Author's response to reviews

Title: Patterns of physical co-/multi-morbidity among patients with serious mental illness: a London borough-based cross-sectional study

Authors:

Charlotte Woodhead (charlotte.c.woodhead@kcl.ac.uk)
Mark Ashworth (mark.ashworth@kcl.ac.uk)
Peter Schofield (peter.1.schofield@kcl.ac.uk)
Max J Henderson (max.j.henderson@kcl.ac.uk)

Version: 2
Date: 29 April 2014

Author's response to reviews:

RE: Manuscript 4789763301157841: Patterns of physical co-/multi-morbidity among patients with serious mental illness: a population-based cross-sectional study

Dear Reviewer,

We appreciate the thoughtful positive review and have amended our paper in light of the comments made. In response to the specific points:

Responses to reviewer Comments

1. It looks like the physical health conditions were selected based on QoF reporting (rather than Read Codes) or some other analysis of routine primary care data? To some extent, this might explain some of the differences found in this study compared to our study (Smith et al, 2013, BMJOpen). It would be good for the authors to address this point more fully in their discussion. For example, could the fact that the QoF is incentivised have affected recording for certain conditions?

We accept the reviewer’s point but unfortunately we were not able to include non-QoF based outcomes in our study with the available database extraction. As such we are unable to determine whether there were disproportionate differences in the prevalence of physical outcomes that are/are not incentivised.

However, we might suspect that if incentivising were to have an effect this would act to increase the recorded prevalence of outcomes in our study. As we found that all except two outcomes were less prevalent in our sample than in the study by Smith et al. based on Read codes, such bias would have led to an underestimation of the difference in the two samples and thus a more conservative interpretation of the difference between the two samples.

Future work with a more detailed database extraction will include non-QoF outcomes and we agree that exploring whether incentivising influences recording
is useful. The discussion of the paper has been amended under ‘comparisons with other research’ to mention the impact that incentivising may have on recording.

2. Could the authors state the 5 conditions covered by QoF but which were not available for this study (and why)?

The extracted data used for the current study was originally extracted for the purposes of another study specifically looking at cardiovascular outcomes, so did not include all available QoF outcomes.

The following statement has been added in the methods section under, ‘physical health conditions’, p5. “Clinical areas for which there was no available information were: rheumatoid arthritis (introduced in the 2013/14 guidelines), osteoporosis, learning disability, and palliative care, peripheral arterial disease. These outcomes were not included in the database extraction used here.”

3. The prevalence of diabetes in SMI patients of 12.9% is interesting because it is higher than in many other studies (usually about 8%). Again, could this be something to do with the QoF? Clearly ethnicity could play a role but the OR for diabetes in SMI versus non-SMI remained significant even in the fully adjusted model.

We agree that this is an interesting finding – as noted in point 1, we would expect QoF incentivising to increase recording; however, all outcomes except diabetes and CKD were lower in our study than the most comparable study by Smith et al. using Read codes.

Ethnicity is likely to play an important role, our data reveals considerable variation in diabetes prevalence by ethnicity and interestingly, this variation is greater among the SMI group. In the SMI group, the prevalence of diabetes ranged from 5.1% (‘other’ ethnicity) to 22.2% (Asian/Mixed Asian) (data not shown). As noted in the paper, the higher prevalence of diabetes in our study is likely due to the greater proportion of diabetes among those ethnic groups which make up a substantially greater proportion of the Lambeth population than in other population-based studies and the strength of association between SMI and diabetes is likely linked to these differences.

However, as noted by the reviewer, despite considerably attenuating the effect size, the association between SMI and diabetes remains after adjusting for ethnicity (and obesity). Furthermore, the prevalence of diabetes in our sample overall (4.2%) is lower than that for Lambeth as a whole from Public Health England published information using data from the Health Survey for England (6.4%).

Together, these factors suggest that there may be under-recording of diabetes in our sample overall; that there may be an interaction with ethnicity; and, that other factors linked to SMI status are associated with SMI. Although outside the scope of the current paper, future work with more detailed database information using linked primary and secondary care data from Lambeth will aim to detangle some
of these issues, for example the role of antipsychotic prescribing.

4. The LDN database also has prescribing information. It would be of interest to see how adding antipsychotic medication to the models might influence the results, e.g., with respect to diabetes.

The authors very much agree that it is of interest to look at anti-psychotic prescribing. The main aim of this specific paper was to examine patterns of morbidity by SMI status so we feel that including antipsychotic prescribing information is beyond the scope of the paper. Further, antipsychotic prescribing may not be well coded in primary care; subsequent work with more detailed database information using linked primary and secondary care data will aim to explore this issue in depth.

5. The main aim of this paper was to compare patterns of multimorbidity between SMI versus non-SMI. The authors acknowledge that they only had 12 conditions available, so it might be more accurate to say that, from the 12 conditions they were able to study, the overall pattern of multimorbidity was similar between SMI and non-SMI.

Thank you, we agree with this point and have amended the conclusions to include the phrase “among the 12 health outcomes included in the current study” (under conclusions, paragraph 1, p13); and the abstract to include “among those conditions considered” (under abstract, ‘conclusions’, p2) in the revised manuscript accordingly.

6. Very minor point: Table 4 is missing the note for what the fully adjusted model (2) includes.

Thank you, this has been amended in the revised manuscript (Table 4, p20).

7. Another minor point is that these data are from a very specific locality (Lambeth) – perhaps Lambeth should be in the title rather than ‘population-based’.

Agreed, the title has been amended to include the phrase ‘London borough-based’ rather than ‘population-based’ in the revised manuscript.

Editorial comments

1) Figure 1 - the lines could be replaced by bars if the authors accept.

Certainly, Figure 1 has been amended to replace lines with bars (Figure 1, p19).

2) I have seen the data was extrapolated for the computerised medical records for almost all GP practices in Lambeth and probably this effort does not require Ethical approval. However, I am kindly asking the authors to make an explicit statement on this.

Thank you, the following statement has been added to the revised manuscript. “Ethical approval: National Research Ethics Committee: concerning the use of
general practice data and practice specific quality indicators. Chairman’s action.”
(Above results section, p6)

3) The external validity of the study has not been discussed in the manuscript. To what extent the study findings extrapolate in other districts in the UK?

The strengths and limitations section of the discussion acknowledges the generalisability of the study findings beyond Lambeth as a limitation of the study (p13).

Again, we thank you for your thoughtful comments on the paper and hope that the above responses are to your satisfaction. We can confirm that all authors have read and agreed the amendments to the revised manuscript.

Yours Sincerely,

Dr Charlotte Woodhead, Dr Mark Ashworth, Dr Peter Schofield and Dr Max Henderson.