Numerous statistical methods have been applied over the years to try to enhance the early detection of an influenza epidemic/pandemic in the midst of the usual seasonal influenza cases. This paper is just proposing another of these statistical techniques. On the basis of simulated data, the proposed new method does not demonstrate complete superiority to the other approaches, and perhaps most telling, it performs rather less well when using real data from the recent H1N1/2009 influenza pandemic. Hence, as with most things in real life, it is likely that a combination of methods will be used to assess the situation.

This does not necessarily detract from the presentation of a new approach. However, the limitation, as with any statistical application, is the quality of the data, i.e. in this case, how many of the ILI cases present to healthcare services and how many of these are reported to surveillance system. Even sentinel GPs may fail to report every case for various reasons. This is the real limitation of such statistical approaches - when the comprehensiveness of the data is questionable.

For early pandemic vs seasonal influenza detection, the natural heterogeneity of the data, due to patients'/doctors' differences in behaviour, e.g. some being stoical Scots and not presenting to healthcare services until they are on death's door, as well as poor or delayed reporting of such ILI cases to the surveillance service from sentinel or other GPs, can all confound early detection of pandemic influenza activity.

Given the above, I would ask the authors to modify their text slightly as follows:

Major Compulsory Revisions - none

Discretionary Revisions - none

Minor Essential Revisions

Background: Last sentence, 1st paragraph.

Please rephrase or remove the reference to the 'containment of an outbreak at its source'. I know this has been advocated by one of the coauthors (Neil Ferguson), but this has been shown not to work in most situations. The rather desperate attempts to contain local outbreaks of the pandemic virus using mass prophylaxis has led to unnecessarily large numbers of adverse reactions to the antiviral drug...
oseltamivir that was used for this purpose early on in the 2009 pandemic, particularly in children (see refs below). Now, in retrospect, this type of mass prophylaxis needs to be carefully balanced against the severity of illness induced by the virus in question. Not only may this be unnecessary in terms of adverse effects on individuals, it is also extremely expensive for governments and public health agencies to fund.


Discussion: 2nd paragraph.

This paragraph seems a bit odd. Earlier in their Results, the authors propose that with a 1% case reporting rate, their WCR approach with a mean detection time (MDT) of 4 weeks is superior to the Cusum or ILI threshold methods with MDTs of 5 weeks each. The in this 2nd paragraph of the Discussion, they then question whether, in fact, a delay of up to 12 weeks is, in fact, poor? If it is not, then presumably whether the MDT is 4 or 5 does not really matter, so all the methods (WCR, Cusum and ILI threshold) are equivalent? The reasons they give thereafter to explain the poor performance of their WCR approach can be applied to all 3 approaches to various extents.

There is no need for the authors to feel 'embarrassed' about the poor performance of their model using the real H1N1/2009 data, which is how this paragraph seems to read to this reviewer. Rather, I would ask the authors to incorporate some of my more general points above and re-write this 2nd paragraph to be more objective and suggest that, in any case, on a practical, daily surveillance level, all 3 statistical methods may well be used in combination if there is any suspicion of unusual influenza activity. This paper just offers an additional tool in the influenza surveillance armamentarium - it does not have to be significantly superior to all other methods (which it is not) to be of use, but can act as a check on the results from other, older statistical methods used to analyse such data.

Level of interest: An article whose findings are important to those with closely related research interests

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