Reviewer's report

Title: Incidence and risk factors for influenza-like-illness in the UK: Online surveillance using Flusurvey

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Reviewer: Matthew Biggerstaff

Reviewer's report:

This manuscript describes a UK-based surveillance system that ascertains the presence of Influenza-like illness (ILI) among an internet cohort. This system complements current healthcare-based ILI surveillance. In this report, the authors describe the system, present the ILI incidence results (including multivariate modeling), and discuss some of the findings. I think that the results and the survey are valuable and provide a low-cost mechanism to ascertain ILI among those who may have not sought healthcare and to see how certain demographic characteristics affected the report of ILI. I do have some concerns before I would recommend the paper being published, however.

Discretionary Revisions

*There are a couple of articles you may wish to reference that show similar findings to your analysis also justify how healthcare seeking varies by demographic and other characteristics:


Minor Essential Revisions

*In top graph of Figure 1, the range for the y-axis scale does not cover the range of incidence values reported. Also, the age categories reported on the figure legend are also not mutually exclusive (e.g. 46–65 and 65+). The bottom graph of Figure 1 is small, difficult to read, and the legend overlays the line graphs. It also says the survey ran from November 22, 2012, to April 14, 2013, but the x-axis starts at December 2, 2012.

*The table titles need to be more much more descriptive.

*It would be helpful if the authors included confidence intervals for the proportions presented in the paper.

Major Compulsory Revisions
Overall, the manuscript reads a bit wordy, and I think parts of the introduction, methods, and results can be trimmed. It would improve readability if the authors went through these sections again to see if there are ways to reduce the word count.

The use of two ILI definitions in the document adds to the overall wordiness of the manuscript without adding a lot of useful information since the trends among both definitions are similar. I recommend selecting one definition to present in the results of the manuscript and move the other definition to an appendix (highlighting any major differences in the results section). The selection of the ILIfever definition is a bit confusing since it isn’t the WHO or US CDC definition and doesn’t seem to be a standard definition for ILI. I recommend citing the source of the definition and justifying its use or changing the second ILI definition to match the WHO/CDC definition.

Further clarification and revision of the how the multivariate models are specified is needed. How were the risk factors included in the model selected? Did the authors account for clustering in the household in the model? I also think the model needs to be stratified by age (children versus adults) since multiple variables in the models don’t really make sense for children (e.g. employment status, child contact outside home, smoker [depending on prevalence in children], and underlying medical conditions [again, depending on prevalence in children]). The current models also have categories for both “contact with children” and “live with children”. It might be worth combining those groups together or at making a composite variable. Finally, you mention in the methods that the odds of reporting ILI was adjusted by the number of reports an individual submitted, but I don’t see that variable included in the results tables.

Some of the result section and a lot of the tables provide regional differences in ILI, but to someone not familiar with all the regions of the UK, it doesn’t really seem to be all that meaningful because no further discussion or context is given. I recommend either dropping the regional presentations, moving them to a supplemental section, or providing some discussion as to why the differences might have occurred or why the authors think the regional differences are important.

It might be helpful to note how many of the observations were self-responses versus proxy responses and how that varied by demographic groups. The number of households ascertained over the course of this survey would also be helpful. There are a lot of symptoms asked about each week and proxy respondents may not be as accurate.

I think the results paragraph and the discussion paragraph that revolve around the effect of school closure on incidence of ILI need to be either dropped or significantly cut. This is a very complex issue, and I don’t think the authors have supported their conclusions. I also don’t feel like this section fits in with the stated objective of the study. If the authors want to keep these results, then I think more a more rigorous statistical analysis is needed (and perhaps the inclusion of more years of data, showing the same trends). This article by Cauchemez et al. may...
provide a good example: http://www.ncbi.nlm.nih.gov/pubmed/18401408.

*I think the discussion section needs to be expanded and the paper’s results put into broader context. The introduction of this paper presents this system as an alternative to healthcare surveillance or at least a way to achieve a sample of persons with ILI that is unbiased by healthcare seeking. However, some of the findings in the discussion section do not relate back to this stated objective, and the authors currently do not devote much space to placing their findings into context, making it difficult for the reader to know how the finding from this paper compare and contrast with other research. For example, the authors only devote one line to discussing the finding that receiving a vaccine was associated with lower reports of ILI. This is a large issue and warrants a longer discussion. I also think it would be helpful if the authors placed the findings of this paper into context with the other years of this surveillance, among the other countries in the Europe-wide system, and the U.S. internet cohort (FluNearYou) and compared their findings to other systems that have attempted to measure ILI outside of the healthcare system.

*Some discussion for the reader on the sensitivity and specificity of ILI during an influenza season is needed since the results are presented for the entire season and weekly. This will help place the burden of influenza vs ILI into context (readers without much knowledge in this area may assume all ILI is true influenza infection).

*I am a little confused why the paper is focusing on the use of public transport as a risk factor for ILI. Has this been shown before or was it a major concern at some point? This needs some context to help the reader understand why it is mentioned.

*I think the overall response rate and the impact of the representativeness of the respondents need to be addressed in the limitations section and a better understanding of the weekly response rate is needed. The authors state that respondents could report from November 22 to April 14, which is around 20 weeks, but the mean and median number of weeks reported in this study was 6 and 8.5. Therefore, it doesn’t appear like respondents consistently reported using this mechanism. Therefore, it would be helpful to see the % of respondents reporting by week plotted out (maybe as a supplemental table or figure) and to analyze the characteristics of participants who were dropped for only reporting one week versus those who were kept in the analysis. I also think there needs to be some discussion on the impact of dropping the first symptom report in the incidence analyses. First, there needs to be some justification for doing this (were the first responses in fact always ill responses or did it track with the overall incidence of ILI among your respondents who had reported >1 time). Second, I think it would be helpful to mention how dropping these responses affected your overall incidence calculations since dropping those reports will likely have an impact on your weekly numbers.

*The third paragraph of the limitations section (risk factor analysis restricted by . . . ) is a bit confusing to read. I am not sure how the omission of questions from
the survey or the wording of questions explains the odd findings of the model, which may be more related to the specification of the model. Please clarify this paragraph.

*The concluding paragraph at the end of the discussion doesn’t seem to match the main focuses of the discussion section. Some of the issues brought up in the concluding paragraph would be useful to mention in the discussion section, but at this point, the discussion section and the concluding paragraph cover the same topics.

*Table 4 doesn’t really seem to fit with the overall stated objectives of the paper. I would drop since this system was not designed to monitor for vaccine uptake.

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

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.