Author's response to reviews

Title: Leave entitlements, time off work and the household financial impacts of quarantine compliance during an H1N1 outbreak

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Author's response to reviews: see over
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Dear Editor

Thank you for the opportunity to resubmit our manuscript “Leave entitlements, time off work and the household financial impacts of quarantine compliance during an H1N1 outbreak”. We also wish to thank the reviewer and editors for their helpful comments. We have revised the manuscript in light of these comments. Details of these revisions are outlined below. We hope you will consider our revised manuscript for publication.

Yours sincerely,
Anne Kavanagh

AUTHORS’ RESPONSE TO EDITOR AND REVIEWER COMMENTS

Title: Leave entitlements, time off work and the household financial impacts of quarantine compliance during an H1N1 outbreak

Comments from our Section Editor:

The participation rate is dramatically low (27%). Collection of data is retrospective which likely introduces recall bias. One statement in the abstract (and the manuscript) is flawed as one cannot conclude to a difference without statistical significance.

In response to the editors’ and reviewer’s comments, we have revised the abstract and the main text of the manuscript to (a) avoid making claims of difference when statistical significance measured at the conventional 5% level is not met, and (b) consider the implications of low participation rates and power limitations in greater depth in the Discussion section of the revised paper.

We note the potential for recall bias, however it would not have been possible to collect this data prospectively because the timing of onset of a pandemic cannot be known in advance.
We fast-tracked the ethics review, questionnaire development and recruitment to move into the field as soon as feasible. We refer to the potential for recall bias in the limitations section of the discussion.

Comments from our Associate Editor:

Although the hypothesis that is tested is extremely relevant and innovative (no similar attempt has been published before) the data collected and used (response rate of 27%), the important potential for selection bias, the retrospective and quite late data collection and the fact that many of the results are not statistically significant reduces very much the interpretation. However, publication as a research note would be a good balance to reconcile between the innovative approach and the lack of clear results.

The Editorial Board’s recommendation was to resubmit so we have done this. We have addressed the issue regarding statistical significance and recall bias in the paper (see editor’s comments above). Further, the helpful revisions suggested by the editors and the reviewer have allowed us to strengthen the manuscript in relation to areas mentioned in this comment by the Associate Editor.

Reviewer's report

Major Compulsory Revisions

The manuscript by Kavanagh et al addresses the effect of parental leave entitlements on time taken off work, financial impacts and compliance to quarantine recommendations. Although the research question is of high interest and the manuscript is generally well written, the fact that the survey response rate (27%) is considerably low limits the generalizability of the results. The results from a survey with a low response rate can be questionable because of potentially low representativeness of the participants who responded. It would be advisable to report the differences between respondents and non-respondents in this manuscript as well and not only mention this in the Discussion section.

We have expanded the paragraph in the Discussion on the limitations arising from the low response rate, and also moved it up to earlier in the discussion so it is more prominent. Unfortunately, the survey had to be administered through schools due to privacy
constraints. We had no direct contact with respondents or non-respondents and are unable to characterize non-respondents in more detail. We have included a statement to this effect in the Discussion. In an earlier publication from this study we reported an analysis of response rates by school socioeconomic status. We now cite the findings from this earlier analysis (that less advantaged schools had lower response rates) in the Discussion and make reference to the implication for generalisability. We also now mention early in the Discussion that the study is likely to be underpowered. In addition, we report the response rate in the revised Abstract, so the reader is aware of it early on.

The previous publication from this study should be introduced in the background of the paper.

We now cite the two papers already published (in this journal) from this study in the final paragraph of the Background section. Specifically we state:

In earlier publications from this study we examined compliance with the quarantine measures, and the information that affected households received about these measures [7,8].

Also, it would be helpful to explain whether there was any attempt to calculate an adequate sample size, taking account of an estimate of non-response rate, or discuss whether other measures to obtain higher response rates were considered.

The target population for this study was households in which a child had been asked to go into home quarantine, from all schools in Melbourne affected by class closures during the outbreak. Thus, our sample was a type of census and, as such, necessarily limited by the number of affected schools in the state. We have ensured this is clear in the revised manuscript.

With respect to efforts to boost response rates, under ‘Survey Administration’ we describe the incentives offered to both schools and households boost participation. We now also make reference to this in the Discussion where we discuss limitations associated with the low response rate, and possible reasons for non-participation.
A description of the research tool is lacking. I consider this as equally important to the description of the data collection process. I would like to see a paragraph reporting the steps followed in the development and testing of the tool (including reliability and validity). The addition of the questionnaire as supplementary material would help the reader to appraise the tool as well. Alternately, if the tool is adopted from a previous study, a reference would help.

Only limited testing of the survey instrument was deemed to be feasible due to the need to conduct the survey at the earliest opportunity after the school closures so as to minimise recall bias. We have included the following paragraph at the beginning of the ‘Survey Administration’ subsection of the Methods to describe this:

We tested a draft version of the survey instrument for comprehension, length and ease of administration with three participants from eligible schools, and made minor modifications based on their feedback. Due to the need to administer the survey as soon as possible after the school closures occurred, so as to reduce recall bias and maximise participation, more extensive testing was not feasible.

There is no paper version of the questionnaire. The questionnaire was conducted online or, in very small number of situations, by Computer Assisted Telephone Interviews (CATI). The questionnaire has many skips making it very difficult to read in paper form. Although we believe including the instrument in the publication would confuse readers rather than help clarify, we are happy to provide a copy of the CATI and defer to the Editor’s judgement regarding its inclusion as an appendix. We have provide detailed descriptions of the questions of relevance to this analysis in the methods section.

The wording of the relationship between leave entitlements and time taken off work is misleading both in the abstract, the Results and the Discussion sections, and should be revised.

We have made substantial revisions to the manuscript which we hope now improve its clarity, including with respect to the relationship between leave entitlements and time taken off work.
**Minor Essential Revisions**

**P-values should be shown consistently across the manuscript.**

We have now shown p values throughout the manuscript, and also included a reference to their calculation the methods.

**The response rate should be reported in the abstract so that the reader can easily find the limitations of the survey.**

We have now included the response rate in the Abstract.

**The time when the survey was conducted should be reported in the Methods section. Was it November 2009?**

The following sentence has been added to the Methods (survey administration) section:

The finalised survey was administered during November and December 2009.

**Discretionary Revisions**

**Abstract:**

**The objectives of the study are not clearly stated: a rephrasing would make them more straightforward. Also, adding “to quarantine recommendations” after compliance (line 4 in Background) would make it clearer for the reader.**

We have amended the Background section of the Abstract so that it now reads:

The Australian state of Victoria, with 5.2 million residents, enforced home quarantine during a H1N1 pandemic in 2009. The strategy was targeted at school children. The objective of this study was to investigate the extent to which parents’ access to paid sick leave or paid carer’s leave was associated with (a) time taken off work to care for quarantined children, (b) household finances, and (c) compliance with quarantine recommendations.

**In the methods section, the number of respondents from whom information was collected should be stated.**

The methods section of the Abstract now reads as follows:
We conducted an online and telephone survey of households recruited through 33 schools (85% of eligible schools), received 314 responses (27%), and analysed the subsample of 133 households in which all resident parents were employed.

**Background:**

I would recommend that you use the name given to the pandemic by the Advisers to the WHO Consultation on the Composition of Influenza Vaccines for the Southern Hemisphere 2012 (26 September 2011): influenza A(H1N1)pdm09. ([http://www.who.int/influenza/gisrs_laboratory/terminology_ah1n1pdm09/en/index.html](http://www.who.int/influenza/gisrs_laboratory/terminology_ah1n1pdm09/en/index.html); Van Kerkhove, Ferguson. Bull World Health Organ 2012)

We have taken the reviewer’s advice and made this change.

In addition, several organizations define quarantine, isolation and social distancing as different measures. For example Dutta (2008) in the World Bank Policy Research Paper The effectiveness of policies to control a human influenza pandemic: a literature review defines social distancing as “A disease prevention strategy in which public health authorities limit social (face-to-face) interaction to reduce exposure to and transmission of a disease. These limitations could include, but are not limited to, school and work closures, cancellation of public gatherings and closure or limited mass transportation” and quarantine as “The physical separation of healthy people who have been exposed to an infectious disease – for a period of time – from those who have not been exposed” (isolation is also defined in Jefferson et al, Cochrane Database Syst Rev. 2011 (7):CD006207, or [http://trainingcalendar.ct.train.org/panflu_toolkit/documents/Resilience/PanFluQuarantineIsolation](http://trainingcalendar.ct.train.org/panflu_toolkit/documents/Resilience/PanFluQuarantineIsolation). Please clarify, since these terms seem to be considered as interchangeable along the manuscript. If in Australia there are different definitions, it would be recommendable to explain the differences in international definitions here.

We had originally been using social distancing as a broader term which covered, among other measures, home quarantine. We thank the reviewer for highlighting the ambiguity that this created, and we have now made a distinction between the two by making various small changes throughout the manuscript.
Additional studies to [1-4] should be cited to better place this study in context (e.g., Barr et al, BMC Infect Dis 2008; 15;8:117; Eastwood et al, Bull World Health Organ 2009;87(8):588-94 or others). Referring to the cost-effectiveness of physical measures would add valuable information too (e.g., Perlroth et al. Clin Infect Dis 2010 50: 165–174). At the reviewer’s suggestion we have included the Eastwood 2009 reference to the Background. We have not added references to cost-effectiveness of quarantine and social distancing measures as we think it might distract from the main focus of our paper.

Methods:

Paragraph 2: ‘Oseltamivir’ should not be capitalized as it is not a brand name.

We have changed this.

Paragraph 2 and 3: ‘(Department of Health Victoria quarantine guidelines, 4 June 2009)’ should be converted in a numeric citation.

We cited this source in this way because the guidelines are no longer publicly available.

Statistical analyses: Add a reference to Stata 11.0. such as ‘(City, State: Name of company)’.

We have added this in-text reference.

Results:

3rd paragraph: The first appearance of Figure 2 seems unnecessary.

This reference to Figure 2 has been removed.

Discussion:

It may be argued that an average parent would prioritize the care of a child for a short period of time over financial losses (and indeed over societal benefit), especially in an uncertain and potentially dangerous situation such as the 2009 pandemic. This could also explain the lack of association between access to leave and compliance with quarantine.

We acknowledge the reviewer’s point. However our argument about the lack of association between access to leave and compliance is that parents appear to have taken time off work,
rather than making other childcare arrangements, to ensure compliance with quarantine (they did this even though most children placed in quarantine were not diagnosed with influenza, i.e. they were contacts rather than cases, and therefore unlikely to be symptomatic). We included the following sentence in the discussion to highlight this point:

While some of this behaviour may have been driven by the need to care for sick children, there were no confirmed influenza A(H1N1)pdm09 diagnoses in the vast majority (94%) of households in our sample. This suggests that, absent the strict quarantine restrictions, other childcare options may well have been attractive to parents to enable them to attend work during the period of school closure.

**Paragraph 4:** As mentioned before, it would have been useful for policymaking to collect cost data as well. With the current results, paragraph 4 may go beyond the scope of the paper. I would present this information in the background of the paper though. In addition, I would cite more recent papers than [12], covering the cost-effectiveness of strategies to tackle influenza pandemics (e.g., Perez Velasco et al. PLoS One 2012; 7(2): e30333; Lugnér, Postma. Expert Rev Pharmacoecon Outcomes Res 2009; 9(6):547-58).

We have decided to remove the last sentence of this paragraph in response to this comment, and this led us to make a further change earlier in the same paragraph.

**Paragraph 6:** It seems that some limitations are attributed to the previous publication from this research. It would be better to re-write this section to make clear that they are shared by the two reports.

We have re-written the limitations section of the discussion, in response to this and other comments from the reviewer. We no longer make reference to the previous publication, and instead expand on the limitations of the study generally. References to the other publications from the parent study are now confined to the Background section (as per earlier comment).