Author’s response to reviews

Title: Economic consequences of Japanese schools’ recovery certificate policy for seasonal influenza

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Economic consequences of Japanese schools’ recovery certificate policy for seasonal influenza

Dear Editor,

Thank you for the opportunity to revise our manuscript. Once again, I appreciate the careful review and constructive suggestions from the reviewers.

Following this letter are the reviewer comments with my responses. Changes made in the main body of manuscript are marked using track changes.

Thank you very much for your consideration.

Sincerely,

Shinya Tsuzuki, MD, MSc

Reviewer reports:

Clemence Tarirai, BPharm, MPH, MTech(PharmSci), DTech(PharmSci) (Reviewer 1): Thank you for the opportunity to review the manuscript: Economic burden for "Recovery Certificate" from seasonal influenza in Japan.

My comments are:
1. The value of the Japanese "recovery certificate" has been questioned in a number of writing (some quoted by the author), hence it's not new information. However, the author attempted a different and interesting approach by looking at the negative economic impact posed by the requirement to provide a "recovery certificate" in order for school-going children <15 years old to come back to class.

Thank you for your comments. I have responded to each of your questions and comments below.

2. The manuscript's theme is narrow and specific to the Japanese economics policy-makers audience, hence it would be appropriate for publication in a local (Japanese) medical, economics or labor journal once revised.

As you pointed out, this manuscript deals with a health policy issue specific to Japan. However, this does not mean that this theme must be published in a local journal. It is useful for us to extend our knowledge about public health policy in foreign countries because a deeper understanding of these issues might provide insight into challenges we face in our own respective countries. I have personally been interested in public health policy regarding primary care overseas and this is exactly the reason I came to learn of the differences in attitude toward influenza between Japan and other developed countries. Therefore, I believe that country-specific public health policy issues are worthy of publication in international journals.

3. The inclusion and exclusion criteria are ill-defined. The methods described are for a specific set of population groups (I.e. school-going patients less than 15 years old, their caregivers and healthcare providers), whereas the title is too general for the Japanese population. Hence, the title (aim) and the methods are partially disconnected.

Thank you for pointing this out. Another reviewer also mentioned the inconsistency between the title and the content of the text. Accordingly, I have changed the title of the revised manuscript to “Economic consequences of Japanese schools’ recovery certificate policy for seasonal influenza”.

4. The descriptions of the methods (and maths) for estimation of the variables studied e.g. sample size for different target groups are not clear (lines 121-125, 159-160). Perhaps, the author should provide properly explained mathematical expressions/equations for defining and assessing the parameters studied while clearly stating the assumptions made or providing references for excluding possible confounding variables.

As suggested, I have added three equations and a more detailed explanation to the “Methods” section of the revised manuscript.

5. There is a disconnect between the methods and the data presentation. One example is the use of different age group categories in the methods compared with those in the results, <5 years old
vs. <6 years old. The statistical data analyses is mentioned, but it is unclear what was being analyzed e.g. dependent, independent, indemmediate, and confounding variables, and their link to the outcome (cost burden)?

Thank you for this comment. Although there are no data about one-year age stratified surveillance data, I introduced population statistics data of Japan [1]. As explained in the revised manuscript, I stratified 5-year age group surveillance data into five 1-year age group data based on the age structure in Japan. As for discussion about dependent/independent variables, these factors are not defined in cost analysis because this type of analysis (economic evaluation) has a different model structure compared with, for example, multiple regression analysis in the social sciences. Of course, various confounding factors might impair the robustness of the results in any kind of model, but sensitivity analyses are usually conducted in order to compensate for the considerable uncertainty in economic evaluations.

6. The manuscript is laden with grammatical, contextual and punctuation errors e.g. Background section: bold lines 42, 44, 47, 49, 50, 51, 52-59 (context?), 62, 79, 181 (chapter?), 204 and the rest of the manuscript. It requires major English language editing.

The errors you pointed out have been corrected accordingly, and the revised manuscript was proofread by a native English speaker.

7. The manuscript was written by a single author, but the author refers to oneself as 'we' throughout the articles: bold lines 81, 89, 90, 104-105, 111, 113, 118, 129, 131, 136, 138, 141, 145, 147, 155, 158, 161, 204, 205, etc.

This has been corrected accordingly.

8. The author makes speculative statements (might?, seems?) in the Results and Conclusions with definitive reference to the findings to support the arguments e.g. the statement in bold line numbers 30-31 in the abstract and conclusion, lines 69-70.

This has been revised accordingly.

9. The author makes inaccurate comparisons in lines 181-183 and 208-210. For instance, are total costs (burden) values reported over the period 2013-2018 or are average annual costs (burden) reported for the the period 2013-2018? (the latter can be meaningfully compared with data for 2015).

I have updated all the results as average annual values.
Pieter De Boer (Reviewer 2): This manuscript titled "Economic burden of "Recovery Certificate" from season influenza in Japan" estimates the costs of a "recovery certificate" policy for schoolchildren in Japan. The paper is interesting and helpful to understand the Japanese school policy on sick leave of children. The calculations are straightforward and clearly described. Nevertheless, I have some additional comments/questions which may improve the paper.

1) On first sight, the title did not ring a bell on what I could expect from the content. It may be a difference of taste, but there are three aspects I do not like so much. 1: Burden. I am not sure whether a recovery certificate causes an economic burden, because a burden has a negative load and that reflect too much the opinion of the authors. I would prefer the word "consequences". 2: "recovery certificate": I think the certificate does not cause the burden, but it is the "recovery certificate policy". 3) It is not clear whether the study deals with sickness leave on schools, because it could also deal with working adults. So I would modify the title to "Economic consequences of the recovery certificate policy from seasonal influenza in Japanese schools ". If the authors agree, these terms should be changed throughout the whole manuscript.

Thank you for your insightful suggestions. I have updated the title based on your suggestion.

2) It is not clear to me what the criteria of Japanese schools are in order to ask for a certificate. Do they use something similar to the usual ILI criteria like fever combined with a respiratory symptom? Are children sampled in order to confirm influenza infection?

Thank you for your question. This is a situation specific for Japan. Japanese physicians and patients prefer a definitive diagnosis of influenza and physicians almost always use rapid influenza diagnostic (or, antigen detection) tests (RIDTs or RIADTs) [2]. These tests are different from virological tests, but they take less time (about 10 minutes) and are comparatively reliable in terms of specificity [3]. Therefore, most influenza patients in Japan are diagnosed not as having ILI, but as having influenza. Schools in Japan require children who are diagnosed as having influenza to submit a certificate. I have also added an explanation about this situation on lines 47-50 in the manuscript.

3) Currently, the recovery certificates issued every year was estimated by the total number of symptomatic influenza patients under 15 years old, with a reference to weekly reports of influenza virus detection. How did the authors use exactly that information in order to estimate the number of cases and subsequently the number of certificates? What are the number of ILI cases and the proportion of detected influenza viruses? In line with comment 2, would the number of certificates not be higher than the number of symptomatic influenza cases, as there may be other pathogens that cause ILI symptoms and schools presumably can not judge whether the infection is influenza or not.
In line with the answer to comment 2, Japanese surveillance data represent the exact number of symptomatic influenza patients who visited any health facility. That is why I used these numbers as a proxy for the total number of patients, even though they are diagnosed by RIDTs instead of virological test.

4) The assumption of half day leave for a caregiver is a strong assumption that could be added to a sensitivity analysis.

I agree with your suggestion and have added a sensitivity analysis about the half-day leave assumption.

5) I like the adjustment for grandparents. The authors have been very thorough about this aspect.

Thank you for pointing out this issue. I have added another reference to descriptive research which mentions the proportion of grandparents who could help their grandchildren on behalf of parents [4]. According to this article, 72% (regardless of cohabitation status) of parents request that their parents (i.e. their children’s grandparents) take care of their children when the children are sick. Considering this, I assumed that 72% of parents do not need to take leave for a physician revisit. This produced a more conservative result than the previous version of the manuscript showed.

Although there were some limitations in the article (written in Japanese, with the target population limited to a single district, etc.), I also included this factor in the sensitivity analyses in order to compensate for the limitations.

6) Page 10, line 155. This should be discussed in the paragraph in which the number of certificates is determined. The source of this percentage seems not that reliable. Please clarify how this poll is conducted and who were the respondents.

First of all, I apologize that my expression was somewhat confusing. Under my assumption, all nursery schools and primary/secondary schools require students to submit a certificate, but some health facilities issue a certificate without charging a revisit consultation fee and/or documentation fee. The reference I cited mentions the proportion of physicians who issue certificates without charging such fees. An explanation about the proportion of certificates required has been added on lines 140-145 of the revised manuscript.

7) It is not clear to me whether Table 1, the number of document issued, is methodology or a result. In results page 10 line 167 states the number of patients of 7253100, and table 1 5135738 documents, but how this conversion has been done is not really clear to me. Also the first paragraph of the results seems to have a lot of methodology like proportion of people with fulltime job, and other percentages mentioned in line 169-173.
As discussed in my answer to the previous question, my approach to the “Methods” and “Results” sections was confusing. I have corrected the number of documents issued as explained in the equation (1) of the revised manuscript, and moved the first paragraph of the “Results” section to the “Methods” section.

8) I think the authors should be careful not to exaggerate their personal opinion throughout the paper that they not like this policy, but be a little more neutral. For instance: Page 5, line 71: "To make matters worse", Page 13, line 205-206. "our study offers an opportunity to tell a little bit weird custom in Japanese healthcare system to other countries' people.

In accordance with your recommendation, I have modified these expressions in the revised manuscript.

9) Figure 2 is not so clear. Smaller cost intervals would provide more information on the distribution, while also the exponential in the legend could be replaced by millions US$

I believe you may be referring to Figure 3. If that is the case, I have revised Figure 3 in accordance with your recommendation.

10) Some minor edits:

- page 16, line 259: societal burden to our society -> 2 times society
- page 2, line 26: IssuRance
- page 2, line 49: THE "recovery certificate"
- page 9, line 137: 12 YEARS

I have corrected these accordingly. Additionally, the revised manuscript has now been professionally edited by a native English speaker familiar with this area of research.

References

