Author's response to reviews

Title: Chronic disease prevalence and care among elderly in urban and rural Beijing, China - a 10/66 Dementia Research Group cross-sectional survey

Authors:

Zhaorui Liu (zhaoruiliu@gmail.com)
Emiliano Albanese (emiliano.albanese@iop.kcl.ac.uk)
Shuran Li (lishuran@bjmu.edu.cn)
Yueqin Huang (dengy@mail.tsinghua.edu.cn)
Cleusa P Ferri (cleusa.ferri@iop.kcl.ac.uk)
Fang Yan (emillyyf@163.com)
Renata Sousa (renata.sousa@iop.kcl.ac.uk)
Weimin Dang (dangweimin@bjmu.edu.cn)
Martin J Prince (martin.prince@iop.kcl.ac.uk)

Version: 2 Date: 2 June 2009

Author's response to reviews: see over
Dear BMC Public Health Associate Editor,

Thank you for your letter. We very much appreciate the comments and suggestions of you and the reviewers and have revised our paper accordingly. Our responses and changes are summarised below:

**Associate Editor comments:**

1. **Provide clear research question(s) in the Introduction.**
   We now linked the summary of the background literature more explicitly to certain unsettled research questions. Our aim and objectives are now clearly listed, meeting the Referees’ comments and suggestions and consistent with the descriptive nature of our study.

2. **Several sub-sections in the Methods include numbered lists (bullets). Please reconsider and use less of such lists.**
   We have converted several of these bulleted lists to free text, and have reduced the number of categories and sub-categories of assessment in the methods section.

3. **Why Poisson regression? Why not logistic regression?**
   We acknowledge that in cross-sectional studies logistic regression is often used to generate odds ratios as adjusted estimates of association with exposures of interest. However, when comparing prevalence across groups, prevalence ratios (PRs) can be directly calculated using Poisson or log-binomial regressions. Odds ratios tend to overestimate the prevalence ratio when the outcome is common (as was the case for several of our analyses), and can produce inconsistent confidence intervals and uneven results when adjusting for confounding. Hence prevalence ratios are generally more appropriate, and Poisson regression working models are a robust method for generating them.

4. **The number of Tables is too large and Table 6 is really too big. Please consider a smaller number of Tables that equally well bring the message!**
   We have deleted original Table 3 “Use of health care services in the three months preceding the interview” and provided the relevant data in the text.

   We have also removed the stratification by dementia status from Table 6, simplifying it considerably, and provided some details of the differences by dementia status in the text. We are keen to present these analyses, since they indicate the importance of dementia as a determinant of disability and dependency, and justify the decision to control for dementia status, as well as for age and gender, when comparing informal care and carer burden between rural and urban sites.

5. **Check the English.**
   Other than a few typographical errors, we did not find any problems. Both the original
version and the current revision were extensively checked and edited by the last author, a native English speaker.

6. Include more context information in the background section of your abstract, in addition to the aims of your study.
Done.

7. Include a 'Competing interests' section between the Conclusions and Authors' contributions. If there are none to declare, please write 'The authors declare that they have no competing interests'.
Competing interests are now disclosed.

Reviewer 1 (James Laditka)'s comment
Provide comments about hypertension prevalence differences between urban and rural for dementia prevalence future trends.
In accordance with his suggestions we have now cited studies indicating that hypertension is a prospective risk factor for dementia, and our own review of global prevalence in which we had hypothesised that improvements in prevention and control of cardiovascular risk factors/disease might indeed modify the projected increases in numbers of people with dementia in LAMIC undergoing the health transition.

Reviewer 2 (Bei Wu)’s comments
1. Given the study was conducted in Beijing, it might be more appropriate to revise the title using the term such as older residents in rural and urban Beijing, instead of “in urban and rural China.”
We have changed “China” to “Beijing, China”.

2. In the abstract, the authors stated that “chronic disease diagnoses were more prevalent.” Given the nature of the self-reported survey, I would not use the wording “diagnoses.”
Actually, our point was that self-reported impairments were more common in urban Xicheng, whereas formal diagnoses made on the basis of clinical examination/structured interview had a similar prevalence between the two settings. To clarify this point we have changed the sentences in the abstract to read ‘Chronic diseases were more common in Xicheng, when based on self-report rather than clinical assessment. Risk exposures were more common in Daxing.’. In contrast to many other surveys of older people in China, our survey of chronic diseases was only partly based on self-report, enabling us to draw this important distinction.

3. More discussion on the reason(s) that the prevalence of self-reported impairments are much lower in rural areas, and self-rated health was much better. Citing the reason as under-reporting or selective mortality may not fully explain this
finding. Actually we cited under-reporting only with reference to self-reported clinical diagnoses, rather than self-reported impairments and subjective health. We expect that you are referring to the observations of Amartya Sen and others on the discrepancies between self-reported health and objective morbidity, and, by extension the limitations of self-reported morbidity as an index of need. This is an important point, and we have now cited and quoted his seminal BMJ editorial on this point,

4. Caregiver stress need more elaboration, perception may be different in urban and rural China
We have now provided the requested references regarding the cross-cultural validation of the Zarit Burden Interview. We agree that it would be appropriate to acknowledge the possibility of measurement bias between urban and rural settings, and have now done so. Nevertheless, circumstantial evidence reviewed in the discussion makes the higher levels of strain in urban Xicheng appear quite plausible.

5. For example, the authors used the term “social protection.” Does this refer to informal long-term care arrangement? Similarly, it is unclear to me what the term “social care” (table 6) means.
A fuller definition of “social protection” is now provided in the background section of the paper. It refers in fact to the raft of informal and formal (i.e state) provisions to protect the most vulnerable. The concern for China, born out by some of our findings in both Xicheng and Daxing, is that coverage of state provision of, for example, pensions and health insurance may not be increasing at the necessary pace to keep step with diminishing availability of financial and practical support from family. ‘Social care’ refers to non-medical care, which in the Daxing and Xicheng is synonymous with informal care. We have clarified this in a revision to the title of Table 5 (original table 6) “social care” with the more explicit and descriptive “Levels of disability and dependency, informal care arrangements and carer strain (among those identified as needing care), by site”.

6. More detailed description on the data source would be helpful.
We have made relevant changes in the text as appropriate and given further details, and quoted our Protocol Paper (previously published in BMC Public Health).

7. How was disability measured? What does the WHODAS II stands for?
We now provide fuller details and added ‘WHODAS II’ in full before introducing the scale.

8. The limitations of the work are not clearly stated when comparing urban and rural sites.
We have clarified further the particular nature of the Daxing site, and the reasons why it cannot be taken as representative of rural sites in China more generally.

Reviewer 3 (Iris Chi)’s comments
1. Research questions should be clearly listed.
Our aim and objectives are now clearly listed.

2. How's the background information linked to the current study is unclear.
We now linked the summary of the background literature more explicitly to certain unsettled research questions. Our aim and objectives are now clearly listed, meeting the Referees’ comments and suggestions and consistent with the descriptive nature of our study.

3. How can this study contribute to the existing knowledge?
See 5. below

4. More detailed description on the study design and measures should be provided.
We have made relevant changes in the text as appropriate and given further details, and quoted our Protocol Paper (previously published in this very Journal).

5. What is the focus of your study? This paper is trying to cover three huge research areas. Given the page and word limitations, neither of these areas can be fully developed in this manuscript.
The foci for this study are the rural/urban differences in health status, health service utilisation, and informal care. For older people, these three elements are very much inter-related, and we feel that they are served better by being presented in a single manuscript. Other studies that have addressed just one or other of these elements in isolation have failed to provide a coherent overview of chronic diseases, their consequences and their management, and how these might vary between urban and rural populations. We have done our best to improve all sections of our manuscript, to simplify the presentation of descriptive data, and to sharpen the discussion.

6. There is great variation in rural areas in China. Chinese Census divides rural areas into 4 different levels. Which level is your sample belong to?
We have added relevant sentences in the text. In brief, our sample Daxing, one of the rural areas of Beijing, can be considered as Class I (the richest one).

7. Too many tables. Urban rural bivariate comparison table is not necessary. It does not add to our knowledge.
We have cut one table (original table 3), and considerably reduced another (original Table 6). We cannot see how a paper of this kind could be published without a full description of the sociodemographic and health characteristics of the samples in the urban and rural sites, and this is more conveniently provided in tables than text. One of the advantages, presumably, of an online journal is that there is less of a premium on space than with print journals?

8. Paper should be carefully proofread and edited before submission.
Done
Thank you again for all the comments and suggestion. If you think we need add more information, please let us know.

Best Regards,

Dr. Zhaorui Liu
Dr. Emiliano Albanese
Prof. Yueqin Huang and
Professor Martin Prince
for and on behalf of the 10/66 Dementia Research Group