Author's response to reviews

Title: Internet-based surveillance of Influenza-like-illness in the UK during the 2009 H1N1 influenza pandemic

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Version: 3 Date: 16 September 2010

Author's response to reviews: see over
Author’s covering letter for re-submission

Dear Dr Chap,

Please find enclosed our revised manuscript *Internet-based surveillance of Influenza-like-illness in the UK during the 2009 H1N1 influenza pandemic*, and below a point-by-point response addressing the concerns of the referees. We would like to thank the referees for their comments and suggestions; following these we have made a number of changes to the manuscript (details below). We feel that we have covered all of the points raised, and look forward to hearing your decision.

Yours sincerely,

Natasha Tilston

**Title:**

Internet-based surveillance of Influenza-like-illness in the UK during the 2009 H1N1 influenza pandemic

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**Version:** 2  **Date:** 16th September 2010
MAJOR COMPULSORY REVISIONS

1. Why only mentioning advantages of internet based data collection and not a major disadvantage namely the lack of validity of the data (especially if one is interested in the specific type of flu virus)

The paper mentions in some detail the disadvantages of internet based data collection (for instance on P 5 “However, it is not clear exactly what is being measured by the internet-based surveillance systems, as the sample may not be representative and it is not clear how to calculate the appropriate denominator (see later).” And in the abstract “However, the requirement for participants to have internet access and to actively participate calls into question the representativeness of the data.”. However we had not discussed the validation of the data, this has now been added to the text, please see Discussion, P.15.

2. One would expect that the authors would focus a) on the gap between the number of GP-based reported cases and the ‘true’ number of cases in the population and b) on the better timeliness of the internet information. In the end one could state whether the costs of this internet based flu-survey operation (staff, recruitment, data collection and processing) would outweigh the benefits, by arguing that either the current GP based information service could be replaced by the internet survey or that the additional information collected by the internet survey is so valuable that it should be continued.

(a) There is no ‘true’ number of cases. In England, the HPA estimates the weekly number of cases. However, comparison of the estimates to serology and modelling data suggest that these ‘true’ estimates were underrepresented by a factor of 10. Nevertheless the trend in these numbers over the course of the epidemic reflects the relative incidence rather well (Donaldson et al. 2009,
We therefore compared our estimates of incidences using the different measures, to these numbers to test how well Internet-based surveillance works.

(b) These internet-based surveillance systems are not intended to be a replacement to traditional systems, but rather a useful adjunct. The cost of running this system in 2009 included set-up costs to develop the website and test it (~ 12 weeks full time equivalent), and a further 1 hour per week for website and database maintenance, salary for three staff who had to send weekly reminders (~3 hours each per week), update the webpage (couple of hours every morning) and analyse the data. The costs are therefore relatively low, in comparison to many GP-based systems. We have now mentioned the relatively low cost in the discussion on page 15 “as they are a relatively low-cost method of collecting ILI data, and allow access to cases that do not necessarily seek formal care.”

3. The paper does not focus on this population/GP attendance gap (I’d very much appreciate if it would) and the internet data do not seem to be more timely than the GP data. Finally the validity problem (the big disadvantage of questionnaire based data) is not touched at all; if a policy making authority is interested in the type of flu virus circulating (as in case of a pandemic); the internet survey data are far too general.

The aim of the paper is to test how to measure incidence most accurately using an Internet-based survey, please see Background, P.5 and the abstract “aim of assessing the reliability of the data, and to evaluate methods to correct for possible biases”. We have now discussed validation methods; please see highlighted text (Discussion, P.15).

4. My conclusion from the paper is that the paper does not succeed in proving what the value of internet based surveys is in this case (pandemic flu surveillance) and certainly not what it adds to the existing data collection structure.
The Paper does not set out to prove that Internet-based surveillance is better than GP based surveillance, but rather to test how well it performs when compared to these traditional methods of surveillance so it may serve as a useful adjunct. Please see Discussion and Conclusions, P.16.

5. I also propose that intermediate ways of data collection (eg via NHS Direct in the UK) are included in the discussion.

A comparison of the flusurvey data with the NHS Direct Data cannot be made as the NHS Direct changed their system for data collection between the 4th July 2009 and 4th October 2009 due to increased call volumes (that is, the system was overwhelmed by demand which led to a failure to collect data consistently over the course of the epidemic). See http://www.hpa.org.uk/web/HPAwebFile/HPAweb_C/1266228133118 and an example of this effect in the figure below for “cough” and “fever” calls (the figure is taken from the above report).

MINOR ESSENTIAL REVISIONS
1. Some typical UK features (like what is the Pandemic Flu Services and how does that influence the Flu reports) should be explained for a non-UK audience.

   An explanation of the National Pandemic Flu Service (NPFS) has been added to the text, P. 11 (comparator estimates of incidence).

2. Please explain in more detail how the case assessment numbers of HPA are being calculated; it is a crucial element in the paper (it serves as a sort of gold standard) but it is not explained

   This has been explained; please see highlighted text, P. 11 (comparator estimates of incidence).

3. I miss references to the use of NHS Direct data

   See comment above.

4. ref 18 is strange (a handful of initials)

   Apologies; this has now been corrected.
MAJOR COMPULSORY REVISIONS

1. The system was initiated during the 2009 pandemic and children (and seniors) are underrepresented. Incidence rates during the pandemic were dramatically skewed towards children. Is this a problem and is it possible that the system would do better in a typical seasonal epidemic where the age distribution of cases is perhaps less skewed? Also, recruitment of participants may have been relatively easy in 2009 given the media attention and public interest generated by the pandemic, but it may be different in the case of seasonal influenza epidemic. It would be useful to discuss how this system could be expanded to monitor seasonal influenza.

We have commented on this in some detail, please see highlighted text (Discussion, P. 15). There are potentially differences in recruitment, although we don’t want to speculate on what they may be.

2. The authors enrolled 5,000 participants to participate in this survey. What was this number based on? Is there anything like a sample size calculation for internet-based surveillance systems?

We do not have a target sample size. There is no practical limit to the number of participants who could take part in the survey, and no cost to expanding the size of the survey. We did not perform a sample size calculation, and to our knowledge there is no sample size calculation for internet-based surveillance systems.

3. In the censored internet sample (the most reliable), 2,369 participants completed 17,532 questionnaires, resulting in about 7.3 reports per participant over a 24-week study period. This suggests that participants do
not report symptoms on a weekly basis as instructed, and presumably they are more inclined to return their questionnaire when they are sick (or one of their contacts is). It would be interesting to plot the weekly number of questionnaires returned, overlayed with the weekly number of ILI reports (unweighted), to gauge whether reporting is strongly linked with disease. Also, a histogram displaying the distribution of number of reports per participant would be nice.

The issues of motivation for reporting was covered in some detail in the analysis, investigating the effect of different assumptions. As noted, participants are likely to be more motivated to take part when sick. We have included the suggested figures, please see Figure 4 and Figure 6 (Results, P.12).

4. Can the authors provide an estimate of reinfection rates in their system (=2 or more reports of ILI between July and December 2009)?

Yes. We’ve added them to the text, please see Results, P.13.

MINOR ESSENTIAL REVISIONS

1. Abstract/results: would be nice to include a quantitative measure supporting the agreement between the internet system and the HPA estimates (e.g., the correlation measures)

   A correlation measure with its P-value and confidence intervals has now been added to the Abstract/results (P.2, line 15).

2. Last sentence of intro: RCGP not defined
We have re-worded the last paragraph of the introduction. Please see highlighted text for RCGP explanation (P11, comparator estimates of incidence).

3. Rapid onset of fever (top of p.7): how was this defined?

Rapid onset of fever was defined as self-reported rapid onset (the wording of the question was “did your fever come on quickly?”).

4. The internet surveillance system was initiated in July 2009 and caught only half of the pandemic summer wave in the UK. Is this taken into account in the estimates of attack rates presented in Fig 6?

Yes, this has been taken into account, please see highlighted text (Results, P.13).

5. Add P-values to correlation coefficients presented in results

P-values and confidence intervals have now been added to the correlation values (P.12).

DISCRETIONARY REVISIONS

1. It would be interesting to compare the attack rate estimates generated by internet system with those of serosurveys conducted in the UK after the first wave (Miller et al, Lancet 2010). It would be also interesting to compare the merits of internet-based and telephone-based disease surveillance during the 2009 pandemic (eg, telephone surveys in NYC during the spring wave).

A comparison of the flusurvey attack rates with those of serosurveys (Miller et al. 2010, Lancet) cannot be made since the Flusurvey did not capture the first wave of the pandemic. We have however, compared our attack rates with that
produced in Baguelin et al, 2010, Vaccine paper, which closely follows that of Miller et al. 2010, Lancet.

2. I understand where the authors are going with the SIS and SIR concepts p.11; however, traditional influenza GP surveillance systems consider that influenza is a SIS infection (ie, such systems use a denominator based on the number of GPs and their catchment population, and tally the number of ILI cases for a given period of interest)... In the interest of comparing with GP-based or HPA estimates, isn’t it best to use the SIS definition?

Yes, we have used the SIS definition when comparing with RCGP and HPA case estimates; please see highlighted text (P.11, Defining a denominator for incidence estimation). The point that was being made is that there are different ways incidence can be calculated from this kind of data, and it is unclear as to which would be the most appropriate.