Author’s response to reviews

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

Authors:

Ozan Isler (ozanisler@gmail.com)
Burcu Isler (burcubayrak85@gmail.com)
Orestis Kopsacheilis (orestiskopsacheilis@gmail.com)
Eamonn Ferguson (eamonn.ferguson@nottingham.ac.uk)

Version: 2 Date: 27 Sep 2019

Author’s response to reviews:

Faculty of Science
School of Psychology
The University of Nottingham
University Park
Nottingham
NG7 2RD
t: +44 (0)115 951 5361
f: +44 (0)115 951 5324
e: psychology-enquiries@nottingham.ac.uk
www.nottingham.ac.uk/psychology

27th September 2019

Re: PUBH-D-19-01368R1: Self-regard motivates patients with high-risk perceptions to vaccinate more than altruism: Exploratory evidence from a field experiment

Dear Dr Szunyogova,,

We thank the editor and the two reviewers for their insightful feedback. We have provided below detailed responses to each issue raised and we made substantial revisions to the manuscript noted in highlights. As further clarification, we have added a theory section to the manuscript, providing a simple utility model that suggests that social-benefit messages may be counterproductive among those with high risk perceptions. Finally, we now refer to the “altruistic vaccination hypothesis” as “prosocial vaccination hypothesis” since the former name, although used in the literature, may be misleading as prosocial vaccination may provide benefits to self as well (e.g., such as protecting kin or those in reciprocal relationships). We detail below the exact changes we have made as well as highlight these in yellow in the revised manuscript. Thank you for your consideration.
Jurgen Maurer (Reviewer 1):

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.

RESPONSE

We have revised our Figure 2, which now reproduces the messages in full, including the risk group information. Since there is no longer the need to reproduce the pamphlets used in our study in the supplement, we deleted previous Figure S1 & Figure S2 from the supplementary files.

We appreciate and accept both reviewers’ comments indicating that the distinction between the two experimental conditions (i.e., separation of the self-benefit focus from the social-benefit focus in the frames) could be conceptually clearer. We would like to point out that we were motivated by already existing frames used in the field, in particular, the pamphlets used by the UK national health services, which we note in the manuscript (page 7 line 1) and provide in our supplement (now labelled as Figure S1). Furthermore, we deemed it would have been ethically questionable to refrain from providing information about the risk groups that participants may belong to, which by definition puts emphasis on self-regard for those who perceive themselves to be in the risk group.

In short, we accept that the social-benefit frame can be seen as a mixed-motive frame, and we have added this to the discussion as a potential limitation (page 16 lines 1-4). Nevertheless, we believe that in a treatment comparison between the two frames any differences that will be observed is due to the additional effects of the social-benefit messaging. For this reason, we continue to refer to this frame as “social-benefit”, which highlights the difference between the two frames, while acknowledging the potential difficulties in interpretation in the 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.
We have clarified the purpose of the free vaccination ticket feature of our study (page 8 lines 16-18). This is an exploratory feature of the study that was added to inquire if social ties could be used to promote vaccination. However, as we emphasize in the paper (p12 lines 9-10), no tickets were utilized. We interpret this outcome as further supporting our preliminary conclusion that social-benefit motives are not very promising among high-risk populations, and we now note this view on page 12 lines 11-13).

It is commented that the ticket option may be counterproductive by motivating deferral. As we now emphasize on page 8 lines 18-21, any information about the tickets were given to patients only after their vaccination decisions were elicited. Hence their decisions could not have been influenced by any deferring option provided by the tickets. It was clearly stated on the tickets that the free vaccines can only be used on the same site until an expiration date, allowing measurement of ticket-base vaccine uptake.

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.

RESPONSE
We completely agree that analyses both with and without these covariates should be reported; we revised Table 2 to also include the unadjusted estimates. We acknowledge this important point by also citing the main reference on the topic by Simmons et al. (2011) on page 11 line 3. None of our findings depend on the inclusion/exclusion of these variables, which we note in the text (page 11 lines 1-3).

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.

RESPONSE
We have now taken steps to improve the clarity of our statistical analysis: Table 2 presents 3 different logistic models (1 simple model and 2 interaction effect models). These models are now separately labelled on Table 2 and in the Results section, and further described in the table caption as well as in the subsection named Analysis under Methods (page 9 lines 3-12). We have also revised the Table 2 title to be more descriptive of the analysis undertaken.

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

RESPONSE
We revised the title, which now refers to influenza.
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".

RESPONSE
Revised.

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

RESPONSE
We believe this information is already in the abstract: aOR = 1.63 and the associated p = 0.108 refer to the difference in vaccine uptake proportions across the two treatment groups.

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.

RESPONSE
We replaced this reference with a more appropriate one—a recent report on seasonal influenza vaccination by ECDC (2).

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

RESPONSE
We deleted this sentence as part of our shortening of the introduction as recommended.

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

RESPONSE
Revised.

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.

RESPONSE
Thank you for the useful references, which we added together with reference (26-28).

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.
RESPONSE
We have shortened the introduction (by shortening the 3rd and 4th paragraphs in the section). We also deferred the introduction of the concept of stratified medicine until the discussion section.

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.

RESPONSE
We have revised the titles and notes for both tables as suggested.

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.

RESPONSE
Table 1 is revised as suggested.

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.

RESPONSE
We clarified the construction of the high vs low risk group on page 8 lines 11-15.

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

RESPONSE
We now provide further detail on page 8 lines 9-11.

Lynne Sturm (Reviewer 2):

The authors are to be commended for probing further into the possible role of altruistic/prosocial intentions in decisions to vaccinate, and better yet, actual vaccination behavior. They target an important subject group of adults considered at medical risk for flu-related complications.

Conceptualization

1. What is meant by the flu vaccine being "... a reliable ... solution" (p.3)?

RESPONSE
We deleted the word “reliable” from this sentence, which is now revised as “Even though the influenza vaccine is widely available and affordable, its global uptake remains low”.

2. The altruistic vaccination hypothesis is conceptualized as an emphasis on the "added social benefit" of vaccination above that accrued by benefit to self" (p.S). The current wording of the social benefit message does not seem to reflect "added value" per se. It implies that the benefit to self is secondary and in the service of benefit of society. "By getting the flu vaccine you can gain immunity against flu.
You will thus lower the odds of transmitting flu to others around you.” In turn, the visual portrays solely "Protect those around you" rather than "Protect yourself and protect those around you." I would argue that a more balanced message about protecting others links the two, with protection of others as a true added value. For example, "By getting the flu vaccine you can gain immunity against the flu and can also lower the odds of transmitting flu to others around you. So, protect yourself and protect those around you." Please address.

RESPONSE
We agree that the meaning of the frames used in our study is open to interpretation and that more precise conceptualizations of the self-benefit vs. social-benefit distinction are possible. In particular, our social-benefit frame can perhaps be seen as positioned somewhere between a mixed-motive message and a pure social-benefit message. As we detail our position in our response to comment #1 of Reviewer 1 above, the contents of our frames were driven by messages actually in use by the UK health services to maintain realism and relevance to policy making (page 7 line 1). We note the limitation regarding interpretation of the pamphlets on page 16 lines 1-4. We have also removed the word “added” on page 4 line 19.

Methods

1. Why were subjective risk perceptions measures after the pamphlet intervention rather than prior to it? The authors wanted to test whether self-risk perceptions moderate the effect of the prosocial message on vaccination behavior, in which case measuring risk perception should have occurred prior to the intervention, not after. Please address.

RESPONSE
The main reason we have elicited our subjective risk measure after the vaccination decision was because both pamphlets provided information about Turkish Ministry of Health’s official conditions for being categorized as under risk of death from influenza related diseases (see Figure 2a). In other words, our subjective risk measure was intended to capture understanding of risk group membership after information needed for making inferences about belonging (or not) to the risk group is provided. Note that this subjective risk measure is not “risk perception” as usually measured in the literature but rather is about the understanding of being in objective category of risk group.

Nevertheless, we do agree that elicitation of the risk group perception after the vaccination decision may have its own limitation, which we now acknowledge on page 16 line 4-16, where we also note the result of the statistical tests showing, for example, that risk group perception does not depend on the frame manipulations. Hence, we believe that Models B and C on Table 2 capture the idea that understanding of the risk group classification moderates the effects of framing manipulations. As we note in the title and the manuscripts, these models were exploratory, and we did not want our exploratory measures to potentially confound our primary behavioural measure (i.e., vaccination uptake). As further discussion, please see our response to point 5 below.

2. Strengths of the methodology include using a clinical sample with actual vaccination behavior, and limiting extraneous verbal interaction between the researcher and the patients (p.8).
RESPONSE
Thank you.

3. Where did the vaccination rate for the prior year of 16% (p.10) come from?

RESPONSE
We asked participants in the questionnaire (Q1.7) “Did you get vaccinated for influenza last flu season?” Among the 244 participants in the study (i.e., including those who were already vaccinated for the study season) 16% reported to have vaccinated for influenza in the last season; we have noted this information on the text and referred to the SI (page 12 lines 6-7). For clearer comparison with this baseline rate, we have revised the overall vaccination rate by the end of our study to also include the 244 participants, which is 44% (page 12 lines 4-6). We have also revised the section in discussion where the 16% figure as reported for a second time, as not to provide duplicate information.

4. Offering a free vaccination ticket to share with friends or families is an intriguing outcome measure of prosocial attitudes. However, a follow-up contact with patients in which they report whether they distributed the ticket might be a better measure than measuring whether anyone returned to the hospital or not. Patients can distribute tickets without the recipients making their way back to the hospital.

RESPONSE
We agree that follow-up contact with patients would be more accurate as we now have no way of knowing whether the tickets were distributed but not utilized or whether they were not distributed to begin with. We note this limitation on the text (page 12 lines 10-11).

5. Please indicate the relationship between medical assessment of risk and the patient self-assessments of risk.

RESPONSE
Medical assessment of risk involved an infectious disease specialist (one of the coauthors) assessing each participant whether they are in the high risk group for influenza related diseases as defined by the Turkish Ministry of Health. These conditions are now listed on Figure 2a as well as on page 8 lines 14-15. Patient self-assessment of risk involved asking participants in the questionnaire (Q2.2) “Are you in the risk group for flu?”. Hence both assessments related to belonging to a high risk group. To provide more detail on the relationship between the two measures, we now report additional data in Results (page 9 lines 17-21) and refer to them in Discussion (page 15 lines 3-8).

Results

1. Presenting qualitative data in the supplementary information section does not seem appropriate and should be omitted. If patient descriptions of reasons for and against vaccination was part of the study, it should, in my opinion, be included in the Introduction of study aims (p.5), methodology described in Methods, findings summarized in the Results and discussed in the Discussion.

RESPONSE
We agree that the for and against data is insightful enough to include in the main text. We have now sections in the abstract (page 2 lines 16-17) introduction (page 4 line 24), methods (page 8 lines 8-9;
page 9 line 13), results (page 11 line 11 to page 12 line 2 and Table 3) and discussion (page 15 lines 15-17) of this data. However, we chose to keep the rest of the qualitative data in the SI since we believe that omission of data elicited may not be an appropriate open science practice and that reporting this data in the main text would be too distracting from our main goal of comparing alternative nudges.

2. In the Abstract, please rewrite to clarify non-significant findings (e.g., "42% of the patients in the self-benefit frame chose to receive vaccination as compared with 34% of the patients in the social benefits frame but the difference was not statistically significant (aOR= ... ").

RESPONSE
Revised as suggested.

a. On page 12, the following statement is misleading: "Inconsistent with the hypothesis of altruistic vaccination, the social benefit message was not found to increase vaccination as compared to the self-benefit message. Instead, vaccination was 8 percentage points higher among those who received the self-benefit message than those who received the social benefit message, which was not statistically significant." Please rewrite.

RESPONSE
Revised for clarification.

3. Might another way to think about the findings be that addition of the altruism message did not depress acceptance of the vaccine? If part of our public health mission is to educate the public about community immunity (although not referred to as such in this paper), this is actually an encouraging finding.

RESPONSE
This is an interesting possibility, and given that the vaccination decisions were elicited in a context of low transaction costs, a probable outcome. Unfortunately, our study is not equipped to causally identify whether the altruism message might have depressed acceptance of the vaccine because we do not have a no-intervention pure control condition (where patients were not exposed to any nudges). We note this limitation of our study on page 15 lines 20-22.

We hope that we have addressed the reviewers’ comments satisfactorily and that the paper is now more suitable for publication on BMC Public Health.

Yours sincerely,

Prof Eamonn Ferguson on behalf of the authors.