Author's response to reviews

Title: Is Anybody Doing It? An experimental study of the effect of normative messages on intention to do physical activity

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Version: 4 Date: 30 April 2014

Author's response to reviews: see over
Seville, 30 April 2014

Victorino Silvestre  
Journal Editorial Office  
BioMed Central

COVER LETTER - REPLY TO REVIEWERS

Dear Mr Silvestre

Thank you for sending us the reviews to our paper on 31 March 2014.

We have read the reviews with great interest and have taken note of all of the reviewers' comments, as well as your additional request to improve written style of the paper and adhere to the journal's style.

We have made all the necessary changes in the new manuscript, which you will find attached. We have also had the manuscript proofread by two external copy editors.

In addition, please find below our point-by-point reply to the reviewers. Should you have any concern or need for clarification, please do not hesitate to let us know.

Best regards

René van Bavel
POINT-BY-POINT REPLIES

Reviewer: Sandra Coulon

Major compulsory reviews

1. We take note of the first point, namely that the manuscript requires further work, in particular:

   - "The reader needs to be better walked through the existing empirical evidence for relations between descriptive norms and PA". We can see how the previous version was too telegraphic and skipped over this information, which is very relevant for placing the current paper in context. We discuss this issue in greater detail in paragraph 5 of the Background (p. 4).

   - "Has a study like this ever been done before?" The closest is the study by Priebe and Spink (2012), which had some ambivalent results (initially they found an effect of normative messages but were unable to confirm this in an extended study). We mention this report and these findings in paragraph 5 of the Background (p. 4).

   - The "previous studies testing a normative message", mentioned in the Discussion, refer to Goldstein et al.'s (2008) study on hotel towels, the Behavioural Insights Team's (2012) study on tax compliance (which we explain in paragraph 4 of the Background, p. 4), and the above-mentioned Priebe and Spink (2012) study. These are all referenced in paragraph 2 of the Discussion (p. 14) for greater clarity.

   - We have revised the manuscript throughout to seek out typos and address the need for greater clarity, more transition sentences, and more definitions. We have also added more definitions in paragraph 4 of the Background (p. 4), particularly on the 'provincial effect'.

2. We take note that findings or differences could be partly explained by different base rates of physical activity (PA). To control for this, we introduced the IPAQ scores into the ANOVA model (see paragraph 4 of Results, p. 12). As a result, the transformation function for intention was changed to ensure normality of the residuals of the regression (paragraph 5, p. 12). As expected, IPAQ was predictive of intention. However, the difference in means of intention between the treatment groups and the Control Group, i.e. the main result of the study, remained as before. The country difference between Bulgaria and Croatia remained and, in addition, Romania showed a higher intention than Croatia. By introduction PA into the ANOVA model, we rule out the possibility that previous PA behaviour accounts for these differences (paragraph 6, p. 13 and new Table 4).

3. The rationale for selecting Bulgaria, Croatia and Romania has been highlighted and brought forward to the Background (paragraph 6, p. 5). However, not all the justification for selecting these countries is relevant in this section. Information about the high levels
of Internet usage, for example, fit better in the Methods section. We are sensitive to the claim about "making a sweeping statement of a few countries" without further explanation or empirical evidence. We provide greater background information, including some comments about public health infrastructures in south-eastern European countries after 1990, in the Background (paragraph 6, p. 5). Empirical evidence is also included in this paragraph, namely a Eurobarometer report which compares attitudes towards public health campaigns in these three countries vs. other European countries. This was also present in the previous version, but it was not so prominent.

**Minor essential revisions**

4. We agree that the manuscript overstated the link between intention and behaviour. A more extensive, nuanced and well-referenced explanation of this link is now included in the Methods section, in the sub-section entitled *Measuring intention to engage in physical activity* (p. 9). The justification for using intention is also better articulated. This issue was also raised by reviewer Amudha Poobalan.

5. On the context of normative messages, it is true that public health interventions through mass media might not be able to fully capitalise on these findings. On the other hand, we believe that the Internet and mobile applications show particular promise for delivering such individually-tailored messages. We included a statement to this effect in the Conclusion (p. 16).

6. On changing the phrase "intention to behave", the text has been modified in the Methods section so that "intention to engage in PA" is used when not referring specifically to the intention to behave construct.

7. The results that young (16-18) Croatian women dropped out more was misleading and has been removed. Their drop-out rate was in fact comparable to the drop-out rate of women of similar age in other countries. However, the drop-out rate of young (16-18) men in Croatia was unexpectedly low (9 out of 73), which, by comparison, gave the impression that the drop-out rate of young women in Croatia was too high (see Table 1). We have reviewed the data collection process in detail and have tried to find an explanation for this low drop-out rate among young (16-18) men in Croatia, but have not arrived at a convincing explanation. We believe this difference in the drop-out rate is simply due to low numbers in the "16-18 year-olds" segments of the sample, and would probably not persist in a larger sample.

8. Regarding "@1" placeholders for the images used in the experiments: we added a legend to the figures in order to clarify that @1, @2 and @3 refereed to age, gender and country. We also placed Figures 1(a), (b) and (c) together in a Word document (labelled "Figure 1") to avoid confusion.

9. We understand that Figure 4 might give the impression that time is being represented on the x-axis, and understand that it might raise possible confusion. However, it is also the standard graph in the literature for presenting ANOVA. This graph would allow the reader to immediately detect interactions, as they would appear as lines crossing over one another. While this graph does not show any such interactions (they are not significant in our study), we would prefer to be consistent with the literature and present the graph with
lines and not bars. However, we can still make this change should the reviewers feel strongly about it.

10. More modest first sentence of Conclusion: done.

Discretionary revisions

1. On the use of the word "perplexing" – agreed, changed to "unexpected" throughout.

2. Mention of injunctive norms – agreed, reference to injunctive norms were not entirely appropriate in the original version. However, reviewer Amudha Poobalan requested a discussion on how this crucial age group might perceive the construct of 'descriptive norm'. To address this comment, it has been necessary to include injunctive norms, albeit just to mention how ineffective they are among young people, and how descriptive norms hold greater promise.

3. Conjecturing on the increase in PA following exposure to the normative message. We agree that self-monitoring is an important concept for health behaviour change, and have mentioned as a complement to the priming explanation (paragraph 2 of Discussion, p. 14). However, the authors are currently undertaking a more detailed examination of the reasons for the effect of the negative message as part of a separate analysis. We thank the reviewer for bringing the issue of self-monitoring to our attention and will seek to include it in our future analyses.

Reviewer: Amudha Poobalan

Major compulsory revisions:

1. Methods

- A sentence has been inserted in the Methods section (paragraph 2, p. 6) to explain how the sample size was calculated.
- A better explanation of the recruitment process, in particular what was meant by randomisation and stratification, is included in the Methods section (paragraph 3, p. 6).
- The issue of using intention as a proxy for PA has been addressed. We agree this link was not treated properly in the manuscript. A more extensive, nuanced and well-referenced explanation of this link is now included in the Measuring intention to engage in physical activity section (p. 9). This issue was also raised by reviewer Sandra Coulon.

2. Results and related tables

- Regarding the request for more information on the numbers in each of the control and intervention groups, a new table (Table 2) has been added, presenting the breakdown of participants by country, age and treatment group.
- Univariate one-way ANOVA analysis, with subsequent post-hoc analysis, was added to the paper. Results are presented in a new table (Table 3), and comments are provided in the Results section (paragraph 3, pp.11-12).
• Comments to the poor fit of ANOVA were added to the text. The last paragraph of the Results section (p. 13) acknowledges the low fit and the last paragraph of the Discussion (p. 15) comments on it.
• The direction of the effect, which is positive for both the positive and the negative normative messages, has been highlighted throughout the Results section (pp. 12-13).
• Results of differences between Treatment 1 and 2 (now renamed Positive Message and Negative Message Groups for greater clarity) were added to the Results section for the univariate analysis (paragraph 3, p. 11) and multivariate analysis (paragraph 6, p.13).

3. Discussion

• Regarding the drop-out rate: please see answer to Sandra Coulon (point 7 above).
• Comments on the poor fit of ANOVA and its implications were added to the last paragraph of the Discussion (p. 15).
• Regarding differences between Bulgaria and Croatia and possible role of other factors: please see reply to Sandra Coulon (point 2 above).
• Regarding the way in which this crucial age group "perceives the construct of descriptive norms": we find it difficult to address this issue in the Discussion. We agree that young people are not particularly receptive to subjective norms (or, more precisely, injunctive norms). Dr Poobalan's own contribution to the literature on this matter, which guided our initial investigation, makes it clear. But this is one of the reasons why we have opted for a study on descriptive norms in the first place. Young people might not respond to what they think others think they should be doing (i.e. the way subjective norms is operationalized in TPB), but they might well be receptive to what others are de facto doing, and simply copy that behaviour. In our opinion, this issue should be covered in the Background rather than the Discussion, since it has guided the design of the study (we include it in paragraph 3, pp. 3-4).

Minor essential revisions:

1. Typo and Grammar errors:
All corrections were taken on board. The confusion with the figures comes from the fact that Figure 1 is actually comprised of three pictures (i.e. the three screen shots composing the positive message). Since each of these pictures is a separate file, we had labelled them 1(a), 1(b), and 1(c). However, together they composed Figure 1. To avoid this confusion, we have now placed Figures 1(a), 1(b) and 1(c) together in a Word document, labelled "Figure 1". Figure 2, therefore, remains as Figure 2 and will not be re-labelled as Figure 4.