Author's response to reviews

Title: Mass social contact interventions and their effect on mental health related discrimination

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Author's response to reviews: see over
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Dear Mr Dizon and Editorial Board at BMC Public Health

We appreciate the opportunity to revise and resubmit our manuscript entitled, “Mass social contact interventions and their effect on mental health related discrimination” for consideration for possible publication in *BMC Public Health* (MS: 1454957308685033). We have carefully considered the reviewers’ comments and we have responded to each issue. Our responses are numbered in the order in which they appeared for each reviewer. Changes in the text are tracked in blue font.

This paper represents original, unpublished material and it is not being considered for publication elsewhere, nor will it be submitted elsewhere while under review. The final version of the manuscript, including the order of authors, has been approved by all authors.

Thank you, in advance, for considering a revised manuscript for publication. Do not hesitate to contact me if you require additional information. I may be reached by mail, telephone, fax, and email listed below.

Kind Regards

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Editorial Comments

1. Manuscripts should include a “Competing interests” section. This should be placed after the Conclusions/Abbreviations. Please consider the following questions and include a declaration of competing interests in your manuscript:

   We have now included a competing interests section at the end of the manuscript stating that the author(s) declare that they have no competing interests.

Referee 1:

Reviewer's report:
The authors presented the results about the impact of population-level social contact intervention on improvement of stigmatizing behaviors, subsequent campaign engagement and willingness to disclose mental health problems. The results were based on 403 participants who were in the national campaign activities. The design of the study/intervention was theoretically driven. The results were interesting - presence of facilitating conditions predicted improved stigma-related behavioural intentions and subsequent campaign engagement 4-6 weeks following social contact. Contact, however, was not predictive of future willingness to disclose mental health problems. It seems to suggest that social contact alone may not reduce stigma, which makes sense to me because stigma may be related to the nature of the conditions. Depression, anxiety, addiction problems and schizophrenia are different and people have different perceptions about these conditions. Although this may not be the best design for evaluating the impact of the intervention, the study still provides important information which is useful for future studies. The limitations of the study were well acknowledged.

1) I think that, in the Results section, the authors should also present the magnitude of the change in RIBS scores and of subsequent campaign engagement. This will set the benchmarks for future studies to compare. With regression results alone, readers cannot see how much have been changed.

   Thank you for this comment. Tables 3 and 4 already include the odds ratios which is a type of effect size. Table 3 provides the probability odds of improved RIBS score (i.e., ratio of the probability of improved RIBS score to the probability of an unimproved RIBS score) and table 4 provides the probability odds of subsequent campaign engagement. This is now further explained on page 15.

Referee 2:

Reviewer's report:
Thank you for the opportunity to review this stimulating paper where the authors try to document an important topic: impact of an innovative program meant to change the stigma of mental illness. Truly innovative is the strategy to “normalize” disclosure in spontaneous interactions between people with mental illness and the
public. The investigators make a valiant effort to evaluate the program’s impact, in
the process illustrating the difficulties of conducting sound research like this in more
global and less controlled situations. They use some solid measures of stigma change
and have some interesting findings. I offer the following comments for their
consideration.

1. Generally, I would encourage reorganizing the Intro; it meanders a tad from the
impact of contact (factors that mediate contact), the necessity to disclose, and ways in
which contact is hampered because people choose. Perhaps most interesting in the
Intro, and not well-developed, is the innovative idea of “normalizing” disclosure. I
think, as hinted on page 2 of the Intro, it is a supportive atmosphere for self-
disclosure. What makes it “supportive?”

   Thank you for this helpful feedback. We have now revised the Introduction
   substantially and have elaborated further on the idea of “normalizing
disclosure” and the meaning of a supportive environment.

2. I found the description of TTC, Time to Get Moving (TGM), and Rainbow at the
end of the Intro and in the Methods a bit hard to follow. The first sentence in the
Methods is redundant with comments in the Intro. The text states volunteers came
together at each event. Who are the volunteers? What events? How was TGM
integrated with physical activity events?

   We have now revised the description of TTC, Get Moving and the Roadshow
to remove redundancy between the methods section and the introduction and
to clarify the integration of physical activity and more clearly describe the
events and purpose of the volunteers.

3. A bit of work is needed to carefully lay out who the participants are in the study:
seems to me you are distinguishing between disclosers and those disclosed too. Some
kind of pithy way is needed to distinguish these groups. In the same section, you refer
abruptly to questionnaires: what are these questionnaires?

   A note is now made to refer readers to the description of the questionnaire
which follows the section discussing study participants. On page 11, under
presence and quality of social contact, we now specify that those with
presence of social contact reflect 2 groups i.e., people with mental health
problems meet someone without mental health problems, ‘discloser’ at the
event and vice versa ‘those being disclosed to’. We do investigate whether
there were differences in outcomes among people with and without mental
health problems in the regression models, the sample sizes were too small to
investigate an interaction between social contact and presence of mental
health problem (Also see Referee 3, point 4 below)

4. You then describe the items/scales that comprise the questionnaire a page or two
later. The reader needs to be a bit oriented about the significance/meaning of the
constructs that comprise the scales. What does intended stigmatizing behavior mean?
A page later: seems a bit redundant by reintroducing the RIBS.
We now provide only a brief description of the survey aims (page 11), i.e., that the questionnaire assesses the presence and quality of social contact and its relationship with various stigma related outcomes and then orient the reader to the more detailed section on the significance/meaning of the measures below.

5. I personally think the section on Analysis strategies would work better directly woven into the Results; i.e., describe specific stats as used.

We appreciate this comment, however, we feel that the organisation and structure may be clearer to describe the analyses section at the end of the methods in one place. We are happy to change this, however, it is felt that incorporation into the results would be better.

6. I think more attention to detail is needed. Table 1 is never mentioned in the text. At one point you say “predictors which predicted.”

We apologise for these oversights. We have now added a reference to Table 1 in the text and revised the sentence on page 16 to state: there were no significant factors, however which predicted an increase in reported propensity to disclose a mental health problem either at work or to family and friends.

7. Please remind readers what “facilitating social contacts” are in Table 3. I don’t understand what “subsequent behavioral actions” are in Table 4.

We have now specified that facilitating social contact conditions refer to the specific conditions theorised by Allport and Pettigrew as being associated with optimal social contact and provide a note at the bottom of tables 2 and 3 detailing this. We have also added a note which specifies that subsequent behavioural actions refer to the following actions: visiting the TTC website, pledging support via the TTC visual pledge, talking with others about the TTC event, or following TTC on Facebook or Twitter.

8. I did not see analyses that compared the effects of TGM and Rainbow which surprised me.

Type of intervention, i.e., Roadshow or Time to Get Moving, were included as a regression covariate to see if there was a difference in the outcomes by type of event, however, this variable was not significant. This is now mentioned in the analysis section.

9. Hedge the Discussion a tad; I might say, “TTC events MAY HAVE facilitated intergroup contact between people with and without mental health problems…”

Thank you for this suggestion. We have now revised the sentence on page 16 to state that: “These data suggest that the TTC events may have facilitated inter-group contact between people with and without mental health problems in a meaningful way.”
10. I think there are two significant limitations to the study that need to be added to the Discussion:
   a. The very, very small percent of people who participated in the study at the events and even smaller group that followed up;
   b. Which may have underpowered some analyses

   *We have now made an explicit statement in the limitations section on page 18 about the limited response rate and the potential for underpowered analysis.*

**Referee 3**

Overall I think that this is a valuable paper that genuinely adds to the existing body of knowledge in the area and should certainly be published. It builds upon and adds to theory; it develops our understanding of what works in hectic real life situations, and it tries to look at longer term changes and behavioural intent and discrimination proxies, addressing weaknesses of many previous studies. The commitment to involving people with experience is commendable and the design is clever and egalitarian. However I feel that some of the claims made should be toned down.

1) There are numerous weaknesses (almost inevitable in real life situation) in the data, eg:
   - It is not pre-post (a missed opportunity or do you feel this would affect the interaction?)
   - The small sample sizes as a % of the audiences and selection biases in the study are acknowledged but then are not reflected in the claims made
   - Nor are there controls
   - Drop off is very high
   - Vast majority already know and have contact with people with MHPs Etc

   *Thank you for clearly outlining these weaknesses. We now include or emphasise each of these weaknesses in the limitations section. We have also suggested that future studies might test these relationships using large scale interventions among a broader and more representative population with larger sample sizes and incorporate use of control groups in order to further test the claims made from these study data.*

3) I really don’t think that the data does demonstrate effect, so much as tentatively indicate- and it would be much better to say that. I would tone down the claims of effect and really reinforce the fact you’ve developed an approach that is valuable. In fact, you might also want to say that it didn’t worsen things (see, and perhaps cite, Quinn et al, 2011, Impact of a mental health arts festival on stigma and discrimination, Acta.- where some real life contact events actually had a negative effect)

   *We have now revised the discussion to make conclusions more tentative and have also emphasised the development of this approach and need to develop this research further to better understand these findings in the conclusions section. We also refer to ‘associations’ rather than ‘predictors’. We appreciate the suggestion of the Quinn reference which is helpful for contextualising our results*
4) In addition you should probably acknowledge lack of specificity as a further limitation as people respond to people with different MHPs differently, and this could be important here.

*We agree this is a limitation and have now stated in the limitations section that investigation of whether the associations found vary by type of mental illness might also be important. Due to the limited sample size, however, we were not able to investigate this factor in these data.*

5) You state that it is not representative in terms of demographics and the main concern for me would be BME communities. Partly due to socio-cultural differences in some parts of some communities that can lead to conceptual differences about MI and beliefs such as inheritability and contagion. Also the more common issue of dual stigma/discrimination due to racism that may complicate likely ‘contact’ effects (see, and perhaps cite, intro’ in Knifton et al, 2010, Community Conversation. SPPE)

*We have now added a comment reflecting the need for investigating these issues among diverse ethnic groups in the limitations section in addition to appropriate references.*

6) Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
Yes, very clearly. It is great to see people actually going back to the original studies to look at the conditions under which contact works, this is usually skimmed over. There are several papers over the last half century suggesting the impact of contact seems to be greater for the majority than the stigmatized minority and you could explore your different results here a bit more

*Although our findings suggest that, individuals without mental health problems remembered their interaction better than individuals with mental health problems we are not able to explore this issue further with these data, we now acknowledge this more clearly as a limitation due to power in the limitations section.*

7) Do the title and abstract accurately convey what has been found?
There are two issues:

a) The first is that is not discrimination as in the title. Reported behavioral intent and campaign activity are not discrimination they are stigma or discrimination intent

*We have now revised the title to reflect this point: Mass social contact interventions and their effect on mental health related stigma and intended discrimination*

b) Secondly the claims are slightly overstated (see note in “Are the discussion and conclusions well balanced and adequately supported by the data?”)
The discussion and conclusions have now been revised (please see points above).