Reviewer’s report

Title: Avoidance Behaviors and Negative Psychological Responses in the General Population in the Initial Stage of the H1N1 Epidemic in Hong Kong

Version: 2 Date: 8 October 2009

Reviewer: James Rubin

Reviewer’s report:

Thank you for sending me this fascinating article to review. The team responsible for it have previously published some very interesting and useful papers on related issues, and this adds nicely to the weight of evidence that they have presented regarding public reactions to emerging infectious diseases. That said, I do have a few suggestions which I hope may be useful in improving the manuscript.

Discretionary Revisions

1. The measures of distress that were used seem a little unconventional, although I note that they have been used successfully in several of the authors’ previous studies. Perhaps some of the unusualness is due to the translation of the items for the sake of this paper? For example, it seems a little odd to ask someone if they are feeling “emotionally disturbed,” or “panicking very much.” Do the authors have any data to show how these items perform when compared with more conventional questionnaires that are used to measures psychological distress? Are there any data about their psychometric properties that could be used to support their use? If not, I do not see this as a major weakness, but it might be something worth mentioning in the discussion [and possibly addressing in any future research].

A more important concern though, is the use of these items to categorise someone as not just ‘distressed’ but also as showing “signs of severe mental distress” (p9, first para & p11 second para) or “severe emotional distress” (p9, second para, and tables). If these items have not been validated against any previously validated scale, or against clinical assessments, then this statement seems a bit of a stretch. Perhaps it would be more in keeping with the actual questions used just to say “high levels of distress.”

2. The authors have pooled data together from three separate surveys spanning one month in order to conduct their analyses. During this time, I am sure there were many developments in the unfolding swine flu outbreak in Hong Kong, and in the way the Government and the media reacted to it. Are the authors confident that the associations between their independent and dependent variables did not change over time, from one survey to the next? They very possible did not, but it would be reassuring to have this checked and a short statement added to the text to confirm this (if true).
3. I was surprised that 63.4% of the population reported avoiding visiting hospitals, and that 63.3% were avoiding travelling abroad. How exactly were these questions phrased? Presumably these were behavioural intentions, rather than actual behaviours? For example, I might say that I am currently avoiding hospitals – but this has nothing to do with swine flu, it is because I am not ill and do not have anyone in hospital who I need to visit. Equally, I am not going to be travelling abroad in the next few months, so it is hard to say if I am ‘avoiding travelling abroad.’

4. From my reading of the literature, and again the authors will know this research better than I do, one interesting aspect of the SARS outbreak was that use of hospitals fell because people were concerned about contracting the illness there. This may have had an impact on their health. It might be useful to work this point into the discussion, to suggest that avoidance has impacts that are not just economic?

5. The authors suggest (p11) that a vicious circle may operate, with avoidance of going out resulting in increased distress. I am not so sure – would it not result in decreased distress as people have taken effective action to reduce their likelihood of contracting the disease? Can the authors provide a supporting reference for their vicious circle argument?

6. The authors suggest that they expect the prevalence of negative psychological responses to increase over the coming months as the number of cases rises. However, could the case not also be made that press coverage showing that the disease is actually relatively mild will assuage public concerns? Obviously only time (and more surveys!) will tell.

Minor essential revisions

1. The authors say (e.g. abstract and 2nd para of intro) that “panic” was prevalent in the Hong Kong community during the SARS outbreak. My understanding is that panic represents irrational, selfish behaviour that disobeys pre-existing social norms (see, e.g., Sheppard et al Terrorism and dispelling the myth of a panic prone public. J Pub Health Policy 2006;27:219-245). The authors are better informed than I am about how the Hong Kong public reacted during the SARS outbreak, but are they sure that ‘panic’ is a technically accurate term? For example, was avoidance of public areas really an irrational response to SARS that contradicted social norms? Panic seems a strong term to use in this context, and I am unconvinced if it is justified.

I note that the authors did use the phrase “panicking very much” as one of the questions in their interview. However, our experience in the UK is that people interpret the word “panic” to mean just “fear.” As such, even if people say they are panicking, it doesn’t necessarily mean that they are behaving irrationally.

2. I would suggest that authors are more explicit in the manuscript that descriptive data relating to the first 550 participants has already been published.
in the Journal of Infection. I am quite content that this does not constitute
duplicate publication – it seems entirely justified in the circumstances for the
authors to have put their first data into the public domain as quickly as possible
and to be presenting a more in-depth analysis (as in this manuscript) at a later
date. However, I do think that it should be made clearer to readers what the link
is between the J Infection paper and the current paper.

3. The sampling strategy (taking random telephone numbers from the telephone
directory) would be seen as relatively low quality if performed in the UK, because
a large proportion of UK residents choose to be omitted from the directory – and
those who make this choice differ from those who do not on several key
socio-demographic (and presumably psychological) characteristics. I am not sure
if this applies in Hong Kong, but UK readers, at least, will assume that it does.
Could the authors clarify what percentage of the Hong Kong population are listed
in the directory? I appreciate that 95% have a fixed line phone line, but are all of
them in the phone book?

4. I am intrigued by the response rate calculation. The authors say that 1,345
eligible respondents were identified and 999 completed the interview, giving a
response rate of 74.3%. This seems remarkably high for a telephone survey.
Calculating response rates for telephone surveys is much more complex than it
appears (see e.g. Galea & Tracy, Participation rates in epidemiologic studies.
Ann Epidemiol 2007;17:643-653.) Can the authors provide a little more
information in this section? In particular, how many phone numbers in total were
called? How many attempts were made to contact each number? How many
didn’t result in a conversation with someone at the other end? Of those which did
result in a conversation, how many people were ineligible? Of the 1345 eligible
respondents who were identified, why did 346 people not go on to complete the
interview. Adding this information may make the response rate seem lower, but
that would not necessarily imply a problem with the sampling.

5. The authors state (p11) that “avoidance behaviours were more likely to be
psychological reactions associated with anxiety due to H1N1.” Again, I am not
sure if the authors’ terminology is strictly accurate here. After-all, they controlled
for anxiety in their multivariate analyses (well, distress at least) and yet the
associations with e.g. gender, age etc remain despite this. So saying that these
associations are mediated by anxiety isn’t actually supported by the data. It could
be that they are mediated by other psychological variables however – e.g.
perceived impact, perceived ability to cope etc etc. For example, research by our
team (Rubin et al BMJ 2009;339:b2651) also identified associations between
perceptions of swine flu and behavioural reactions, even when we controlled for
anxiety about swine flu.

6. p11 – the authors discuss “a few international studies documenting strong
levels of anticipatory anxiety” at the early phase of human avian flu. However,
several studies have now released results which show the actual responses of
various international populations in terms of behaviour, perceptions, anxiety and
the associations between them. I do feel that these studies would provide a much
more concrete comparison for the author’s study, both in this paragraph and
throughout the rest of the discussion (e.g. in preference to SARS or H5N1 studies when discussing the role of age (p12)). Examples include our own work showing the link between perceptions and behaviour in the UK (Rubin et al BMJ 2009;339:b2651), work by Robert Blendon in the US examining public perceptions of the outbreak (see www.hsph.harvard.edu/news/h1n1/ for a summary of their results, which as far as I know have not yet been formally published in the peer reviewed literature), and by Sandra Quinn, also in the US, also looking at perceptions of the outbreak (Quinn et al Biosecurity and Bioterrorism, 2009:7:3 fast track paper on-line doi:10.1089/bsp.2009.0041). Other studies which specifically look at public reactions to H1N1 are also on-going and may have been published by the time the authors come to revise their manuscript; these might also be worked in. The authors themselves note that “similar data will be obtained from other countries and can be compared with ours:” the data are indeed already available, and there is nothing to stop the comparison being made!

7. I would recommend a read-through to make sure the English is ok (e.g. ensuring that tenses agree) – it isn’t too bad, but it does need a bit of polishing up before publication.

**Level of interest:** An article of importance in its field

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

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

**Declaration of competing interests:**

The only potential competing interest I can see is that I have suggested that the authors cite one of my own papers.

Other than that, I declare that I have no competing interests.