Dear Dr Aburto,

We have now received all of the peer reviewers' reports for your manuscript. Unfortunately, based on the remarks of the reviewers and the editorial team, we do not feel that the manuscript fulfils the requirements of Article (Original Research) for The Lancet Public Health.. Therefore, on this occasion, we have decided not to publish your manuscript.

The comments we received from the reviewers are presented below:

Reviewer #1: Many thanks for asking me to review this article on excess deaths in COVID

Comments:

Abstract: Generally clear and well written. I would add the specific numbers for the fall in lifespan inequality in the findings section

Funding: Perhaps something to take up with the editorial team but there seems to be multiple sources of funding and you may chose to expand these further.

Implications of Findings: Please see my comment in the discussion, however, the last sentence, seems unnecessary here and not the true focus of this paper

Introduction: Could you add a sentence after the 'Gini coefficient' explaining what it is for the lay reader Last paragraph, some this information should be moved to the methods, such as the dates of data collection etc

Methods: I am not a demographer, so the specific methods will need that level of review and another member of the reviewing team. However, I could not see any mention of a sensitivity analysis which you include in your results and I would have expected to have seem that here.

1st paragraph: Why did you chose these 6 ages groups, please justify this.

Results:

1st paragraph: following from the above comment in the methods, why include the group 0-14 years if you then are just going to discard it Otherwise, clear and well written.

Discussion:

I think you need a separate paragraph about weaknesses. You note in the methods that reporting rate for death is higher than historically rate of reporting; within one week (approximately 81%) however, the additional 19% that are not reported should be highlighted as a potential weakness/confounder as you do not state what effect this may have had on your findings (I note that you do touch on this in the second paragraph of the discussion).

You don't discuss the sensitivity analyses in the discussion

Pg 8, line 5. Please replace fragile with frail, which is the population that this reference refers to.

Third paragraph: Disease of the trachea. This seems a bit strange. This is a rare cause of death and you should expand/explore this further. Also please justify why excess deaths due to 'unexplained symptoms...' are attributable to COVID. I agree that they probably/possibly are, but it is simply stated as fact and therefore needs some supporting text.

Last paragraph, second line. 'Massive efforts' please either rephrase this or justify what you mean by this. It is not very objective as it stands.

Last sentence; re low cost protective measures. This feels a little out of place both here and the article in general, as it is an article about the demographics of death in COVID. If you wish to include it, I would not simply have it as a last sentence. To that end, while I know it is not the Lancets style to have a separate conclusion section, the article ends abruptly and I would suggest that you finish it with a couple of summary sentences

Many thanks again for asking me to review this.

Reviewer #2: It is worthy of investigation which proves the damages of COVID-19 from another perspective, especially on elderly and male. However, as it has mentioned in the manuscript, the saturation of health facilities or the accessibility of health facilities during the pandamic could potentially increase the death from other non-covid-19 causes, say overestimate the damage of COVID-19 itself on mortality. Actually, it could be a major reason for the high mortality after COVID-19 pandemic. Therefore, i would suggest authors of this article to stratify/assess the scale of the saturation of health facility and compare it to other places/countries with mortality over COVID-19 outbreak.

Reviewer #3: Life expectancy, based on applying age-specific death-rates in 1st 6-months of pandemic-year 2020 to child born in 2020 is artefactual as these pandemic rates shall not apply when child is 40, 50 , 60 etc.

Lifespan inequality is insufficiently well described - unfamiliar to me, and still unclear. Not convinced that similar artefacts as best life expectancy have been obviated.

Supplementary material page 2 specifies the 4 models used & gives their central estimATEs (95% uncertainty) for excess deaths. Neg Binomial is wider than, & includes, Poisson - corresponding CIs are non-overlapping which suggests Neg Binomial fits better than Poisson but is not the model features in RESULTS. The so-called empirical is itself model-derived, the model based on how expectations for influenza excesses are computed. Pandemic excess is less by around 10,000 using this 4th model. Smoothing functions (model 4 excepted) are unexplained, estimates not provided & model fits also absent.

in Supplementary, I was surprised to note strongly patterned (weekly or daily?) estimates for excess deaths (see Figures)- these patterns are not apparent in weekly totals. Text lacks adequate explanation of Methods & results accordingly take insufficient account of the methodological diversity. Smoothing functions may provide the explanation but they are not detailed as to form or estimated parameters.

All analyses based on registration-week (sometimes 1-26, mostly 10-26) but authors do not discuss sufficiently uncounted coroner-referred deaths which are not registered in E&W until coroner's investigation is complete, which can takes weeks, months or years; and in pandemic with occupationally-associated COVID deaths at issues, those still under investigation are currently uncounted & unaccounted for by Chief Coroner. More discussion of this is warranted.

Authors' Introduction & WHat's New read as though biostatisticians (and indeed the press) had not been monitoring excess deaths by gender & age-group throughout the pandemic period from week 11 onwards and contrasting excess deaths between E&W vs Scotland (where late registration does not apply).

Reviewer #4: This well written paper estimates excess mortality from covid-19 and to my knowledge, is the first to study impact on life expectancy.

I have a few minor comments the authors may wish to consider:

- It would be helpful to clarify how the LE estimates during 2020 where estimated as my understanding is that data was not available for 5 year age groups.

- I was not sure if the following sentence in third paragraph of results is correct or wrong way round, please check. As 'respectively' implies 28.7% for males and 33.2% for females.

"These numbers are 28.7% (27·8, 29·5) and 33.2% (32·3, 34·1) higher, respectively, than the baseline."

- It is interesting that the sensitivity analysis shows estimates of excess deaths are higher when using the first three approaches than from approach 4 (which is similar to that used by the ONS in their reports). It might be worth highlighting this point in the discussion. Also, I was surprised that the negative binomial method gives such different figures compared to the Poisson approach.

Reviewer #5: This is an interesting analysis which aims to compare excess mortality with other metrics: life expectancy and lifespan inequality. There are three major issues with the analyses and conclusions:

1. Whether these metrics truly are more comprehensive.

The abstract background talks about misclassification of COVID-19 but actually no other section of the abstract talks about misclassification or correct classification. Rather, this manuscript is setting out a different set of metrics for studying the effects of the pandemic but does not include direct or indirect effects or morbidity. Therefore I disagree that the life expectancy metric is that much more comprehensive than excess deaths per se. The GBD and DALYS/QALYs methods should be better referenced. There is also the issue that most of the deaths are in older people and in people with underlying conditions (mostly over the age of 50). Therefore, it is questionable whether life expectancy at birth is the best metric.

2. The findings regarding excess mortality are not new I am not clear why the authors are redoing analyses of excess mortality which have been done by ONS assuming a Poisson distribution. ONS has published week-by-week data for all-cause and cause-specific mortality since March, which has been used in other analyses, e.g.:

Banerjee A, Chen S, Pasea L, Lai A, Katsoulis M, Denaxas S, Nafilyan V, Williams B, Wong WK, Bakhai A, Khunti K, Pillay D, Noursadeghi M, Wu H, Pareek N, Bromage D, Mcdonagh T, Byrne J, Teo JT, Shah A, Humberstone B, Tang LV, Shah ASV, Rubboli A, Guo Y, Hu Y, Sudlow CLM, Lip GYH, Hemingway H. Excess deaths in people with cardiovascular diseases during the COVID-19 pandemic. Medrxiv. Preprint. 2020. Online 11/6/2020. <https://www.medrxiv.org/content/10.1101/2020.06.10.20127175v1>

3. There is no mention of direct versus indirect deaths The pandemic is causing deaths through infection (COVID-19 deaths) and indirectly through changes in health systems and in individual behaviours. The latter may end up causing far greater deaths over the long-term than COVID-19 per se. This has been modelled for cancer, cardiovascular disease and several other diseases in the UK and other countries.

Although this has not been a positive outcome, I would like to thank you for giving us the chance to consider your manuscript, and I hope this does not discourage you from considering The Lancet Public Health again in the future.

Yours sincerely,

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