Author's response to reviews

Title: Early detection of influenza outbreaks using the DC Department of Health's syndromic surveillance system

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Author's response to reviews: see over
Dear Dr. Graham,

We appreciate the second opportunity to revise our manuscript, “Early detection of influenza outbreaks using the DC Department of Health’s syndromic surveillance system” (MS 1325449738303918). Below you will find a detailed response to each comment made in the reviews of our manuscript. We believe that the topic we address and the methods we employ are of great interest to the journal’s readership and are eager to see the results of our work published in *BMC Public Health*.

Thank you again for your careful consideration of our submission.

Sincerely,

Beth Ann Griffin, Ph.D.
Michael Stoto, Ph.D.

**Reviewer:** Michael Jackson

**Major compulsory revisions**

None.

**Minor essential revisions**

1. Combining the two former papers into one forces some information to be relegated to appendices or to supplementary on-line material, as the authors have done. However, the main manuscript should ideally be a stand-alone document. Overall the authors have done an admirable job with this. However, one place where information should be moved from the appendix to the main paper (or vice versa) is the description of the simulated outbreaks. The appendix describes how the simulated outbreaks were generated (using $x$ added standardized counts). However, there are places in the main text and in the figures (page 13, second paragraph and figure 1, for example) where the authors refer to outbreaks of size $x$. This becomes confusing for readers who have not read the appendix before the results. For clarity, it would help to either move the description of the outbreak simulations to the main text, or to move discussion of outbreak size to the appendix or other supplementary material.

We thank the reviewer for his careful review of both the main manuscript and the supplementary materials. Given the comment above, we have moved the details on how outbreaks were simulated to the main manuscript.

2. On page 15 at the top, the authors say “we recommend using $(k,l) = (0.25,0.20)$ since it does outperform $(k,l) = (0.25, 0.40)$ even if slightly. Recommending specific values for parameters to be used in other contexts conflicts with the discussion: “The optimal parameters and detection algorithms that we found in this analysis, of
course, apply only to the particular data sets analyzed...the results cannot be
generalized to other data streams...” (page 21, third paragraph).

We have corrected the recommendation in the results section (now on page 16) to read:
“we recommend using \((k, \lambda) = (0.25, 0.20)\) for the D.C. data since it outperforms \((k, \lambda) = (0.25, 0.40)\) even if slightly.”

(3) An additional limitation that should be mentioned is that setting the false
positive rate assumes there were no true outbreaks during the non-flu seasons. If
there were outbreaks, this would cause the false positive rate to be overestimated.

In response to this comment, we now state in our limitations paragraph on page 25 that
“Our method for setting the false positive rate assumes that there were no true outbreaks
during the non-flu seasons.”

(4) On page 17, the authors use CUSUM and EXPO to confirm that the “unspecified
infection” is the most sensitive for detecting the onset of seasonal influenza. Why
was MV CUSUM not also used here?

The MV CUSUM is not used in the analyses shown in Figure 5 since the analyses only
include univariate streams of data. We clarify this point by writing on page 19: “The
flagging of the \textit{univariate} detection algorithms is represented by symbols (+ for CUSUM,
\circ for EXPO) plotted according to the day they flagged on the horizontal axis and along
different fixed values on the vertical axis to help distinguish more clearly between the
algorithms being compared.”

(5) On page 12, middle of the page, the following sentence has errors and is
incomplete: “Initially, we examined how well each of the eight syndrome categories
did along [sic] at flagging the beginning of the flu season depending.”

We have corrected this text. It now reads: “Initially, we examined how well each of the
eight syndrome categories did \textit{alone} at flagging the beginning of the flu season.”

Discretionary revisions
(1) Figure 1B is apparently for the unspecified infection syndrome based on text and
figure title in the manuscript, but the title in the .ppt file with the figures suggests
the data come from the gastro syndrome.

We apologize for this error. This graphic was incorrect in our prior submission. The files
are now up to date and Figure 1B is for unspecified infection as described in the
manuscript.

(2) In the second paragraph of the introduction, a number of studies are
summarized in which the performance of syndromic surveillance is presented as an
improvement over other systems. A number of comparative statements are made, in
which the comparison is not made clear. For example, “…respiratory syndromes
represent an early indicator of impending influenza morbidity and mortality, sometimes by as much as three weeks” (page 4, second paragraph). Does this mean three weeks in advance of the actual influenza season, in advance of other surveillance methods, or something else? A similar issue exists for the description of the study by Olson et al.

We have edited the text in this paragraph on pages 4 to read: “Brownstein and colleagues [3] show that children and infants presenting to pediatric emergency departments (ED) with respiratory syndromes represent an early indicator of impending influenza morbidity and mortality, sometimes with as much as a three week lead on EDs serving adults [3].” Moreover, we have edited the description of the Olson et al study on page 5 so it now reads “Olson and colleagues note that age-stratified analyses of ED visits for fever and respiratory complaints offer the potential earlier warning of the arrival of epidemic influenza (compared to non-stratified analyses) because they allow for detection of the characteristic age-shift of pandemic influenza [9].” We believe this clarifies the comparisons being made.