Author's response to reviews

Title: Burden and Correlates of Non-Communicable-Diseases among rural residents: A cross-sectional study in Hebei, China.

Authors:

Junjun Yang (wx2yyji@163.com)
Wenya Yu (yuwenya2002@sina.com)
Qiang Zhou (739976299@qq.com)
Tanmay Mahapatra (drtanmaymahapatra@yahoo.com)
Lei Chen (gdpuchenlei@163.com)
Yiqiu Li (lijiqiu1974@163.com)
Xiaoyan Zhang (zhanxiaoyan1970@yeah.net)
Sanchita Mahapatra (raysanchita@yahoo.co.in)
Yuying Yan (yanyuying1965@126.com)
Weiming Tang (weimingtangscience@gmail.com)

Version: 3
Date: 1 May 2015

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

Part 1, Response to reviewer 1

Major Compulsory Revisions

1. This manuscript describes a cross-sectional survey to estimate the burden and identify the correlates of NCDs in rural China. 6003 rural residents aged 15 and above from 36 villages of Shijiazhuang in Hebei province of China completed a self-administered questionnaire. The sample is large, but the authors did not introduce how or based what the counties, the villages were selected? And how these participants were recruited? What random sampling methods were used in the study? If this was a representative sample of rural residents from these sites? These should be clarified under Methods.

Response: We completely agree with the concern expressed by the reviewer. In the revised manuscript, we have presented the step by step recruitment process in details mentioning the sampling design so that the random selection method of the counties, villages and thus a representative sample of the residents of those rural areas are clarified.

2. Please clarify what the response rate was of your study, as it is unclear if 6003 rural residents were recruited, why the total numbers of certain variables were not consistent, such as “Exercise in last 12 months”, “Weight change in last 12 months” in Table 1?
Response: We are sorry for not being clear about this previously. In the revised manuscript, we have added the response rate for our study. The non-consistent numbers in table 1 and 2 were due to the different number of missing responses in different questions, which we have clarified under Table 1 and Table 2.

3. I wonder why so these age ranges were used. According to the definition of United Nations and China, the persons aged 65 and above are classified as the elderly. As we all know, the elderly are more vulnerable to chronic diseases, why did not you analyze the prevalence of NCDs among the elderly group?

Response: We are sorry for this confusion. As per the definition of United Nations and China, persons aged 65 and above are classified as the elderly. However, our study was not only focused on elderly people, instead, we surveyed everyone aged 15 or more. Thus we stratified our participants into four age groups. Age is a well known risk factor for NCDs, thus we did not put much attention on the prevalence of NCDs among different age groups. Instead, while determining the adjusted association between other socio-behavioral risk factors and risk of occurrence of NCDs, we adjusted for age which is a known confounder for the association between other socio-behavioral risk factors and risk of occurrence of NCDs.

4. The author mentioned in the Method that “simple ordinal logistic regressions were performed for univariate analysis [Odds ratio (OR) and 95% CI]. However, there were no such results in the manuscript.
Response: We are unsure about the reason for this confusion. As mentioned in the manuscript, and highlighted in the revised manuscript with the reviewer, the crude ORs in Table 3 for measuring the strength of association between non-communicable-diseases (NCDs) and their correlates, are the results from simple ordinal logistic regression results of univariate analysis.

5. The Abstract is reasonable. Unfortunately there are problems with some expressions. Besides, the conclusions should be rewritten taking into account the nature of the study.

Response: We are sorry for this issue. We have modified the abstract in the revised manuscript accordingly to solve the linguistic issues in the expressions and we have re-written the abstract to taking the nature of the study in to account.

6. As this study was performed in 2010~2011, please also report in the discussion whether there have been any changes since then, or the refreshed data is required.

Response: As per the suggestion of the reviewer, we have discussed this issue in the limitation part of the revised manuscript.

Minor Essential Revisions

7. The aim of the study was not clear, and this should be clarified under Introduction.
Response: We are sorry for not mentioning it clearly in the previously submitted version of the manuscript. In the revised one, we have modified the last paragraph of introduction part to make the aims more clear.

8. Page 10, Paragraph 1: This is difficult to understand. 5th para 1th sentence onward: This should be written more clearly.

Response: According to the suggestion of the reviewer, the sentence has been rewritten.

9. There are a number of sentences with grammatical errors which should be corrected by a first language English speaker or copy-editor.

Response: We are sorry for these grammatical errors. One co-author of this study whose first language is English has corrected these grammatical errors.

10. Results vary in presentation and in tables with one or two decimal places – be consistent, I would use.

Response: We are sorry for this inconsistency. As per the suggestion, we used one decimal place throughout the revised manuscript.
Part 2, Response to reviewer 2

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

1. It seems inacceptable the omission of Confidence Intervals in what constitute the more solid results (prevalence estimations). Confidence Intervals are present only in relation with odds ratios whose use in rather conflicitive because of the cross-sectional nature of the study and the temporal ambiguity that prevented the authors from drawing causal inferences, as they themselves pointed out in their discussion.

Response: We completely understood the concern of the reviewer. In the revised manuscript, we have added 95% Confidence Intervals for the prevalence of diseases.

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

2. In the section named “Recruitment” there are a couple of little problems:
   a) We can read: “In order to get precise and valid estimates from a representative sample, at the design phase, we decided to select 1 person per1000 residents of rural areas of Shijiazhuang using stratified sampling with probability proportional to size.” However, the strata mentioned there are not mentioned later. How was this stratification?
b) In the next sentence, it is said: “According to the 2010 population census of China, there were 7,355,238 persons living in the rural areas of Shijiazhuang, and about 5.5 million of them were aged 15 years or more[11] (Reference).” I don’t understand what does the allusion to “Reference” mean.

Response: We are sorry for these mistakes. In the revised manuscript, we have clarified the details about the strata and the stratification methods. Further, we have deleted “Reference”, which was inadvertently present within the text.

3. When informing about “Correlates of NCDs”, the authors use the abbreviation OR sometimes and COR in other occasions. Since “COR” is not defined, I guess it means “crude odds ratio”. In any case, a uniform notation would be mandatory.

Response: We are agreed with the reviewer. In the revised manuscript, we have mentioned and “Crude OR”

4. I do not think it is convenient to repeat results in the discussion section. For instance, the entire first paragraph of this section could be eliminated.

Response: As per the suggestion of the reviewer we have deleted the first paragraph of the discussion section in the revised manuscript.

5. I suggest to reconsider the use of the expression “our sampling technique …”
was planned for recruiting a representative population” (page 19), which sounds rather rare.

Response: As per the suggestion of the reviewer, we have rephrased this expression in the revised manuscript to make it more meaningful.

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

6. The authors include explicit p values in the foot of table 3, and implicitly when they say more than one time that results lacked statistical power. It seems that the later resource is the way they have chosen to explain the fact that nullity is within the interval confidence. Concerning the first of these things, I strongly suggest reconsidering use of p values. They are not necessary at all. The authors do not need to use p values (presently undervalued) which are considered inconvenient by most of the present standards, as it easy to corroborate looking at International Committee Of Medical Journal Editors Uniform Requirements For Manuscripts Submitted To Biomedical Journals. Some recent papers and guides are recommended to be read by the authors in this concern. For example, the following four:

• Nuzzo R. Scientific method: statistical errors. P values, the 'gold standard' of statistical validity, are not as reliable as many scientists assume. Nature 2014, 506:150-152.


Response: We understood the viewpoint expressed by the reviewer. As per the suggestion, we have removed P-values from the revised manuscript.

7. In relation with the second issue (mention to small power), it is certainly astonishing to read it, since the sample size was huge. The problem with this statement is that it is true; more specifically, it will always be true when no significant results are obtained, and, therefore, sterile. It is not fortuitous that it is impossible to come across with the opposite (and equally tautological) statement: “A significant difference between groups has been detected; however, perhaps with a smaller sample size, this difference would have proved to be not significant”. Such a double standard is itself an unequivocal sign of the ritual employment of NHST.

Response: We understood the point the reviewer is making. Thus as per the comment, we have removed these kind of statements from the revised manuscript.
Part 3, Response to reviewer 3

A. Major Compulsory Revisions

1. Methods section:

1.1. The authors have missed describing the study setting, and other important
descriptions of the source population, which could have provided a general
picture of the livelihood and lifestyle of the study population. Thus, such a
description should be provided.

Response: We are sorry for these mistakes and in the revised manuscript, we have addressed all
these as suggested.

1.2. The first paragraph of the methods section tells that the authors “decided” to
pick a sample of 1 per every 1000 population of #15 yrs old. And the authors
have repeatedly claimed about the representativeness of the selected sample.
Can you provide any justification for why the 1 per 1000 sampling approach was
adopted?

Response: We are sorry for these confusions. We have modified this paragraph, and explained
why and how 1/1000 people were recruited.

1.3 In general the sampling technique requires further clarification. The authors
mentioned that stratified sampling technique was used. If so, what was the
stratifying variable, and how many units were there in each strata…?

Response: We agree with the reviewer regarding this shortcoming. We have revised the write up regarding sampling technique, and mentiond that we stratified the population at county and village level, and reported the proportion of the units that was selected for sampling.

1.4. In line 6-9 (the methods section) the authors mentioned that PPS sampling method was used. However, the administrative structure of the study area/province is not provided, which makes it understanding the sampling procedure difficult. Thus, the sampling scheme requires further clarification, with what constitutes a sampling unit in each sampling step, and how many strata were included in the selection.

Response: We are sorry for this mistake and in the revised manuscript we have provided description of the sampling technique and mentioned the sampling steps.

2. Is there any justification on the validity of the method how mental state was measured?

Response: We agree with the reviewer that the validity of the estimation method of the mental state of the participants were not well-established, thus, we did not count mental health problems among the NCDs. We have discussed this in the revised discussion part.
3. Potentially, the use of many inter-related predictors in the logistic model may inflate / deflate the estimates due to multicollinearity. Was there any effort the authors did to identify the effect of multicollinearity in the multivariate analysis?

Response: We are not sure about the origin of this confusion. There could have been the scope of multicollinearity in the multivariate analysis if we would have included many inter-related predictors in the regression model. However, in our data analysis, we built one model for one variable, and adjusted for age, gender, education and marital status. Thus, multicollinearity should not be a problem for our study.

4. Was the predictor variables assessed for outliers, influential observations and normality? This may be of interest, as many of the predictor variables were continuous.

Response: We definitely agree with the point raised here by the reviewer and we had assessed our data for outliers of predictor variables during data analysis.

5. Who did the data collection for the clinical measurements? Who processed the clinical samples? How were they handled and transported?

Response: We are sorry for not providing these information in the manuscript submitted earlier. In the revised manuscript, we have clarified that nursing staffs at the study site conducted the
clinical measurements, collected, processed and performed the rapid testing. Since the results were based on rapid testing, no transport problem was involved in our study.

6. A subject was considered as having NCD if either he/she reports to have any chronic disease, or if the test results of the measurements (including BP, and blood examinations) revealed to have NCD. In this sense, how consistent and valid the outcome definition is? Moreover, self-report can relatively have better sensitivity for some NCDs than others.

Response: We are agreed with the reviewer regarding the problem of outcome definition in our study. But we believe this is a problem with any similar studies involving NCDs with a larger sample size. We have discussed this problem in the discussion part.

Results:

1. In Paragraph 1 and 2 (results section) or table 1, the authors would have benefited from a more formal statistical testing in showing if there was a difference in the distribution of the demographic and behavioural variables between men and women. Additional statistical test should be undertaken.

Response: We agree with the reviewer. We have performed additional data analysis, and have included 95% CI for the distribution of the demographic and behavioural variables for both men and women.
2. In the prevalence of NCDs section: paragraph 1 and 2: the prevalence determined by the clinical measurement during the study and that of self-reported should be explicitly stated.

Response: As per the instruction of the reviewer we have clarified this in the revised manuscript.

Discussion:

1. in paragraph 4 & 5, the authors have made definitive conclusions such as;
“alcohol drinking were strong correlates for the development of NCDs. These findings probably emphasized the need for policy implications such as raising tobacco taxes, legislation of health warnings, laws to maintain smoke-free work environment.” Though exposures such as smoking and harmful use of alcohol are established risk factors of NCD, which risk factor is a significant contributor in a specific population requires in-depth study. In addition the design of the study doesn’t allow establishing causation association and many cases that can threat validly of the association were not ruled out. So, how do you justify making such recommendation/conclusion?

Response: We understood the concern expressed by the reviewer. Thus we have removed such recommendation/conclusion from the revised manuscript.

2. Interpretation of the findings is limited to simple comparison with other studies. There lacks a focus of how the methods used and the nature of the study
population might have contributed to the comparability or variation of findings.

Response: We are sorry for this shortcoming in our interpretation. In the revised manuscript, we mentioned about the comparison between the methods and the nature of the study population between our study and others.

B. - Minor Essential Revisions

1. In the results section of the abstract –include the unitd for the BMI reported (kg/m2)

Response: We have added this in the revised abstract.

2. In the abstract, the authors can provide mean±SD or median (IQR) for age, than <30 years and >30

Response: We agree with the reviewer. We have added this in the revised abstract.

3. In line-6 of the methods section, says “Reference”, provide appropriate Reference

Response: We have added the relevant reference for this sentence.

4. The abbreviation PPS should first appear along with the first use of its form in
line probability proportional to size.

Response: In the revised manuscript, we have added the abbreviation after using its full form “probability proportional to size”.

5. In paragraph 2 of the screening section—there is inconsistency in using brackets: (, [ and lots of missing punctuations.

Response: We have corrected this in the revised manuscript.

6. Instead of screening, which is more than a simple cross-sectional examination, the authors can use another appropriate term.

Response: We agree with the reviewer. We changed “screening” to “measures” in the revised manuscript.