Author’s response to reviews

Title: Disaggregating catastrophic health expenditure by disease area: cross-country estimates based on the World Health Surveys

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Lin Lee, DPhil
Chief Editor
BMC Medicine

Dear Dr. Lee:

Thank you for the opportunity to resubmit our paper “Disaggregating catastrophic health expenditure by disease area: cross-country estimates based on the World Health Surveys” for consideration for publication by BMC Medicine. In this revised manuscript, we have carefully addressed each of the comments raised by the reviewers.

Attached to this letter you will find a point-by-point response to the reviewers’ comments. We have fully responded to and incorporated all the feedback from both reviewers, which has significantly strengthened the manuscript.

The two reviewers highlighted key areas for improvement. First, they commented that more justification for and interpretation of the regressions conducted was warranted. As a result, we
have added substantial explanation to the Methods and Discussion sections on regression selection, choice of controls, and results interpretation.

Second, the reviewers suggested contextualizing our findings with a health system lens to enhance their policy relevance. We have therefore added a panel to Figure 1, depicting the distribution of catastrophic health expenditure by disease with pooled financing, which captures the extent to which a health system is organized to protect people from the financial risks of disease. The health system implications of our analysis are also discussed at greater length in the Discussion section.

Finally, we made additional minor changes per the reviewers’ suggestions, including: more detail on the “other” disease category, justifying our choice of survey and methods for estimating disease-specific catastrophic expenditure, and contextualizing our findings with the published literature.

We believe that depicting the distribution of catastrophic health expenditure by disease is a critical input to conversations about universal health coverage (UHC) and the Sustainable Development Goals (SDGs). Understanding this distribution can help policymakers better design essential health benefits packages, insurance programs, and other policies to reduce the burden of out-of-pocket expenditures and illness-driven impoverishment.

Thank you for your consideration of our work. We believe that this paper is of high relevance to policy-makers and researchers invested in charting a path to UHC and thus of great interest to the BMC Medicine readership.

Please do not hesitate to contact me if you have any questions.

Kind regards,

Annie Haakenstad, MA

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Enclosures: Point-by-point Response to Reviewers
This manuscript conducts an economic analysis of catastrophic health expenditures (CHE) in LMICs using data from the World Health Survey (2002-4). The authors determine household income and then remove food expense and use the net as the available household income. Then they determine the proportion of this net that is spent on health care during the last month. They find substantial CHEs are across all countries with a range from 15% to 30%. They report a positive correlation between CVD CHE and the global burden of disease level of CVD burden in country. A large proportion of the CHE is due to causes that cannot be determined. They were not able to look at health care coverage in their analyses. They conclude that financial risk protection from disease may not always align with national disease burdens.

Thank you for your careful review and helpful comments.

With the SDG focus on universal healthcare coverage, this is a well-conceived and well written manuscript on a very important and timely topic. There are some areas where in could be improved including a better explanation of how the age of the WHS data may be affecting the results and the large proportion of medical cause being in the other (not defined) category.

Thank you for the positive comments on our manuscript. We have added more details on what could be captured by the “other” category in the Discussion section (page 15, paragraph 2). We have also expanded on the limitations related to the age of the dataset in the Discussion section (page 15, paragraph 3).

While this type of study is invaluable in the context of rolling out UHC, the older data from the WHS created challenge to its current relevance. It seems this may have been the only standardize dataset that covered so many countries that is currently available for analyses. It would be helpful if the author could discuss the dataset selection and the decision to go with the WHS - including a more detailed understanding of what some of the limitation might be. There may be country-specific data that could be compared to the WHS data for an analyses similar to what was done - to reassure that the older data is at least similar to what we have more currently.
The WHS was the only survey conducted in more than 6 countries that captured data on why care was sought, associated that care-seeking with spending and thus could be used to estimate CHE by disease. A number of country-specific surveys, such as the 74th National Sample Survey in India, as well as the WHO's Survey on Global Aging and Adult Health (SAGE) surveys in 6 countries asked similar questions. Thus, to-date, using WHS was the only way to empirically compare CHE estimates by disease area across a wide range of countries. We have added more information justifying the use of the WHS and the limitations of other existing datasets in the Methods section (page 5, paragraph 3).

Reviewer #1

Comment #4

Also, what might be important and enlightening would be to try to describe the level of healthcare coverage and/or the healthcare financing across various countries in 2002-2004. Likely it will have change much since but this may provide better insight into some of the findings and trends.

We fully agree with the reviewer. We have now added a component to Figure 1 examining the distribution of CHE by disease by levels of pooled financing, defined as the sum of government health expenditure and prepaid private expenditure as a share of total health expenditure. This allowed us to compare across countries this crucial health financing feature that could have an effect on CHE incidence.

Reviewer #1

Comment #5

The analyses of looking at the GBD country burden and the correlation to CHE for a specific disease may be pushing the data beyond it limits. More explanation of why this approach is justified would be helpful. It is a very interesting finding of the direct correlation of CVD burden and CHE. However, it is challenging to understand why injury and communicable diseases are more flat in their correlation. A more detailed explanation of why we think this is the case would be useful. Again, with this situation, if a few country have better data from other sources, it would be helpful to confirm these types of profiles.

Thank you for this comment. We have now added further justification of our approach. (page 8, paragraphs 1-2). We believe an important contribution of quantifying CHE by disease is to examine the extent to which it reflects disease burden. The finding of the relationship between CVD prevalence and CVD-related CHE may reflect a growing availability of CVD care,
including more intensive care (e.g. advanced diagnostics and surgery) where CVD burden is higher. Because of the advanced care possible, it is also possible to spend substantially more, thus driving up CHE. Countries’ financial protection policies may also not be keeping up with spending, lagging behind the increases in CVD spending. The fact that a similar proportion of individuals are affected by injury CHE, even as injury incidence increases, can reflect the adaptation of financial protection measures to the larger proportion of individuals affected by such disease burden as well as less need to use expensive treatment. More discussion on this aspect of our analysis was added to the Discussion section (page 8, paragraph 2).

Reviewer #1
Comment #6

Another major challenge is sorting out the issue of the "other" disease category. More discussion on how this may be impacting the conclusions would be useful.

We have added more discussion of the “other” category in the Discussion section of the manuscript (page 13, paragraph 1).

Reviewer #1
Comment #7

The profile of CHE at 15% in LICs, 30% in LMICs, and 15% in UMICs probably needs better explanation. We would think this would be a positive relationship with the largest CHE proportion in the LICs. However, the assumption is that in LICs, care may not be available, or, if it is, it is being forgone. Again, looking at some example of individual countries would be reassuring that this is a valid explanation.

While surprising, our results are consistent with the existing literature: Wagstaff et al. (2018) detect a statistically significant and positive association between GDP per capita and CHE, which they postulate is related to greater service availability, more use of expensive technology, and higher prices. Our results differ slightly: Wagstaff et al. (2018) have more of an inverse U-shaped curve, which is related to two components. First, the results in table 1 are population-weighted. The lower-middle income country results are driven predominately by India, which has some of the highest CHE rates globally; Wagstaff et al. (2018) do not use population weights in their regression analysis. Second, compared to Wagstaff et al. (2018), we used a different CHE measure – 40% of capacity of pay—which is more sensitive to CHE among the poor (which constitute a larger share of the population in low- and lower-middle income countries). When we replicated Wagstaff et al.’s results we obtained a similarly positive direction on the CHE metrics
they used, although our results were not statistically significant since our sample size was smaller. We have added further discussion about this relationship between GDP per capita and CHE and references to the literature in the Discussion section (page 13, paragraph 2).

Reviewer #1

Comment #8

Specific Comment: Suggest to grafts of the data in table 2 using the fully control model.

We considered this suggestion very carefully, but concluded that including the suggested controlled fit would make the figures more difficult to interpret. Because the coefficients did not vary substantially, the fits represented in the figure were also a very close depiction of the controlled results.

Reviewer #2

Comment #9

I was recently asked to review 'Disaggregating catastrophic health expenditure by disease area: cross-country estimates based on the World Health Surveys." I quite like the premise of the manuscript, but find the manuscript to be a bit confusing. It seems to want to look at CHE by disease, but then it adds in a few regressions. I did not really find the discussion in the manuscript to be particularly clear on the value of these regressions, or even the need for them.

Thank you for your helpful comments and insightful suggestions which significantly strengthened our manuscript. We have now added more justification for the regressions and carefully discussed the related findings in the updated manuscript (page 8-9).

Reviewer #2

Comment #10

Firstly, I am a bit skeptical that a 'past 30 days' analysis tells us quite what is claimed it does. If it is to do so, one must be willing to assume that (a) illness is randomly distributed across time and place, (b) the depth of illness, and, thus, the costs associated with the illness are also randomly distributed, and that (c) it is entirely appropriate to ignore those who did not suffer any illness in the past 30 days. Given the number of countries involved and the number of overall observations, the first two, in particular, are reasonable assumptions. The third, is also probably
ok, but if they have actual health spending (and they would generally), then shouldn't they be included?

Health spending by individuals without an illness, such as a general check-up, would be included in the “other” category. All health spending was considered in our analysis, but we were limited by the response options in the survey as to the reason for health spending. We now mention this in the limitations section.

Reviewer #2

Comment #11

The bigger concern I have regarding these assumptions is that there was one illness that was a serious problem in Africa, in particular, around that time. Yet, the authors do not give much thought to HIV/AIDS. In African countries, this is likely to be an important component of the 'other' category, due to the stigma associated with it. Thus, I think additional thought in that regard is necessary.

Thank you for this important comment. We agree that the stigma associated with HIV/AIDS could theoretically affect a respondent’s choice in responding to a survey question about why care was sought. However, HIV/AIDS was not a response option in this case and thus any spending on this health area would be delegated again to the “other” category. Because HIV/AIDS was not a response option, we do not include it in our discussion or analysis, but we agree that this could be playing a role in CHE and is an important topic. We added mention of this limitation (page 15, paragraph 3).

Reviewer #2

Comment #12

Secondly, although I like the premise of the paper, it is far from clear that there is a need to report the findings across World Bank income categories. In terms of financial protection, I would have more interest in the type of health system and implied coverage. For example, did people have access to private insurance, social insurance, is the public system 'free', does it require referral up? Some attempt was made to address that in the regressions, but my basic point here remains.

This is a very helpful comment. We agree that the organization and financing of the health system plays a major role in financial risk protection provided in countries. We have now added a figure that grouped countries by the share of health expenditure that was pooled, both from public and private sources (the sum of government health expenditure and prepaid private
expenditure as a share of total health expenditure) in Figure 1c. This sheds light on how health financing factors could play a role in these patterns.

Reviewer #2

Comment #13

We are really not given much reason for the set of variables included in the regressions or the type of data subsetting used in the presentation of the research. To oversimplify, I would like to see some effort to convince me that the correlations we might uncover are important, and the reason they are important. Similarly, if we find there is no correlation, can we get some insight as to why that is meaningful? On the other hand, if the paper is simply about describing CHE by disease type across countries, I would prefer that to remain the focus. I find the regressions to be a digression, partly due to the lack of context around the contribution they supposed to make to the research question.

Thank you for these comments. The regressions are an effort to examine whether the relationships between share of the population affected by the different diseases and disease-specific CHE are robust to important health system features. We use basic characteristics of income and health financing, First, we control for the income level of the country. In countries with higher incomes, people may be less vulnerable to CHE. Income is also an important determinant of certain disease areas and could thus be an omitted variable in the regression. Second, we control for defining health systems features, including the amount invested by the government in health and the share of health spending that is pooled. Both these controls are intended to capture how much a country invests in financial risk protection measures. These investments could also have an impact on the types of diseases prevalent and affect the share of the population affected by disease-specific CHE and thus be an omitted variable in the regression. We have added further context and justification for the regressions (pages 8-9).

Reviewer #2

Comment #14

Thirdly, it is suggested that the analysis is limited by lack of information on the health system. Yet, most of the countries used in this analysis have had papers published regarding CHE in those countries. We have now also compared our findings with the existing literature on CHE (see Wagstaff et al. (2018) and Raban et al. (2013)). As we noted in the Introduction section, countries, probably using expenditure data. Those papers are likely to describe, at least to some degree, the health system. More importantly, I think some level of comparative work
surrounding previously published papers that have used other data and the analysis here would be beneficial. It is difficult to compare across disease areas with existing estimates.

We conducted a review of all literature on CHE by disease, and cited a published review (Kankeu et al. 2013) that found that comparisons across disease-specific CHE studies were not feasible. More specifically, existing literature used methods, like snowball sampling of hospitalized patients, or bottom-up costing, that were not comparable with our methods and executed in a way that tended to overestimate OOP spending. For example, Essue et al. used a bottom-up OOP costing method and estimated that half of all CHE cases globally were due to cardiovascular disease — this was implausibly high given all the other disease driving up CHE such as HIV/AIDS, malaria, maternal and child health, etc. Second, we have added additional text in the Discussion section (page 14, paragraph 2) as well as a justification of our approach for the regressions (pages 8-9).

Reviewer #2

Comment #15

I also would like to see more discussion of the bootstrap approach. The underlying data generating process is fairly complicated, as there are different countries and different strata structures. How did the bootstrap take all of that into account?

We constructed the bootstrap with the complex survey design in each country in mind. The WHS was implemented as a multi-stage clustered survey. We designated the different strata used to select clusters, which was indicated in the WHS microdata and thus could be used directly to resample. We resampled at the strata level, integrating uncertainty related to the larger strata used in each country. This allowed us to maintain the national representativeness of the survey while also quantifying uncertainty of unique metrics. Stratification included different regional considerations, including rural/urban distinctions and the different provincial/state/county administrative configurations used in each country. The bootstrap was thus adapted to take into account the different sampling strategies that occurred in each country. We also used the survey weights to scale up estimates to be nationally representative at each stage of the analysis, including estimates of CHE by disease. We took 1000 draws of the underlying data in this fashion, constructing a distribution of each indicator with the uncertainty in the underlying data. We calculated the median, 5th and 95th percentiles to characterize the uncertainty in our estimates. Finally, to scale up to a global level, we used the population of each country at the time of the WHS to weight the respective estimates. This was particularly critical for representing the contribution of China and India to the distribution of CHE by disease. We have added further details on the bootstrap to the Methods section.
Reviewer #2

Comment #16

There is another issue that may or may not impact on the outcomes. As I have shown for the case of South Africa, the choice of equivalence scale does not matter much. However, it could matter across countries, as there might be additional forces at play that I did not consider in my South African focused research. I am not aware of any attempt to think more comparatively about that, and I am not suggesting you should change your question to consider that one. However, some thought on that front is probably needed. The reference, if you are interested: Koch, Steven F., "Catastrophic Health Payments: Does the Equivalence Scale Matter?" Health Policy and Planning 33(8): 966-973, October 2018. http://dx.doi.org/10.1093/heapol/czy072.

Thank you for pointing us to this reference. We have now added references to issues related to equivalency in both the Methods and Discussion sections.

Reviewer #2

Comment #17

I have a few minor editorial suggestions. Page 3 line 56 - many rather than may. Page 3 line 58 - have rather than has. Page 4 line 11 - Should try to be consistent with the use of citations throughout the text, Jan et al (2018) is not common referencing in health journals.

Thank you very much for these editorial changes, which we have now incorporated.