



Economic growth and child malnutrition

Authors' reply

Although we appreciate the interest of Stephen A O'Connell and Caroline Smith in our work,¹ we have to refute nearly all of their claims. Most importantly, their claim that they re-examine the same data and reproduce our results is incorrect. Their analysis is based on 121 aggregate-level data points without any covariates, whereas ours was based on the full microdata and a large set of covariates from 121 Demographic and Health Surveys, which included almost half a million observations. Their analysis re-examines very similar aggregate data as previous aggregate-level studies,^{2,3} and comes to more or less the same conclusions as these studies. It was the main innovation of our study to analyse the association between childhood undernutrition and economic growth at the micro level, accounting for individual-level characteristics, incorporating the repeated-within-country observations of gross domestic product (GDP) per capita in the statistical design by clustering standard errors and showing a large number of sensitivity analyses. None of the points made by O'Connell and Smith question the robustness of our findings.

Their claim that a few unusual observations push our results towards zero is incorrect and also related to their exclusive reliance on a bivariate aggregate analysis. If one just correlates 121 observations, some outliers can make a difference. This, however, bears no relation to our analysis, in which we also presented a large number of robustness checks and subsample analyses— including samples without the observations from the surveys in question—in all of which our results remain robust. For the outcome variable of stunting, O'Connell and Smith exclude one observation from Armenia and one from Madagascar and claim that these two observations push our results towards zero.

In table 3 of our study,¹ we also reported population-weighted results, in which these two observations basically do not play any part (because of their small population compared with countries such as India), and still come to the same conclusions as everywhere else in the paper. The GDP per capita variable that we used was also chain-linked and therefore their claim that we used the inappropriate measure of real GDP is also without support.

Finally, O'Connell and Smith claim that we dismiss economic growth and that we place economic growth and direct health interventions into opposition. Such statements are misrepresenting our work. We say that, during the period and in the countries we studied, economic growth did not contribute much to reducing childhood undernutrition. We do not deny that economic growth—eg, additional resources in the economy—has a strong theoretical potential for reducing childhood undernutrition. However, this potential relies on two basic assumptions. First, the additional resources must reach those in need, either through trickle-down effects of raising household incomes or through increased government spending. And second, the additional resources must be spent in ways that are effective for reducing childhood undernutrition, either through household spending on health and nutrition, or on government programmes. If the additional resources generated by economic growth are used in this way, then clearly economic growth and direct health interventions are not in opposition. This apparently was not the case for the sample of countries and years that we studied.

We declare no competing interests.

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