Author's response to reviews

Title: Effects of changes in health insurance reimbursement level on outpatient service utilization of rural diabetics: Evidence from Jiangsu Province, China

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Author's response to reviews: see over
Dear Armee:

We must thank you and all other reviewers for the critical feedback. We feel lucky that our manuscript went to these reviewers as the valuable comments from them not only helped us with the improvement of our manuscript, but suggested some neat ideas for future studies. Please do forward our heartfelt thanks to these experts. Based on the comments we received, careful modifications have been made to the original manuscript. All changes made to the text are in red color. In addition, we also have an expert English expert to check the English for the revised version. We hope the new manuscript will meet your magazine’s standard. While we have detailed point-to-point responses to each member of the review team, we first provide below a quick summary of the major changes in this revision.

Summary of Major Changes in this Revision
1. The data and methods were improved and also added more specific explanations. Propensity Score Match (PSM) was made in order to enhance comparability between two groups. And corresponding model estimations and interpretation of results have been done newly.
2. Hypotheses have been revised according to the suggestions of reviewers.
3. Most contents in the sections of Discussions and Conclusion were rewritten according to the suggestions of reviewers.

Responses to Reviewers’ Comments
Thank you very much for your very clear, thoughtful, and constructive comments on the previous version of our manuscript. As you will see, we have greatly benefited from your feedback and the revised manuscript has significantly improved in clarity of both the theory and the contribution. The major changes to the paper have been summarized earlier in the response note. We respond below to your specific comments.

Referee Xuezheng Qin:

Major Compulsory Revisions:

Q1. The DID analysis requires the “parallel trend” assumption, i.e., the treatment group and the control group will have the same utilization pattern in the absence of the reimbursement policy reform. Thus, the authors should convince the readers that the two groups of patients are comparable in the above sense. This can be done, for example, by doing a statistical test that shows whether the two groups are significantly different in baseline characteristics.
Answer: Thank you very much for your positive assessment of our work. Propensity Score Match (PSM) was made in order to enhance compatibility between two groups. However, because the policy variable was divided into three groups for the estimation model of yearly total outpatient expenses and PSM is not suitable for three groups, we did not use PSM before DID estimation about yearly total outpatient expenses.

Q2. Another concern on the DID model: If patients can travel to other counties to seek care, then the DID analysis may not be valid. For example, if sicker patients can travel to counties with higher reimbursement rate to cover their higher medical cost, then the increase in reimbursement level will increase the county's medical expenditure simply because there is a change in patient mix rather than the hypothesized effects. The authors should take this possibility into account and make some efforts to address this potential issue.

Answer: Thank you again for your keen observations. The DID analysis in this study should be valid. Firstly, the research objects were rural common patients with type 2 DM. There is no obvious difference of health service quality for these patients such as treatment and drugs are prescribed among different sample counties. According to the interview on administrative staffs of NRCMS, the case that patients go to other counties' corresponding health institutions to receive health service rarely happens because patients only have less than half of the reimbursement rate in their own county than that in other counties. Thus, in the situation that no big gap between the service quality of different counties and the limited reimbursement rate in other counties, patients without serious complication are fairly less likely to seek health service outside their counties. Secondly, patients only can be reimbursed in their local cooperative medical management center they belongs to, thus patients from in other counties will not be recorded in the database we researched. Thirdly, usually, only patients with serious complication will go to higher level health institutions out their own counties to seek treatment. In fact, the number of those patients is very few. In addition, those patients should also be excluded in our research objects due to their serious complication.

Q3. In China, there is also a difference between the nominal and the actual reimbursement rates. Although a nominal rate can be fixed for all DM patients, the actual reimbursement rate can be different from person to person depending on what services/drugs are prescribed. The authors should be able to use the NRCMS claim data to check whether the actual reimbursement rates are on
average close enough to the nominal rates defined by the three counties. Otherwise, the assumption on the difference between “treatment” and “control” groups in the DID model can be invalid.

Answer: Thanks! The reimbursement rate in this research is policy reimbursement rate. All services and drugs included in the scope of reimbursement can be reimbursed according to this rate. Because some services and drugs are not included in the scope of reimbursement, the actual reimbursement rate can be different from person to person depending on what services and drugs are prescribed. However, the increase of the policy reimbursement rate will surely lead to the increase of the actual reimbursement rate, because the scope of reimbursement in sample counties from 2010 to 2011 is constant. Our research goal is to analyze the effects of policy reimbursement rate in different health service institutions on the utilization of outpatients with type 2 DM, so we do not pay much attention to the actual reimbursement rate. In addition, the policy reimbursement rate and actual reimbursement rate of the majority of outpatients is equal because the research objects were rural common patients with type 2 DM, which is in very different with that of inpatients.

Q4. Second, the econometric methodology also has several issues. For example, the authors estimate the choice of health institutions separately, using each type of institution as the dependent variable in one regression. This approach will lead to different “base choice” in different regressions, making it hard to compare the results across regressions. A better approach to model patients’ facility choice is to consider all facility types jointly by using discrete choice models (such as Multinomial Logit) or Conditional Logit models or sequential choice models (such as ordered probit model). The sequential model implicitly assumes that there is a natural ranking among 3 types of health facilities (county hospitals, THC, and village clinics).

Answer: The data was used is two years of panel data of every patient. We used these data to analyze the effect of the change of the reimbursement rate in the second year on the probability of utilization of the institution through comparison between two years. Discrete choice models or sequential choice models cannot estimate the net effect of policy variables. Therefore, DID models were used in this study.

Q5. Also, the authors use different policy dummies for different analysis. For estimation on outpatient medical expenditure, the policy dummies are Diff-count 2 and Diff-count 3; for
estimation on health facility choices, the policy dummy is Diff-rate. Why not use the same policy indicators in both analyses?

Answer: This is also explained in the below explanation for another reviewer.

Q6. Third, the authors should be more careful with the interpretation of results. For example, in the conclusion, the authors interpret the increase in outpatient expenditure as "improved access to outpatient services". However, I don't think these two concepts are the same, as increased expenditure can also result from an increase in price. To prove that access to medical care is in proved, the paper needs to show that patients with DM are more likely to seek treatment with the increase in reimbursement level, but this kind of information is not currently provided in the paper. Another related complication is the concept of "moral hazard": when faced a higher reimbursement rate, a patient may increase her consumption of both necessary and non-necessary medical care, resulting in welfare loss due to "overconsumption". To see whether the increase in utilization is "improved access" or "overtreatment due to moral hazard", the authors can use the NRCMS claim data to see what types of services are being increasingly used with the higher reimbursement rates.

Answer: Thanks, this has been revised in the manuscript.

Q7. Similarly, for the interpretation of non-significant impact on patient health facility choice, the authors argue that the reason is "due to the relatively small changes in reimbursement relative to patients' total out-of-pocket expenses and the small differences in fees charged by the different institutions". However, as mentioned in the introduction section, the three types of facilities differ significantly in quality and cost, thus if maximum reimbursement amount / rate is raised, more patients should choose the "higher quality" facilities and pay higher cost. Why is it not the case?

Answer: Thanks, this has been revised in the manuscript.

Q8. Fourth, some policy implications are not well rooted in the empirical findings. For example, "measures to improve the quality of care and scope of services at lower-level healthcare institutions, such as...", should be taken if the goal is to avoid patients overwhelming county hospitals and to
locate care closer to where rural people resided.” I cannot find any evidence from the regression results to support this kind of policy suggestions.

Answer: Thanks, this has been revised in the manuscript.

Minor Essential Revisions:

Q1. Introduction section: in the statement of two hypotheses, rather than simply saying “reimbursement for outpatient services will affect total outpatient expenses and utilization of different types of healthcare institution”, the authors should clarify on the expected directions of such impacts (i.e. increase or decrease) and provide theoretical foundation for such expectation.

Answer: Thanks, this has been revised in the manuscript.

Q2. Method section: vector X in the regression equation only contains age, sex and severity of illness. Some other important factors are missing that can also contribute to the medical expenditure or health facility choices. These include, for example, income, education, household size, marital status, employment status, etc. Although the authors acknowledge that these variables are not available in the dataset, they should further discuss whether this variable omission problem will lead to estimation bias in the DID framework.

Answer: This has been revised in the manuscript.

Q3. Method section, since P is a dummy variable, it cannot indicate the “extent” of policy change, it can only indicate the “treatment status”.

Answer: Thanks, this has been revised in the manuscript.

Q4. At some places, the authors say the paper is using two year panel data, and in other places the authors say it is based on repeated cross-sections. The paper should make it clear what kind of data is being used. If the same cohort of patients is being followed in 2010 and 2011, then this is panel data, otherwise, the data should be labeled as repeated cross-sections.
Answer: Yes, the same cohort of patients was followed in 2010 and 2011. This has been revised in the manuscript.

Other language improvements have also been made.

Discretionary Revisions:

In the method section, the authors stated that “difference-in-differences (DID) with matching was used to make model estimates”, but I did not find any “matching” step in the estimation. From the regression equations, I can see that the current model is a standard DID model. In fact, it would be better if the author can perform the DID with matching (using propensity scores) to make the results more robust.

Answer: Thanks, this has been revised in the manuscript.

Reviewer: Su Liu

Major compulsory revisions:

Q1. The current literature review as part of the introduction is weak. The authors claimed that “to the best of our knowledge, there are no studies that analyze the impact of outpatient reimbursem ent level changes on health service utilization.”

In fact, the literature on how changes in insurance copayment or coinsurance rate might affect utilization is quite extensive, the most seminal one being the RAND experiment (see Manning et al. 1987 AER paper). Evidence of moral hazard has been well established internationally. It’s important to include it in the literature review, and address how the paper contributes to its advancement.

Answer: Thanks for your valuable suggestions; this has been revised in the manuscript.

Q2. Equation 3 (though it was not denoted) that was used to describe the model for this study has some serious problems:

Answer: Thank you again for your keen observations. Equations have been divided into two kinds and were interpreted respectively and this is also explained in the following explanation for another reviewer.

Q3. I do not understand why in Table 3, each of the counties with policy changes was estimated separately (through Diff-amount 2 and Diff-amount 3), whereas in Table 4, they are combined together (through only one Diff-rate)? The changes in the reimbursement rates in Gaoyou and
Rudong are quite different – they should have been estimated separately in Table 4. Why didn’t the authors do so? There needs to be a solid explanation here.

Answer: Yearly max un-reimbursed amount means the highest compensation amount of total outpatient medical expenditure of a single patient in a year, which includes all outpatient medical expenditure in all health facilities. In short, yearly max un-reimbursed amount is not related to the kind of health institution. However, reimbursement rate was different among three levels of health institutions, and the change of reimbursement rate between two years was also different among three levels of health institutions. Thus, for estimation on outpatient medical expenditure, reimbursement rate cannot be used as the policy dummies. At the same time, for estimation on health facility choices, yearly max un-reimbursed amount cannot be used as the policy dummies.

Theoretically, yearly max un-reimbursed amount has positive impact on total outpatient medical expenditure, and the change of reimbursement rate at every level health institutions have positive impact on corresponding utilization at this type of health institution. Therefore, we used the change of yearly max un-reimbursed amount for outpatient services to estimate the model of outpatient medical expenditure, and used the change of outpatient reimbursement rates at different health institutions to estimate the models of utilization of different types of healthcare institution.

Q 4. I have a few concerns about how findings are interpreted and policy recommendations made in the discussions:

a. Paragraph 3, the authors said, “generally speaking, raising yearly max un-reimbursed amount for outpatient visits by rural residents with type 2 DM should be advocated”. I generally agree with this, but one must notice, the current policy did not just raise the reimbursement for people with chronic conditions, it’s for all people – should one worry about the negative impact of moral hazard? Or suggest differentiated policy treatment?

Answer: Thanks for your thoughtful comments. This has been revised in the manuscript.

b. Paragraph 4, the calculation of the average after-reimbursed OOP expenses per patient seems to be wrong! If one were to use simple multiplying as suggested by the paper, 152.3 x (1-22.8%) = 117.57, NOT 34.72. Unless I completely got this wrong, the OOP difference between different levels of institutions should be much larger than what the paper currently described. Please double check. In fact the data provided in Table 5 do not seem to be consistent with the text description. If Table 5 is correct, and consider this is per outpatient visit, for someone from rural with DM, the difference does add up to be quite significant.

Answer: Thanks, this has been revised in the manuscript.
c. Paragraph 5, The authors said “Compared with those aged 18-30, patients in younger and older age groups were more likely to visit village clinics. Therefore, THC s and county-level hospitals should be made more accessible to younger and older patients, in order to facilitate better access, if their health conditions require care at higher-level institutions.” I think this policy recommendation sounds very strange: who do you mean by “younger and older” patients, everyone except 18-30 aged? There could be a simple explanation behind this finding: 18-30 are much more likely to be migrant workers working in counties rather than staying behind in villages (like children and elderly). They chose the institution because it’s more easily accessible (there could be other reasons as well, but I doubt it’s due to conditions requiring care differently).

There could be a simple explanation behind this finding: 18-30 are much more likely to be migrant workers working in counties rather than staying behind in villages (like children and elderly).

Answer: Thanks for your thoughtful comments. This has been revised in the manuscript.

d. Paragraph 7, regarding the finding on females, if I’m understanding this right, Y2+Y3+Y4 should be equal to 100%, right? So if females are less likely to visit village clinics and THCs, by definition, shouldn’t they be more likely to visit county-level hospitals?

Answer: It is true according to the new estimation results. This has been revised in the manuscript.

Minor essential revisions:

Q1. There are quite a few grammar mistakes and typos throughout the manuscript. Suggest authors review it carefully and the language to be edited thoroughly.

Answer: Thanks, the language has been edited thoroughly by a native English speaker.

Q2. One of the paper’s contributions was said to be “the first attempt to conduct a tracer illness study in order to control possible biases associated with studying several diseases together.” I didn’t quite follow this argument in the beginning (in the abstract, as well as end of paragraph 5 under introduction). As I read the paper, I think what the authors meant simply is that previous studies have looked at a large population without controlling for heterogeneity across different diseases. Regardless of whether I interpret this right, I think the authors should make a better effort explaining the argument and claiming this contribution.

Answer: Thanks, this has been revised in the manuscript.
Q 3. In describing the hypotheses, it would be more helpful if the authors could attempt to specify the direction of expected changes (whether it's positive or negative), as opposed to just saying “change of X would affect Y”.

Answer: Thanks, this has been revised in the manuscript.

Q 4. Data. Paragraph 1. “(The data collected by us are available upon request)” – is this data different from “the province-level NRCMS management database”? There is only one single data source, right?

Answer: Yes, this has been revised in the manuscript.

Q 5. Clarification please:
   a. How were the type 2 diabetic outpatients identified in the data? Through diagnosis code, such as ICD-9?

Answer: Type 2 diabetic outpatients identified in the data according to ICD-10 code. This has been clarified in the manuscript.

   b. Why was the analysis limited to individuals aged 10 years and over? Is this due to data availability or some other concerns?

Answer: Individuals aged 9 years and under suffer few type 2 DM and their information are usually not complete. This has been clarified in the manuscript.

   c. Data. Paragraph 2. Figures quoted here for proportions of visits to different level of institutions referred to just Jiangsu or the whole China?

Answer: We accept the referee’s suggestion; this has been clarified in the manuscript.

Q 6. Table 2. Suggest add in descriptive statistics for the outcome variables as well.

Answer: We accept the referee’s suggestion; this has been revised in the revised manuscript.

Q 7. Table 5. See my earlier comment 4b in the last section. Also please clarify if the numbers presented here are “average total medical expenses” or “average OOP expenses”.

Answer: We accept the referee’s suggestion; this has been revised in the revised manuscript.
Answer: They are average OOP expenses. This has been clarified in the manuscript.

Discretionary revisions:

Q1. Results. Paragraph 1. It would be good if the authors could attach a dollar value to the “9.1% increase in total outpatient expenses as a result of 40 yuan increase in max reimbursement amount”. I was also wondering how many patients exceeded the max amount, or just stayed barely under? That might have more policy implications than just the percent increase.

Answer: It means that compared with the patients whose max reimbursement amount remained unchanged, there were 9.1% more likely to increase the total outpatient expenses for the patients whose yearly maximum reimbursement amount increased by 40 yuan. 9.1% is likely and a dollar value cannot be obtained.

Finally, we appreciate very much for your time in editing our manuscript and the referees for their valuable suggestions and comments. I am looking forward to hearing from your final decision when it is made.