Author's response to reviews

Title: Perceived risk, anxiety, and behavioural responses of the general public during the early phase of the Influenza A (H1N1) pandemic in the Netherlands: results of 3 consecutive online surveys.

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Author's response to reviews: see over
Rotterdam, November 2010.

Dear Ms Norton,

We would thank the reviewers for assessing our revised manuscript entitled ‘Perceived risk, anxiety, and behavioural responses of the general public during the early phase of the Influenza A (H1N1) pandemic in the Netherlands: results of 3 consecutive online surveys’. The reviewers were very positive and concluded that, the revised version has been significantly improved. However, as mentioned by the associate editor, there are a few minor points that must be addressed before our paper can be accepted for publication in BMC Public Health.

In this letter we give a clear point-by-point response to the comment of the reviewer. In the second revised manuscript you will find the changes we made as a result of these comments.

If you have any further questions, please don’t hesitate to contact me.

Your sincerely,

On behalf of all co-authors,

Marloes Bults.
Comment reviewer 1:

Discussion

1. **Reviewer original Q.** Has there been any test retest reliability of the 1-5 scale?

**Reviewer request:** Test-retest of behavioural scales is important given the scale is limited (1-5) and a change from 5 to 4 may have a greater influence that a change from 15 to 14 on a scale of 1-15 and that human’s response to items relating to attitudes, perceptions etc may not be stable. Given you have already undertaken the survey in a pandemic situation please make note of the lack of test-retest as a potential limitation in your Discussion.

*In the discussion we now describe that: ‘The validity of the questionnaire used in this study was not tested through a test-retest design, because the Influenza pandemic was ongoing and thus perceptions were not stable over time’.*

Methods

2. **Reviewer original Q.** Mention the number of steps from the first to the final model.

**Reviewer request:** You have used a backward deletion model. Like the other two alternatives (forward and stepwise) there are controversies with this approach. A model should improve over the null model and giving the number of steps to identify the final model the readers can consider: the incremental improvement from initial model to final model; downward bias in the p-values and SE of the final step. The readers should be given sufficient information to evaluate the final model and usually includes statistical test of individual predictors (including SE and p-values), a goodness of fit statistic and validity of the predicted probabilities. Please replace “taken out” with ‘removed’.

*As suggested by the reviewer we now give statistical test of the individual predictors (including SE and p-values) and a goodness of fit statistic ($R^2$). In the revised version, ‘taken out’ has been replaced with ‘removed’.*

3. **Reviewer original Q.** The development of several composite scales is usual practice and Table 2 explains each item for each construct. However, once you make a decision to use a composite scale an examination of the individual items (as in Table 2) undervalues the construct. I would move Table 2 to an appendix with the Cronbach alpha for survey 1 and remove all analysis between the survey periods.

**Reviewer request.** Behaviours (such as intention to comply with advise) are complex requiring theoretical framework to predict which independent variable(s) are most significant in intention to comply. A central tenet to applying a theoretical framework is the premise that individuals behave (e.g. intend) after a complex inter relationship between components of the model has occurred. Presenting individual items (Table 2) ignores this underlying tenet and assumes each independent variable is important in isolation. Yet, we know variables (composite scores) will fall out of the model because these are not significant influences on intention in the presence of others. A such, Table 2 provides no insight into complex cognition unlike a presentation in a table of the 3 models for intention for each period (with Beta, SE, p-values).
**Tabel 2** does not describe influences on intention. It describes frequencies of variables which are necessary to give a clear answer on the first objective of this study, namely ‘to identify trends over time in risk perception, feelings of anxiety, and behavioural responses (survey 1-3). The p-values described in table 2 do not reflect univariate analyses but they describe time trends in perceptions and behavioural responses over a period with changing risks and publicity (April - August 2009).

4. Reviewer original Q. With sample sizes relatively large and so I am surprised all constructs of a 1-5 scale were recoded into a dichotomous scale. Have you attempted a multiple linear regression entering the independent constructs as 1-5 scales (sum all items and divided by total number of items to get the scale back to 1-5) with the dependent variable on 1-5 scale. Then present the range, median and interquartile range for each in a new Table 2. Use the interquartile range to standardize the beta coefficients (IQR*beta coefficient prior to exponentiation) of each of the significant predictors in the two models. This way you can compare the effects of each construct on the dependent variable because each construct is now standardized.

Reviewer request. Looking at the authors’ response I believe they have missed my point. When you provide your Multiple Logistic Regression in a table form your odds ratios will be crude odds ratios and each estimate of effect will not have been adjusted for the interquartile range. Hence the odds ratios for each significant predictor cannot be compared until you multiple the beta by the IQR before you exponentiate – this way the readers can compare the odds for “believing in the efficacy of measures” with “self-efficacy” etc.

As suggested by the reviewer, standardization of the ORs will lead to ORs that are comparable in size with each other. However, the adjustment will depend on the distribution of the summary scores as we found in our dataset and therefore will be less comparable with results of other studies. Dichotomization as we performed also leads to effect sizes that can be compared to each other and interpretation are much easier and results are better communicable. We have again consulted C.W.N. Looman (senior statistician at Erasmus Medical Centre in Rotterdam, and co-author over 150 articles). In his opinion the way of standardization as suggested by the reviewer is not usual in this kind of research. He concluded that dichotomization does lead to less power, but the way of analyzing as performed in our study leads to results which are much easier to interpret and better communicable. In his opinion, these advantages outweighs the disadvantages.

**Results**

5. Please note:
1) older age (30-49yrs: OR 1.77, 95%CI 0.94-3.35) includes unity and as such is not significant, remove it from results.
2) include p-values for each.

As the reviewer advised older age (30-49 yrs: OR 1.77, 95% CI 0.94-3.35) has been removed from the results. In Table 3 p-values for the significant variables are given.