**Peer Review and Communication History**

**MS Title**: Social Affect Regulation in University Students During the COVID-19 Pandemic

**Author Names**: John P. Powers, Megan Burnham, Hannah Friedman, & Kateri McRae

**Submitted:** Apr 20, 2022

**Editor First Decision**: Revise & Resubmit before external peer-review

Dear Dr. McRae,
Thank you for submitting your manuscript, “Social Affect Regulation in University Students During the COVID-19 Pandemic” to Collabra Psychology. Editor in Chief Simine Vazire and I have both read it closely. I think that the MS is promising, but Simine and I both feel there is a major issue that needs to be addressed before sending the MS out for review.

The issue pertains to the tests of Hypothesis 2: that “changes in social reappraisal support due to the pandemic will correlate with mental health symptoms” (quoting from your preregistration). You do get four (barely) significant p-values consistent with this hypothesis. But you also get one that is significant in the wrong direction. And, presumably, the remainder (9, if I am understanding the number of tests correctly) are not siginificant in either direction. I just don’t think this is strong evidence one way or the other. Correcting for multiple testing (as you should, since you are running, if I understood this right, 16 tests) nothing is significant. I think you need to update the paper to make this clear and soften the claims around what you found here.

I hope that you are willing to make these changes. If you do, I am happy to send the MS out for full review.

Best,

Yoel Inbar
Senior Editor, Social Psychology

**Author Response**
May 3, 2022

Authors’ revisions and responses to comments from the editors

Dr. Inbar:

We appreciate your prompt and thoughtful comments, and that you think the manuscript is nearly ready for full review. Our response is marked \*\*\*.

**Comments from Editor**

The issue pertains to the tests of Hypothesis 2: that "changes in social reappraisal support due to the pandemic will correlate with mental health symptoms" (quoting from your preregistration). You do get four (barely) significant p-values consistent with this hypothesis. But you also get one that is significant in the wrong direction. And, presumably, the remainder (9, if I am understanding the number of tests correctly) are not siginificant in either direction. I just don't think this is strong evidence one way or the other. Correcting for multiple testing (as you should, since you are running, if I understood this right, 16 tests) nothing is significant. I think you need to update the paper to make this clear and soften the claims around what you found here.

\*\*\*We have now corrected the correlation analyses for multiple testing as you recommended. Therefore, there are no longer any significant correlation results. We have updated the abstract, results, and discussion to reflect this change in results. We have updated the methods to describe the correction procedure, to update the corresponding power computation, and to make it more clear that there are 20 correlation tests in total. Finally, we have updated the supplementary material to describe this deviation from the preregistered analysis methods.

**Editor First Decision**: Revise & Resubmit

July 27, 2022

Dear McRae,

I have now received 2 reviews of your manuscript, “Social Affect Regulation in University Students During the COVID-19 Pandemic”, from researchers with special expertise in emotion and self-regulation. I also independently read the manuscript before consulting these reviews. The reviewers had mostly positive reactions to your manuscript. I agree that your manuscript has important strengths and also that there are some issues that need to be addressed. I therefore encourage you to submit a revised version for further consideration at Collabra: Psychology.

The reviewers did an outstanding job in their reviews. I will highlight issues I think are particularly salient here. In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewers’ comments, outlining each change made in your manuscript or providing a suitable rebuttal.

1. Both Reviewers and I felt that Hypothesis 3 did not fit well with the rest of the paper. Reviewer 1 noted that the rationale for this hypothesis was not well-developed in the introduction, and Reviewer 2 made some excellent points about the challenges of drawing meaningful conclusions from comparisons with normative data. I also didn’t see Hypothesis 3 listed in the pre-registration (see also Point 2), suggesting that if this analysis is included at all, it should be presented as exploratory. However, given the other concerns, I would be inclined to cut Hypothesis 3 from the paper entirely.
2. In some cases, the pre-registration does not seem to match the analyses in the paper, or it is too vague to tell if it matches. For example, the pre-registration says: “We hypothesize that we will again observe a relationship between the frequency of communication with core social contacts and the degree to which these contacts are rated as helpful for reappraisal.” In the paper, this analysis seems to have been done in three ways: for pre-pandemic data, peri-pandemic data, and then for the change between the two. Because the pre-registration is not specific, it is unclear which of these analyses was/were pre-planned. Similarly, the pre-registration specifies that communication frequency will be assessed across multiple modalities, but does not specify whether these will be analyzed separately or as an aggregate. Also, as mentioned in Point 1, I did not see Hypothesis 3 (“mental health systems would not differ from population normative data”) in the pre-registration. To address these kinds of inconsistencies, it would be helpful to clearly divide the results into “pre-registered” and “exploratory” sections.

In summary, I think this is a promising manuscript and, I hope you will revise it for further consideration at Collabra: Psychology. I look forward to receiving your revision. Please see the instructions below for submitting your revision.

Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to re-submission.

If you have any questions or difficulties during this process, please contact the editorial office at editorialoffice@collabra.org.

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Alexa Tullett

# Reviewer 1

##### Rating scale questions

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  | ✔ |  |  |  |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  |  | ✔ |  |

##### Open response questions

### Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

“Social Affect Regulation in University Students During the COVID-19 Pandemic” is an observational study examining how one’s frequency of communicating with their social contacts influences their affect regulation ability, particularly reappraisal. The authors assessed whether the amount of communication with contacts influenced social reappraisal support across several communication modalities, whether changes in social reappraisal support conducted in several communication modalities since the advent of the pandemic related to mental health symptoms, and whether mental health symptoms in the sample would differ from published norms. Participants completed a battery of questionnaires online, reporting their depression and anxiety symptoms, and they described the number and types of contacts they had. The authors found that more frequent contacts predicted higher social reappraisal support ratings, that participants perceived receiving more social reappraisal support after the pandemic began than before, and that mental health symptoms were worse than those of published norms.

I think the work has a few strengths. First, it provides a timely look into the social aspects of affect regulation and helps to increase the literature in that field in a fairly unique manner. Second, it compares social reappraisal support across several different communication mediums (text, voice, in-person, and the person’s overall communication). There was the potential of finding out where social affect regulation might work best, to be replicated in either the laboratory or in a diary study. I also wondered whether it were possible to compare the effectiveness of individual forms of reappraisal with social forms.

I have few issues with the manuscript. The main thing is the following. I see the Introduction of the manuscript contains reasoning behind the author’s first and second hypotheses. However, it was unclear why the authors would believe there would be no differences in mental health symptoms from population normative data. I think the Introduction section would benefit from some added reasoning behind the lack of a difference. Perhaps the authors could draw from the reasoning relayed in section 2.3.3 regarding the context of the pandemic.

Overall, I think the manuscript provides some interesting findings that could generate further questions about reappraisal.

# Reviewer 2

##### Rating scale questions

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  |  | ✔ |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  |  | ✔ |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  |  | ✔ |  |

##### Open response questions

### Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

As far as the writing/narrative, the design and execution of the study, and the statistical analyses go, I have virtuall no comments. The authors have been extremely thorough, pre-registered their hypothesis and exclusion criteria, and all other methodological choices are well justified. The multi-level analysis is both appropriate and contains all of the information I find most important with one minor exception, and claims are largely not overstated. Well done.

The few comments that I have are as follows:

1. The minor exception for the MLM info is that I typically find it best practices with these models to at least state that the assumptions of the model were tested and met, or if not met, accounted for (see Maas & Hox, 2004).
2. My gut reaction to the testing of hypothesis 3 is that it’s almost a little too detached to be relevant here. I understand the difficulties of getting accurate normative data but the number of changes between college students now and those in as far back as 1983 are countless. Attributing any change to COVID seems a stretch in terms of claims. That said, it is consistent with other recent research and I’m not questioning that mental health has deterioriated in young people since the pandemic started. I think a good compromise here would be to leave the analysis as is but to simply note in the discussion that these changes are likely an ammalgamation of changes over several decades of American history.
3. The lack of evidence for hypothesis 3 I think may actually be understated in this paper. The authors discuss how this is atypical compared to previous research and that it is likely due to the extremity of changes that came with the pandemic, but although this makes sense, I don’t believe there is extant evidence that a sufficiently large decline in mental health can render otherwise helpful strategies useless. The exception to that might be in relation to traumatic events which, one could argue, the social isolation of the pandemic was for many people; could be a relevant avenue to discuss but not strictly necessary in my opinion. Instead, I would at least like to see the numbers for these statistical tests as they are completely absent from the manuscript currently. Any type of Bayesian analysis or examination of effect sizes that might be able to shed light on whether this is a power issue or a strictly null effect are warranted here since this was a pre-registered hypothesis.

This is a very interesting and compelling paper, you should be proud of the work you’ve done here!

**Author Response**
Aug 5, 2022

Authors’ revisions and responses to comments from the editor and reviewers

We appreciate the reviewers’ valuable and thoughtful comments. We believe the resulting revisions have substantially strengthened the manuscript. We discuss our responses to each point, in turn, below (our responses marked \*\*\*).

**Comments from Editor**

1. Both Reviewers and I felt that Hypothesis 3 did not fit well with the rest of the paper. Reviewer 1 noted that the rationale for this hypothesis was not well-developed in the introduction, and Reviewer 2 made some excellent points about the challenges of drawing meaningful conclusions from comparisons with normative data. I also didn’t see Hypothesis 3 listed in the pre-registration, suggesting that if this analysis is included at all, it should be presented as exploratory. However, given the other concerns, I would be inclined to cut Hypothesis 3 from the paper entirely.

\*\*\*In response to these comments, we have now moved everything related to Hypothesis 3 from the main manuscript to the supplementary material. We feel this action is more appropriate than removing it from the article entirely since this hypothesis was preregistered (it is the latter component of the first hypothesis in the preregistration). It is now encompassed within Hypothesis S2, more similar to how it was presented in the preregistration. We have also tried to address both reviewers’ concerns through additional revisions to this supplementary material, including explaining our reasoning behind the supplemental hypotheses and noting that changes in students over the past several decades may have contributed to some of the effects we observed. Regarding the rationale for the supplemental hypotheses, in short, we had observed some effects potentially related to the pandemic in our previous study; given that the current study was conducted much later in the pandemic and at a time when restrictions were more relaxed, we expected these effects to be reduced if not altogether absent due largely to habituation and other adaptations.

2. In some cases, the pre-registration does not seem to match the analyses in the paper, or it is too vague to tell if it matches. For example, the pre-registration says: “We hypothesize that we will again observe a relationship between the frequency of communication with core social contacts and the degree to which these contacts are rated as helpful for reappraisal.” In the paper, this analysis seems to have been done in three ways: for pre-pandemic data, peri-pandemic data, and then for the change between the two. Because the pre-registration is not specific, it is unclear which of these analyses was/were pre-planned. Similarly, the pre-registration specifies that communication frequency will be assessed across multiple modalities, but does not specify whether these will be analyzed separately or as an aggregate. Also, as mentioned in Point 1, I did not see Hypothesis 3 (“mental health systems would not differ from population normative data”) in the pre-registration. To address these kinds of inconsistencies, it would be helpful to clearly divide the results into “pre-registered” and “exploratory” sections.

\*\*\*We thank the editor for highlighting these places where we needed to make the manuscript more clear. Regarding Hypothesis 1, while the corresponding hypothesis in the preregistration does not go into detail regarding measures, the analysis plan in the preregistration does provide more detail and specifies that we had planned to use pre-pandemic communication and change in communication as predictors. In a previous set of peer reviews for this study, we received a strong recommendation to present a more comprehensive set of models by also including models with peri-pandemic communication frequency as predictors. We have now made this much more clear in the manuscript. We felt that it would significantly disrupt the flow and clarity of the paper to partition the methods and/or results into whole preregistered and exploratory sections, given that the 2 instances of exploratory analyses are closely related to other analyses that were preregistered. Therefore, we added a sentence to the Analyses section of the methods indicating that any analyses that were not preregistered would be explicitly noted as exploratory. We then added text to the descriptions of the analyses and results to carefully indicate which analyses were exploratory vs. preregistered. Somewhat similarly, where we discuss our computed measures in the preregistration (“Indices” section), we do note that these are computed separately for each communication modality. It’s true we did not explicitly go on to state that these measures would never be aggregated in analysis. This is because one of our modality measures was already explicitly an “overall” communication category that serves the same function as a computed aggregate of individual modalities would have. Since we note at the end of the introduction and describe throughout the methods that analyses were run separately for the different communication modalities, we hope that this provides sufficient clarity regarding this issue, but let us know if you feel additional clarification is needed. Finally, as noted in our response to point #1, Hypothesis 3 was included within the first hypothesis of the preregistration.

**Reviewer #1**

3. I wondered whether it were possible to compare the effectiveness of individual forms of reappraisal with social forms.

\*\*\*We believe the reviewer was discussing ideas for future work with this comment rather than highlighting a point to be addressed in the current study and manuscript. Just in case, we wanted to respond that we agree that this could be a very interesting extension of this work in the future, although we don’t think we have the data to properly address this question in the current study.

4. I see the Introduction of the manuscript contains reasoning behind the author’s first and second hypotheses. However, it was unclear why the authors would believe there would be no differences in mental health symptoms from population normative data. I think the Introduction section would benefit from some added reasoning behind the lack of a difference. Perhaps the authors could draw from the reasoning relayed in section 2.3.3 regarding the context of the pandemic.

\*\*\*See our response to point #1.

**Reviewer #2**

5. For the MLM, I typically find it best practices with these models to at least state that the assumptions of the model were tested and met, or if not met, accounted for (see Maas & Hox, 2004).

\*\*\*We appreciate this reminder for thorough reporting, and we have now added a statement to the description of these analyses in section 2.3.1 regarding meeting assumptions for MLM.

6. My gut reaction to the testing of hypothesis 3 is that it’s almost a little too detached to be relevant here. I understand the difficulties of getting accurate normative data but the number of changes between college students now and those in as far back as 1983 are countless. Attributing any change to COVID seems a stretch in terms of claims. That said, it is consistent with other recent research and I’m not questioning that mental health has deteriorated in young people since the pandemic started. I think a good compromise here would be to leave the analysis as is but to simply note in the discussion that these changes are likely an amalgamation of changes over several decades of American history.

\*\*\*We agree that we were too quick to overlook the potential influence of changes over time in this population in the effects we observed for some of the mental health measures. In response to comments from the editor and both reviewers, everything related to Hypothesis 3 has now been moved to the supplementary material (see our response to point #1 above) and is now encompassed under Hypothesis S2. We feel that this move appropriately reduces the emphasis on these results. Nevertheless, we still followed your recommendation when discussing these results in the supplementary material by noting how they may also reflect changes over time since the normative data were established. In addition, where we first introduce these comparisons with normative data, we further qualify that they were only intended to be interpreted as a rough indicator of atypical deviations on these measures.

7. The lack of evidence for hypothesis 3 I think may actually be understated in this paper. The authors discuss how this is atypical compared to previous research and that it is likely due to the extremity of changes that came with the pandemic, but although this makes sense, I don’t believe there is extant evidence that a sufficiently large decline in mental health can render otherwise helpful strategies useless. The exception to that might be in relation to traumatic events which, one could argue, the social isolation of the pandemic was for many people; could be a relevant avenue to discuss but not strictly necessary in my opinion. Instead, I would at least like to see the numbers for these statistical tests as they are completely absent from the manuscript currently. Any type of Bayesian analysis or examination of effect sizes that might be able to shed light on whether this is a power issue or a strictly null effect are warranted here since this was a pre-registered hypothesis.

\*\*\*We appreciate the reviewer prompting us to report statistics for these correlations. We had removed some statistics related to these results during a previous revision, but we agree that since this was a preregistered hypothesis, the statistical values warrant reporting at least in the supplementary material, even though they did not meet our threshold for significance. We have now added a correlation table of these results to the supplementary material, taking care to note there the corrected threshold for significance and that none of these results met that threshold. We reference this new table in results section 3.1 in the main manuscript. Regarding the interpretation of these results, as you note, we do raise the possibility that negative impacts of the pandemic outweighed any positive effects of social reappraisal. Importantly, we also discuss the possibility that this discrepancy with previous findings could be because social reappraisal is only one component of a larger set of social support behaviors. In other words, we carved out and examined a more granular component of social support, potentially resulting in smaller effects than the larger construct. Related to both of these possibilities, we agree that these null findings may be due to insufficient power as you suggest. When discussing our power analyses, we note that our corrected threshold for significance corresponds to an r value of .24 (i.e., we could detect correlations of .25 or larger). When examining the new correlation table, it is then apparent that we observed a handful of negative correlations (greater social reappraisal support correlating with lower mental health symptoms) approaching .2. Thus, these effect sizes seem to have been a little too small to reach significance in the current study. We have revised some of the language around the discussion of these findings to make this more clear.

**Editor Final Decision:** Accept

Aug 16, 2022

Dear Kateri McRae,

I have now had a chance to read over your manuscript “Social Affect Regulation in University Students During the COVID-19 Pandemic”, along with the letter describing the changes you made. Thank you for your responsiveness to the concerns that the reviewers and I raised. I am happy to say that your paper is now officially accepted for publication in Collabra: Psychology. Congratulations on this excellent work, I think it will make an important contribution to the literature and I look forward to seeing it published! I hope your experiences with Collabra: Psychology have been positive and that you will continue to consider it as an outlet for your work.

As there are no further reviewer revisions to make, you do not have to complete any tasks at this point.

You will be receiving separate correspondence regarding any production and technical comments, data deposits, as well as publication charges. We work with the Copyright Clearance Center to process any applicable APC charges. Please note that your APC transaction must be completed before your article gets published.

You will have an opportunity to check the page proofs before we publish your article. Thank you again for publishing in Collabra: Psychology.

Sincerely,
Alexa Tullett