Author's response to reviews

Title: Prevalence of mood and anxiety disorder in self reported irritable bowel syndrome (IBS). An epidemiological population based study of women

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Author's response to reviews: see over
Response to reviewers’ comments

Comments in italics, our responses in plain text.

Reviewer’s report
Title: Prevalence of mood and anxiety disorder in self reported irritable bowel syndrome (IBS). An epidemiological population based study of women
Version: 1 Date: 10 May 2010
Reviewer: Vladeta Ajdacic-Gross

Reviewer’s report:
This is an interesting epidemiological paper on the association between mood and anxiety disorders and IBS. The data came from a medium sized epidemiological survey with a sample size with some more than 1000 (female) participants. The analysis was well designed with respect to the state of the research and included extensions to differentiate the results (current vs. lifetime occurrence of psychiatric diagnoses and IBS; comparison of IBS with diabetes and hypertension; exploratory analysis with specific psychiatric diagnoses). A critical reading of the manuscript revealed a few major and several minor suggestions.

Major issues:
• The manuscript did not explain why this study is based merely on a sample of women. To my understanding, the Geelong Osteoporosis Study includes males and females. On page 16, last paragraph, the authors remarked that the “inclusion of women only obviously restricts the generalisability of our findings”. In fact, diverging findings in men and women might provide new clues to a better understanding of diseases which burden women more than men.

Unfortunately, data for men are still being collected, and will not be available shortly. The study was originally set up for studies of osteoporosis, which is why it was originally focused on women.

• In contrast to the analysis design, the interpretation of the results appears to be quite unidimensional. Firstly, it misses that not only psychiatric disorders but also IBS may represent heterogenous conditions. Secondly, the interpretation does not consider that there are hypothetically several different interrelations and causal paths, which may coexist. This may be due to the heterogeneity of the diseases, but also to different temporal sequence patterns. To my opinion, the conclusions are somewhat rash. I also wondered that the authors did not include hypotheses, which might be derived from their recent paper on nutrition and affective disorders (Felice N. Jacka et al.: Association of Western and Traditional Diets With Depression and Anxiety in Women. Am J Psychiatry 2010; 167:1–7).

Minor issues:
We agree, and we have thus changed the interpretation to also include the perspectives here highlighted. The heterogeneity of IBS (which we are unable of capturing) is included in the limitations. We have also included the nutrition hypothesis and the reference (Jacka et al. Am J Psychiatry 2010). Our sample is generous has rich data on nutrition, enabling us to examine IBS in relation to nutrition.
to explore if this might be a causal pathway between IBS and common mental disorder. No such association was detected. We have mentioned this in the methods under potential confounding factors.

**The sample of this study is larger than in most other studies examining IBS and psychiatric disorders. However, with respect to the heterogeneity of these diseases even much larger samples would be needed. Thus, this study is now midway.**

Yes, we agree, and we hope and believe there will be more studies available soon.

**The paragraph on page 5/6 ("Despite many studies ...") was difficult to read and might need some reworking: a) the studies with negative results were quoted twice; b) the sentence next to last introduced a fourth issue (or should alternatively be moved to the second issue two)**

We have changed these sentences in accordance with the comments by the reviewer.

**Dose-response relationship (page 14, paragraph 14): I guess that a nested model (differentiating the subgroups 1-3 vs. 4) would work at least as well as a linear or curvilinear relationship. Thus, the analysis paves the way for two quite different interpretations.**

We agree with the reviewer that it does seem to be 1-3 vs 4 association in the figure. However, the figure might be too rough (without confidence intervals or any other analysis to back up the statement of dose response association). Therefore, we have added confidence intervals to the figure, and increased the number of categories from quartiles to deciles for GHQ. Pearson correlations across the GHQ deciles were 0.14 and 0.16 (both p<.001) for current and lifetime IBS respectively. Excluding the two last deciles (testing for dose response associations within sub-clinical symptom levels of common mental disorder) revealed weaker, but still significant associations r=0.07 (p=.040) and r=0.09 (p=.008) for current and lifetime IBS respectively. These are better arguments for the alleged dose response association, and we have changed the text accordingly. Finally, we also examined and found curve-linearity in this association. This is added to the methods, the results section, a new figure has replaced the original one.

**The exceptional finding, that there was no association between IBS and bipolar disorder might turn out to be revealing in future studies and thus deserves eventually some more emphasis in the Discussion.**

We have expanded on the issue. The most likely explanation is that this is a type 2 error, as the total number of cases with bipolar disorder was n=24 only, whereof only one had current IBS and two lifetime IBS. However, if we added only a couple of more cases with IBS and bipolar disorder, it would be consistent with the other conditions. We have expanded on this issue in the discussion.
• Association of IBS and GHQ-12 (page 17): from a methodological point of view, this association is not surprising. It might be explained by the fact that summary scores often work better than specific variables in association analyses.

We agree, but the psychometric reputation of screening instruments is still not by far as good as that of clinical diagnosis. We have changed the text a bit, and given examples from other areas (prediction of mortality and disability) to illustrate this issue further.

• Page 18, paragraph 3: This paragraph is misplaced between paragraphs discussing the associations. Eventually, it could be omitted.

The paragraph is omitted.

• Table 1:
The first row has no labels and no overall figures in the first row. Please indicate also that the %-values pertain to the rows.

Additions according to advice made.

• Table 3:
The first row has no labels. Please indicate also that the %-values pertain to the rows.

Additions according to advice made.

• Table 3:
The title announces “lifetime IBS, hypertension and diabetes” but only the IBS figures were shown. Please provide the lacking figures.

We are sorry, this was a cut/paste error. We have no figures for lifetime (but not current) diabetes and hypertension, simply because these conditions usually are chronic. The title is changed.

• Tables 4 and 5:
To what do the %-values pertain?

Lables are added to the text.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: I declare that I have no competing interests.
Reviewer's report

Title: Prevalence of mood and anxiety disorder in self reported irritable bowel syndrome (IBS). An epidemiological population based study of women

Version: 1  Date: 26 April 2010  
Reviewer: Bodil Ohlsson

Reviewer's report:

General comment

This is an interesting report dealing about an actual area. I think that the study is well written in all aspects without one. I do not understand if the subjects are interviewed personally or by questionnaires sent home to them. Please clarify this in the methods by one heading called study design. If the subjects are interviewed personally, this must have taken a lot of time. Were the subjects contacted by mail or phone? Please clarify how it practically was performed.

Subjects were interviewed personally (and yes, it took a lot of time). They were contacted by mail and phone. A detailed description is now added to the text.

As the psychosomatic aspect was not examined, the authors can not propose IBS as a psychosomaric disease. The psychiatric diseases may be secondary to chronic abdominal pain. We do not know what come first, the IBS or the psychiatric problems.

Yes, we completely agree it is not possible to determine causality in this association on the basis of the current study. We have added to the interpretation of the finding that abdominal pain or IBS preceded the psychiatric condition.

May be this information can be extracted from the interviews.

Unfortunately, it is not possible to extract this information from the interviews.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: I declare that I have no competing interests