Author’s response to reviews

Title: Health status in a transitional society: urban-rural comparisons from a dynamic perspective in China

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Author’s response to reviews:

Reviewer #1: Using the 2005-2013 Chinese General Social Survey data, the research in question focused on temporal variation in self-reported health among rural and urban Chinese residents. The authors used the cross-classified random effects models (CCREM) to separate total changes into age, period, and cohort (APC) variations. They found distinct age, time period, and cohort patterns in health between rural and urban residents. The authors clearly described their data and results. However, I have concerns about the validity of the method that they used. As a result, I remain skeptical about their empirical conclusions. I described my thoughts below.

Q1: First, the manuscript would benefit from a compelling theoretical front end to motivate the study. The urban-rural disparity is certainly an interesting topic, but simply exercising a method that has not been widely used in a research area does not automatically warrant a contribution. The authors need to be clear about what an APC analysis would contribute the literature.

This is an inspirational suggestion. Just as the reviewer says, a theoretical structure or some contributions to existing theories will be beneficial in a study. Actually our study has made some efforts in this part. For example, we discuss the cumulative advantages/disadvantages effects and age-as-leveler effects theories, and we try to demonstrate these two theories are not competing and they will make sense in different conditions. In addition, the life course theory, which was proposed by Glen H. Elder in 1970, can be treated as the theoretical cornerstone of cohort effect. However, we did not present it in our paper before. Therefore, we introduce this theory in the literature part of our paper: “The life course theory, proposed by Glen H. Elder in 1970, can be treated as the theoretical cornerstone of cohort effect. Based on this theory, individuals’ life course was nested in the context it lived, and individuals’ behaviors were the consequence of social change [49]”.

Our results also prove that the health variation is highly in line with the social change, which reflects the strong power of the life course theory.

Q2: Second, the authors briefly mentioned the difficulty in identifying APC models, but apparently were not updated with new methodological discussions in this area. They claimed that
"with the development of statistics technology, this conundrum has been solved reasonably" (page 4). They also believed that CCREM "could not only remedy the limitation that the confounding of age and cohort effects in cross sectional studies, but offset the limitation of most previous longitudinal studies that only considering period effects but overlooking the cohort effects" (page 14). These statements are misguided. Recent research has shown that the CCREM cannot solve the identification problem (see, e.g., Bell and Jones 2014, 2015a, 2015b, 2017; Luo and Hodges 2016). It means that the estimated APC effects from a CCREM, like all other APC models, are arbitrary without theoretical justification. Because the substantive conclusions in this research entirely depend on the methodological validity of the CCREM, I remain unconvinced about their conclusions about temporal variations in health. Assuming that they would continue to be interested in using APC methods, I highly recommend the authors to read closely the methodological work mentioned below.

Thank reviewer and we admit that this is a valuable suggestion for us. Recently we have read literatures the reviewer recommended and some others related. After reading more literatures, we absolutely realize that we still cannot solve the APC identification problem well through the method we use. Actually, we, all the way, believe that in order to reflect real life properly, any model needs some reasonable assumptions that fit facts well.

When we run our model in the beginning, we set APC model with an equal-width interval for cohort groups, a five-year cohort forms. Then we changed and adjusted the aggregation method and finally use the form presented in the manuscript the reviewer sees. Just as some other researchers put (Bell & Jones, 2014), we also found that different aggregations of cohort would produce distinct cohort effects.

Maybe the CCREM method, firstly proposed by Yang and Land in 2006, is just a transition in the way we disentangle the APC conundrum reasonably. But just as Box and Draper put “all models are wrong, but some are useful”. We argue that our results are referable and credible more or less. Firstly, we set the cohort groups with unequal-width intervals, but we do not set them at liberty, we set them on the basis of Chinese history in the 20th century in order to fit the real life better. This can be treated as a rationale justifying our constraints, as Luo and Hodges (2016) stressed, in order to make our estimation more reliable. Similar strategy was used by some other researcher (Chen et al., 2010). Details can be seen in the paragraph below Table 1. Then, the period effect of health is not linear. Just as Reither et al. (2015) put, linear effects can hardly exist in real world. So the cohort effect we estimated is reliable more or less (we express this view in discussion part).

Given these intricate factors, and based on reviewer’s suggestions, we determine to revise our manuscript as follows:

(1). We still use the HAPC-CCREM to analyze our data, but correct our statement on this method in our manuscript. We delete the states that treat CCREM as a reasonable solution to APC conundrum, and add some statements on recent discussion related to advantages and disadvantages of this method. Also, we state why we choose HAPC-CCREM and what we do to confirm the validity of this method in our manuscript more clearly. For example, we express why we use unequal-width intervals of cohort groups (fitting the Chinese history and making our
interpretation meaningful; making the variance smaller within cohort groups and larger between cohort groups)

(2). We admit our method limitation in the last paragraph, the limitations part. “Finally, according to previous studies, the method we use, HAPC-CCREM, still cannot well solve the APC identification problem. Though we have taken several measures, such as unequal-width intervals of cohort groups, to make our results reliable, we should treat the results in this study with caution”

References:


Certainly, we also revised some other expressions in order to make our study more precise. And we hope the edition now can well meet the requests. And thank the reviewer for these valuable suggestions again.

Reviewer #2: Thank you for the opportunity to review this manuscript. The paper examines the variation trends of age, period, and cohort effects on health status among Chinese residents comprehensively, and analyses the dynamic comparisons of urban-rural disparities in health on these three temporal dimensions. The rationale for the study is clearly presented. The methodology of the paper appears sound. This is a paper that has good potential and covers a very important topic, and it can be accepted by the journal. There are three suggestions offered to the authors:

Q1: The word "SHR" should not be abbreviated in Introduction part.

It is a good suggestion, and we should not use SRH when it first appears. According to the reviewer’s suggestion, we use its full name “self-rated health” when it first appears in the main body, and we write its abbreviation “SRH” in a bracket following (“In the longitudinal studies that used self-rated health (SRH) as outcome variable, scholars majorly concentrated on how the gender disparities [7, 24, 30]…”). In the following contents we use SHR directly.
Q2: The statistical method should be added.

Thank the reviewer. We mainly use HAPC-CCREM, and we introduced it in the method part before. Maybe our previous introduction was not so clear and detailed. Based on this suggestion and another suggestion from another reviewer, we introduce and discuss this method more in the introduction part and readjust some expression in the method part, moving the statements of centralization and stepwise regression strategy from result part to method part. We hope these revisions can well meet the request of the reviewer.

Q3: The content in Discussion part should be simplified, some significant points are not prominent.

Thanks for reviewer’s valuable suggestion. We agree that the discussion part really seems to be a little long and should be simplified. After revision, the words number declines from 2582 to 2210. We mainly simplify some long-winded expressions, and details can be seen in our discussion part.

Certainly, we also revised some other expressions in order to make our study more precise. And we hope the edition now can well meet the requests. And thank the reviewer for these valuable suggestions again.

Reviewer #3: Please find the comments below:

Q1: Introduction could be more concise.

This is a useful suggestion, and we indeed write something dispensable in the introduction part. According reviewer’s suggestion, we simply our introduction and reduce the words number from 547 to 348. Thanks.

Q2: Some faults exit, please proofread the manuscript, such as,

Line 15 page 8, SRH should be spelt out when first used in the main text

It is a good suggestion, and we should not use SRH when it first appears. According to the reviewer’s suggestion, we use its full name “self-rated health” when it first appears in the main body, and we write its abbreviation “SRH” in a bracket following (“In the longitudinal studies that used self-rated health (SRH) as outcome variable, scholars majorly concentrated on how the gender disparities [7, 24, 30]…”). In the following contents we use SHR directly.

Line 54 page 11, countries or counties?

This is really a fault of us. The correct expression is counties not countries, and we have already corrected it. Thanks for this suggestion.
Method section:

Q3: Line 54 page 11, I recommend to describe the 125 counties /districts, including how many province they were sampled from, the socio-economic information, etc.

    Thank the reviewer. We also think this is necessary. Since 7 surveys are not the same in sampling, we add more details in this part. Details can be seen in the “data and source” part.

Q4: Line 25 page 13, how did you score SRH, clarify.

    Thanks for this good suggestion. Accordingly, we correct and make our statement of SRH more clear, changing “including very poor, poor, fair, good, and very good” into “including very poor=1, poor=2, fair=3, good=4, and very good=5”

Q5: What software was used to analyze your data?

    We use SAS9.4 to analyze our data. We add this information to our manuscript according to this suggestion.

Q6: It' important to overview the general information of participants in urban and rural areas in each cross-sectional survey (including number of participants, male%, age groups...). Please list them in a table.

    Thanks for this useful suggestion. Accordingly, we list a period-cohort specific table, Table 2, to display some information in each cross-sectional survey, including number of participants and mean scores of SRH within each cohort group.

Q7: It's surprise to see the remarkable decline in 2011. The explanation of Fukushima nuclear disaster for this finding is a little farfetched. Could that due to investigator bias or selection bias? Again, please list the general information of participants of all surveys (2005-2013)

    After listing the general information of participants of all surveys (2005-2013), we also find a low level of SRH in 2011 (see table 2). The reviewer argues that it mostly results from the investigator bias or selection bias. We admit that it might be one reason, but we still insist that the reason we have discussed in the paper is possible to some extent.

    Just as we put in the footnote before, in another study (be submitted), based on the World Value Survey (WVS) 1990-2012, we compared the SRH variations among China, Japan, Russia and America with IE algorithm. And we found that the SRH deteriorated greatly among Japanese residents in early 2010th, and the health of Chinese residents just worsened slightly, while we could found significant declines of health among Russian and American residents. Details can be seen in the following figures (ordinate is the coefficient of IE estimation, larger means healthier).
The WVS is a large-scale survey in the world, and the result based on it is also in line with our result. Thus we speculated that health related international events would influence health abroad through a specific spatial model, with nearer distance being impacted more significant.

With caution, we think the suggestion from the reviewer is reasonable more or less. So we add this possibility in the footnote “Certainly, the low level of SRH might also be caused by the sample bias or selection bias.”

Q8: Figure 2a, the title is uncompleted.

Thank the reviewer. The title of Figure 2a, Urban-rural disparities, is completed in fact. It just seems to be uncompleted. Accordingly, we change it into “Urban-rural disparities alone” to avoid misunderstandings unnecessary.

Q9: For figure 1c and 4, I suppose most of the participants with older age were included in the early surveys, while the younger ones were included in the later surveys. If so, the disparity was more likely to be impacted by policies issued at the point of the surveys.

Thanks for the suggestion. Since we use the cross-sectional data from 2005 to 2013, the phenomenon that most of the participants with older age were included in the early surveys and the younger ones were included in the later surveys is hardly to be true. Actually, with the life expectancy improved, the mean age is larger in more recent survey. Details can be seen in table 2 or the following table.

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<tbody>
<tr>
<td>Age (mean)</td>
<td>44.697</td>
<td>42.405</td>
<td>43.189</td>
<td>47.292</td>
<td>48.085</td>
<td>48.807</td>
<td>48.524</td>
</tr>
</tbody>
</table>

Q10: Line 51 page 20, what exactly was the difference between urban and rural regarding social insurance? Please clarify.

Thank the reviewer. The social insurance here means social safety net between urban and rural China. Given the existence of household registration system, the double-track system has profoundly impacted Chinese daily life. As for social safety net, new rural cooperative medical system (in rural area) and urban medical insurance (in urban area) are two different systems, though they have combined together in recent years, the distinct impacts has lasted long. Generally speaking, social safety net in urban China has stronger protective effects on residents’ health, for most medical resources get together in urban area. In fact, these two medical systems precisely reflect the double-track system in China, under which residents have two different lifestyles between rural and urban areas, including various aspects like economy, culture and so forth. They all influence health, and, to some extent, we just use medical system as an example, which is directly related to population health.
Q11: Line 23 page 22, please describe the health system reform in 2009.

Thank the reviewer. Based on this suggestion, we add some description on the new healthcare reform in the footnote “The New Healthcare Reform, started in 2009 and is now underway, has profound effects on the medical system in China. It proposed the guiding ideology, basic principle and global purpose of the healthcare reform, and especially stressed the equality of urban and rural areas in healthcare services allocation.”

Q12: The conclusion should be concluded based on your main findings and combined with the current health policies, rather than giving general suggestions.

This is a valuable suggestion. In the last but one paragraph, we conclude two main results and propose some suggestions from a macro perspective. Certainly, these general suggestions are not essential. According to reviewer’s suggestion, we determine to delete these general suggestions and move these two main results to the beginning of conclusions and discussion part.

Certainly, we also revised some other expressions in order to make our study more precise. And we hope the edition now can well meet the requests. And thank the reviewer for these valuable suggestions again.

Reviewer #4: This study used the HAPC-CCREM method to explore the self-reported health with the age, period and cohort effects, then compared the urban-rural difference, and influential factors. The method is valid and innovative, and the findings in the study provide evidence for historical influence on health.

Abstract:

Q1. Page 2 line 26: the sentence "Urban-rural disparities of health amplified with age, and cumulative effect and age-as-leveler effect were not contrary substantially, they would come across in distinct conditions" was not very clear.

Thank the reviewer. Just as the statement in the literature review, cumulative advantage/disadvantage and age-as-leveler theories are two widely used theories when discussion the population disparities of age effects (e.g. Chen FN, Yang Y, Liu GY: Social change and socioeconomic disparities in health over the life course in China: a cohort analysis. American Sociological Review 2010, 75(1): 126-150.). Previous studies treat them as two competing theories (Zheng L, Zeng XH: Gender differences in SES and health gradient in China: a life course longitudinal study. Chinese Journal of Sociology 2016, 36(6): 209-237.), but our results suggest that they are not competing, they will make sense in specific situations respectively. So we state that “though these two theory perspectives were developed relatively, they were not competing substantially” in discussion part.
Since the reviewer proposes this question, we check this expression again and do find it improper here, for it contains some subjective judgments that should not appear in result part. So we state it in detail “Urban-rural disparities, as well as work status and gender disparities, in health amplifies with age, which is in line with cumulative advantage/disadvantage effect; while marital status disparities in health converges with age that conforms to age-as-leveler effect”

Q2. Page 2 line 17: "but compared xxx" I think it should not be "but".

Thank the reviewer. After careful examination, we find that it’s because we lose a “not” in line 16. After correction, this sentence is “this study not only explored the age, period and cohort effects of self-rated health independently, but compared the health status between rural and urban areas…”

Introduction:

Q3. Page 3 line 26: Please explain the concept of "Universal health", and the sentence "no universal health, no overall well-off".

This two are both words with Chinese features. “Universal health” means that each resident should be healthy, and it underlines the universality of health status. “No universal health, no overall well-off” means that only keeping healthy can make us be rich essentially, in other words, if we do not have a healthy body or keep healthy, no matter how much money we can earn, we are not really rich. These two expressions both stress the importance of health. Since they may be misunderstood by individuals from other nations, we adjust our statement as “Chinese government has put forward the conception of universal health, underlining the significance of population health”. We really appreciate the reviewer.

Q4. Page3 line 32: Was the medical reform conducted and finished in 2009, or it was conducted since 2009?

It is a useful suggestion, and it is us that misused the preposition. We correct our expression, changing “in 2009” into “since 2009”. Thanks.

Q5. Page 4 paragraph 1: This paragraph was about the longitudinal analysis, and cohort effect, but it also includes health disparities, which was introduced in the following paragraph. I suggest if author could only discuss the longitudinal analysis and cohort effect in this part, and discuss the disparity together with the following paragraph.

This suggestion, as well as the following Q10, is related to the structure of our manuscript. Our design or structure is that: firstly discussing health disparities (rural-urban disparity) in a cross-sectional level, then overviewing the health disparities from a temporal perspective (age, period, and cohort effects of health in rural and urban areas). However, we should discuss or overview
the development and advantages and disadvantages of various age-period-cohort (APC) methods, especially HAPC-CCREM. So we develop our manuscript as the reviewer see. We think it will be strange if we directly introduce the development of APC models after introduction part.

According to suggestions from the reviewer and other reviewers, we determine to adjust our manuscript as follows: first discussing health disparities (rural-urban disparity) in a cross-sectional level, then giving a brief introduction to APC model, finally overviewing the temporal trend of health and health disparities from a temporal perspective (age, period, and cohort effects of health, as well as the dynamic urban-rural disparities).

We hope this revision can well meet the reviewer’s requests. Thanks.

Q6. Some paragraphs had subheadings, but some did not. Please keep consistent.

According to the form request of Population Health Metrics, subheadings do not need to add serial number, so it seems a little strange that some paragraphs have subheadings but some do not. Thank the reviewer.

Q7. Page 4 line 51: maybe "have gained much attention" instead of "attracted most focuses".

This is a good suggestion, and we have corrected it based on reviewer’s author.

Q8. Page 5 line 15: I think social status, income were not resources that unfair distributed? Some public resources, like healthcare, were unfairly distributed.

Thank the reviewer’s suggestion. After consideration, we correct our statement as “such as healthcare and culture or education resources etc.”

Q9. Page 5 line 18: The statement "the urban-rural disparities in health was the most important one" do not have enough support evidence. Please add more clarification.

Thanks for this suggestion. Actually we indeed do not have enough evidence to support this statement, so we decide to delete this sentence to keep the accuracy of our expression.

Q10. Page 6 last paragraph: This paragraph talked about the longitudinal study, age, period and cohort effects. So it may be better to combine the information introduced in page 4 paragraph 1 and the current paragraph together.

Details can be seen in the answer to Q4.
Q11. Page 7 last paragraph: since authors did not use the HAPC-GCM in the current study, why introduced it here which might cause confusion for readers.

Thank the reviewer. After revision, we just mention the HAPC-GCM and do not introduce it more. We list several related methods, including HAPC-GCM and others, in order to clear that much effort has been done in the past several decades.

Q12. Authors may re-consider the structure of the introduction which was not organized very well.

This is a valuable suggestion. We have read several other papers and reorganized the structure of the contents before data and method part. Thank the reviewer.

Method:

Q13. Page 11 last paragraph: Were the surveys multi cross-sectional or longitudinal? Because authors have discussed a lot about longitudinal analysis. Please clarify.

Thank the reviewer’s suggestion. CGSS each time is a cross-sectional survey, and samples are almost different each time. So when we stacking each cross-sectional survey, 7 times from 2005 to 2013, it becomes a repeated cross-sectional survey. However, when we analyze the temporal trend through repeated cross-sectional data, we also can call it longitudinal analysis. It belongs to longitudinal analysis substantially. Based on reviewer’s request, we adjust related statement as follows:

Changing “7 times surveys” into “7 times cross-sectional surveys”;

Changing “Launched in 2003, the CGSS was the first comprehensive, continuous large-scale investigation project” into “Launched in 2003, the CGSS was the first comprehensive and large-scale investigation project”.

Q14. Page 13 first paragraph: It is better to briefly introduce each cohort authors have proposed. So readers could understand what happened in the specific cohort.

Thank the reviewer. After discussion, we do not think we should briefly introduce each cohort group, for we have already named each cohort group and each name implies what happened during corresponding cohort group. Also, other researcher did not briefly introduce each cohort group they proposed (Chen FN, Yang Y, Liu GY: Social change and socioeconomic disparities in health over the life course in China: a cohort analysis. American Sociological Review 2010, 75: 126-150.). So we do not think we should introduce each cohort group.
Q15. Page 14: In the equation, authors included party and work as predictors, but in the following part (Line 57) there were not no party and work variable. Please keep consistent.

Thank for the suggestion, and now we correct this error. We change female into gender, rural into hukou, party into politics status, married into marital status, work into work status. We also write all of the predictors, including age, age2, gender, hukou, politics status, marital status, education and work status, to keep the equation and the statement consistent.

Results:

Q16. Page 15 second paragraph: Most of the first paragraph of the results was about method. So it may be moved to the method part.

This is a suggestion that is of value. Accordingly, we adjust the expression of this paragraph and move it to the method part. Thanks.

Q17. Table 1: It may better to present the frequency for some variables, like gender, hukou. What did authors mean by primary, junior and senior in education?

Thanks for this valuable suggestion. Accordingly, we add more information of the variables, including independent and dependent variables, into table 1: category, frequency and percentage.

For education, the expression of its category before may be unclear, so we correct it. Education has five categories: illiteracy, primary school, junior high school, senior high school, college or above. We believe this can be clearer.

Q18. Table 2: Page 16 line 9: the ICC was 9.66% in the text, but 0.1004 in the Table 2. Please keep consistent.

Thanks and we have corrected it. The right ICC is 10.04%.

Q19. Page 17 line 46: Did the health level increase after 1977 cohort instead of 1984 as described in the text? Authors may check the statements in the text and the results in the Tables and Figures again to keep consistent.

Thanks and this is absolutely our fault. We have corrected this sentence and the right one is “after 1977 cohort, the residents’ health…”

Q20. Figure 2: It is better to combine a-d into one figure.
We admit that this is a useful suggestion. But we tend not to combine them into one figure, for it might be not such clear for readers to read.

Certainly, we also revised some other expressions in order to make our study more precise. And we hope the edition now can well meet the requests. And thank the reviewer for these valuable suggestions again.