severity etc.

Response: We understand that this could be an advantage for the reader. However, we only have data on diagnoses for the full data set (110 patients) and this is presented in the previously
mentioned article [1]. For the participants in the research interviews we did not ask about their problems.

Q14. Also any data on how the MINI assessment matched up with FPs provisional diagnoses based on patient stories.
Response: Yes, this is an interesting issue that we coped with in the previously mentioned article [1].

Q15. There is little use of the qualitative data in the findings section. I would like to see quotes to back up each of the key points made.
Response: Thank you for the proposal. We have included a table with sample quotations for the COM-B components (Table 5). This means that most quotations in the text itself are omitted.

Q16. The authors state that, to their knowledge, there has been no study of referral to a therapist for diagnosis. This may well be the case. However, there are similar systems running in other countries such as the UK for assessment and treatment of mental health problems in primary care and I feel that their relevance needs to be discussed here. Also, the authors should look to literature on collaborative care for depression and consultation liaison models.
Response: We think that the actions listed for comment 2b also takes care of this comment.

Q17. Some limitations of this study are considered but there are some other issues which seem to be missed, such as the lack of fidelity to the referral model in one PHCC.
Response: We have added a paragraph about the low number of patients included. Concerning lack of fidelity to the intended referral process in PHCC2, we have revised the manuscript to clarify that referral for assessment only was not mandatory (highlighted lines 140-152 and Table 3).

We are looking forward to receiving your response concerning this revised manuscript.
Best regards,
Agneta Pettersson
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