

Author's response to reviews

Title: Psychiatric services in primary care settings: a survey of general practitioners in Thailand

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Author's response to reviews: see over



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The Editor
BioMed Central Family Practice

Re: MS: 2083961765956832 'Psychiatric services in primary care settings: a survey of general practitioners in Thailand'

Dear the Editor:

Thank you very much for giving us the opportunity to revise our manuscript. We would also like to thank the reviewers' for giving us a very helpful comments. The BMC suggestions for formatting have been reviewed in detail and minor changes have been incorporated.

The revised manuscript has addressed the reviewers' comments point by point as follows:

Reviewer 1: Steve Kisely

General

1... the discussion should be expanded to include international comparisons of primary care in countries other than the UK. The absence of such a discussion is surprising given the authors themselves cite a reference to the WHO study of psychological problems in general health care settings, which although 10 years old, would have a lot of this detail (Ref #6). This would give the work more context.

Response: We tried to include data from developing countries in the discussion as suggested. We have done a new review of the literature and captured more references by trying to use articles from developing countries as many as possible.

2. I also wondered why the authors used DSM rather than the ICD-10 Primary Care Version, which would seem to be a more appropriate classification. In this regard I was surprised that GPs were unfamiliar with mixed depression and anxiety (Para 3, page 10) since this is a common presentation in primary care.

Response: We agree that ICD-10 Primary Care Version is more appropriate for primary care patients. However, we used DSM in our study because it is the psychiatric classification system that is taught in the medical schools in Thailand. Therefore, GPs in Thailand are unfamiliar with mixed depression and anxiety which is the term classified in ICD-10.

More details have been added (4th paragraph, p 4 and 2nd paragraph, p 7).

3. The reference (#9) refers to psychiatrists – did it include GPs too?

Response: We have deleted this reference (Udomratn P: Mixed anxiety and depressive disorder : An illness that psychiatrists should not overlooked. Journal of the Psychiatric association of Thailand 2000, 45:99-109) as it refers to Thai psychiatrists that many of them are still not familiar with the term mixed anxiety and depressive disorders.

4. .. One remarkable feature was the relative absence of newer psychotropics such as SSRIS or atypical antipsychotics. The authors only mention at the availability of a generic form of fluoxetine. What about the other SSRIs? Are there barriers to receiving psychotropic medication in Thailand?

Response: Details on newer psychotropics in Thailand have been added (4th paragraph, p 7).

Major Compulsory Revisions

5. More details on the Thai health system - are they free at the point of delivery? Are there charges for prescriptions?

Response: We have added details on the Thai health system that has changed since health reform in 2001. At present, under the universal coverage scheme, patients pay 30 Baht (US\$ 0.70) per visit or per hospitalization at point of service, through a contract model. Details have been added (2nd paragraph, p 3).

6. More international comparisons to give the work more context

Response: We have made international comparisons where possible, for example: prevalence of mental disorder in primary care in developing countries (5th paragraph, p 6), alcohol problems (last paragraph, p 6), depressive disorders in primary care (2th paragraph, p 7) and the benzodiazepine use problem in primary care (2th paragraph, p 8). We would like to do some more comparison however the manuscript has been already considerably long.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: No

Reviewer 2: David Armstrong

General

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. The paper needs some language corrections before being published. The required changes are too numerous to describe.

Response: We have sent the manuscript to a native English speaking colleague for language revision.

2. The paper was not well referenced. It would be useful if findings from similar studies from countries at least at a similar stage of economic development were provided and compared. As it is, bald statements such as GPs thought 10.8% of their patients had psychiatric problems is difficult to interpret without some reference point. There is mention of page 9 that prevalence was "similar to studies in other countries" but no references were provided (and the next sentence in fact contradicted this by pointing out that depression was more common elsewhere).

Response: We have done a new review of the literature and captured more references by trying to use articles from developing countries as many as possible. We have revised the background section to reflect the picture of our country as a developing country.

We have made a comparison between the perception of psychiatric illness among Thai GPs and the prevalence of mental disorders in primary care from other developing countries (5th paragraph, p 6).

For depression, we have revised the mentioned section as suggested (2nd paragraph, p 7).

We did not provide data on prevalence rates of depression and anxiety from each country because there were wide variations among them and probably not the main point here.

3. Statistical tests are described in the methods section but only p values are given in the text. With this sort of sample size many associations will be significant; what we need to know is the effect size. For example, there is a reported association ($p=0.01$) between numbers of patients seen and reported prevalence of psychiatric diagnoses. This association needs a scatter plot and/or a Pearson r so the reader can see how strong this association really is.

Response: We have revised our statistical analysis and report as suggested. The association between the number of patients examined per day (<20, 21-30, 31-40, to >70) and reported prevalence of psychiatric diagnoses was analyzed using ANOVA (3rd paragraph, p 5). We have also included consultation rate as an independent factor in a multiple regression analysis using prevalence as the dependent variable. However, the result showed no statistical significant.

4. The most surprising finding of the paper was that despite GPs with high consultation rates reporting more psychiatric diagnoses, time was seen as the biggest barrier to making diagnoses. This was later discussed rather lamely as a mystery. What is needed is more (statistical) analysis to examine this association in more depth. For example a multiple regression using prevalence as the dependent and consultation rate and time barriers as independent variables would allow better understanding of this problem. Indeed, most of the results in this paper would benefit from more detailed multivariate analysis as at the moment the paper is descriptive rather than explanatory.

Response: We have reexamined the data as suggested. Results of this new analysis have been shown in the results section (4th paragraph, p 5). A multiple regression analysis using prevalence as the dependent variable was done as suggested (6th paragraph, p 5 and 1st paragraph page 6, Table 2). There was a weak association between consultation rates and psychiatric diagnoses and barrier problems. Since this study was an opinion survey, it may not reflect the real-world practice. We would like to focus our discussion only on the outstanding results. Therefore, we decide not to elaborate this issue in our manuscript (6th paragraph, p 6).

5. There is also a clustering problem in the data as several respondents might be drawn from the same community hospital “ but sorting this out might be a statistical step too far unless the authors have expert statistical advice. (I think the authors can get away without doing this though.)

Response: In hospitals with several doctors, we sent only 2 copies of questionnaire to each hospital (1st paragraph, p 4).

6. It seems that a fifth of the respondents were “specialists” rather than GPs. Should these respondents have been included in the analysis? Or should the title (and theme of the paper) be something different?

Response: We agree to exclude these respondents from the analysis. We have added sentences : “Sixty seven of the respondents (15.4%) took further special training in various fields such as medicine, surgery and orthopedics. We excluded them from our analysis because their training might affect their perception and practice. As such, a total of 367 GPs were included in this study” (1st paragraph, p 5). Data of GPs who did not take further special training were used in the study accordingly.

7. There were some results given at the beginning of the Discussion section that should be in the Results section (and again included in a more detailed multivariate analysis).

Response: These sentences were deleted as it was the reiteration of data in the result section.

8. In the discussion the authors claim that Thai GPs should be using SSRIs rather than amitriptyline. This remains a contested question across much primary care and it is not

sufficient to simply assert this is what should happen. Adequate reference to this debate should be provided if this recommendation is to be sustained.

Response: We have added sentences and references to support our contention (1st paragraph, p 8). References 25-31 have been added.

9. The authors also advocate that patients are screened for psychiatric disorder by nurses (using a Thai developed instrument). Again I think we need more justification for this recommendation (which is, again, not based on any of the study findings). The key question is whether screening would result in more benefit than harm for patients. Again there is a considerable literature on this topic and the authors need to engage with this before offering such apparently evidence-free policies. Subject to satisfactory solutions to the above problems I think the paper could make a fair contribution to the primary care literature.

Response: We totally agree with the reviewer. This section has been revised to suggest the significant role of non-medical health workers (line 2-14, p 9).

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests

Thank you for your consideration. We look forward to hearing your reply.

Sincerely yours,

Manote Lotrakul, M.D.