

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	The association of socioeconomic status with financial burden of disease among elderly cardiovascular patients: evidence from the China Health and Retirement Longitudinal Survey
AUTHORS	Li, Chunyan; Young, Belinda-Rose; Jian, Weiyang

VERSION 1 – REVIEW

REVIEWER	Adrianna Murphy London School of Hygiene and Tropical Medicine
REVIEW RETURNED	02-Aug-2017

GENERAL COMMENTS	<p>The following sentence should include references: “Studies in various LMIC have well documented the relevance of socioeconomic factors on health outcomes and cost-of-illness among patients.” Or, if the references are #s 1 and 2 of the subsequent sentence in the next paragraph, then this sentence should be joined to that same paragraph.</p> <p>The expression ‘lower conditions’ doesn’t really make sense and should be replaced – do you mean poorer socioeconomic conditions?</p> <p>The expression “patients who paid their last outpatient/inpatient visit because of CVD” is used a few times and isn’t really proper English. An alternative could be “patients whose reason for their last outpatient/inpatient.</p> <p>In the sentence “It is worth noting that all information at community level was directly collected from local officials through a constructed questionnaire.”, the expression ‘it is worth noting that’ is not necessary and should be removed. This fact is not simply ‘worth noting’ but crucial information about the methods of the study.</p> <p>“All data cleaning and statistical analyses were done by Stata 14.1” should be “All data cleaning and statistical analyses were done using Stata 14.1”</p> <p>Some English editing would be helpful. For example, for the sentence: “Regarding the health service utilization, there were about 15% of CVD patients (2011:334/2568, 2013: 463/3056) that reported being hospitalized at least once in the past year because of CVD, and 10% of the CVD patients (2011: 260/2568, 2013: 279/3056) used outpatient services because of CVD at least once during the past month.”, I would suggest: “Regarding health service utilization,</p>
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	about 15% of CVD patients (2011:334/2568, 2013: 463/3056) reported being hospitalized at least once in the past year because of CVD, and 10% of CVD patients (2011: 260/2568, 2013: 279/3056) used outpatient services because of CVD at least once during the past month.”
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REVIEWER	Frank Sloan Duke University USA
REVIEW RETURNED	12-Sep-2017

GENERAL COMMENTS	<ol style="list-style-type: none"> 1. The current version of the paper lacks sufficient focus. What specifically is the value-added of the paper? What does the paper add to what is currently known? 2. It takes too long to get to the findings and then it is not clear what the takeaways are. 3. The role of health insurance coverage in determining out of pocket cost must be described. Also, there are important differences in coverage between urban and rural areas. What about the role of public provision of care? 4. The cost concepts need to be described more precisely. 5. The paper is in need of editing. There are a number of writing errors.
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REVIEWER	Baowen Xue UCL Research Department of Epidemiology & Public Health, London, UK
REVIEW RETURNED	16-Oct-2017

GENERAL COMMENTS	<p>This paper investigates the inequality in economic burden of disease among elderly cardiovascular patients in China. This work therefore aligns with an increasing number of studies suggesting that CVD patients with higher SES spend more on health services on average, but are less likely to encounter household catastrophic health expenditure. Overall, I think it is an interesting study; however, I have concerns/questions detailed below that should be addressed prior to acceptance.</p> <p>1) Introduction: One unique/important thing in China is that urban and rural residences have two different systems of health insurance and pension. The author mentioned a bit of urban-rural difference in health insurance policy in the Measures. But I feel it should be described in Introduction.</p> <p>2) Method: OOP payment and Catastrophic health expenditure</p> <ol style="list-style-type: none"> 1. How different types of OOP payment (in table 3) were measured should be described more clearly in the Measure. For example, the author uses per capita cost and per capita annual health expenditure in Table 3& 4. But this was not mentioned in the Measure. Did per capita values were calculated as total values divided by household size? <p>SES indicators</p> <ol style="list-style-type: none"> 2. Rural-urban is the first SES indicator. Is this rural-urban based on the residential region or the HuKou? 3. Occupation was categorised into 5 groups: agricultural work, employed, self-employed, retired, and unemployed. How about those farmers who were no longer working? Are they in the
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	<p>agricultural work, or retired, or unemployed? I think in the Chinese context, many farmers cannot conduct formal retirement, so they just work until they are too old or too sick. These group of people may be particularly vulnerable to catastrophic health expenditure occurrence.</p> <p>4. Total household living expenditure was used as the third SES indicator. In the more developed countries, total household wealth is commonly used. I suggest the author to explain why choosing total household living expenditure, and provide some references. I remember that there are several papers have discussed that total household living expenditure may be a more appropriate measure in developing countries, such as in China.</p> <p>3) Statistical analysis:</p> <ol style="list-style-type: none"> Page 16: Not sure if "multilevel log-linear regression" is a standard way of describing the model. I feel it is just a multilevel linear regression, and log transferred the outcome. Page 16: "to study the association of individual SES characteristics with the increase in annual OOP payment among CVD patients". What does "the increase" mean? I think this manuscript only assessed the association between SES and annual OOP payment, not the increase in OOP. <p>4) Results:</p> <ol style="list-style-type: none"> Page 17 first paragraph: "In the CHARLS database, a total number of 17,708 ... ". This paragraph should be moved to methods. Page 19: The median numbers of OOP payment by service type is reported in Table 2. Actually, it was reported in Table3 not in Table 2. Besides, this paragraph did not describe the results very clearly. The description in the text should be matched with the tables. For example, "... but the inpatient cost for a single visit increased by 450 Yuan/visit (22.0%) per capita". I think it should be the direct inpatient cost. Page 20: 'According to the multi-level linear regression analysis (Table 3)'. Again, this should be Table 4. Page 20: The author said " the OOP payment among the richest 20% households was almost 0.85 times higher compared to the poorest 20% of households in 2011, and 1.17 times higher in 2013 ($p < 0.05$)." I feel the explanation of the coefficient is not correct. If this study used multilevel linear regression, and log transferred the OOP payment. Then, the 0.852 in table 4 means that the log transferred annual OOP payment of the poorest 20% of household is 0.852 higher than the richest 20% of household. It's not 0.85 times higher, as it is not odds ratio. Page 22: The author said "the higher the level of household living expenditure, the stronger protective effect there is for CVD patients' families against CHE occurrence." I don't think a descending trend of odds is enough to support this conclusion. An interaction test is preferred. <p>5) Tables:</p> <ol style="list-style-type: none"> Table2: Table 1 shows that there are higher % of people aged ≥ 75 in year 2013 than 2011. So age effect should be taken into account when comparing the results between 2011 and 2013. Then, Table 2 should show the age-standardised prevalence of CVD. Also, table 2 can be moved to appendix, as your study sample is only CVD patients. Table 4: Tables should stand alone, complete and informative in itself. e.g. Is "per capita annual health expenditure" in table 4 the same as " Per capita total OOP payment in one year" in table 3? If yes, then the author should choose one, and use it consistently. Table 4&5: This study used a 'multilevel log-linear regression
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	<p>analysis, i.e. a two-level random-intercept model with individuals at the first level and communities at the second level'. So the 450 communities were treated as the second level. I feel the way of showing of fixed effects and random effect in table 4 and 5 is a bit misleading. Urban-rural area is actually in a high cluster than the 450 communities, so it is not appropriate to say urban-rural is a community level fixed effect. I suggest the author to use urban-rural difference throughout the manuscript.</p> <p>4. Footnote: I think the * in the footnote should indicate the P value not the 95% CI. For example, on page 21 the author said the retired/receded group (OR=1.34, 95%C.I. 1.00~1.78) is statistically significant. But we did not see a * in the table. Is the P=0.05 or <0.05?</p> <p>6) Discussion:</p> <p>1. The author used a lot of space to (e.g. the whole page 27) talk about the Chinese policy, but did not discusses how their research findings are related to the policy.</p> <p>2. The urban-rural difference in occurrence of catastrophic health expenditure is significant in 2011, but not in 2013. I suggest the author to discuss possible explanations.</p> <p>3: From table 1, we can see the % rural sample increased from 51.8 in 2011 to 53 in 2013. In a country with very fast urbanization, the % rural sample should decrease. Perhaps indicating some bias due to drop-out.</p>
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REVIEWER	Sirirat Anutrakulchai Faculty of Medicine, Khon Kaen University Khon Kaen, Thailand
REVIEW RETURNED	19-Oct-2017

GENERAL COMMENTS	<p>I have some comments as these details;-</p> <p>1. The strength of this study is that information used for analysis represents the national Chinese data; so, the results provide a clear overview of the socioeconomic effect on payment situation. However, due to the retrospective nature of the study, some data might not be complete which the authors should mention.</p> <p>2. In Table 3, "Per capita OOP payment for outpatient care in one year" was used 2 times. Should be corrected as inpatient?</p> <p>3. Regarding to occupation issue in individual level, what are the reasons that the Unemployed group had higher odds of encountering catastrophic spending than the agricultural group?</p> <p>4. Are there other explanatory issues for the lower CHE occurrence in the urban residence or high SES group? For example,</p> <p>a. they may get more benefit from their health scheme therefore they pay less for health or</p> <p>b. they have higher absolute salary amount therefore although they pay more for CVD treatment but it's still less than 40% of total their incomes or</p> <p>c. they get better health, therefore they pay less proportion for illness when comparing with their incomes.</p>
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REVIEWER	Carlo Piccinni Researcher at CORE (Collaborative Outcome Research) - a CINECA partener Casalecchio di Reno (Bologna) - Italy
REVIEW RETURNED	26-Oct-2017

<p>GENERAL COMMENTS</p>	<p>I read with great interest the article by Chunyan Li and Weiyang Jan entitled “The association of socioeconomic status with economic burden of disease among elderly cardiovascular patients: evidence from the China Health and Retirement Longitudinal Survey”.</p> <p>The article is well written and clear in each part: I very appreciated the efforts by authors to full-fill the STROBE checklist.</p> <p>This work was aimed to investigate the possible association between cardiovascular diseases and economic aspects in China. The research was conduct by using data coming from a longitudinal survey called CHARLS (the China Health and Retirement Longitudinal Study). The objective of the study and the results obtained are certainly of great interest for policymakers and public health researchers, especially due to the important changes that are coming in China in the last years also in Health governance. In order to ameliorate the manuscript I suggest the following revisions:</p> <ol style="list-style-type: none"> 1. The main drawback of this manuscript is that correctly recognised by authors as first limitation (i.e. the estimation of OOP payment by using the self reported cost in the medical visit). This limitation, and others are due to the choice to analyse data coming from a survey (i.e. CHARLS) and they could be overcome by use real world data (e.g. health administrative databases). Therefore, I suggest to add in the discussion section a paragraph concerning the importance and the need to use (or to establish if missing) health administrative databases. These data sources are certainly useful to better address the changes in health governance, other than to generate scientific evidence for many clinical issues. 2. The last paragraph of Introduction (from page 8 line 56 to page 9 line 25) should be moved in Discussion section. 3. page 9 line 38, please complete the name of CHARLS by adding “Retirement”. 4. The Discussion should be reduced in order to allow the reader to better focus the main findings of this study and their consequences. 5. The issue concerning the multi-morbidity should better described in the study, due to it well-known that the number and the type of comorbidities strongly influence the healthcare resource utilization and the relevant costs. I suggest to add a table describing the comorbidity characteristics of studied cohorts.
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VERSION 1 – AUTHOR RESPONSE

Editorial Requirements:

Comment1: please include a statement detailing the ethics approval.

Response1: now included in Page33, after the statement of competing interests.

Reviewer1:

Comment1: the following sentence..“studies in various LMIC”..

Response1: The reference for the sentence "studies in various LMIC" should be the same as its following sentences. Revised accordingly.

Comment2: explanation of " lower conditions"

Response2: yes it means lower socioeconomic status. Revised accordingly.

Comment3: expression of "patients who paid their last outpatient/inpatient" isn't proper
Response3: Thanks for suggestion! Revised to "patients whose reason for their last outpatient/inpatient" accordingly.

Comment4: "it is worth noting"..should be removed.
Response4: Revised accordingly.

Comment5: "all data cleaning ..done using Stata"
Response5: Revised accordingly.

Comment6: English editing
Response6: This version is proofreaded and revised by a native speaker.

Reviewer2:

Comment1. The current version of the paper lacks sufficient focus. What specifically is the value-added of the paper? What does the paper add to what is currently known?

Response1: The significant value of this paper is using the latest data from a national representative household survey, presenting calculation of both direct and indirect health cost among Chinese patients of cardiovascular diseases, different groups of patients' odds of encountering catastrophic health expenditure, and how these health burdens were associated with patients' socioeconomic status. The results would be a reference for Chinese health policymakers to adjust the health reform policies, as well as contribute future cross-country research. The significance is discussed at the end of the introduction in the latest manuscript.

Comment2. It takes too long to get to the findings and then it is not clear what the takeaways are.
Response2: the part of introduction was reorganized and shortened.

Comment3. The role of health insurance coverage in determining out of pocket cost must be described. Also, there are important differences in coverage between urban and rural areas. What about the role of public provision of care?

Response3: the role of health insurance types covering different populations is included in the introduction, Page6.

Comment4. The cost concepts need to be described more precisely.

Response4: formulas to calculate cost are included in Methods, on page10~12.

Comment5. The paper is in need of editing. There are a number of writing errors.

Response5: This version is proofreaded and revised by a native speaker.

Reviewer3:

Comment1: One unique/important thing in China is that urban and rural residences have two different systems of health insurance and pension. The author mentioned a bit of urban-rural difference in health insurance policy in the Measures. But I feel it should be described in Introduction.

Response1:Thanks for suggestion! the introduction is reorganized accordingly.

Comment2: How different types of OOP payment (in table 3) were measured should be described more clearly in the Measure. For example, the author uses per capita cost and per capita annual health expenditure in Table 3& 4. But this was not mentioned in the Measure. Did per capita values were calculated as total values divided by household size?

Response2: formulas to calculate cost are included in Methods, on page10~12.

Comment3: Rural-urban is the first SES indicator. Is this rural-urban based on the residential region or the HuKou?

Response3: according to the protocol of CHARLS, it's based on the residential region.

Comment4: Occupation was categorised into 5 groups: agricultural work, employed, self-employed, retired, and unemployed. How about those farmers who were no longer working? Are they in the agricultural work, or retired, or unemployed? I think in the Chinese context, many farmers cannot conduct formal retirement, so they just work until they are too old or too sick. These group of people may be particularly vulnerable to catastrophic health expenditure occurrence.

Response4: a clearer definition of the 5 occupation types is included on page13.

Comment5: Total household living expenditure was used as the third SES indicator. In the more developed countries, total household wealth is commonly used. I suggest the author to explain why choosing total household living expenditure, and provide some references. I remember that there are several papers have discussed that total household living expenditure may be a more appropriate measure in developing countries, such as in China.

Response5: Yes, the guideline for analyzing household survey data in developing countries, published by the World Bank, suggests to use household expenditure as a economic SES indicator instead of income or total household wealth. This is explained in the Methods section on page13.

Comment6: Not sure if "multilevel log-linear regression" is a standard way of describing the model. I feel it is just a multilevel linear regression, and log transferred the outcome.

Response6: thanks! Revised accordingly.

Comment7: Page 16: "to study the association of individual SES characteristics with the increase in annual OOP payment among CVD patients". What does "the increase" mean? I think this manuscript only assessed the association between SES and annual OOP payment, not the increase in OOP.

Response: this was our mistake. We did not measured the increase. Revised accordingly.

Comment8: Page 17 first paragraph: 'In the CHARLS database, a total number of 17,708 ...'. This paragraph should be moved to methods.

Response8: revised accordingly.

Comment9: Page 19: The median numbers of OOP payment by service type is reported in Table 2. Actually, it was reported in Table3 not in Table 2. Besides, this paragraph did not describe the results very clearly. The description in the text should be matched with the tables. For example, "... but the inpatient cost for a single visit increased by 450 Yuan/visit (22.0%) per capita". I think it should be the direct inpatient cost.

Response9: Thanks! Revised accordingly.

Comment10. Page 20: 'According to the multi-level linear regression analysis (Table 3)'. Again, this should be Table 4.

Response10: Thanks! Revised accordingly.

Comment11: Page 20: The author said " the OOP payment among the richest 20% households was almost 0.85 times higher compared to the poorest 20% of households in 2011, and 1.17 times higher in 2013 ($p<0.05$). " I feel the explanation of the coefficient is not correct. If this study used multilevel linear regression, and log transferred the OOP payment. Then, the 0.852 in table 4 means that the log transferred annual OOP payment of the poorest 20% of household is 0.852 higher than the richest 20% of household. It's not 0.85 times higher, as it is not odds ratio.

Response11: Thanks for pointing this mistake out! We revised our interpretation words.

Comment12: Page 22: The author said "the higher the level of household living expenditure, the stronger protective effect there is for CVD patients' families against CHE occurrence." I don't think a descending trend of odds is enough to support this conclusion. An interaction test is preferred.

Response12: We limited our interpretation of this point, and indicated the overlaps of 95%C.I. of ORs.

Comment13: Table2: Table 1 shows that there are higher % of people aged ≥ 75 in year 2013 than 2011. So age effect should be taken into account when comparing the results between 2011 and 2013. Then, Table 2 should show the age-standardised prevalence of CVD. Also, table 2 can be moved to appendix, as your study sample is only CVD patients.

Response13: We included the age effect in our limitation discussion on page23-24, and moved table2 to appendix.

Comment14: Table 4: Tables should stand alone, complete and informative in itself. e.g. Is "per capita annual health expenditure" in table 4 the same as " Per capita total OOP payment in one year" in table 3? If yes, then the author should choose one, and use it consistently.

Response14: Thanks! Revised accordingly.

Comment15: Table 4&5: This study used a 'multilevel log-linear regression analysis, i.e. a two-level random-intercept model with individuals at the first level and communities at the second level'. So the 450 communities were treated as the second level. I feel the way of showing of fixed effects and random effect in table 4 and 5 is a bit misleading. Urban-rural area is actually in a high cluster than the 450 communities, so it is not appropriate to say urban-rural is a community level fixed effect. I suggest the author to use urban-rural difference throughout the manuscript.

Response15: we did perform a two-level regression, and include the urban-rural difference at the second level. Thought urban-rural could be a higher cluster as compared to other community-level indicators, we think it could be also treated as a community characteristic for acknowledging the great variance within-urban or within-rural areas. We revised our words into "urban-rural difference" through the manuscript as the reviewer suggested, but still keep it as it was in our statistical analysis models.

Comment16: Footnote: I think the * in the footnote should indicate the P value not the 95% CI. For example, on page 21 the author said the retired/receded group (OR=1.34, 95%C.I. 1.00~1.78) is statistically significant. But we did not see a * in the table. Is the P=0.05 or <0.05?

Response16: Revised accordingly.

Comment17: The author used a lot of space to (e.g. the whole page 27) talk about the Chinese policy, but did not discuss how their research findings are related to the policy.

Response17: we wanted to connect our finding to the recent health policy reform. But thanks for the suggestion! We shortened our discussion accordingly.

Comment18: The urban-rural difference in occurrence of catastrophic health expenditure is significant in 2011, but not in 2013. I suggest the author to discuss possible explanations.

Response18: we provided some possible explanation for the difference, mainly about the policy change between 2 years, in our discussion.

Comment19: From table 1, we can see the % rural sample increased from 51.8 in 2011 to 53 in 2013. In a country with very fast urbanization, the % rural sample should decrease. Perhaps indicating some bias due to drop-out.

Response19: Table1 only included CVD patients. The changing trend of urban-rural ratio in the general population in urbanization process does not necessarily influence the ratio in CVD patients during only 2 years.

Reviewer4:

Comment1: The strength of this study is that information used for analysis represents the national Chinese data; so, the results provide a clear overview of the socioeconomic effect on payment situation. However, due to the retrospective nature of the study, some data might not be complete which the authors should mention.

Response1: It's right that our study analyzed secondary data. We included the limitation of CHARLS in our discussion of limitation.

Comment2: In Table 3, "Per capita OOP payment for outpatient care in one year" was used 2 times. Should be corrected as inpatient?

Response2: Yes. It should be inpatient. revised accordingly.

Comment3: Regarding to occupation issue in individual level, what are the reasons that the Unemployed group had higher odds of encountering catastrophic spending than the agricultural group?

Response3: As unemployed group has no income source (a clear definition of unemployed group was added in the Methods part), they are more likely to encounter CHE as compared to agricultural workers who could earn money through selling products.

Comment4: Are there other explanatory issues for the lower CHE occurrence in the urban residence or high SES group? For example,

- a. they may get more benefit from their health scheme therefore they pay less for health or
- b. they have higher absolute salary amount therefore although they pay more for CVD treatment but it's still less than 40% of total their incomes or
- c. they get better health, therefore they pay less proportion for illness when comparing with their incomes.

Response4: Thanks for proposing the alternative explanations. Added in our discussion accordingly.

Reviewer5:

Comment1: The main drawback of this manuscript is that correctly recognised by authors as first limitation (i.e. the estimation of OOP payment by using the self reported cost in the medical visit). This limitation, and others are due to the choice to analyse data coming from a survey (i.e. CHARLS) and they could be overcome by use real world data (e.g. health administrative databases). Therefore, I suggest to add in the discussion section a paragraph concerning the importance and the need to use (or to establish if missing) health administrative databases. These data sources are certainly useful to better address the changes in health governance, other than to generate scientific evidence for many clinical issues.

Response1: Thanks! We added this in our limitation discussion!

Comment2: The last paragraph of Introduction (from page 8 line 56 to page 9 line 25) should be moved in Discussion section.

Response: revised accordingly.

Comment3: page 9 line 38, please complete the name of CHARLS by adding "Retirement".

Response: revised accordingly.

Comment4: The Discussion should be reduced in order to allow the reader to better focus the main findings of this study and their consequences.

Response: the discussion part is reorganized accordingly.

Comment5: The issue concerning the multi-morbidity should be better described in the study, due to it well-known that the number and the type of comorbidities strongly influence the healthcare resource

utilization and the relevant costs. I suggest to add a table describing the comorbidity characteristics of studied cohorts.

Response: the co-occurrence of other NCDs is included in Table1.

VERSION 2 – REVIEW

REVIEWER	Adrianna Murphy LSHTM UK
REVIEW RETURNED	04-Dec-2017

GENERAL COMMENTS	<p>Major comments</p> <p>Thank you to the authors for this submission. I think this is an important study and, overall, is well described.</p> <p>The most important issue I would like to comment on with respect to this paper is the exclusion of any data on health insurance status or type of health insurance in the analysis (although some of this is capture by employment). In the introduction, the authors frame the research in terms of a potential failure of insurance reforms in China to sufficiently cover the poor, and claim that different types of insurance packages may provide more or less cover. In the conclusion the authors state that insurance must be made more comprehensive to protect the poor. While I do not disagree with these statements, I do not understand why the authors would not have tried to estimate the effect of insurance on expenditure and CHE in order to support their hypothesis and conclusions.</p> <p>Methods</p> <p>I find some of the method descriptions confusing.</p> <p>a) It is mentioned that the authors: “first estimated the per capita health expenditure for outpatient/inpatient service each time.... and then applied multilevel regression analyses to explore the association between one’s economic burden and socioeconomically influencing factors”. What does ‘each time’ mean? Did they sum all expenditure for all visits over the interview period? And how was ‘economic burden defined’? Was it simply the sum of expenditures or the proportion that expenditures occupied of overall consumption?</p> <p>b) It is mentioned that the CHARLS only interviewed individuals aged 45 and older, but in the next paragraph the authors state that they excluded individuals aged 45 and</p>
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younger for this study. If they are not included in the CHARLS in the first place, why do they need to be excluded for the analysis?

- c) How was household expenditure estimated? Was it self-reported? For the last year or last month?
- d) As this is a longitudinal study, are the same households included and compared in 2011 and 2013? This should be clarified.

Results and Discussion

The finding that people spent more on outpatient services than inpatient is surprising, even if you account for higher frequency of outpatient visits. Most studies tend to find higher outpatient expenditure, because these services are simply much more expensive (e.g. operation vs check-up). This finding should be discussed with possible explanations. Are some inpatient services covered whereas outpatient services are not? Or do you really think the result is simply due to a very high frequency of outpatient visits?

Minor comments

I believe in the 'setting' section of the Abstract some information should be given about the Chinese context, not only the survey.

The following sentence should include references: "Studies in various LMIC have well documented the relevance of socioeconomic factors on health outcomes and cost-of-illness among patients." Or, if the references are #s 1 and 2 of the subsequent sentence in the next paragraph, then this sentence should be joined to that same paragraph.

The expression 'lower conditions' doesn't really make sense and should be replaced – do you mean poorer socioeconomic conditions?

The expression "patients who paid their last outpatient/inpatient visit because of CVD" is used a few times and isn't really proper English. An alternative could be "patients whose reason for their last outpatient/inpatient.

In the sentence "It is worth noting that all information at community

	<p>level was directly collected from local officials through a constructed questionnaire.”, the expression ‘it is worth noting that’ is not necessary and should be removed. This fact is not simply ‘worth noting’ but crucial information about the methods of the study.</p> <p>“All data cleaning and statistical analyses were done by Stata 14.1” should be “All data cleaning and statistical analyses were done using Stata 14.1”</p> <p>Some English editing would be helpful. For example, for the sentence: “Regarding the health service utilization, there were about 15% of CVD patients (2011:334/2568, 2013: 463/3056) that reported being hospitalized at least once in the past year because of CVD, and 10% of the CVD patients (2011: 260/2568, 2013: 279/3056) used outpatient services because of CVD at least once during the past month.”, I would suggest: “Regarding health service utilization, about 15% of CVD patients (2011:334/2568, 2013: 463/3056) reported being hospitalized at least once in the past year because of CVD, and 10% of CVD patients (2011: 260/2568, 2013: 279/3056) used outpatient services because of CVD at least once during the past month.”</p>
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REVIEWER	Sirirat Anutrakulchai Faculty of Medicine Khon Kaen University Thailand
REVIEW RETURNED	10-Dec-2017

GENERAL COMMENTS	The authors already clearly improved the manuscript. I have no any comment.
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REVIEWER	Baowen Xue UCL Department of Epidemiology and Public Health, UK
REVIEW RETURNED	10-Dec-2017

GENERAL COMMENTS	<p>The revised manuscript has been much improved. I only have a few minor points as follows.</p> <p>Page 8 & Page 11: the first time you use the abbreviation of OOP in the manuscript, need to spell it out. Same as OTC.</p> <p>Page 15: The author first said there are 3 community-level control variables, then described 4 community-level control variables, and then put 3 community-level control variables in the table footnote.</p> <p>Page 20: “ For outpatient services in 2011...” need to be clear it is the indirect cost.</p> <p>Page 20: “According to the fourth quartile...”. It is the third quartile</p> <p>Page 20: “In 2011, CVD patients from the wealthiest...” I suggest adding coefficients as well. Need to report the Higher 20% group as well, as it’s also significant.</p>
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	Page 22 : " ...significantly lower among patients from more wealthy households." Need to at least show the ranges of ORs for these 3 groups of wealthy people.
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REVIEWER	Carlo Piccinni CORE (Collaborative Outcome Research)
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REVIEW RETURNED	11-Dec-2017
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GENERAL COMMENTS	The manuscript has been revised accordingly to all my comments and requests. I have no further comment.
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REVIEWER	Frank A. Sloan, Ph.D. Duke University USA
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REVIEW RETURNED	21-Dec-2017
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GENERAL COMMENTS	<p>Review of "The Association of Socioeconomic Status with Economic Burden of Disease...." MS bmjopen-2017-018703.R1</p> <p>General Comments</p> <p>This paper uses data from a national survey of households in China to assess the cost of cardiovascular disease (CVD) to households in families with this disease. The survey is useful in being national, being a panel, and having many variables not available in clinical data bases. However, as the authors acknowledge, CVD is self-reported. More importantly, the list of CVD-related items is not complete. It contains many elements of ischemic heart disease, but not other forms of this broad category of diseases, e.g., heart murmurs, arrhythmia, and TIA (stroke is included). There is no measure of severity of disease although this would certainly affect cost. Better data are available for other countries, but perhaps not for China.</p> <p>The paper does not describe the system of health care financing in China sufficiently. The paper discusses changes between 2011 and 2013. But it is not clear why these changes occurred. Were there changes in insurance coverage or do these changes reflect changes in the samples?</p> <p>It is not clear what inferences should be made for public policy based on these results? Do the results for CVD, even with the measurement errors, generalize to care as a whole?</p> <p>The survey only provides detail on the most recent visit. Generalizing from one month to the entire year seems shaky. Also, a visit might involve some investigation of CVD but this may be incidental to investigation of other health issues.</p> <p>The writing of the paper could be improved.</p> <p>Specific Comments</p> <p>p. 2. Lines 51ff. What make of changes between 2011 and 2013?</p> <p>p. 3. Line 31. Not clear what the policy implication is. If OOP payment is high, what comes next?</p> <p>p. 5. Line 24 and elsewhere. Economists would prefer the use of the term "financial" to "economic" in this context. Economic refers to the allocation of scarce resources.</p> <p>p. 5, Line 27. "perpetual" Not correct term.</p>
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	<p>p. 6. Line 21. Is CVD in the cited study defined the same way your study does?</p> <p>p. 8., line 4. "health reform is not performed as well as it could" This statement is much too general. Catastrophic spending... Is it defined in the Shandong study in the same way as your study. Basis of your definition of "catastrophic spending." Why select 40%. Is this standard?</p> <p>p. 10. DA007, ED018, etc. Far too much detail for a published study especially when so many other pertinent details are not included.</p> <p>p. 10, line 37. The sample is larger in 2013 than in 2011. Are the two samples comparable? Could some of the changes occurred due to aging of the sample?</p> <p>p. 11. Line 11. Self-buying. Poor English.</p> <p>p. 11. Line 24. Nursing costs during inpatient hospitalization is a direct cost.</p> <p>p. 12. Line 51. "education level might be negatively associated" There is a huge amount of literature on this topic.</p> <p>p. 13. Top. Much more literature.</p> <p>p. 13. Here and elsewhere there are tense changes. Categorized=past tense; is=present tense. These are examples.</p> <p>p. 14. "It should be noted.." You note it so the reader thinks you think it should be noted.</p> <p>p. 16, line 44. Results will depend on how CHE is defined. Is your definition of CHE what others use?</p> <p>p. 17, last line. Patients. Individuals are patients when they are hospitalized or visit a physician. Generally, they are individuals, not in the role of patients.</p> <p>p. 18, line 24. Clarify point.</p> <p>p. 20. Bottom. Why should the results for 2011 differ from those for 2013? Was a difference expected? If this is so, then what would one expect for more recent years?</p> <p>p. 21. What are the reasons for the urban-rural differences? Prices, insurance, demographic factors???</p> <p>p. 24. Line 14. The paper has not discussed cost control policies? Which ones?</p>
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VERSION 2 – AUTHOR RESPONSE

THE FOLLOWING IS Point-by-Point response

Reviewer #1

#1 The most important issue I would like to comment on with respect to this paper is the exclusion of any data on health insurance status or type of health insurance in the analysis (although some of this is capture by employment). In the introduction, the authors frame the research in terms of a potential failure of insurance reforms in China to sufficiently cover the poor, and claim that different types of insurance packages may provide more or less cover. In the conclusion the authors state that insurance must be made more comprehensive to protect the poor. While I do not disagree with these statements, I do not understand why the authors would not have tried to estimate the effect of insurance on expenditure and CHE in order to support their hypothesis and conclusions.

#1 Response: We thank the Reviewer for their feedback. The two core findings of this study are: (1) among CVD patients, poor households have higher odds of having CHE as compared to richer households; and (2) annual outpatient expenditure is higher than annual inpatient expenditure. Since the health insurance schemes are awarded based on SES indicator (e.g., urban resident specific health insurance), we use SES as a proxy variable to begin to understand its association with OOP

Payment. However, we cannot estimate the effect of insurance on expenditure and CHE because of the differences in health insurance benefit packages by province. To address this comment we added a section describing the health insurance design, revision, and enactment process more fully on page 29-30:

“It should be pointed out that although almost all the provinces adopt the structure of three basic insurance schemes, the benefit design regarding co-payment rate, deductibles and covered services might vary greatly across provinces and even cities in one province. Citizens from richer provinces normally have better insurance schemes, as well as more access to higher-quality health services. This is partly due to the political development and enactment of medical insurance schemes. Briefly, the Chinese Central Government devises general national health policies and guidance documents, then the individual province government enacts its own specific health policies and guidance (e.g., copayment rate, annual maximum limit). Thus, the health care benefits may vary by province. Future studies should build on our work by doing a cross-comparison of provincial policies and its association with CVD inpatient and outpatient services by employment, economic, and education status.”

#2 (in Methods) It is mentioned that the authors: “first estimated the per capita health expenditure for outpatient/inpatient service each time.... and then applied multilevel regression analyses to explore the association between one’s economic burden and socioeconomically influencing factors”. What does ‘each time’ mean? Did they sum all expenditure for all visits over the interview period? And how was ‘economic burden defined’? Was it simply the sum of expenditures or the proportion that expenditures occupied of overall consumption?

#2 Response: We thank the Reviewer for their feedback and have revised the wording to be more understandable. Specifically, we changed “each time” to “per visit”. Briefly, the annual health expenditure for outpatient service was calculated by the cost for the last outpatient visit * number of outpatient visits during the past month*12; the expenditure for inpatient service was calculated by the cost for the last inpatient visit * number of inpatient services during the past year. Two outcome variables were used to define one’s financial burden (previously ‘economic burden’, but now changed to ‘financial burden’ to address another’s comment): (1) the annually monetary medical cost for each CVD patient, including outpatient and inpatient and self-medication cost; and (2) catastrophic health expenditure, defined as spending over 40% on health for each household. We have clarified the calculations on page 12-13.

#3 (in Methods) It is mentioned that the CHARLS only interviewed individuals aged 45 and older, but in the next paragraph the authors state that they excluded individuals aged 45 and younger for this study. If they are not included in the CHARLS in the first place, why do they need to be excluded for the analysis?

#3 Response: We thank the Reviewer for noticing this. CHARLS included primary respondents (aged 45 and above) and their spouse (if any, and could be younger than 45). Respondents who were younger than 45 took a proportion about 5%. So we need to exclude the data entries of those who aged less than 45 from the raw database. We have clarified this on page 10.

#4 (in Methods) How was household expenditure estimated? Was it self-reported? For the last year or last month?

#4 Response: In CHARLS, household expenditure is measured by a series of questions that included food expenditure, durable goods purchase, utility, communication and transportation cost. Interviewees were asked to report the cost estimate for a specific item during the past week (e.g., food expenditure), month (e.g., communication and transportation cost) or year (e.g, furniture purchase). We have included this information on page 14-15 of the manuscript.

#5 (in Methods) As this is a longitudinal study, are the same households included and compared in 2011 and 2013? This should be clarified.

#5 Response: We thank the Reviewer for their feedback. We have added additional wording to explicitly state that CHARLS is a longitudinal survey (p. 9-10). In our analysis, we included both the same households that agreed to follow-up, and newly interviewed households. In this latest submission we include the exact number of retained participants and the overall retention rate (p.10-11)

#6 (in Results and Discussion) The finding that people spent more on outpatient services than inpatient is surprising, even if you account for higher frequency of outpatient visits. Most studies tend to find higher outpatient expenditure, because these services are simply much more expensive (e.g. operation vs checkup). This finding should be discussed with possible explanations. Are some inpatient services covered whereas outpatient services are not? Or do you really think the result is simply due to a very high frequency of outpatient visits?

#6 Response: We thank the Reviewer for their comment. To address the comment we have added more discussion about this unexpected finding. Briefly, in addition to the annual outpatient utilization rate being higher (as compared to inpatient), outpatient services has higher deductible, copayment-rate and lower maximum reimbursement limit than that for inpatient service. To date some insurance schemes do not cover outpatient services in certain provinces. A similar acknowledgement concerning the lack of emphasis on outpatient services has been made (Yip & Hsiao, 2009). According to Yip and Hsiao (2009) "As chronic diseases impose a growing share of the burden on the population in developing countries, it is not necessarily true that insurance coverage focusing on expensive hospital care alone is the most effective at providing financial risk protection."

#7: I believe in the 'setting' section of the Abstract some information should be given about the Chinese context, not only the survey.

#7 Response: It is now changed into "A nationally representative survey - The China Health and Retirement Longitudinal Survey (CHARLS) - was conducted in 28 provinces of mainland China in 2011 and 2013." Due to words limitation, we couldn't include detailed information here, but more information about the survey and sampling setting could be found in the methods section.

Reviewer #2

#1 There is no measure of severity of disease although this would certainly affect cost. Better data are available for other countries, but perhaps not for China.

#1 Response: We thank the Reviewer for their feedback. While we were not able to measure the severity of a single disease from the CHARLS data set, we created a proxy variable that we could inform us of the number of cooccurring non-communicable diseases. We used this variable within our regression models.

#2 The paper does not describe the system of health care financing in China sufficiently.

#2 Response: We thank the Reviewer for their comment. We have added more background information about the healthcare financing in China (p. 6-7). Specifically, we discuss how the health care system is funded, and the unique differences among the insurance schemes.

#3 The paper discusses changes between 2011 and 2013. But it is not clear why these changes occurred. Were there changes in insurance coverage or do these changes reflect changes in the samples? It is not clear what inferences should be made for public policy based on these results? Do the results for CVD, even with the measurement errors, generalize to care as a whole? The survey only provides detail on the most recent visit. Generalizing from one month to the entire year seems shaky. Also, a visit might involve some investigation of CVD but this may be incidental to investigation of other health issues.

#3 Response: We thank the Reviewer for their feedback. We have revised the paper to more clearly show the differences in CHE by socioeconomic status indicator and by year, after controlling for demographic variables within and across samples. Our paper aimed to show the financial burden associated with CVD OOP payment (from outpatient services utilization) by socioeconomic indicator. Since the health insurance schemes are awarded based on SES indicator (e.g., urban resident specific health insurance), we use SES as a proxy variable to begin to understand its association with OOP Payment. To address the reviewer's concern, we have now included generalizability and estimation as limitations within our discussion section (p.30, p.24-25).

#4 The writing of the paper could be improved.

#4 Response: We thank the Reviewer for their comment. We have worked to improve this version of the manuscript.

Specific comments:

#5: p. 2. Lines 51ff. What make of changes between 2011 and 2013?

#5 Response: In the discussion section, we mention changes that occurred, such as price factor (p. 25). With this in mind, we make an effort to report findings by year, in addition to across years. Due to the abstract word limitation we could not provide more details on the changes.

#6: p. 3. Line 31. Not clear what the policy implication is. If OOP payment is high, what comes next?

#6 Response: Based on the reviewer's comment we had added more information to clearly state the policy implication (p.3, p31). Briefly, the policy implication is that mitigating the disparities in OOP

payment between outpatient and inpatient services can lead to better protection from CHE among patients with CVD. Thus, policymakers should lower copayment and indirect costs associated with outpatient services. While some CVD patients may use in-patient services, all CVD patients use outpatient services multiple times a year.

#7: p. 5. Line 24 and elsewhere. Economists would prefer the use of the term “financial” to “economic” in this context. Economic refers to the allocation of scarce resources.

#7 Response: Thank you for this suggestion! All changed accordingly.

#8: p. 5, Line 27. “perpetual” Not correct term.

#8 Response: We thank the Reviewer for their comment. We have changed “perpetual” to “consistent” (p. 5)

#9: p. 6. Line 21. Is CVD in the cited study defined the same way your study does?

#9 Response: We thank the Reviewer for noticing this. The definition of CVD in the cited study is different to the one we used in analysis. A clear definition in the cited study is now added to the manuscript (p.6).

#10: p. 8., line 4. “health reform is not performed as well as it could” This statement is much too general. Catastrophic spending... Is it defined in the Shandong study in the same way as your study. Basis of your definition of “catastrophic spending.” Why select 40%. Is this standard?

#10 Response: We agree with the Reviewer that it is important to provide less general comments. To address this comment, we deleted the sentence. We use the O’Donnell’s book of “Analyzing Health Equity Using Household Survey Data” and publications from the World Health Organization (WHO) to define catastrophic spending. Both WHO and O’Donnell suggest a cutoff point of 40%.

#11: p. 10. DA007, ED018, etc. Far too much detail for a published study especially when so many other pertinent details are not included.

#11 Response: We thank the Reviewer for their feedback. We have removed the item numbers and briefly include the question itself. This can be found on page 10-11.

#12: p. 10, line 37. The sample is larger in 2013 than in 2011. Are the two samples comparable? Could some of the changes occurred due to aging of the sample?

#12 Response: We thank the Reviewer for their feedback. Although the majority (69.1%) of the 2,568 patients enrolled in 2011 were retained in 2013. In 2013 (the first follow-up), CHARLS did recruit new households with comparable demographic background to replace those who dropped out. We keep

both follow-ups and newly interviewed participants into analysis. We use “age” as a control variable within our regression models (p. 15).

#13: p. 11. Line 11. Self-buying. Poor English.

#13 Response: We thank the Reviewer for their feedback. We have altered all incidences of “self-buying” to “buying.”

Reviewer #3

#1: Page 8 & Page 11: the first time you use the abbreviation of OOP in the manuscript, need to spell it out. Same as OTC.

#1 Response: We thank the Reviewer for their feedback. We have spelled out the OOP(p.9) and OTC(p.11) abbreviations.

#2: Page 15: The author first said there are 3 community-level control variables, then described 4 community-level control variables, and then put 3 community-level control variables in the table footnote.

#2 Response: We sincerely thank the Reviewer for noticing this and have made changes to both the manuscript (p. 15) and footnote of Table 3 and Table 4 (p. 34-35) There are 5 variables: (1)per capita annual net income, (2) regional location (east/middle/west part of China), (3)availability of healthcare services, (4) availability of transportation services, and (5) population size.

#3: Page 20: “ For outpatient services in 2011...” need to be clear it is the indirect cost.

#3 Response: We thank the Reviewer for their feedback. We have edited the page 21 to clearly show that we are discussing indirect cost.

#4: Page 20: “According to the fourth quartile...”. It is the third quartile

#4 Response: We thank the Reviewer for noticing this and have altered the wording to reflect that it is indeed the third quartile (page 21).

#5: Page 20: “In 2011, CVD patients from the wealthiest..” I suggest adding coefficients as well. Need to report the Higher 20% group as well, as it’s also significant.

#5 Response: We have added coefficients and have reported the higher 20% group as well. These changes can be found on page 21.

#6: Page 22 : " ...significantly lower among patients from more wealthy households." Need to at least show the ranges of ORs for these 3 groups of wealthy people.

#6 Response: We thank the Reviewer for their feedback. We have added the Odds Ratios at a 95% confidence interval for each of the three groups. The changes can be found on pages 23.

Thank you Dr. Sirirat Anutrakulchai (Reviewer#4) and Dr. Carlo Piccini (Reviewer#5) for your insight and comments on our last version.

VERSION 3 – REVIEW

REVIEWER	Adrianna Murphy London School of Hygiene and Tropical Medicine
REVIEW RETURNED	04-Feb-2018

GENERAL COMMENTS	I would like to thank the authors for their responses to my earlier comments. I am satisfied with their revisions. I am not suggesting any re-analysis using different measures of catastrophic spending, but for future work the authors may want to consider current debate of the advantages/disadvantages of different approaches to measuring this indicator. These are discussed most recently in the Dec 2017 World Bank/WHO Global UHC Monitoring Report: http://www.who.int/healthinfo/universal_health_coverage/report/2017/en/ .
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REVIEWER	Baowen Xue UCL Research Department of Epidemiology & Public Health, London, UK
REVIEW RETURNED	09-Feb-2018

GENERAL COMMENTS	The manuscript has been revised accordingly to all my comments and requests. I have no further comment.
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