Author's response to reviews

Title: Would school closure for the 2009 H1N1 influenza epidemic have been worth the cost?: a computational simulation of Pennsylvania

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Author's response to reviews: see over
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Dear Editors:

On behalf of the authors of the paper entitled “Would school closure for the 2009 H1N1 influenza epidemic have been worth the cost?: a computational simulation of Pennsylvania” I would like to thank you for your time and consideration and the opportunity to revise our manuscript. The comments made are very much appreciated and have helped us to improve the manuscript. Listed below are point-by-point responses to each of the Editors’ comments.

Reviewer #1:

1. Comment on any potentially important differences in demography, school location/makeup in Pennsylvania population between the year 2000 (data used for synthetic population) and year 2009 (data used for influenza parameters and costs). If no major changes have occurred in the last decade that might affect conclusions, a sentence to this effect would strengthen paper.

Response: According to the US Census Bureau’s 2005-2009 America Community Survey, the age distribution of the population in the state of Pennsylvania did not change significantly.

Added the following text on page 5:

“The US Census Bureau’s 2005-2009 America Community Survey results show that there were insignificant changes to the age distribution in Pennsylvania between 2000 and 2009.[23]”

2. Comments on use of number-of-cases (rather than proportion of students) as a parameter for school closure trigger. If schools in the study area are all close in size, or if number of cases is a more realistic criterion than proportion, a sentence to this effect would strengthen paper.

Response: Our work with public health officials (such as author Ronald Voorhees, Director of Epidemiology at the Allegheny County Health Department) during the 2009 H1N1 pandemic, and our group’s previous research on school closure in Allegheny County [see ref 7] suggests that the use of cases as an individual trigger to school closure may be appropriate and realistic to how school closure would be performed in
the state of Pennsylvania. Sensitivity analysis of other trigger mechanisms were performed but not included in the paper due to them not have a significant effect on the outcome of the study.

Added the following text on page 6:

“In consulting with public health officials in Pennsylvania during the 2009 H1N1 pandemic, this scheme may be consistent with how school closure would be implemented in the state during an epidemic, and sensitivity analysis of other closure triggering mechanisms showed no significant change in the results.”

3. Did both the ABM and economic model take into account calendar timing of school closure and its relation to school holidays and breaks (when schools are closed already)? If so, what was assumed about activities of school-age children and mixing dynamics during school breaks? Clarification of this point will strengthen paper.

Response: It is assumed that the outbreak of the pandemic starts when students are in school for the fall (which is consistent with surveillance data gathered by the CDC). Students do not go to school on the weekends in the model. If a child does not go to school, their school-based activities are not taken into account for that day, but all other possible transmission places are considered. Given the focus of the study was on responsive school closure and the length and timing of the 2009 H1N1 epidemic, we chose not to consider extended holidays such as Thanksgiving or Christmas breaks as it was unlikely to change the outcomes of the research.

Added the following text on page 6:

“Consistent with surveillance data collected during the 2009 H1N1 epidemic by the CDC, it is assumed that when the epidemic starts, schools are open. On weekends, students do not go to school and instead have increased activity in their neighborhoods and communities. The peak of the epidemic occurred well before possible extended holidays such as Thanksgiving and Christmas and so these were not considered.”

4. Previous studies cited here have accounted for presence of multiple adults (with different employment status) in households with school age children, have argued that population-average wage by industry assumptions are not always good approximations of earnings, and have accounted for the likelihood of partial productivity among absentees through work-from-home. Clarification of how these issues were included in the analysis (via synthetic population data or Monte Carlo simulation), or why they were excluded, will strengthen the paper.

Response: As mentioned in the paper, we do stratify the populations in our economic model to account for some of these factors. We break down families into single parent, dual-income, and single income families, which is derived from data gathered from the Bureau of Labor statistics. We also apply a factor to account for families that have more than one school aged child to prevent double counting absenteeism for those families. We do not make an attempt to account for parents that work from home, as we did not feel we had reliable data to account for this factor. A sensitivity analysis of this factor
has been performed, and even if 50% of the families could work from home, the general conclusions of the work would still not change.

The following sentence was added to on page 16:

“The economic model does not include an attempt to account for persons who may be able to work-from-home, which may reduce the productivity loss estimates of school closure but is unlikely to change to conclusions of this research.”

5. An additional potential impact of school closure identified by some of the cited research is absenteeism among health-care workers, which might affect treatment or other mitigation strategies. Inclusion or estimation of this effect in the ABM epidemic model would be a nice addition to the paper.

Response: This is an excellent suggestion and one our group plans to explore in the near future. However, our current ABM is not able to account for this statistic, and adding this feature would require changes to the program outside of the scope of this work. We do have health care workers in the model, and their absenteeism due to influenza is included (i.e. if a health care worker becomes symptomatic with influenza, there is a probability that they will stay home). We have plans to look at this effect in future research.

Reviewer #2

1) One of the reasons school closure was seen to have an impact in some circumstances during 2009 (e.g. in the early Japanese outbreak – see Nishiura et al Eurosurveillance 14(22):19227) was that children seem to have been disproportionately affected by the H1N109 strain. A strong age-specific impact can be seen in most pre-vaccination serosurveys (e.g. Wu et al. Clin. Infect. Dis 51:1184, Miller et al, Lancet 375: 1100). Following up some of the references in the paper, it appears that this model also incorporates higher transmission from children, however this point is not discussed. If the investigators simulate an outbreak without school closure, how does the attack rate compare across age groups? Could they comment on the extent to which this model has been calibrated to the 2009 pandemic strain?

Response: The model does indeed have this effect taken into account. As described in detail in other publications, the calibration scheme we are using is consistent with other models produced by the MIDAS research network. Also, as you have pointed out, the model shows a significantly higher percentage of incident infections among children than the rest of the population. A figure has been added showing the attack rate of school-aged children versus the rest of the population for the three $R_0$’s explored. As to the extent the model has been calibrated for the 2009 pandemic strain, the model basic disease parameters such as latent and infectious period are similar to that estimated for the 2009 H1N1 pandemic.

Added new Figure 2 and added the text to page 6:

“The model is calibrated using empirical data from the 2009 H1N1 epidemic and historical epidemiology studies of epidemic influenza. Important to this work and
consistent with other influenza modeling efforts, schools are a critical place for spread of the disease\[21, 22\]. Figure 2 shows the percent of incident infections for school aged children versus the rest of the population as a result of the model without school closures. It shows that across several basic reproductive rates, $R_0$ (the average number of infections produced by an infected individual in a completely susceptible population), that the percent of infections for school-aged children is much higher than that of others in the population. The US Census Bureau’s 2005-2009 America Community Survey results show that there were insignificant changes to the age distribution in Pennsylvania between 2000 and 2009\[23\].”

2) A strong finding from this study is that school closure is too costly for a relatively mild influenza strain. However, it would be valuable to be able to apply these results to possible future pandemic strains. Could the authors provide a bit more detail on how severity might change these results? The case fatality percentage assumed here is between 0.002% and 0.01%; would a much higher severity change these results considerably?

Response: This is an excellent point. We have added to this analysis a set of runs at basic reproduction rates of 1.2 and 2.0 the cost-benefit analysis of epidemics that have 10 and 100 times the case fatality rate of 2009 H1N1 to explore this space. In all of these cases, the cost of closing schools still greatly outweighs the benefits from mitigation of the epidemic. So the results of this sweep along the case fatality rate imply that the conclusions of the paper are robust for much more deadly influenza epidemics.

Added Figure 7 and the following text to page 12:

“A potential factor to consider from the 2009 H1N1 epidemic is that the case fatality rate (CFR) was relatively low, especially compared with that of estimates used in the preparedness planning for avian influenza [1]. To examine the potential sensitivity of the model to increased CFR, rates of 10 and 100 times that of the estimated CFR’s for 2009 H1N1 were explored. Figure 7 shows the net costs for varying durations of school closure with the 2009 H1N1 CFR as well as 10 and 100 times the original value. As expected, increases in CFR lower the net cost as the benefit of the school closure prevents morbidity. This effect, however, is minor when compared to the cost of closing schools, and so the net costs do not drop significantly.”

And added the following text to page 14:

“For CFR of up to 100 times that of 2009 H1N1, school closure still exhibits a net cost.”

3) The results presented in Table 2 concerning the impact of closure length on cost demonstrate that the most cost effective solution – for all values of $R_0$ – is the 8 week closure. This also seems to me to be a valuable finding for informing potential future pandemics, and I feel the authors could stress it further.
Response: Thank you for this observation; the following text has been added to page 14:

“Indeed the cost per case averted drops as school closures are extended to 8 weeks in length.”

And added the following text to page 17:

“As in previous studies, closing schools for at least 8 weeks is necessary to have an effective mitigation of an influenza epidemic.”

Also, thank you for reading the document carefully and pointing out the remaining typos in the manuscript. We have gone through and corrected the errors noted.

Again, thank you for your time and consideration for this manuscript. Your comments are well received have strengthened this work.

Sincerely,

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