Author's response to reviews

Title: Are infant mortality rate declines exponential? The general pattern of 20th century infant mortality rate decline

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Author's response to reviews: see over
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Editorial Team
Population Health Metrics

Dear Editorial Team:

Thank you for the helpful comments on our paper, “Are infant mortality rate declines exponential? The general pattern of 20th century infant mortality rate decline.

With the help of these suggestions we have been able to substantially revise this paper and are submitting it again for your review.

Reviewer 1:

1. The reason of selection of these 22 countries was not known. Why these countries and not other countries? How the differences would be addressed? The reliability of the mortality data of these countries is not the same and this issue didn't mention in the paper. There would be necessary to mention in this regard in the text. We have addressed this issue. We limit our analysis to those countries included in the human mortality database (HMD) for which we also have GDP per capita data. We regard HMD as a more reliable source of historical IMR data and now confine our analysis to HMD.

2. Why were two different databases used? Is there any comparison between these two databases? Was HMD not enough? See response above. Just one database now.

3. Where is table 2 in the text? We have renumbered the tables in the text.

4. Why will we need to know the linearity or non-linearity of IMR over time? Why weren't APC (Age-Period-Cohort) models used? With using APC, there is no need to know about shape of trend.

The rationale for why we need to know the linearity or non-linearity of IMR is now made more clear in our paper. The point is that prior studies of IMR have not used APC models. (When one is interested in “What is changing the survival of infants?” there are not multiple ages (just infants) and the cohort is only observed one time in one period.) We understand the superiority of APC models in disentangling multiple factors behind mortality for older age groups, and plead that we are not responsible for the fascination with infants that has pervaded the literature.

Given that demographers have studied and will continue to study the social determinants of IMR trends over time, we are offering our contribution in hopes that future studies will adopt a more respectful and appropriate transformation to IMR data.
Reviewer 2:

1. I have a serious doubt as regards the data used:
   The title claims that the paper is on twentieth century decline in IMR. However the data used does not fully reflect the twentieth century. In first instance, data for 22 countries is used from an IMR database assembled at the World Bank by Pritchett and Summers. Final year is 1988, however. And for 9 countries the data do not start from 1900 or 1901 onwards. On the other hand data from before 1900 are also included.
   In second instance, data for 34 countries from the Human Mortality Database (HMD) are used (on the probability of dying in the first year, instead of on the IMR…). Final year is 2003/2004/2005/2006. Only for 11 countries the whole twentieth century is included. And again, also observations from before 1900, and after 1999 are included.
   As regards the data used by Pritchett and Summers no link to a website is given, and no further information is given on how the data were assembled. Is there not more recent data available, or can this recent data not be added in the same way as Pritchett and Summers did before?
   As regards the additional data that is being used from HMD: this is very confusing. Especially in the way it is written down in the data & methods part. The probability of dying in the first year is not the same as the IMR, so to me it is not clear why the authors use the probability. From the data on the HMD website it is easy to calculate the infant mortality rates…Please also include the website from which the data can be retrieved in the data and methods.
   In the discussion the authors state that “alternative specifications could be necessary for 21st century populations when IMR has already reached a low level”. Thus in fact the authors acknowledge that the historical period for which the data is included makes a difference.
   My suggestion would therefore be to use data on the IMR for the twentieth century (thus up to 1999).

We have addressed these issues. We limit our analysis to data from 1900-1999. We also limit our analysis to those countries included in the human mortality database and we calculate the IMR from the HMD data. We also provide the link to the HMD website (http://www.mortality.org/). For the GDP per capita data, the data used by Pritchett was originally derived by Angus Maddison and they have since been updated. We use the updated data and also provide the link to the website. (http://www.ggdc.net/maddison/).

2. In addition no information is provided on the source of the GDP data. Obtaining consistent trends in GDP is quite a task. Therefore as a reader I would be very curious to find out which data has been used, and whether attempts were made to control for inconsistencies over time.
   See response above—we now use GDP data from Maddison. Though the details of deriving historical time series of GDP per capita are beyond the scope of this paper, we provide references describing how these estimates were derived.

3. The data used needs more explanation in the data and methods part. Could you also include a little bit more on the selection of countries. Is this pure as a result of data availability?
We have added text about this. Yes, countries were included according to data availability.

4. Do the missing years in table 1 reflect missing information or jumps in the time series? The missing years reflect missing data and this has been indicated in the footnotes to the tables.

5. Then as regards the methods I have also some questions for clarification, especially in relation to the IMR-GDP analysis:
   a) How were the results obtained for the whole sample, in both the analyses?

   The whole sample estimates were obtained by fitting a maximum likelihood model of
   \[
   \frac{IMR_{it}^\lambda - 1}{\lambda} = C + \beta_1(GDP_{it}) + \beta_2(t) + \varepsilon
   \]
   Which amounts to maximizing
   \[
   L = \varphi \left( \frac{IMR_{it}^\lambda - 1}{\lambda} - C - \beta_1(GDP_{it}) - \beta_2(t) \right)
   \]
   Where \( \varphi \) is a Gaussian pdf, GDP is GDP per capita, \( t \) is time, and \( I \) is the index for each country and \( t \) is time in years. The parameters to be estimated by maximum likelihood were \( \lambda, \beta_1, \beta_2 \) and \( C \).
   This model presented did not use population weighting, although the results are not greatly affected by weighting each country-year by its population at the time.

   b) Do the results of the IMR-GDP analysis not also depend on whether GDP or LN(GDP) is taken into account?

   The transformation shown above nests models ranging from logIMR to IMR as a function of GDP. It is possible to simultaneously apply Box Cox transforms to the right hand side at the cost of fitting a second parameter. That would permit one to assess models ranging from logIMR to IMR as a function of logGDP to GDP.

   We tested these models on their own, and felt that they did not shed any new light on the main point of the paper. The focus is what IMR transform fits best. The main purpose of estimating IMR vs. GDP models was to allow readers to determine how important mistransformation of IMR could be.

   For the sake of review, we present below the results of Table 2 with the provision that ln(GDP) is used on the right hand side. Note that the gaps between the coefficients are even larger. However, we prefer to display the table of IMR vs. GDP because it will probably be less distracting to readers to refrain from excessive machinations with both sides of the regression equation.
<table>
<thead>
<tr>
<th>Country</th>
<th>Years</th>
<th>Box-Cox IMR transformation $\lambda = \text{&quot;Best Fit&quot;}$</th>
<th>Log IMR transformation $\lambda = 0$</th>
<th>Coefficient gap</th>
</tr>
</thead>
<tbody>
<tr>
<td>Whole sample</td>
<td></td>
<td>Coefficient on Ln(GDP) per capita</td>
<td>Coefficient on Ln(GDP) per capita</td>
<td></td>
</tr>
<tr>
<td>Australia</td>
<td>1921-1999</td>
<td>-0.841 ***</td>
<td>-0.667 **</td>
<td>-0.174</td>
</tr>
<tr>
<td>Austria</td>
<td>1947-1999</td>
<td>0.216525 ***</td>
<td>0.274</td>
<td>-0.05748</td>
</tr>
<tr>
<td>Belgium</td>
<td>1900-1913</td>
<td>0.150632 ***</td>
<td>0.195 *</td>
<td>0.04437</td>
</tr>
<tr>
<td></td>
<td>1918-1999</td>
<td>-0.07049 ***</td>
<td>-1.084 **</td>
<td>1.013513</td>
</tr>
<tr>
<td>Canada</td>
<td>1921-1999</td>
<td>-0.03752 ***</td>
<td>0.065</td>
<td>-0.10252</td>
</tr>
<tr>
<td>Denmark</td>
<td>1900-1999</td>
<td>-0.0573 ***</td>
<td>-0.999 **</td>
<td>0.941705</td>
</tr>
<tr>
<td>Finland</td>
<td>1900-1999</td>
<td>-0.0884 ***</td>
<td>-1.406 **</td>
<td>1.317599</td>
</tr>
<tr>
<td>France</td>
<td>1900-1999</td>
<td>-0.08434 ***</td>
<td>-1.035 **</td>
<td>0.950657</td>
</tr>
<tr>
<td>Italy</td>
<td>1900-1999</td>
<td>-0.15149 ***</td>
<td>-0.974 **</td>
<td>0.822511</td>
</tr>
<tr>
<td>Japan</td>
<td>1947-1999</td>
<td>-0.02032 ***</td>
<td>-0.597 **</td>
<td>0.576681</td>
</tr>
<tr>
<td>Netherlands</td>
<td>1900-1999</td>
<td>-0.02262 ***</td>
<td>-0.356 **</td>
<td>0.333384</td>
</tr>
<tr>
<td>New Zealand</td>
<td>1947-1999</td>
<td>0.03459</td>
<td>0.859 **</td>
<td>-0.82441</td>
</tr>
<tr>
<td>Norway</td>
<td>1900-1999</td>
<td>-0.00431</td>
<td>-1.08 **</td>
<td>1.075692</td>
</tr>
<tr>
<td>Portugal</td>
<td>1940-1999</td>
<td>-0.3658 ***</td>
<td>0.781 **</td>
<td>-1.1468</td>
</tr>
<tr>
<td>Spain</td>
<td>1908-1999</td>
<td>-0.12748 ***</td>
<td>-0.948 **</td>
<td>0.820516</td>
</tr>
<tr>
<td>Sweden</td>
<td>1900-1999</td>
<td>-0.06166 ***</td>
<td>-0.599 **</td>
<td>0.537338</td>
</tr>
<tr>
<td>Switzerland</td>
<td>1900-1999</td>
<td>-0.00914</td>
<td>0.076</td>
<td>-0.08514</td>
</tr>
<tr>
<td>UK</td>
<td>1900-1999</td>
<td>0.145315 ***</td>
<td>-0.258 *</td>
<td>0.403315</td>
</tr>
<tr>
<td>USA</td>
<td>1933-1999</td>
<td>-0.00895</td>
<td>-0.044</td>
<td>0.03505</td>
</tr>
</tbody>
</table>

c) When calculating the coefficient gap in Table 3 should you not keep the other parameter estimates of the model constant?? I am afraid that I did not fully get this part. This is now Table 2. The specification now uses the same variables and only differs in whether the dependent IMR transform is BoxCox or Log.
The new version of the paper tries to do a better job explaining the purpose of the exercise. The point is to ask, “How wrong could a coefficient estimate be if an analysis always assumed that ln(IMR) is the best transformation to regress against an independent variable of interest?” Since prior literature has focused on the IMR to GDP relationship we ask the question in this context. The coefficients on the left are the best fitting estimates achieved with the flexible functional form (Box Cox). The coefficients on the right are the estimates that a naïve analyst would obtain by assuming that the ln(IMR) transformation was the best one. The final column measures the gap between the two estimates.

d) Also, I found the use of lambda is 0 in relation to the IMR-GDP analysis very confusing. Then – according to the text – the dependent variable is 0/0?? All in all this analysis needs more explanation in the data and methods part.

The revision does a better job explaining why the Box-Cox transformation converges to a log transform in the limit as $\lambda \to 0$. To take this limit it is helpful to compute a Taylor series for both the Box-Cox transform and the log transform in the neighborhood of a constant “$a$”.

$$Box-Cox(y) \approx \frac{a^\lambda - 1}{\lambda} + \frac{a^\lambda}{a}(y - a) + \frac{a^\lambda(\lambda - 1)}{2a^2} (y - a)^2 + ...$$

$$Log(y) \approx \ln(a) + \frac{1}{a}(y - a) + \frac{(y - a)^2}{2a^2} + ...$$

Then one can see that as $\lambda \to 0$ all of the terms in the expansion of Box-Cox converge to the terms in the logarithm function.

6. In relation to Figure 1 it would probably be helpful for the reader to show A0 and B0 for the reader. Also, I would recommend using the same A0 and B0 values for Panel A and Panel B.

This figure is intended to simply illustrate graphically the general mathematics given in the appendix. Putting real number values for $A_0$ and $B_0$ would be less convincing because readers might think that the demonstration is only true for exactly those values.

7. Maybe you can add in the methodology text how you tested the value of lambda for each country and for the pooled sample against the null that lambda=1 and against the null that lambda=0.

The text now makes clear that this is a likelihood ratio test which is distributed as a chi-square. We compute the log likelihood when the model uses the best fit $\lambda$ and take the difference from the log likelihood when $\lambda$ is set at 1 or 0 respectively.

8. Should the part on the implications for analyses of relative versus absolute gaps in IMR over time (currently in introduction) not go to the results? The paper needs a motivating force in the introduction section to avoid losing readers who won’t have the patience to get to the “Why does this matter?” part. Prefer to keep it in introduction to hold some interest.
9. The discussion currently does not include a reflection on the specific results that were obtained, only on the more technical result of this paper. As a reader I would be interested to read more about the reasons why in some countries the decline is exponential whereas in some other countries they are linear.

Page 10 now reflects on what it would mean to observe linear vs. exponential declines in IMR and why it is unlikely that a single country adhered to either single pattern over a century.

10. The outcome on GDP and IMR are not reflected upon. What does for example the coefficient of -0.142 in Table 3 exactly mean and how does this relate to earlier studies on this matter? In the results, what is the implication of the observation that “coefficients from the Box-Cox models tend to be larger in absolute magnitude than those from the logarithmic model”? Based on what is included in the discussion now (obvious discussion of the general trend in IMR, and some repetition of things mentioned in the introduction), I would recommend this article as a short technical article and not as a full article.

There is virtually no news value in redemonstrating a negative relationship between GDP and IMR, and it would try readers patience to “point out” this negative relationship.

This is a very old finding in public health. What is newsworthy is that past demonstrations of the relationship have been biased by from 33 to 100%. We do reflect on the few cases where using a more respectful transformation of IMR can make the conventional negative relationship turn significantly positive as it does in the case of Australia and UK. This is not a general phenomenon, and one cannot claim that in general GDP growth increases in infant mortality. The positive relationship vanishes when the term for the time trend is removed.

With the time trend term in place the regression is explaining changes in IMR on the basis of changes in GDP. There is a growing literature on how some health indicators in some countries improve during recession (Ruhm 2000; Ruhm 2006). But this is not a paper about recessions and infant mortality. (It would be hard to sell a paper focused on recessions based on a finding that occurs in only 2 countries.) Instead, this is a paper about being more careful in how IMR is transformed, because without care phenomena are missed and distorted.

11. In the introduction and in the summary you mention about the Preston curve. As this is very essential to the paper, I do not think it would harm to include a brief explanation.

We have added a brief description of the curve in the text.

12. In the introduction you mention about the implications of whether or not IMR time series require logarithmic transformations. First you mention about the implications for analyses of convergence of IMR both across countries and among subpopulations. Second you mention about the implications for analyses of relative versus absolute gaps in IMR over time. As regards the convergence, do you talk about levels or trends and do you talk about past levels/trends or future levels/trends. I feel that your results have
important implication for assessing future trends as well, which currently is not clearly mentioned in the introduction. As regards the second implication, it would be good to already in the introduction clearly talk about “tracking progress in health equity” as this is what you are actually talking about. Also in the last paragraph of your conclusion, note that your second hazard is about assuming that the TRENDS IN the rate ratio is a complete reflection of the TRENDS IN health disparities between populations.

This is a good suggestion so now we include better coverage in the background section the relevance of the paper to using rate ratios to assess past and future trends in health equity.

12. You mention that Figure 2 shows the pattern of decline in ln(IMR) in 22 European and Latin American countries during the 20th century. From the legend, however, I count only 19 countries, and it seems it is just IMR and not ln(IMR). It is difficult to discern the trends of the different countries. We have redone Figure 2 to include ln(IMR) for the countries we include in our revised analysis.

Sincerely,

David M. Bishai, MD, MPH, PhD
Associate Professor