Reviewer’s report

Title: Model the impact of travel restriction via air, sea, and land for the influenza A H1N1 epidemic in Hong Kong

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Reviewer: Deirdre Hollingsworth

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In this paper the authors discuss the relative impact that land, sea and air travel restrictions would have had on the timing and magnitude of the H1N1 2009 pandemic outbreak in Hong Kong. They model an epidemic in Hong Kong and in a number of connected areas to look at the effect of controls within Hong Kong. They show that very high levels of travel restrictions are required to delay an epidemic, and that the delay depends on whether connections to source areas are targeted first, on when the epidemic gets established in China and on the R0 in connected areas and in Hong Kong. They also show that travel restrictions have most impact when in combination with other control interventions.

There were a flurry of studies on this issue published in 2006/2007 onwards. I have listed several of them below. The authors refer to most of these in their paper, although a couple are missed, notably Colizza et al on cooperation between countries, and Pitman et al on the efficacy of screening in delaying an epidemic. The published papers are focussed mainly on air travel since this is the most rapid method of global spread. However, the general insights are likely to be the same for other methods of travel. Therefore the general insights in this paper are not surprising.

The novel aspects of this paper are the application to Hong Kong in detail and explicit consideration of land, sea and air travel. The authors conclude that the route which is best connected to the source area for the H1N1 pandemic (Mexico/USA, air travel) is the most effective route on which to impose travel restrictions and that the strong land connection to China will have an impact once the epidemic takes off in China. The explicit consideration of the Hong Kong scenario may be of interest to policy makers in Hong Kong.

Minor essential revisions

This study is carefully performed and written. However, it was not clear to me how the R0s for other countries were estimated (In table 2 of the SI it says they are estimated). There is also no presentation of data from the Hong Kong epidemic against which to validate the model.

The first paragraph seems to be out of date – it refers to a ‘recent’ clinical update from 2010 (reference 1), and an update in Hong Kong from week 29 of the pandemic (reference 2). Tables 1, 2 and 3 are difficult to read, and perhaps this
information might be better presented as figures.

References


1. Is the question posed by the authors well defined? Yes
2. Are the methods appropriate and well described? Yes
3. Are the data sound? Yes
4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Yes
5. Are the discussion and conclusions well balanced and adequately supported by the data? Yes
6. Are limitations of the work clearly stated? Could do with a little more detail here.
7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? There could be more summary of previous general insights in the introduction.
8. Do the title and abstract accurately convey what has been found? Yes
9. Is the writing acceptable? Yes

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests