Reviewer’s report

Title: Developing guidelines for school closure interventions to be used during a future influenza pandemic

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Reviewer: Kathryn Glass

Reviewer’s report:

This paper investigates the impact of operation issues surrounding school closure – such as timing, duration and the extent to which closures are simultaneous. They identify that under most conditions, individual school closure is preferable, although long-term, widespread closure may be considered for a highly pathogenic strain. The modelling work is thorough, and the results are informative for policy makers. I particularly like the table of policy recommendations (although I’d suggest changing “0.13% newly infected” to “13 new cases per 10,000”).

Minor Essential Revisions:

1) The authors report the reduction in attack rate for various levels of school closure and accompanying antiviral use. In the results section, this is presented unambiguously – e.g. they report “a maximum of 14% (from 33% to 19%) reduction in attack rate can be achieved...”. However, in the abstract, such a result is simply reported as a “~15% reduction”. My first interpretation of an 15% reduction would be an attack rate reduced from 33% to 28%. I realise there are space constraints, but re-wording to “Attack rates were reduced from 33% to 19% by 8 weeks school closure”, or something similar would avoid ambiguity. As it stands, the abstract suggests to me that school closure has a very minimal effect, but in fact these are quite considerable reductions.

Discretionary Revisions:

2) Could the authors clarify the antiviral distribution strategy? Once antivirals begin to be distributed, are they given continually throughout the outbreak? They note that: “Antivirals eliminate the dependency of activation trigger on school closure duration”. Do they have an explanation of this result?

3) The result that individual school closure is much less sensitive to the timing of the intervention than simultaneous school closure is very valuable. For a new pandemic strain of influenza, we are unlikely to know exactly how many un-reported, un-diagnosed or asymptomatic cases exist, so relying on accurate data to trigger an intervention seems dangerous. Moreover, individual school closure is more likely to be tolerated for a mild pandemic strain, as parents will be aware of local cases. I’d like to see the authors highlight this result – and its practicality – a little more.
4) The authors use age specific infection rates from seasonal H3N2 to calibrate age-specific susceptibility in their model. While this data has higher infection rates in children than adults, the differences are not nearly as extreme as much of the data from the 2009 H1N1 pandemic. For example, serological data for H1N109 in the UK was used to estimate seroconversion rates of around 21% in 0-4 year olds, 42% in 5-14 year olds, 21% in 15-24 year olds, and no significant increase from baseline in the 25+ year olds (see Miller et al (2010) Lancet 375:1100-1108). In Australia and Japan, early outbreaks showed around 80% of cases in under 20 year olds, with school closure seemingly effective in interrupting the first wave in Japan [20]. While I realise this paper is examining more general aspects of timing, duration and type of school closure, I’d be very interested to see the effect of H1N1 patterns of transmission on these results. As far as I can tell, the sensitivity analysis reported in [15] compared the model used here to one in which there was no difference in attack rates with age. Some discussion of the impact of a more extreme pattern of age-specific attack rates would be valuable, and would reassure the reader that the results are applicable to the H1N109 strain.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.