Author's response to reviews

Title: Health System Outcomes and Determinants Amenable to Public Health in Industrialized Countries: A Pooled, Cross-Sectional Time Series Analysis

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Author's response to reviews: see over
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Dear Editorial Team,

Thank you for the detailed review of our manuscript “Health System Outcomes and Determinants Amenable to Public Health in Industrialized Countries: A Pooled, Cross-Sectional Time Series Analysis”.

We found the reviewers’ comments very useful: they helped to improve the manuscript. Below are our responses to each reviewer’s comments and our subsequent revisions.

We have updated the references and formatted the manuscript according to journal style. We look forward to a fruitful collaboration and a favorable decision.

Sincerely yours,

Onyebuchi A. Arah
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REVIEWER 1 (Niyi Awofeso)
Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

First, the use of aggregate data limits the significance of the authors’ findings. It would be more appropriate to analyse the variables chosen from the perspective of low- and high socio-economic groups. Such alternative approach would better focus the public health intervention that might be appropriate vis-à-vis each amenable variable for most of the countries studied (e.g. see Ljung et al; socio-economic differences in the burden of disease in Sweden, Bull WHO, 2005, and Frank et al; investigating explanations of socio-economic inequalities in health, EJPH, 2004).

“We agree with the reviewer that his suggested approaches would be fruitful but we have to point out that our study is ecological in nature because we only have access to aggregated or ecologically averaged data for the countries in question. This point is stressed in the Background, Methods, Discussion and limitations of our study. In the future, when we can get micro-level (individual-level), we will pursue these issues at the individual level.

Second, I agree with the authors’ comments with regards to the limited validity of tobacco data in the OECD report. Similar limitations apply to some of the other variables used. For instance, a tobacco consumption of 11.62 litres per capita (table 1) corresponds to about 80 litres of wine per capita, or about 1 glass per day. This is in fact lower than the 2 glasses/day recommended by Ruut Veenhoven’s Happiness study (http://www.eur.nl/fsw/research/happiness/). I cannot see how a decline in this already low aggregate level would correspond to improved quality of life, at least if Veenhoven’s data are to be believed. This observation underscores the need for a stratified analysis based on socio-economic status, since especially with alcohol and tobacco, the socio-economic differentials are more informative than aggregate values (http://tc.bmjjournals.com/cgi/reprint/12/suppl_2/ii67) .

“Again, we share the reviewer’s concerns but, like we pointed out above, our study is based largely on the OECD Health database which has country-level macro-data which mask intra-country (between-persons and between-regions) variances in levels of the variables; the alcohol and tobacco variables will be good examples. This is among the reasons why we stressed in the Methods and Discussion that our study cannot and should not be used for individual level inferences to avoid cross-level biases. Like we pointed out in the
Discussion, our findings are in line with known ecological associations between health or mortality and these ‘public health’ variables. We have now extended the “limitations” sub-section to highlight some of the data issues (p. 13).

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

It would be valuable if the authors were able to propose specific strategies to address the factors that are amenable to public health interventions. Data exists on the progress (or lack of) made by many of the countries studied with regards to addressing the variables studied. A discussion of success stories and reasons for failure should further strengthen the discussion section of the manuscript.

-We thank the reviewer for his suggestions. We believe, however, that given our discussion which already broaches these issues (p. 12), a further discussion of actual strategies will not only make our article unwieldy (with secondary research questions and answers) but will also detract from our primary focus which is to examine the (macro-)association of these factors with mortality. Therefore, we would prefer to address specific strategies for our comprehensive list of ‘amenable factors’ in a separate article to give them a more detailed treatment beyond just presenting an overview within the current study.

Discretionary Revisions (which the author can choose to ignore)

Although on pages 3-4 the authors stated that they were “not interested in the extent to which public health activities and investments yield value for money, and are more efficient and equitable than medical care”, yet the thrust of the discussion section of manuscript appears to be on this issue (p. 11-12). Also, discussions of ‘proximal’ and ‘distal’ health determinants (p. 4) do not appear relevant to the main thrust of the article.

-We have now eliminated that sentence to avoid confusion (p. 3). We retain the discussions of ‘proximal’ and ‘distal’ health determinants to highlight that the variables we choose are mainly proximal from an ecological perspective and that the use of time dummies may, to some extent, control for the more (stable) distal, but time-dependent determinants such as political and policy influences (p. 4 & 6).

Furthermore, the variances attributable to all-cause mortality and PYLL determined by the authors appear much higher than other studies have found. Given the validity limitations of the data used, the authors may wish to re-examine their statistical methods, or explain why their analysis led to the selected factors accounting for such a high variance.

- High explained variances are rather common in ecological studies like ours (an examination of references 18, 21, 22, 37, and 38 would easily confirm this). In fact, previous studies using similar variables and the same dataset have reported slightly higher values than our study (see for example, Macinko et al. Health Services Research 2003;38:831-865).

REVIEWER 2 (Ellen Nolte)
Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Choice of explanatory factors: The authors acknowledge the limitations of some factors such as Åê–Etototal tobacco consumptionÅ–, å, ë (p. 9, para 3, line 3) and that the chosen factors may at best be taken as proxies (p. 12, para 2); however, given the findings of other work that has looked more specifically at the association between health care input and health (care) output, it strikes me as odd that
the present analysis chose to use physician density and doctor visits as indicators of medical care input (p. 6, para 4), which, in previous work, was found to show, at best, inconsistent results.

-We share the reviewer’s concerns and we now have a more extensive “limitations” sub-section by adding some 5 sentences to the first papraph (p. 13). Largely because they are the most commonly available, especially in our dataset, these factors are the commonest used medical care inputs in such studies as ours (as can be seen in the studies we referenced in our manuscript). We basically only used these factors as proxy measures for statistical control, and our findings mirror those of previous studies that used such factors and our dataset (see for example, Macinko et al. Health Services Research 2003;38:831-865).

Selection of factors deemed indicative of public health policies versus medical care indicators: the authors define collective public health expenditure as a measure of public health policies, complemented by the indicator healthcare coverage, arguing that the latter would be a good indicator of the proportion of the population able to take advantage of services provided by the former (p. 6, para 2). This argument may hold in principle; however, I believe that using this indicator introduces a bias by reflecting access to care in general and, consequently, health care outcomes, rather than public health policies. Also, it is not quite clear how the analysis addresses the particular case of the United States with low levels of coverage in comparison with the remainder of the countries included in the study. According to table 1, the analysis excluded the USA in calculating this indicator; this is however not mentioned and thus requires corresponding explanation in the manuscript. On more general level it would be instructive to learn whether/how the results change if this indicator was excluded from the analysis. In any case, the authors are encouraged to discuss this particular issue more thoroughly.

-We share the reviewer’s concerns and have now included more sentences (p. 8, 9 and 10) to address these concerns. Table 1 only highlights the values for healthcare coverage with and without (in brackets) the USA. Further analysis excluding the USA did not substantially alter the models and this is to be expected given that our matrices capitalized on the variation (in principle further increased by the USA’s values on healthcare coverage) in the data to adjust the standard errors we used in our hypotheses/association testing. We have also included more discussions on the impact of excluding the healthcare coverage variable from models 3 to 5 (p.11)

A crucial problem not really discussed relates to the comparability and reliability of the underlying data. For example, while the indicator physician density as included in the OECD data set should include practicing physicians only the figures for some countries are in fact much broader and include all physicians entitled to practice. Similar issues pertain to the comparability of doctor visits (p. 6, last para). It is not clear from the manuscript which indicators are derived from which source (OECD Health Data 2003 or Annual National Accounts [references 35,36]); however, I assume that similar issues will pertain to the indicator collective (public) health expenditure (p. 6, para 2) and others.

-We have added more sentences to address these issues ( p. 14).

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

In discussing their findings I am somewhat surprised that the author did not acknowledge the evidence emerging from the considerable body of literature that has looked at the concept of avoidable mortality that, while using a different methodological approach has highlight similar issues, namely the importance of appropriate public health policies as an integral part of health system performance.

-We thankful to the reviewer for pointing out this oversight (was actually mistakenly deleted from the manuscript before submission). We now refer to the recent avoidable mortality research and literature (p.13-14) and references 59, 60, 62 and 63.