

Income Inequality and Health: Evidence from Developed and Developing Countries

Reply to Referee 1

We would like to thank the referee for most useful comments and helpful suggestions. In the following, we specify how we are going to take the referee's comments into account when revising the paper. Our reply focuses on the six major points of the referee, listed in the order of the report.

1. We fully agree with the referee that our finding on developed countries contradicts the empirical results of various earlier studies. [Note that, in contrast to the first sentence in the report, we find that income inequality is associated with *better* health in developed countries and *worse* health in developing countries]. We already stress in the current version of our paper that the association of higher inequality with longer life expectancy in high-income countries "is in contrast to most previous studies" (page 19). Moreover, we explicitly rate this finding as "surprising" in the concluding section (page 25). All the same, we propose several revisions to avoid the impression that we are biased towards referencing the minority of studies which are more in line with what we find for developed countries. First, we may stress upfront in the Introduction that most studies find detrimental health effects of income inequality. Second, we will fulfil the referee's request to update our references and highlight recent findings from studies that we missed before. We are most grateful to the referee for alerting us to these additional references. In particular, we will summarize major results from the most comprehensive review of 155 papers by Wilkinson and Pickett (Soc Sci & Med 2006). We also agree with the referee that there are several studies on developing countries. Nevertheless, our reasoning in the last paragraph of page 2 appears to be in line with the conclusion by Kondo et al. (BMJ 2009, doi: <http://dx.doi.org/10.1136/bmj.b4471>; page 8) that "further investigations are needed because of the lack of empirical evidence from many parts of the world, including developing countries." Furthermore, it should be clarified in the revised version of our paper that we focus on comparing the effects of income inequality on health in developed and developing countries. In our view, such a comparison represents an important contribution to the existing literature.

2. In line with the referee's comment, we are going to stress in the revised version of our Introduction that Kondo et al.'s study clearly suggests that "there is an association between higher income inequality and worse health outcomes" (BMJ 2009: page 8). We are also going to make explicit that Kondo et al.'s finding of a threshold effect does not appear to be particularly relevant for most of our sample countries, considering that the Gini coefficients in our sample typically exceed 30.

3. We completely agree with the referee that we should better explain why our findings for developed countries (though not for developing countries) are in contrast to most other studies. In addition to discussing the possible explanations suggested by the referee in the subsequent comments, we would like to stress the importance of methodological choices. Specifically, it appears to us that the panel cointegration approach is most appropriate in the present context in order to account for endogeneity concerns. While the endogenous nature of income inequality is often acknowledged in the related literature, we would argue that it is rarely addressed appropriately in previous empirical analyses. The cointegration approach not only allows us to estimate long-run coefficients in a manner that is free of endogeneity bias, but it is also robust to omitted variables, as we may discuss in more detail in the revised version. Another advantage of the cointegration approach is that it offers meaningful, non-spurious results.

Considering that most economic time series data are non-stationary, it is important to note that the absence of cointegration or residual stationarity can lead to spurious associations in standard panel regressions involving non-stationary variables. Finally, panel cointegration analysis, like conventional panel analysis, allows us to control for any country-specific omitted factors that are relatively stable over time, thereby eliminating the time-invariant omitted-variable bias inherent in cross-sectional analysis.

4. We agree that the choice between market income and disposable income may help explain our findings. We are going to assess this possibility in several steps when revising our paper. For a start, we may assess the correlation between market and disposable incomes over time. As indicated by the referee, this correlation could be weaker for developed countries than for developing countries. This, in turn, could have implications for the comparison of the effects of income inequality on health between developed and developing countries. Next, and more importantly, we are going to replicate our empirical estimations by replacing market incomes with disposable incomes. Preliminary tests seem to suggest that our major results are fairly robust to this modification.

5. In the revised version of our paper, we would like to discuss the issue of premature mortality by referring to the interesting findings of Torre and Myrskylä in their 2011 working paper on a sample of 21 developed countries. However, the Human Mortality Database used by these authors does not provide any information on mortality for specific age cohorts in developing countries (except for Chile). This prevents us from extending the analysis of Torre and Myrskylä to a broader sample, including low-income countries. However, we agree with the referee by concluding that this is an important area for future research once comparable data become available for a sufficiently large number of developing countries.

6. We would like to stress that the cointegration approach allows us to estimate the long-run cumulative effect of inequality on health. As the principal interest is on the long-run effect, it is not essential in our view to be concerned about the variable lags through which inequality will impact health. Nevertheless, as suggested by the referee, we may provide a more detailed discussion of lagged effects in the revised version of our paper. In addition, we may provide additional evidence in the revised version by presenting impulse response functions, which provide a better picture on the timing of effects. In this context, we would explicitly refer to the study for the United States by Zheng (Soc Sci & Med 2012).

Finally, we are most grateful to the referee for having alerted us to several minor issues (points 7 – 13). When revising the paper, these points will be addressed by clarifying the description of the Gini data in Table 1 (point 7); mentioning the importance of even small differences in risk (point 8); referring to Uslaner (2002) on the role of trust (point 9); avoiding unnecessary inferences (points 10 and 11); re-considering the importance of medical care (point 12); and clarifying the definition of per-capita GNP (point 13).