Author's response to reviews

Title: The dynamics of risk perceptions and precautionary behavior in response to 2009 (H1N1) pandemic influenza

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Version: 2 Date: 13 August 2010

Author's response to reviews: see over
August 12 2010

Dear Editor,

Thank you for your review of our manuscript (MS # 8478953634013035) “The dynamics of risk perceptions and precautionary behavior in response to 2009(H1N1) pandemic influenza”. We have read your comments and those of the reviewers and have revised the manuscript. Below, we outline our responses to these comments. We hope we have addressed the reviewers’ concerns adequately. We appreciate the thoughtful comments and we look forward to your response.

Editor’s comments

In addition, please can you state whether your research conformed to the Helsinki Declaration (http://www.wma.net/e/policy/b3.htm), and to local legislation. Please add a paragraph in your methods section stating this, and name the ethics committee which approved the research. Please also indicate whether patients gave informed consent to participate in the study.

Thank you for pointing this out. In methods section, we declared that our research conformed to the Helsinki Declaration and approved by Rutgers University Internal Review Board, and that patients gave informed consent to participate in this study.

Reviewer 1’s comments

General comments
This study presents interesting data and should have been published earlier to guide actions in the second wave of A(H1N1) pandemic. However, due to the limited number of questions on complex concepts like risk perception and the absence of a theoretical model, the interest of this study in a post-pandemic context is limited.

Major Compulsory Revisions
1. What I have found to be the main flaw of this study are the scales and dichotomus scores used. I am really disappointed that willingness to engage in preventive behaviors (vaccination and antivirals as prevention) were amalgamed with intention to engage in curative behaviors (antivirals medications to cure). Those two behaviors are not the same and psychosocial predictors of willingness to engage in preventive vs curative behaviors are known to be different. The dichtomus score representing engagement in precautionnary behaviors is also misleading. The search for information in the medias cannot be consider at the same level that cancelling trips or quarantine measures which have more consequences in the life of individuals. The absence of a theoretical model se fait cruellement sentir à ce niveau. Cronbach scores cannot replace a solid reflexion on the underlying the questions. I would suggest to construct two scales for the willingness to take pharmaceutical interventions (preventive interventions versus curative interventions) and two scales for the engagement in precautionnary activities (information seeking vs “quarantine” measures). Unfortunately, this would implie major changes in the method, results and discussion sections of the manuscript.
We originally chose to use three combined scales for perceived risk likelihood, willingness to take pharmaceutical interventions, and engagement in precautionary activities, because we thought that would help streamline the paper to focus on dynamics of risk perceptions and precautionary behaviors. However, we have no objections to the reviewer’s comments, and have followed her suggestion. In the revised version, pharmaceutical inventions were grouped into two scales (preventive vs. curative intervention), and precautionary activities were also grouped into two scales (information seeking vs. taking quarantine measures).

Accordingly, the analysis has been changed as the reviewer suggested. However, we observed the same qualitative trends in temporal and geographical dynamics as before in many of the scales. For example, we have found declines over time in willingness to accept preventive pharmaceutical intervention and engagement in information activities. Interestingly, this decline was not apparent for willingness to accept curative intervention or in engagement in quarantine precautionary measures (See page 10).

2. Another main irritant of the manuscript is the parallel with media attention. Analysis of the media attention presented in the method section is only about news stories in the newspapers. Few indication are given about the exhaustivity of the logiciel used (are all newspapers in the US recensed in this sofware?, Were the online version included? Was flu the only keywords used? More information should be given to better understand this part of the analysis.). Furthermore, at one point, it is stated that most individuals learned about swine flu on TV (65.9%) and then all comparisions between results of the study and media attention are based upon news stories in the newspaper. Whitout adding references showing that the decline in news stories in the newspapers were parallel with the decline of general attention toward swine flu or the pandemic in all medias (radio, television), results must be presented with precaution and the limits of this analysis should be stated. Correlations between results of the study and decrease in media attention, as presented in the discussion section, should be presented in the lights of these limitations.

LexisNexis is a comprehensive database of US national and regional news media, and our search was conducted in US newspapers and wires, which includes approximately 700 media sources. The search results were classified into several subcategories such as newspaper articles, newswires and press releases, and other categories. It turned out that some news seemed counted in multiple categories, and hence we counted only the number of newspaper articles to avoid double-counting. “flu” was used as the single search term, as it includes both “influenza” and “flu”.

Responding to this comment, more details of the search process have been added in the manuscript (see page 8). In addition, we have shown a correlation in the number of articles between newspaper articles and TV/radio transcripts ($r=0.91$) using the same LexisNexis news search to support our argument of overall decrease in media attention.

3. The affirmation in the paragraph on the implications of survey results for successful responses to a novel influenza outbreak in the discussion section are not supported by results of this study. For example, it is said that acceptance of H1N1 vaccine is likely to be higher among individuals
who have received prior flu vaccines and no information on the past vaccination behavior of the survey respondents were presented.

We apologize for the confusion. For the part of “prior influenza vaccines”, we intended to mention results from earlier studies which found that acceptance of seasonal influenza vaccination is strongly associated with previous vaccine acceptance. We have written the sentences to make the context clearer to avoid readers’ confusion.

4. Table 1 is not adding any new information to the text. This table should be replaced by Additionnal File 1, which is more illustrative.

We have moved Table 1 to Additional File1.

5. Table 4 is not easy to read and the use of t-test to compare dichotomous variable is inappropriate. This table should be re-think and the presentation must be revised.

We have rearranged Table 3 (the previous Table 4) and moved some of the columns to the text. We hope this change helps better understanding. As for the comment about the t-test, we were not sure which variable/measure the reviewer was referring to. The analyses reported in this table use t-test for comparison of continuous variables and chi-square test for comparison of dichotomous variables (i.e. proportions). The current version of the table now reports only correlation coefficients, to avoid confusion.

Minor essential revisions
1. Questions included in the score on perceived likelihood are not pertained on the perceived likelihood that the respondent “himself” will contract the swine flu, which is the determinant of engagement in precautionnary behaviors (see Brewer, already cited in the manuscript). When discussing about the fact that this concept is not related with precautionary behavior, this flaw should be acknowledge.

The first sentence is true. In point 1 under Reviewer 2 below, we explain why we chose to use these somewhat uncommon questions for our analysis. In response to the reviewer’s comment that “this concept is not related with precautionary behavior” we point out that in the regressions to examine predictors of the willingness to take pharmaceutical interventions, the perceived likelihood scale was indeed a significant predictor. I guessed that Reviewer 1 here talked about different pattern in dynamics of risk perceptions and precautionary behavior. When time was controlled, perceived likelihood scale and the willingness to take pharmaceutical interventions was found to be correlated.

2. More information on the recruitment and the limit of web-base survey should be presented. Was the survey firm having a list of individuals that had already consented to receive questionnaires? What is the representativity of their list? Web-based surveys have many limitations that were not presented in the paper. More information on the sociodemographic characteristics of the respondents and the non-respondents should be discussed in the manuscript. The problem of representativity inherent to this method for data collection should be clearly stated.
To respond to the questions the reviewer raised, the survey company had a list of individuals who had consented to receive invitations for surveys, although they were free to decline any invitation they received. This cohort of potential invitees is thus made up of volunteers who may not be nationally representative. The survey company knew the demographic information about each potential participant and issued invitations in such a way that our study sample would have an age and gender distribution that was representative of the adult US population. These details about survey collection have been added in the methods section.

We have also added limitations of our web-based survey that they may have potential bias in respondents in addition to small sample size and self-selection bias that we had discussed in the original manuscript. Analysis on age-sex distribution of non-respondents was provided (see page 5) given information we could obtain from the survey company.

3. The low response rate should be clearly indicated as major limitation of this study.

We have stated it as a limitation in the discussion. Thank you for pointing this out.

4. Beside household size, age and sex, revenue and educational level are found, in many studies, to be predictors of precautionary behaviors. No data about this are presented in the manuscript, this is a limit that should be stated.

We have stated it as a limitation in the discussion. Thank you for pointing this out.

5. In the Table 1, references about the source of data about H1N1 cases should be added.

Presumably this comment is actually about former Figure 1 as former Table 1 was about the number of respondents on each survey day and no information on H1N1 cases was included. We have added the source of H1N1 cases and number of articles in the figure legends.

Discretionary Revisions
1. I would have like to have another reference (maybe a more theoretical oriented paper) to support the idea that individual risk perceptions are important in the control of infectious diseases.

Risk likelihood and risk severity have long been viewed as drivers of health behavior (not just control of infectious diseases) going all the way back to the Health Belief Model and Expected Utility Theory. And of course the Brewer et al (2007), meta-analysis clearly shows that perceived risk a consistent predictor of vaccination behavior.

In addition, we have now cited the following references.


2. The references of the work of Brewer (3-4) are directly related to the Health Belief Model, however this is completeley evacuated of this article. References to less theory-driven papers would have been preferable. More information should be given on the relation between risk perceptions and subsequent behaviors.

We now explicitly reference the Health Belief Model as the original of the central idea that risk perception is an key driver of health behavior (page 3).

3. The sentence on constance of predictors of precautionary behaviors over time should be support by adequate references. Works in the psychosocial fields are mostly supporting the opposite...

We have softened this claim. The point we wish to make is simply that if perceived risk predicts behavior, and perceived risk changes over time, then one might think that behavior would also change over time.

4. I was surprise not to have found any references to the work of Ritvo, who is a leader in the field of risk perception. Indeed, a clear definition of risk perception and its use in this study should be found in the background.

Thank you for the suggestion. We have incorporated Ritvo’s extensive work on vaccines and risk perception. Also we added a sentence to define our risk perception in the background.


5. In the results section, an indication should be given when data cannot be found in the Tables or Figures.

We inserted statistics in text wherever they are not found in Tables so as to avoid redundancy.

Reviewer2's comments

I have reviewed the paper entitled "The Dynamics of Risk Perceptions and Precautionary Behavior in Response to 2009 (H1N1) Pandemic Influenza." This interesting paper addresses an important public health issue which is the distribution and etiology of precautionary behaviors in response to an emerging health threat, by using national survey data. From my viewpoint, the paper should elicit interest from the readers of BMC ID, as well as public health officials and scholars. However, I would not recommend the publication without a few modifications. At this stage, the paper should be slightly strengthened to achieve the standards for publication in
high-ranking academic journal. I think that the following comments may be useful to the authors to revise the manuscript for resubmission.

General comment:
From my viewpoint, a little number of problems associated with both conceptual and methodological issues can be identified. First of all, it seems to me that the authors tend to neglect some important pieces of the existing literature on the conceptualization and measure of risk perceptions. There are a large number of empirical or theoretical papers of importance for this topic that have been mentioned but not seriously considered by the authors. For example, risk perceptions is generally constructed in most social and cognitive models of health behavior as the combination of the perceived likelihood of becoming ill and the perceived severity of the illness. In their questionnaire, the authors introduced a very unusual item to measure the perceived severity of illness, which do not allows them to appraise the perceived severity, and therefore the perceived risk, appropriately. The results of this study needs to be partly reconsidered, or at least discussed, in light of a long line of conceptual works on risk perceptions and behaviors that may explain some apparently paradoxical results.

This is true. Risk perception is often conceptualized as risk likelihood and risk severity. Notably, the Health Belief Model conceptualizes it that way. The Brewer et al. meta-analysis shows that both risk likelihood and risk severity predict vaccination behavior.

When we conducted our survey, H1N1 was very new. There had been so few cases at the time that the likelihood that any one person (say, one of our participants) would get infected was quite low. People often have difficulty estimating very low likelihoods. That’s why we asked risk likelihood questions not about “likelihood that you will get infected with H1N1” but rather “likelihood that H1N1 will reach your community”.

The risk severity of H1N1 that was worrisome early in the outbreak was the high mortality rate. Past research on seasonal influenza by some of the authors of the current paper used items such as “If you got the flu, how severe would it be?” However, in the current study, the primary severity concern was presumably mortality. Thus, our risk severity question asked about the expected worldwide death toll.

In previous literature, risk severity questions have been quite variable. Consider the risk severity items included in the Brewer et al meta-analysis. They list examples including “influenza can cause death” (Nichol et al., 1992) and “if I had influenza, I would not be able to manage daily activities” (Zimmerman et al., 2003). Our risk severity item seems to be within the range of content present in the literature.

Thus, our study used both risk likelihood and risk severity items. These items are a bit different from risk items used in some previous studies, but designed to be apropos to the novel outbreak in its early stages.

Second, one of the scales combines both dichotomous and continuous variables. However, as much I know/remember statistics, this is technically incorrect. Unless the authors could provide supportive statistical references indicating that it makes sense to sum up the responses to these
two types of variables and use the sums as coherent and distinct variables, an alternative approach to measure “pharmaceutical intervention” would be needed.

We are not sure that there is a problem in combining dichotomous and continuous variables so long as they are ordered variables and standardized before combining. Our dichotomous variables were created based on yes/no question, which is a categorical variable but is also considered as an ordered variable as it shows the degree of interest. However, in the revised version, following the comment from the reviewer, we have conducted our analysis based on two separated measures; dichotomous variables to indicate if they respondents showed interest in accepting pharmaceutical intervention, and scale variable to indicate the willingness to pay to receive intervention. Combining Reviewer 1’s comment of separating pharmaceutical intervention into preventive and curative interventions, we now have a total of four measures for the willingness to accept pharmaceutical intervention.

**Major Compulsory Revisions:**

L.5-7, p. 6: the pharmaceutical intervention scale combines both categorical and ordered variables, which seems to me either technically incorrect or, at least, very unusual in psychometrical practices. Can justify this construction from a formal statistical perspective?

We have separated out the scales into dichotomous and continuous variables as explained earlier.

**Minor Essential Revisions:**

L. 24, p. 4: How were study participants contacted? By phone? By email?

It was by email. We have added that in the manuscript.

L.12-13, p. 6: why did not you collect any socioeconomic information about the participants such as education, occupation, race or income? They are generally considered variables of interest in health behavior research.

We have mentioned this as a limitation.

L.10-15, p. 10:’’ Significant predictors for scores on the pharmaceutical intervention scale included perceived risk. […] Sex was not a significant predictor, suggesting that risk perceptions mediate the relationship between the willingness to receive pharmaceutical interventions and sex’. Do you mean “perceived likelihood”?

Thanks for catching this. We have replaced risk perceptions by perceived likelihood.

L.6, p. 11: “Perceptions about the risks associated with 2009 (H1N1) pandemic influenza (…) showed changes over time”. From my viewpoint, the nature and amplitude of this temporal change remains unclear.

We rephrased the sentence in the way that there would be no misunderstanding for readers.
Figure 3: “Proportion of Rs engaged”. What do you mean by Rs?

Rs stood for “respondents”. We have replaced the word by “respondents” in the new figures.

**Discretionary Revisions:**

L. 9-14, p 4: I would also recommend to the authors to read and/or mention the following supportive papers:


*Lau JT, Griffiths S, Choi KC, Tsui HY. Avoidance behaviors and negative psychological responses in the general population in the initial stage of the H1N1 pandemic in Hong Kong. BMC Infect Dis. 2010 May 28;10:139.*


Thank you very much for these useful references. We have cited them in the manuscript.

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Thank you for your time for reviewing and considering this manuscript for publication in *BMC infectious diseases*. We look forward to your review.

We appreciate your careful evaluation of our work and hope that this revision meets with your approval.

Sincerely yours,

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