Reviewer's report

Title: Factors Determining Catastrophic Health Expenditure: Evidence from Georgia and its Policy Implications

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Reviewer: Marc Suhrcke

Reviewer's report:

1. Is the question posed by the authors well defined?
Yes the question is well defined.

2. Are the methods appropriate and well described?
Although I am not an expert on these specific methodologies, I would definitely say that the methods used by the author are appropriate and well defined. I would however say that some more discussion of the measure of catastrophic expenditures would be in order [MAJOR COMPULSORY REVISION], plus some discussion of alternative methods for the assessment of catastrophic health care expenditures. Although the 40%-threshold appears to be widely used in this type of literature, it is limited at least in that it is a rather arbitrary figure, and thus some good reason why other methods are not used ought to be given. Or additional methods should be applied. The recent World Bank published book on health equity measurements by O'Donnell, van Doorslaar et al might be worth citing and looking into.

3. Are the data sound?
Presumably yes but the survey they use primarily is not described in any slightly greater detail and that might just be worthwhile. (DISCRETIONARY REVISION)

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Mostly yes I would say, except, as mentioned under 3 with respect to the description of the survey. (DISCRETIONARY REVISION)

5. Are the discussion and conclusions well balanced and adequately supported by the data?
(MAJOR ESSENTIAL REVISION)
I would say this is an area where more work is needed. I understand that the core numbers presented relate to the 2007 survey and there is no question that this is a potentially extremely valuable and unique source. However, I would recommend to undertake even more efforts at comparing these data from one point in time to a) other data from Georgia and – even more so – b) from other “comparable” countries, despite the limitations that such comparisons may have.
In terms of domestic data I wonder whether the 2001 Living Standard, Lifestyles and Health survey in eight CIS countries (incl Georgia) might be any use as a comparator data (see www.llh.at for some information on the survey and some but not all related publications).

Maybe the paper would also benefit from some introductory graphs that locate the Georgian situation on some of the aggregate indicators (eg share of OOP vs public exp in total health exp) over a longer time trend and compared to other countries. One should seek to avoid the impression that the exclusive focus of attention of the authors is Georgia.

The authors mention the limitations in the comparability of the data between the 2007 and the 2000 survey and attribute this to potential differences in the questionnaire. One might be interested in knowing just what the differences in the questionnaire are? Later in the article there is also a comparison of the FFC index to the 2004 value. Is there also a comparable number from the 2004 survey on catastrophic payments available or can it be calculated? That might be another useful number.

Towards the end the authors very tentatively attribute the reduction in the gradient in health care utilization from 18.3% to 11.7% to “government’s efforts to deliver better coverage to poor”. Could this not also be entirely unrelated and fully explained to reduction in the overall absolute poverty rate (which I am sure has occurred on a significant scale) or even in the relative poverty rate (which I don’t know how it evolved but it presumably declined as well)? Some more mention of general macroeconomic conditions that the government may or may not be responsible for would be worthwhile.

6. Are limitations of the work clearly stated?

(MINOR ESSENTIAL REVISION)  
The authors do definitely point out some limitations of the methods and data used but as alluded to above they could go into more detail on some points, and mention alternative ways of doing it in other cases.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?

I would say yes. As mentioned above there may be a case for looking into the World Bank book on health equity measurement.

8. Do the title and abstract accurately convey what has been found?

(MINOR ESSENTIAL REVISION)  
Perhaps I would put a little less emphasis on “Factors determining..” in the title. The paper seems to be more about first measuring the extent of catastrophic expenditures and of FFC, rather than explaining the factors that are driving it. There is also not so much in the way of policy implications. This is not necessarily a problem but then perhaps it should not be in the title either.

It may be a little confusing to some to have “catastrophic health exp” in the title.
and “fairness in financial contribution” in the objective of the abstract.

9. Is the writing acceptable?
Pretty ok but would benefit from editing.
(MINOR ESSENTIAL REVISION)

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**
I declare that I have no competing interests.