Dear managing board of PCI Registered Reports,

Thank you for the editorial letter received on 14/01/2023 regarding our manuscript, "Stage 2 Registered Report: Stress regulation via being in nature and social support in adults - a meta-analysis."

In this letter, we provide point-by-point responses to the issues identified by both reviewers. To review our improvements, we provide a manuscript with and without track changes on.

We appreciate the feedback provided by reviewers and believe that their suggestions have greatly improved the quality of our meta-analysis. We hope that the new version of the manuscript will now meet the high standards for Stage II acceptance at PCI Registered Reports.

Sincerely, on behalf of all co-authors,

Alessandro Sparacio

Reviews

Reviewer 1: Dr. Felix Schönbrodt

I just stumbled across one sentence, on p. 13: "After all, large samples have large expected sampling variability, leading to imprecise results". Shouldn't that be the other way round?

Authors' Response: We thank Dr. Schönbrodt for his positive feedback overall and pointing this out. We changed the sentence indicated by the reviewer that contained that typo. We have now rephrased the sentence in the following way on p.13:

"After all, small samples have large expected sampling variability, leading to imprecise results"

Reviewer 2: Dr. Siu Kit YEUNG

Comment 2: The authors excluded effects of d = 2.00 or above. This sounds reasonable, but any justification regarding the cutoff based on past research papers? If not, the author can also state the d > 2.00 cut-off is arbitrary. Also, would be great if the authors add citations for the papers with d > 2.00.

Authors' Response: Yes, the threshold is arbitrary. Given the observed set of effects, we think it provides a good trade-off in that it filters out effects that are way off any expected, realistic bounds and on the other hand is still conservative. In the revised manuscript, we acknowledge that the threshold is arbitrary and also provide results of a simulation that helps illustrate how deviant those excluded effects are. The simulation draws on the observed distribution of effect sizes in social psychology, derived from more than 25,000 studies, models a theoretical distribution that approximates this empirical distribution, and provides a frequency expectation for such large effect sizes:

"d = 2.00 is a conservative, arbitrary threshold. Richard, Bond, and Stokes-Zoota (2003) found that the empirical distribution of absolute effect sizes in social psychology is well approximated by a left-sided truncated normal distribution with $\mu = 0$ and $\sigma = 0.55$. Given

this theoretical distribution, $d > 2.0$ can be seen as highly deviant, with a cumulative density of just .0003 (i.e., representing 0.03% of the distribution)."

Comment 3: I think for Hedges g, as it is possible for the value for go over 1.0, there needs to be leading zeros.

Authors' Response: We thank the reviewer for the feedback, we have added leading zeros for Hedges' g in the new version of the manuscript.

Comment 4: Regarding the risks of biases, error, other research quality issues of the literature, the authors may connect the findings to generally prevalent problems in the Health Psychology literature and call for implementations of Open Science practices (e.g. see Hagger, 2019; Norris et al., 2022; Yeung, 2023)

Authors' Response: We thank the reviewer for bringing attention to the potential risks of biases, error, and other research quality issues in the literature. Our general focus is in social, not health psychology. It therefore feels somewhat arbitrary to us to focus on Health Psychology. Nonetheless, we agree with the author's concerns and advocate the adoption of more stringent research methods, but we restrict ourselves to stress regulation on Page 35 of our manuscript, and then briefly relate this to social psychological research in general:

"The issues of high risk of bias, publication bias, and low power are not limited to emotional social support and being in nature and stress. These shortcomings extend to other stress-regulation strategies as well (see e.g., Goessl et al., 2017; Sparacio et al., 2022). Given the pervasive problems related to risk of bias, publication bias, and low power, we support others' suggestions of implementing pre-registration or, preferably, Registered Reports that are reviewed before data collection as a means of obtaining more rigorous and reliable evidence on stress-mitigation interventions, as well as in social psychology more general."

Comment 5: The authors may consider adding a section on Open/Meta Science practices in Health Psychology, future directions for improvement, and/or future Open/Meta Science work needed (e.g. checklists, error/reproducibility check by journals, as in *Meta Psychology*, more assessments of different health psychology literatures)

Authors' Response: We thank the reviewer for his suggestion. On Page 40, we added a new section named "How to further improve the evidence base in stress-regulation research" in which we provided a checklist to verify to what extent the manuscript adheres to open science practices:

> The evidence base surrounding stress regulation would benefit from enhancement in quality, but such a sustained effort requires changes in researchers' workflow. Based on the CORE Lab Lab philosophy (Goncharova et al., 2022), we recommend a number of steps: First, researchers should decide, before data collection, to what extent their research is exploratory or confirmatory. They can then adopt an exploratory (https://osf.io/96vw4/) or confirmatory (https://osf.io/mzg4q/) research template (both of which are imperfect, as no research is fully exploratory or confirmatory). If the research is mostly confirmatory, they can then decide to pre-register by themselves (minimum) or submit as a Registered Report (preferable). If the research is exploratory, they can decide to withhold part of the data for cross-validation. If part of the data is withheld, certain journals are willing to accept this as a Registered Report (see e.g., Wittman et al., 2022 as example of this approach). If confirmatory, before data collection, researchers should prepare their analysis script. After data collection, they should post their analysis script and explain

deviations, if any. Whether exploratory or confirmatory, with the post-data analysis script, they should post the deidentified data, to the extent ethically possible. Before publication, (at least) one researcher should be invited to do code review (who then becomes a co-author of the project). To aid authors in their efforts to embrace this approach, we have prepared a very rudimentary checklist (Table 3) that may be used as baby-steps towards promoting best practices in Open Science. For the underlying reasons for this approach, see https://psyarxiv.com/6jmhe/."

Section/topic	Item#	Checklist item
Confirmatory vs exploratory	2	Choose whether the research project is mostly exploratory or mostly confirmatory.
Pre-registration	1a	Clearly state whether the study was pre-registered or not. If pre-registered, provide the name of the registry (e.g., PROSPERO/OSF) and registration number (if applicable).
Registered Report	1b	Clearly state whether the study was conducted as a Registered Report or not. If yes, provide the name of the registry (e.g., OSF), registration number (if applicable), and link to the finalized Stage I Registered Report.
Open data, code, and materials	3	Provide open data (to the extent ethically permitted) with reproducible code and open materials, stored in an open repository (e.g., PsychOpen CAMA; Burgard et al., 2021).
Code review	4	Ask an independent researcher (preferably outside your lab) to review your code and offer that person authorship.

Table 3: Non-exhaustive checklist to start improving Open Science practices

Comment 6: Regarding the weak effects or null findings (with publication bias correction) for social support, I suggest discussing the following further:

i) How are the findings related to or different from the literature of social support, mental health, mental health conditions, and subjective well-being? For example, you may discuss similarities and differences between mental health, mental health conditions, subjective well-being, and stress, in terms of definitions, operationalizations and measurements, as well as the possible relationships/lack of relationships with social support

ii) How are stress related outcome variables or stress related studies included in this meta-analysis, different from the outcome variables of other meta-analyses that investigated the association between social support and

stress (e.g. Chu et al., 2010; Harandi et al., 2017; Thorsteinsson & James, 1999)? For example, correlational versus interventional?

iii) The above suggestions and questions may also apply to the associations between nature exposure and stress, and nature exposure, subjective well-being, mental health, and mental health conditions.

iv) Apart from adding paragraph(s) about these issues (which would be great), the authors may consider adding a table and/or a figure to facilitating explanation and illustrations.

v) The authors discussed some methodological, data reporting, and error explanations for null finding of emotional social support on stress, which are reasonable, but any other plausible explanations for these findings?

vi) The authors may discuss further regarding future research directions on the potential effect of emotional social support on stress regulation, mental health conditions, mental health, and psychological well-being?

Authors' Response:

We thank the reviewer for these questions. We answer them through bullet points:

I & III) We see the reviewer's point. We tried to come up with an answer that satisfied Points I and III, but after several rewrites, were unable to do so in a succinct but comprehensive manner. Let's take mental health. Just for depression alone, measurement is extremely heterogeneous (see e.g., Fried, 2017). Making a comparison between a literature where there is little consensus on the core concept with the stress literature is therefore very hard to do. Similarly, for well-being, Diener et al (2009) already cite five "new" (in 2009) scales (while similar measures exist that were developed by Kahneman, or on similar constructs, like life satisfaction; Cantril, 1965). The measurement of depression alone will take a chapter to address; to compare depression with stress therefore goes beyond the scope of our article. While we were working through our answer, we did think that learning from these literatures can provide promising avenues for these literatures, as there are some highly powered (cross-sectional) studies that could provide some leads. So, instead of addressing these two questions directly, we have relied on these studies to provide future avenues at p.39 in a new section called "Moving forward: Learning from related constructs". We hope that this works for the reviewer as well:

> In the present meta-analysis, we have focused exclusively on studies investigating the effects of interventions to reduce stress and we don't find effects for emotional social support and stress, and some effects for being in nature and stress (but at high risk of bias). We could potentially learn from closely related constructs, such as well-being and mental health, where some highly powered cross-sectional studies were conducted and could be informative for future experimental studies on emotional social support. In relation to social support and well-being, Golden et al. (2009) find that lacking an embedding into a social network and experiencing isolation are associated with lower levels of well-being. As it pertains to being-in-nature, Soga et al. (2021) find that the frequent use of greenspace and having a green window view from within the home was associated with increased life satisfaction and happiness (both factors that contribute to subjective well being).

Furthermore, stress may be a contributing factor to mental health, it is again but only one aspect. Mental health, again, is a multi-faceted construct, including aspects such as anxiety and depression (which itself is already very heterogeneous, see Fried, 2017), both of which were assessed in our meta-analysis. In terms of social support, a cross-sectional study of 461 participants found that people with no depression experienced significantly more social support from friends and parents than people with moderate to severe levels of depression (Alsubaie et al., 2019). In terms of being-in-nature, viewing a greenspace was associated with lower depression, anxiety, and loneliness (Soga et al., 2021).

While our meta-analysis only focuses on stress and its affective consequences, these highly-powered studies are promising for a literature that has high risk of bias and – for emotional social support – no effect. Based on the cross-sectional studies we cite here, it may well be possible to design a high-powered, pre-registered, experimental multisite study to test the potential effects of emotional social support rigorously (see also Sparacio et al., 2023).

II) We thank the reviewer for pointing to these meta-analyses. The first and most important difference between these meta-analyses and ours was a difference in analysis strategy, while the focus of the meta-analyses were really different, which we now highlight in Footnote 12 on Page 36:

A reviewer pointed out that previous meta-analyses seemed to suggest positive effects of social support (e.g., Chu et al., 2010; Harandi et al., 2017; Thorsteinsson & James, 1999). However, none of these meta-analyses adjusted for publication bias, nor did they remove statistically faulty studies, nor did they make an assessment of risk of study bias. In addition, all of these meta-analyses focused on social support in general (not on emotional social support specifically), with one focusing on well-being (Chu et al., 2010), another on mental health (Harandi et al., 2017), and another only on physiological measures of stress (Thorsteinsson & James, 1999).

IV) We appreciate the valuable suggestion provided by the reviewer to include a table in our meta-analysis. However, after careful consideration, we believe that the inclusion of the suggested table would result in an excessive amount of information and detract from the overall clarity and focus of our study. Instead, we have taken other measures to address previous meta-analyses in our field in our manuscript. Specifically, we have included a footnote on Page 36 that provides a brief overview of the differences between our analysis and previous studies, thus acknowledging the existing literature on this topic. We believe that this approach allows us to maintain the necessary balance between providing a comprehensive overview of previous research and presenting our findings in a clear and focused manner.

V) We appreciate the reviewer's comments and suggestions for alternative explanations. The best answer in this case is: We don't know. At present, the most probable explanation for the lack of effect is the limited statistical power and high risk of bias in the included studies. As we highlighted in our manuscript, the results of our meta-analysis suggest a need for further research to be conducted on this topic, including the use of Registered Reports to address the issue of underpowered and biased studies. We feel that once these improvements are implemented, we can further discuss alternative explanations for null effects, if they are to be present in the literature.

VI) We thank the reviewer for this insightful comment and agree that discussing potential future research directions related to emotional social support and its impact on stress regulation, mental health conditions, mental health, and psychological well-being would be beneficial. Specifically, exploring the potential moderating factors that may influence the strength of the association between emotional social support and mental health and well being could be meaningful. A correlational study by Park et al., (2013) of 1027 adults found seemingly robust associations between perceived support and health, but tested a number of additional three way interaction hypotheses for which the authors did not have sufficient statistical power (including levels of neuroticism, life stress, and culture). First, a good way to address such exploratory questions in the absence of clear hypotheses is by using (conditional) random forests to analyze the data (see e.g., Wittman et al., 2022; Szabelska et al., 2023), as we highlighted at p. 34:

"Most cross-sectional studies studying complex interactions are underpowered and need to increase power. However, even when studies are cross-sectional, causal inferences can be improved, even with modest samples. There are reasonable solutions to such "small N, large P" problems. We recommend to use machine-learning methods to 1) reduce potential confounds by adding predictors and 2) to better extract patterns from data by using random forests to identify the most important predictors of the projected outcome (cf., Wittmann et al., 2021; for a tutorial, see Szabelska et al., 2022). Once identified, researchers can generate relatively precise predictions to be tested in either longitudinal designs or experimental studies to bolster the strength of the causal inference".

Comment 12: Regarding age distribution of studies, I wonder how many % of studies consist of substantial proportion of elderly participants (60 years or above, or 65 years or above)? In my recent Open/Meta-Science assessment (Yeung, 2023) of the health message framing literature (a Social-Health Psychology topic), only ~10% of studies consist of substantial elderly samples. This may have theoretical and practical implications as emotional social support may be more effective for older people, based on Socioemotional Selectivity Theory by Carstensen et al. (1999), and it seems plausible that older people would be more engaged in nature exposure interventions, but I am unaware of related evidence (let me know if there is).

Authors' Response: We agree that this may be an important consideration. The data reported in the primary literature, however, makes it very difficult to examine that. The issue is that studies tend to report only the mean age (and SD) of their sample but do not usually report the age composition by say quantiles, deciles, etc. Therefore, our meta-analysis worked with mean ages extracted from the studies. In the revised manuscript, we reflected on this issue (see also our response to comment 14 below). To best approximate the distribution of mean age in the included studies, here are the age deciles for both types of effect. For instance, 40% of samples collected in Being in nature studies had a mean age of 22.50 or less.

Being in nature

0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
19.91	21.30	21.50	22.20	22.50	29.60	32.00	33.68	34.94	40.90	47.30

Social support

0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
18.30	21.58	26.61	35.17	38.56	39.37	40.19	42.99	49.75	59.88	71.70

This is probably the closest description of age composition that can be derived from the data reported in the literature. We tested the moderating effect of age formally, using a moderation analysis and report the results in the revised manuscript. Please see our response to Comment 13. Also note that some of the numbers for the mean age and proportion of female changed slightly – during the triple check of the code, we found out that the original means/proportions were not weighted properly. The changes, however, do not lead to any substantive changes in the substantive interpretations.

Comment 13:Up to the authors and the editor, not necessary, but may be worthwhile to run an exploratory non-pre-registered moderator analyses for age.

Authors' Response: Thank you for this suggestion. Sure, examining the moderation effect of mean age is something that readers definitely may want to see. We did find a moderation effect of mean age in being in nature. Here, the intervention had a larger effect size for older samples when examining the effect of being-in-nature on reducing stress. No such effect was detected for social support. The results are reported in the respective parts that are devoted to the analyses of heterogeneity by population characteristics.

> "We also did not find a moderating effect of mean age of the sample for the effect of emotional social support on stress, (B = -.002; p = .31). … Neither gender, age, or type of population could thus explain the heterogeneity (and thus the lack) of the effect."

> "There was also a significant moderating effect of age of being in nature on stress, (B = -.01, p = .003), where in older samples, the intervention yielded a larger reduction in stress, on average. Although sampling is rarely representative, the moderating effect of gender and age could at least partially explain the heterogeneity of our effects."

Comment 14: Having a separate "Future Research Directions" section would be great, in terms of addressing research gaps (e.g. more studies with elderly samples, more studies with non-mid-to-high-income-country samples), improving reporting, transparency and reproducibility of research in the literature. I know some of these issues are already discussed in the submitted manuscript, but having a subtitle and a section that discuss these issues would be clearer.

Authors' Response:

We thank the reviewer for the suggestion. We added a section in the newest version of the manuscript called "How to further improve the evidence base in stress-regulation research" in which we provided a checklist to improve transparency and reproducibility of research in the literature.

We also followed the reviewer's suggestion whereby social support may be more effective in an older population on page 36:

"Furthermore, future studies on emotional social support should include older samples, which are currently undersampled, while age could potentially moderate the effects on being-in-nature"

Comment 15:Regarding benefits of Registered Report format, the authors may cite Soderberg et al. (2021).

Authors' Response: We thank the reviewer for the suggestion, we cited the study of Soderberg et al., (2021) at page 35.

Comment 16: "3 in the United States, 1 in Poland, 1 in Malaysia, 4 in the United Kingdom, 1 in Japan, 1 in Finland, 1 in Germany, 1 in China, 1 in the Netherlands, 1 in South Korea, 1 in China, 1 in Italy, and 1 in Denmark" (p. 10) - 1 in China or 2 in China?

Authors' Response: We thank the reviewer for spotting the mistake, we removed the repetition in the revised version of the manuscript.

Comment 17: "there was a slight effect of gender" (p. 9) – please specify the direction of findings

Authors' Response: We specified the direction of the effect at page 25:

"We found a small and negative relationship between gender and the efficacy of being in nature in reducing stress ($B = -.01$, $p < .001$), meaning that the effect of intervention was stronger for women as compared to men (while we again point to problems in relation to representative sampling). No gender effect was detected for social support."