Reviewer’s report

Title: Limits of the social-benefit motive among high-risk patients: A field experiment on influenza vaccination behaviour

Version: 1 Date: 19 Aug 2019

Reviewer: Lynne Sturm

Reviewer's report:

Summary:

The paper presents interesting data from an experimental study comparing messages referring to self-protection vs. altruism to motivate influenza vaccine uptake in a hospital setting in Istanbul, Turkey. While the study findings of the paper appear valid and valuable to the research community, I believe that the paper could benefit from some clarifications and revisions to further increase its value prior to its publication.

Major comments and suggestions:

1. I am not sure if the paper really performs a comparison between messages referring to self-protection vs. altruism, as the altruism message also seems to contain information about objective risk group criteria. Perhaps the study is more a comparison of pure self-protection messaging vs. mixed messaging (in a relatively low educated population), which may also explain the findings. The authors should be much clearer about the message content in the paper text (possibly reproducing the messages in full, as these are obviously key). Also, if my hunch is correct, the alternative explanation should at least be acknowledged as a potential alternative interpretation in the paper's limitation section.

2. The use and purpose of the "free vaccination ticket" is unclear, especially since the paper states that influenza vaccine is anyway cheap in Turkey. Also, the ticket seems to encourage potential deferring of the vaccination decision, which seems to be counterproductive and makes it challenging to check for vaccination at follow-up if vaccination was performed at a different setting. At a minimum, the study should clarify how vaccine uptake was ultimately measured.

3. I think that the study should not only report results from analyses that control for two potentially endogenous variables (asking for recommendation and asking for pamphlet to be read) across the intervention, as these may be so-called "bad controls". In my view, the paper should report the results that omit these "control variables" as well as analyses that use those "control variables" outcomes and in a last step, perhaps the results that use those variables as controls, as it is currently done in the paper.

4. The analysis section is very unclear on how the statistical models underlying Table 2 are exactly implemented. Are the results based on one model with interaction effects or based on sub-sample analyses? The currently analysis section and the description of Table 2 make it very challenging to
understand exactly what type of analysis was performed on how to interpret the findings. These issues should be clarified both in the text and in the design, title and notes of Table 2.

Minor/editorial comments and suggestions:

5. Title. I think the title should make clear that the paper only considers influenza vaccination and I would therefore include "vaccinate for influenza" in the title

6. Abstract (Background). "costs to public health and hospital operations" should be more comprehensively formulated. I would replace it by "costs to public health and health systems".

7. Abstract (Results). Presenting a p-value for the vaccine uptake proportions across the two treatment groups (rather than just for the aOR) would be useful.

8. Reference (2) is perhaps not ideal here as it includes data from the 2009/H1N1 pandemic. Maybe the authors could consider a more recent reference making the same point in the context of a "regular" influenza season.

9. P. 4, line 7: "invites a focus" sounds strange. Consider revising.

10. P. 4, line 12: "with risk perception particularly" should be replaced by "with risk perceptions being particularly"

11. The motivation of the paper could be further strengthened by adding more influenza vaccination-specific references for the intention-behavior gap beyond the more generic reference (26). The papers of Harris et al (2009), Maurer (2016) or other related papers could be useful here, the latter as it also focused on SES-gradients that may be relevant to the low education setting studied here.

12. Overall, the introduction is very long and partially repeated in the discussion section. Repetition should be avoided. I would shorten the introduction and relegate the placement of the study into the related literature to the discussion section. In general, the paper does not separate section as clearly as it should and could benefit from some more detail in the description of measures and methods.

13. Table titles and notes should be more comprehensive to highlight more clearly what is shown in the tables and to make the tables more standalone.

14. Table 1 should include p-values for tests of differences in variables across the two treatment arms. In my view, the table should also exclude the 22 participants that were already vaccinated as they were excluded from the treatment and analysis.
15. P. 7, line 10ff. The information on the construction of high risk groups vs low risk groups is unclear and presented in more detail later. The variable coding should be more clearly explain here and dropped later.

16. P. 8, line 34ff. It would be useful to have some more detail on how risk perceptions were measured for this study.

Harris, K. M. et al. (2009). Do people who intend to get a flu shot actually get one?. Journal of general internal medicine, 24(12), 1311.


Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

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If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

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