Author's response to reviews

Title: The care of patients with subthreshold depression in primary care: Is it all that bad? A qualitative study on the views of general practitioners and patients

Authors:

Matthias Backenstrass (matthias_backenstrass@med.uni-heidelberg.de)
Katharina Joest (katharina_joest@med.uni-heidelberg.de)
Thomas Rosemann (thomas_rosemann@med.uni-heidelberg.de)
Joachim Szecsenyi (joachim_szecsenyi@med.uni-heidelberg.de)

Version: 2 Date: 30 July 2007

Author's response to reviews: see over
Comments to the Reviewers:

To Mr. Cuijpers:

Major compulsory revisions:

1. A major problem of the study is that the GPs selected the patients. The authors acknowledge that in their Discussion but they do not oversee the consequences of this. Possibly, a majority of patients with subthreshold depression are not diagnosed by their GP and possibly these patients have different views about their GP. This study does not rule out this possibility in any way. It would have been much better to screen some patients in the waiting room and select the ones with subthreshold depression.

   ♦ The Reviewer is completely right, when emphasizing the significance of the selection bias. It would indeed have been better to screen patients in the waiting room in order to rule this bias out. Therefore, we discussed this shortcoming of the study even more detailed and critically under the limitations in the discussion section (page 20). We also interpreted the significance of the results with more caution.

2. The citations are too elaborative. I would suggest that the authors reduce the text of the results section to half of what it is now. The points they make can also be made without all this text.

   ♦ The results section and the citations were shortened as far as this was possible (e.g. two rather long GP citations for diagnosis were deleted, see page 8/9/10; One patient citation for diagnosis was deleted, see page 15, and one for treatment, see page 16.) However, since this was a qualitative study, the citations need to be elaborative in order to validate the authors’ statements and assumptions. Also, the scientific value of a qualitative study depends on quotations. The other reviewers did not mention the length of the text as a problem.

3. The English language is very bad and should be edited by a native speaker

   ♦ The language has been thoroughly edited by a native speaker.

4. The authors should describe the limitations of the study much more elaborately. I already mentioned the fact that only patients who were diagnosed by the GP were included. Another important limitation is that this study was conducted in a small region in Germany, and may not be generalized to others countries. For example, St. John’s wort is not routinely prescribed in many other countries outside Germany.

   ♦ The limited generalizability of the results to other European countries indeed is an important point, which was not sufficiently discussed. We included this limitation in the discussion section (see page 20) and discussed this using the prescribing of St. John’s wort as an example. However, with a radius of 300 km, the region in which we conducted the interviews was not that small.
5. The relation between the GP and the patients is stressed as an important issue in several parts of the paper. This is of course an important aspect, but should not be overestimated. Especially because here only patients were selected by the GP. It is no wonder that these patients stress their good relation with their GP. But what about the other patients with subthreshold depression who were not diagnosed by their GP or who do not have a good relation.

- Again, the reviewer is right when indicating that the GP-patient-relationship should not be overestimated because of the selection bias. We included this limitation of our conclusion regarding the relationship in the limitations part (see page 20) and also discussed the significance of the selection bias in more detail in the discussion section (see point 1 of the comments, see page 20 in the manuscript).

6. On the second page of the discussion, at the top, the authors say that the validity of the diagnosis strategy of the GP is confirmed by the fact that the patients had some form of current or past depression. First, this is not specified in the results section of the paper (only current diagnoses are reported). Second, 5 out of 24 patients (more than 20%) had no diagnosis at all. That is not just something.

- Indeed our presentation of the sample was not elaborated enough which might have been confusing. According to Mr. Jorm’s suggestions (see comment Nr. 5 to Mr. Jorm), we now present only the data of those 20 patients of which we could analyze the tapes. Therefore, we included a table with a diagnostic sample description of the 20 patients and also stated that the (now) four patients not fulfilling criteria for a current mood disorder at the time of the interview had fulfilled them in the last three months (see Subjects, page 5).

7. At the third page of the discussion section, the authors say that they could not confirm the finding from other studies that GP’s make no difference between the treatment of subthreshold and threshold depression. They can not make such a statement without examining also how patients with threshold depression are treated.

- We deleted any statements concerning a comparison of subthreshold and threshold depressive patients. The reviewer is right, no such conclusions can be drawn from our data.

Minor Essential Revisions:

1. The abstract overestimates the results of the study, when it is said that past criticisms must partly be turned down. That can not be done on the basis of a qualitative study aimed at generating hypotheses.

- We changed the conclusion drawn in the abstract and stressed the fact that the results generate hypotheses.
2. In the introduction, the authors should mention that minor depression is a diagnostic category in the ICD, which is used in primary care more than the DSM

- Indeed, not the DSM-IV but the ICD-10 is commonly used in European primary care. However, in order to comply with the standards of the international discussion on subthreshold depression, we decided to make diagnoses according to the DSM-IV. Moreover, as far as we know, minor depression does not exist as a category in the ICD-10. ICD-10 differentiates three episodes differing in severity. However, there are differences between the mild depressive episode of the ICD-10 and the minor depression of the DSM-IV.

3. More information should be given about the diagnostic status of the patients. Was only the mood disorder section of the SCID taken or also other sections? Were there no patients with dysthymia? Did not one of the patients have an anxiety disorder or a personality disorder?

- In order to give more information on the diagnostic status of the patients, we included a diagnostic description of the sample in the results section (see table 1). We also elaborated on the diagnostic proceeding with the SCID in the methods section (see Instruments, last section, page 6).

To Mr. Jorm

Major Compulsory Revisions:

1. The language needs editing by a native speaker.
   - The language has been thoroughly edited by a native speaker.

2. Some of the limitations are well described in the Discussion. However, there are others (i) the sample of the GPs is from Germany and practice there may not be typical of other countries, e.g. the preference for herbal treatments; (ii) it would have been useful to have a comparison group of patients with major depression, otherwise it is impossible to know how specific some of the findings are to subthreshold depression (e.g. patients’ and GPs’ treatment preferences).
   - The limited generalizability of the results to other European countries indeed is an important point, which was not sufficiently discussed. We included this limitation in the discussion section (see page 20) and discussed this using the prescribing of St. Johns’ wort as an example.
   - It would have indeed been useful to directly compare subthreshold and major depressive patients in order to find out how specific our findings are. We included this point in the limitations in the discussion section (page 21, last paragraph). See also point 7.
3. What does it mean to say the GPs were selected “at random”? What exactly does “doctors cooperating with the department” mean? This is worth knowing to judge how biased the sample was.
   - We elaborated on this in the Subjects section (see page 4)
4. Under “Subjects”, it is unclear who the “they” were in “they were given a list.
   - The “they” has been substituted by “The GPs” (see page 4)
5. Given that only 20 out of 24 patients had their data included, it would have been more appropriate to give the characteristics of the 20 rather than the 24.
   - Yes, it would be more appropriate to only give the characteristics of the 20 patients whose data were included. We therefore changed this in the text (see under subjects) and also included a table for better sample description.
6. I could not understand the point being made in the sentence “Moreover, since most GPs treat these patients with counseling…” Surely if the GPs made physical diagnosis, they would not have used counseling as a major mode of treatment, so the diagnosis does make a difference.
   - We changed the sentence for better understanding. What is meant here is that whether or not the GP communicates the depression diagnosis to the patient does not influence the treatment strategy (see page 18, third last sentence).
7. The conclusion that “the result that GPs tend to use therapeutic talk…is opposed to studies…” does not seem justified, given that the present study did not examine MDD patients as a contrast.
   - The reviewer is right, this conclusion was not justified on the basis of our data. We deleted it.
8. In table 1 (now table 2), there are no numbers given in brackets for the reasons under “Not satisfied with diagnostic proceedings”
   - We added the corresponding numbers

To Mrs. Maxwell
Discretionary Revisions:
1. My only real concern with the paper is the applicability of some of the findings to other international contexts (e.g. prescription of herbal treatments). The authors do not discuss their findings outwith the cultural context in which they are generated.
   - This is an important point which we missed to discuss. Therefore, we included this limitation in the discussion section (see last third of page 20, see also comments to Mr. Cuijper, point 4 and to Mr. Jorm, point 2)
2. The diagnosis of depression is mostly the result of a “negotiation” between doctor and patient. Many patients often reject this diagnosis. The authors do not appear to consider this,
either in their approach to recruiting and the questioning of respondents or in the presentation of the findings. For example, the section where they discuss that it was not important to be given a diagnosis, to me reflects attempts by patients to distance themselves from the diagnosis.

- This is also an important point which we did consider but did not present. Therefore, we included this issue using one patient quotation (see page 13, end of “Diagnosis” section) and also mentioned this in the discussion section (see 18, fourth last sentence).