Author’s response to reviews

Title: From social adversity to sympathy for violent radicalization: The role of depression, religiosity and social support

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Author’s response to reviews:

Dear Editor,

thank you for the opportunity to revise our manuscript. We also thank the Reviewers for their helpful feedback and suggestions. Please find below our point-by-point reply (in italics) to the concerns they raised. We hope that you find that our revision satisfactorily addresses all the critical points that have been highlighted. All changes in the manuscript have been put in red font text.

On behalf of all authors

Reviewer reports:

Editor:

In the last decades, public violence increased substantially in countries where this was previously rather rare. Besides the disruptive societal effects, such violence (e.g. terrorist attacks) has a substantial adverse effect on health and wellbeing of people, not only on the directly, but also on the indirectly involved, and on the population as a whole, making this phenomenon indeed an important public health issue. Although violent radicalization not always leads to violent acts, it often precedes these. Therefore it is important to gain more insight in the different paths leading to violent radicalization, making your manuscript very timely and relevant, and interesting to the readers of our journal.

However, the reviewers have some important comments and questions, which you can find below.
Please answer them as concretely as possible. Adding to these, I would like you to elaborate a bit more on: 1) the low response rates, the (lack of) representativity of your study, and the possible biases, and the possible consequences of these for your findings and conclusions, and 2) possible actions that concretely can be undertaken in the population of your respondents to prevent radicalization, based on your findings.

Response: The low response rates, and associated potential biases are now more thoroughly presented and discussed in the manuscript (p. 19), and a more detailed explanation can be found in our response to Reviewer 1’s comment (see below). Following the Editor’s and Reviewers’ comments we also expanded our implication section (pp. 19-20).

Reviewer #1: Dear Authors,

This article focuses on a college population - in this case a young population (majority is 16-21 years old) and mainly female, third or more generation immigrants and not religious. The text is balanced as it describes well several sides of this topic and emphasizes nicely what this study can cover (and what not). It is clear that this study covers the sympathy of violent radicalization (SVR), not the factors that might be mediators/moderators to actual violent radicalization. So, even though media focuses often on Islam/Muslims, this study will not be able to make any hard statements about any such relationship, as the study population only has 6% Muslims. The authors are also not formulating any of these statements as such, although any conclusion on "no mediation" of religion between social adversity and SVR should not be mixed up with the usual "media focus" on "Islam/Muslims" and "radicalization". The authors balance this well in their conclusions of the main text, in my opinion better than in the conclusion part of the abstract.

The weakest point in the study is probably the response rate, varying between 2 and 19% of the college populations (N=1894). If using the full data sample, it is even less (N=1190) although the characteristics between the full sample and the full data sample are similar. Can there be given a clarification why there still are missings for some variables in the full data sample (Table1)? How was it handled in the analyses?

Response: Unfortunately, our response rates are within the range reported for most online surveys, especially for such a delicate investigation. However, we expect that these rates may even be higher than if we were to administer these surveys directly to the students. The online form provides a sense of anonymity and allows for better accuracy in the answers. Overall, the characteristics of the population are also comparable to the population of college students in Quebec, which indicates that there is limited potential for differential response leading to potential selection bias. In any case, whether a population is representative or not does not limit inferences from associational studies as already discussed within the field of epidemiology and biostatistics (See Rothman et al. 2013: https://www.ncbi.nlm.nih.gov/pubmed/24062287). Regarding missing data, we did run some analyses to investigate any differences between the complete data sample and the overall sample of the study, and our statistics indicate that the two samples were comparable. We would have liked to run our analyses using multiply imputed datasets to avoid any potential issues, however, there is no theoretical development yet that incorporates such multiple imputations with the mediation analysis within the counterfactual framework.

As reviewer I am not familiar with all the used instruments, which all have references to earlier studies. Is it possible to provide the range (min-max) of the continuous instruments, to have a faster and better understanding of the means in table 2? Additionally for Table 2, is it possible in the table (or it's
footnote) to indicate which instrument was used in the table (as discrimination and "radicalization" have 2 instruments). NB: "sympathy for violent radicalization" is more correct than "radicalization".

Response: Thank you for your comment. We provided the range of the continuous instruments in the description of the measures (pp. 7-10), and substituted “Radicalization” with “Sympathy for Violent Radicalization (SyfoR)”, and “Discrimination” with “Discrimination (continuous score)” in Table 2.

Can the authors specify somewhat more the multilevel model that was used (line 26 page 10) for presenting the results of line 30-46 page 13? It would be an added value (and easier to understand) for most readers to have a table with a traditional approach added to the manuscript. Does a traditional approach also support/suggest the presence of mediation, e.g. change in coefficient of the exposure when adding the mediator in the model based on non-predicted data?

Response: The traditional approach will be biased, especially in terms of confidence intervals. The traditional analysis in this case violates one of the important assumptions that is that the outcome is independent between subjects (i.e. each subject’s response is independent from other subjects). In our setting, the responses of our participants are not independent from each other for those within the same colleges. The multilevel analyses allow taking into account this correlation between subjects within the same institution that is due to higher order factors. For example, two students selected from the same institution are expected to respond more similarly than two students selected from different institutions as there are other institutional factors that can impact the response. Therefore, any statistical analysis of such data must account for the intra-institutional correlation, and failure to do so can result in misleading inferences. This been said, the traditional approach is believed to lead to the same results, but with tighter confidence intervals, which may lead to a higher number of significant results. We clarify this point now in the methods section (p. 10).

Without prejudice to the above comment, the authors present a nice stepwise presentation of their statistical approach - from univariate analysis, DAG’s to multilevel analyses and finally quantification of the mediation within a counterfactual framework. Something that is confusing are the moderation analyses: I assume that they were performed in the multilevel models? Maybe it would be easier for the reader to first present the "basic models" (previous comment) and "moderators" to finalize it with the "mediators" (Monte Carlo approximations etc.)? Are all presented instruments/variables used to predict the mediator and outcomes in the counterfactual framework?

Response: We agree with the Reviewer that moderation analyses must come after the main multi-level models to avoid confusion, as these were ran using the multi-level analyses. We also present the moderation before the mediation findings in the results section. Regarding the variables included in the mediation models, we used the same set of covariates inferred from the DAGs, in addition to the potential mediators.

Final comment, the CI of proportion mediated can be below zero or exceed the 1 - theoretically that is impossible. Is this a modelling issue, e.g. bad choice of the models (linear, non-linear or semiparametric) or a mathematical issue? How were the models chosen?

Response: One of the important issues with the traditional Baron/Kenny mediation method (in addition to not handling mediator-exposure interactions), is that it cannot account for such negative mediated effects. Actually, even the proportion mediated can be theoretically negative. A negative mediation (sometimes referred to as suppression) is one that increases the effect instead of reducing it. These actually can arise when actually the effect of the exposure on the mediator has an opposite direction to the effect of the mediator on the outcome. In other words, if there were no effect of the exposure on the
mediator, the effect of the exposure on the outcome would have been even bigger. Such counterfactual methods allow for this kind of effects to be detected, although we did not find any such effects here, only confidence intervals included negative effects.

Kind regards,

Reviewer #2: An interesting article on a topic of interest, conducted in eight different colleges in eight different areas of Quebec that provides some useful insights into support for radicalization, which in turn can be used to develop strategies to support those at risk.

Some comments for clarity and some areas that it would be good to provide further information are listed below:

With regards to the ages of the students. I would have thought that the majority of undergraduate students would be within the age group of 19-24. The article does not comment if the students were in under-graduate or post graduate courses. Was this not known? Was there a difference in results. Response: Thank you for your comment. The misunderstanding and diversity in age groups is due to the specificities of the Quebecker Cegep school system. In Quebec, colleges (known as Cégeps) are a public educational institution placed between high school and university. Their purpose is to assist in continuing education. These types of schools provide two to three year pre-university programs and vocational career programs to younger students (starting from age 16) as well as older professionals. Colleges do not have graduate students, except if those have opted for a vocational training after completing a university degree, which is rare. This has now been clarified in the manuscript (p. 7).

Given that one of the measures that was found to have an impact was experience of discrimination, and this was experienced in their educational setting - I would have thought that this would be a major finding and recommendation in the paper - that is that discrimination is widely experienced in educational settings, and that action needs to be taken to address racism and discrimination. There is a passing mention in the conclusion - but this is not sufficient. It needs to be drawn out in the analysis in much more detail, and I would suggest be a major finding of the paper.

The analysis notes the link with depression and SVR and on page 17 notes that mental health services and need to be provided in close proximity such as being on the premises on schools and colleges.

I would have thought that addressing the causes of the social adversity, where possible is of equal priority, as well as supporting the individual victims in how they cope and respond to social adversity.

Whilst it is beyond the control of the colleges to address the childhood trauma, and trauma experienced in the country of origin - it is squarely the responsibility and within the power of the educational institution, to work towards eliminating the trauma experienced within the educational institution, such as racism and discrimination, as well as supporting the students in trying to better deal with the trauma associated with instances of racism they are experiencing at their educational institutions.

There is no mention of sorts of trauma being experienced at the educational institutions. Was this captured in the survey, as it would be good to know further information. Was the trauma related to discrimination by other students? Or discrimination by teachers and other more organisational factors?
Response: We agree with the Reviewer that both discrimination and trauma are complex constructs whose experience (and reporting) merit appropriate attention. To answer the Reviewer’s request about the profile of discrimination in school, we have included in this file a table describing the different discrimination events reported by the students who said they were discriminated in school (see Table 1 at the end of this file). Because the instrument we used to assess discrimination is constructed in a way which does not permit associating directly a place with a type of discrimination we cannot affirm that these events were necessarily, or only, happening at school. Nonetheless, these results show that items indicating a feeling of not being respected or valued are at the forefront, while actual insults or discriminatory gestures are rarer. This indicates that forms of ambiguous discrimination predominate. These micro aggressions can undermine a person’s self-esteem but are difficult to denounce because the victim is often perceived as too sensitive. Given the above mentioned limitations we do not include the Table in the paper, but we now mention these results in the main text (pp. 13-14 ), and address their implications in terms of prevention (pp. 20-21).

Some questions, or areas for further investigation include- how do these figures compare with non-migrants, for example with regards to age and depression. Are there similar rates, or information to use as a comparison? Or is this for further study?

Response: The results presented in the study refer to a general sample of mostly third-generation immigrants and above, which means that most of our sample (66%) could be considered “non-migrant”, given that both their students and his/her parents were born in Canada. As mentioned in the results (p. 13), second- and third-generation (and above) immigrant students had more similar SVR scores, reporting higher scores than first-generation immigrant students. Therefore, we controlled for immigrant status (first-generation, second-generation and third-generation and above) in all our analyses. Comparing the associations of depression and age with immigrant and non-immigrant populations was not the aim of the present study, but we now mention its importance in the limitation section as a possible avenue for future research (p. 19).

Expression and editing comments

* page 16, line 19 - there seems to be a word missing in the line
Response: Thank you. We eliminated the word “to” and now the sentence reads (p. 16):
“Taken together, the effects of exposure to violence and to discrimination on SVR invite to take more into account the impact of such human perpetrated adversities in the content and development of violent radicalization prevention programs.”

* page 13 Results section. Some spacing and formatting needed regarding the location of the table. Are tables 1 and table 2 going to be inserted here? If yes, then it needs to be spelled out clearly. If not then reference needs to be made to refer to the tables at the end of the articles.
Response: We added the sentence “Insert Table N here” to clarify the position of the tables in the paper (see p. 13).

* page 13 Mediation and moderation analysis - discussion could be made clearer. A mention made that religiosity not significant, but no further information in this section
Response: The issue of religion is now more thoroughly discussed in the Discussion section (p. 20). In the Results section we clarify that religiosity has a moderating rather than a mediating effect (the order of presentation of results has been changed – moderation results now presented before mediation results).

(see file attached as Response to Editor and Reviewers with the attached Table mentioned above)