Reviewer’s report

Title: Trends in completion of immunization in the context of health reforms: The Tanzania case study

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Reviewer: Enrique Delamonica

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Review of “Trends in completion of immunization in the context of health reforms: The Tanzania case study”

This is a clearly and well written paper with a solid analysis of equity trends in Tanzania. There are some adjustments, suggested below, to improve the paper. After these revisions are taken into account, the paper should be published.

The weakest part of the paper is the connection between the detailed analysis of DHS data and the policy conclusions. While the policy recommendations are reasonable, they are based on secondary data and studies, not based on the analysis of the previous sections (e.g. the discussion of the introduction of health fees). A simple way to address this is to recognize this problem up front and state that the evidence analyzed in the results section supports the conclusion of other studies. As a consequence the policy recommendations from other studies are valid.

The paper analyzes the increase in inequality using two complementary approaches. The gradient along an index of wealth is contrasted in two points in time. First, for the 1996 round of the survey, the gradient is flat. By 2004 it shows higher immunization rates among richer than poorer children. In addition, changes through time in the immunization rates for different quintiles are tested. Both methods point in the same direction, showing the results are robust. The discussion could be complemented with the tests introduced in Minujin and Delamonica, 2004, Wirth et al, 2006 and Balk et al, 2006 and other approaches such as Braveman 1998, Anand et al, 2001, Stecklov et al1999.

The paper does a good job describing the Demographic and Health Survey and the health care system in Tanzania. This is done efficiently in a few paragraphs.

Nevertheless, the health care reforms could be placed in the larger context of recession, adjustment and privatization that started in the 1980s. Also, in a sentence or two, it could be mentioned that changes since early 2000s were accompanied with partial external debt relief and a country-owned economic recovery strategy (MKUKUTA). Given these contextual changes, the authors could consider using also the 1992 round of DHS which is closer to the beginning of the decentralization and adjustment policies. This is important because
otherwise some conclusions could be challenged. For instance, on page 14 it is said that in 1996 there was a situation of equity. However, this would be IN SPITE of several years of adjustment and the policy changes that the authors claim hampered equity.

The methodological discussion about the wealth index could improve with four simple adjustments. First, the authors who originally introduced the wealth index should be mentioned (Filmer and Pritchett, and Gwatkin et al.). Second, the authors should make it clear if they (re)calculated the wealth index or if they used the one reported in the DHS documents and available on-line with STAT compiler.

Thirdly, the paragraph which mentions income and wealth differentials seems to indicate income is better than wealth to measure disparities. Actually, they are different angles on the multi-dimensional phenomenon of disparity. Using a wealth index is not a “(second) best alternative” but a more reliable estimate than income because it addresses more permanent aspects of differentiation (assets) than temporary ones (income) and non-sampling errors would be smaller (e.g. assets can be observed by the interviewer instead of relying on the recollection of past income or expenditures by the interviewee).

Fourthly, it should be made clearer in the text that the discussion is about relative poverty (e.g. “least poor” is the correct way to describe the category). Just a sentence stating that income poverty was estimated at almost 40% in 2000 and that a basic needs approach to child poverty results in an estimate of over 80% of children in material poverty would suffice (Minujin and Delamonica 209 and forthcoming). In this context that the decline in immunization rates occurred in the two lowest quintiles, not just the lowest one, is interesting and could be highlighted further (see table 3).

The authors should also revise some of the language and rhetoric. For example, they are analyzing only one determinant of U5MR (both in the abstract at the end of the first paragraph, the sentence could read “within a context of declining U5MR it is important to analyze the evolution of immunization”). Similarly, the HIV pandemic as a major element preventing the achievement of the U5MR goal is not mentioned.

Finally, the logistics analysis is very interesting. Thus, it would be useful to show what the model used for the calculation looks like. Also, it should be acknowledged it is based on available data and not a theoretical model.

Suggested references:


Balk, Deborah, Emma Sacks, Adam Storeygard, Alberto Minujin, and Meg Wirth, Minujin, A. and E. Delamonica, “Setting the stage for equity-sensitive monitoring of the maternal and child health Millennium Development Goals”, World Health


Minujin, A. and E. Delamonica, “Socio-economic inequalities in mortality and health in the developing world”, Demographic Research, April, Special Collection Number, 2004


**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