

Peer review process documentation

of the article

Between Public Engagement and Discursive Repression:

Media-Related Communication Risks for Scientists in the Context of

Socio-Ecological Crises

by

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Review Round 1

Comments by Reviewer A

Dear authors,

Thank you for the opportunity to read your interesting and timely paper. This paper makes a valuable contribution to the growing literature on science communication, the role of scientists given the climate and nature crisis, and the reasons for and consequences of scientist advocacy. You make a good case for considering the role of discursive repression in how scientists determine whether to put their heads above the parapet and contribute to public discourse. The concept is not something I had come across before and find it to be a useful concept for describing this phenomenon. In particular what I found good about the paper is that it is well written, easy to follow, and you make a strong case for considering the negative impact of this repression on scientists' willingness to speak. As we know, attacks on science and scientists are increasing, and scientists need to feel confident and supported to contribute to public discourse.

The data collected and analyses conducted are appropriate for the exploratory nature of the study, and support your conclusions (with some caveats regarding level of detail – see my comments below on what can be improved). The method and quantitative analysis and results are presented well and are largely easy to follow.

I am happy to recommend this for publication once you have revised the manuscript in line with my suggestions below. Some of these are more significant while others are minor. Overall, I think the manuscript makes an important contribution, and with these revisions it will sit well within the advancing field.

Major Qualitative analysis

The qualitative analysis is underdeveloped. It is mentioned in 2.2.9 but there is no detail on how these open text responses were analysed, how many responses were received, the amount of detail etc. Later, it is mentioned again on line 362 to support the need for organisational backing directing the reader to the Supplementary Details, and again in the Discussion. The level of detail here is not sufficient to support the points. This is compounded by the fact that the open text data is not available publicly. If this is for reasons of anonymity, then I would suggest going through the data and anonymising the responses before sharing, or at the very least sharing more detail (e.g., themes with illustrative quotes if thematic analysis was used). Given the current level of detail regarding the open text responses and the analysis, this is not sufficient to support the claims. You could also include a couple of illustrative quotes if they provide a particularly good example of what participants were calling for, especially if it connects with some of the reasoning behind choosing to speak out or not (only if present of course).

Script for Analysis

The script used for running the analysis is also not present at this time. It is listed but cannot be downloaded. Please double-check and confirm that this is available for download. At present the data is available which is good, but the script is not accessible.

Ethics

Ethics? Who approved the study? This will need to be present in the final version.

Literature

A number of my comments concern the positioning of the manuscript within the recent literature. The authors suggest that relatively few studies have examined scientists' own experiences in engaging in public discourse. While this was largely accurate several years ago, there has been a growing body of work since 2021 that speaks directly to these issues. Alongside Dablander et al.'s work on factors motivating engagement, several studies have examined how scientific identity shapes scientists' emotional responses, perceived constraints, and sense of professional risk when communicating about climate change. This includes survey-based work on how core scientist identity values influence willingness to engage, as well as qualitative research that explores scientists' lived experiences of managing the perceived tension between scientific norms and public advocacy. Some of this research has also taken an ethnographic form, providing insight into the day-to-day strategies scientists adopt to reconcile these roles. This literature would help connect the author's discussion of discursive repression and psychological strain as this literature helps illuminate how scientists actively negotiate and mitigate some of the very pressures the authors describe.

For example, recent survey-based work has examined how scientific identity shapes willingness to engage, highlighting the importance of particular scientist identity constructions for whether and how scientists choose to speak out (Finnerty et al., 2024b). It also, along with (Dablander et al., 2024) who you cite regarding the risk of advocacy politicising science, finds age to be positively associated with engagement, which supports your finding regarding tenured scientists feeling more able to speak out.

In Section 1.2 Ivory Tower versus Activism Dilemma, given that the paper frames "ivory tower versus activism" as a central dilemma capturing the tension between choosing not to speak and being seen as detached versus speaking out and being seen as compromised, I think it is important to incorporate work that has examined this tension directly. This has been explored via interviews with scientists, setting out this dilemmatic framing, and documents the varied ways scientists choose to engage in public discourse while preserving their scientist identities and credibility (Finnerty et al., 2024a).

I think these papers regarding how the scientist identity shapes engagement and the strategies use for navigating the dilemma in particular are important for this paper to engage with, but here are a few more below that I think would help better situate the paper within the wider literature. I have included these suggestions because they fill specific conceptual gaps the authors themselves identify. In my view, incorporating this work would strengthen the manuscript's theoretical clarity and ensure it reflects the current state of research on scientists' experiences of public engagement and how scientist communication is situated within the broader literature. Feel free to use some or all of it as you see fit.

Regarding arguments for institutions supporting scientists engaging in public discourse see (Perrin et al., 2025) and (Gardner et al., 2021).

Regarding how early-career climate scientists see their roles as communicators (van Eck, 2023).

More broadly, you speak to credibility and public trust in science. You may find the following recent review useful for these purposes *Climate Advocacy And Activism By Scientists: A Narrative Review* (Finnerty et al., 2025) as it synthesises the literature from the past few years on what scientists are doing regarding advocacy and activism, why they are doing this, and what the consequences are for public trust in science, how their peers see it, and what the consequences are for the scientists themselves.

Recent qualitative and ethnographic work has also examined how these dilemmas are negotiated over time, offering insights into how scientists manage identity, credibility and risk across transitions into more visible forms of engagement (Finnerty, 2026).

Regarding whether scientists should or should not remain neutral, see this recent perspective (van Eck et al., 2024), which sits nicely alongside pieces by (Oreskes, 2020) and (Fossen, 2025). Van Eck et al here challenge the assumption that scientists should avoid value expression in public communication which may be useful regarding expectations around neutrality.

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Minor

Line 244. You mention frequency of interviews in traditional media or on social platforms. I assume you mean posts with regards to social media, needs correcting.

Table 1. You might consider standardising the variable means and standard deviations to a shared scale, as there are multiple scales present in the table making it difficult to parse at a glance without referring back to the methods sections. I understand that the main aim to is communicate the relationships, but this would help.

Comments by Reviewer B

Thank you for the opportunity to review this manuscript, which examines how scientists experience their own public engagement on environmental issues. The manuscript shows that in addition to positive experiences, scientists report negative experiences, such as personal insults, physical threat and perceived reputational harm (in its entirety termed “discursive repression”), which can ultimately reduce their willingness to engage in public discourse. The study is timely, draws on an interesting sample, and is highly relevant in today’s political landscape. However, I have several substantive and methodological comments that, in my opinion, would improve the quality of the manuscript.

1. **Line 55/56:** Could you please clarify what type of advocacy is being referred to here? While the example is interesting, it would benefit from a bit more background explaining its relevance and what it is intended to illustrate. Alternatively, you might consider omitting the example if it does not add essential information at this stage.
2. **Line 59:** Please clarify what you mean by the “blurred” relationship in this context and how these examples illustrate this blurring?
3. **Line 73:** The rationale for introducing COVID-19 at this point is unclear. It is not evident why the COVID-19 pandemic is repeatedly used as a comparison when the manuscript focuses on climate injustice. If the concept of discursive repression is primarily derived from prior research conducted on societal phenomenon during the COVID-19 pandemic, this should be stated explicitly. Alternatively, if COVID-19 is being framed as part of or analogous to climate injustice, this connection should be clarified.
4. **Starting Line 123:** This section would benefit from a bit more contextualization of the repression literature. You draw on relevant work from several fields of repression research. However, to improve clarity and conceptual grounding of your work, it would be helpful to include a few sentences situating discursive repression more explicitly within the broader repression literature, particularly given that in your Discussion you emphasize expanding our understanding of repression in democratic regimes.
5. **Line 171:** Building on my previous point, you cite Marheinecke et al. (2024) in relation to scientists and discursive repression, although their study focuses on state-driven repression and does not specifically examine scientists. While I agree that the concepts are related and see the relevance of this work in the context of your study, it would be helpful to clarify that this literature comes from a related field and to explain more explicitly how insights from state-driven repression can be extended to scientists’ experiences of discursive repression.
6. Overall, the introduction would benefit from a more focused and clearly articulated framing, especially in leading up to the variables assessed and tested in the study and the focus of the discussion.
7. **Line 249 / Line 271:** Please clarify whether validated scales were used. If not, how were the positive statements and reputational harm statements developed? Additionally, please clarify whether items with factor loadings below 0.5 were dropped entirely from the analyses after the PCA.
8. The Methods section is missing a description of the statistical analysis plan. Please clarify, at the end of the Methods section, which analyses were conducted and for what purpose, how missing data (if any) were handled, and which tests or diagnostics were used to assess model assumptions and fit. Additionally, please specify which analyses were confirmatory in order to test your hypotheses and which were exploratory or post-hoc.

9. Building on my previous point, the beginning of the Results section is somewhat confusing regarding when correlations are reported versus when inferential statistics are presented, and it is not always clear why some relationships are tested while others are not. While the correlation table is relevant, inferences such as one relationship being “higher than” another cannot be drawn from simple comparisons of correlations alone. You might consider first presenting descriptive statistics, followed and clearly separated by your hypotheses (and sub hypotheses) testings.
10. **Figure 1:** Consider using a higher resolution for this and all following figures. Furthermore, the figures are missing a note explaining the figure in more detail, as figures should be able to stand on their own. Please clarify, e.g., what comprises insults, positive experiences, ... and what a violin plot displays.
11. In **Line 318** you state that “these results support Hypothesis 1: A considerable proportion of scientists who take a public stance on socio-ecological crises report experiencing repressive responses.” However, the result you refer to appears to be based on the proportion of statements resulting in negative experiences rather than the proportion of scientists reporting such experiences. While this is an interesting finding, to adequately address your hypothesis, please also clarify the proportion of scientists who reported negative experiences, as opposed to the number of negative experiences reported overall.
12. **Line 326 second hypothesis:** It appears that you are primarily reporting bivariate correlations, which are not sufficient to answer your hypothesis. To do so, you should consider using inferential statistics that control for intercorrelations and other relevant variables. However, if you intend to address the second hypothesis using your mediation analysis, then the mediation model should be used consistently to test (and report on) the hypothesis, and reporting correlations at this stage may not be necessary.
13. **Line 362:** The reporting of the qualitative findings appears somewhat unexpected. It would be helpful to clarify in the Methods section how these qualitative findings are incorporated into the study design and results. Additionally, please explain whether and how these findings inform your suggestions and implications in the Discussion section.
14. **Figure 2:** Please use a higher-quality version of the figure and adjust the font size. In addition, consider using boxplots rather than bar plots, as the figure appears to display means with standard deviations rather than frequencies; boxplots would better illustrate the underlying distributions.
15. In the first paragraph, focus on reporting your results more straightforwardly before moving to interpretation (e.g. “These patterns suggest a complex landscape in which public engagement can be both rewarding and demanding for scientists (Nogrady, 2021; Das NETTZ et al., 2024)”), In addition, the mediation findings seem to not be reported in this paragraph, even though they are highly relevant to your results.
16. **Line 420:** “Although positive engagement outcomes were more frequent than negative ones among our respondents, it appears that negative experiences (especially those threatening one’s reputation) carry more psychological weight and disproportionate influence on future engagement decisions. Even a single serious negative incident can have a chilling effect that outweighs many positive aspects.”: I am not sure where this conclusion is coming from. You report a mediation analysis involving negative experiences, but I do not see a comparison with positive experiences with respect to future engagement decisions. Please clarify whether such analyses were conducted, or revise the wording to reflect only the conclusions that can be supported by the presented data.

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17. **Line 455:** Your finding that tenure is positively associated with public engagement supports this argument, consider adding this to strengthen your argument.

Comments by Reviewer C

Thank you for the opportunity to review the manuscript “*Between Public Engagement and Discursive Repression: Scientists’ Communication Risks in the Context of Socio-Ecological Crises*.” The authors present a survey ($n = 138$) of scientists from German-speaking countries on their experiences with public engagement in traditional and social media, focusing on perceptions of reputational harm, psychological strain, and the role of institutional support in this context. The findings indicate that the surveyed scientists generally report positive experiences, particularly when communicating through traditional mass media. Yet, they also report negative experiences (e.g., insults, reputational threats), especially on social media. Moreover, a lower willingness to engage is associated with insults; this relationship is mediated by perceived reputational harm. Furthermore, the direct negative association between reputational harm and willingness to engage is attenuated when scientists perceive strong organizational support. The manuscript offers interesting insights into a particular case of scientists engaging in public discourse on climate change and biodiversity and clearly holds potential. In its current form, however, the paper would benefit from a comprehensive revision to sharpen its central arguments and provide transparent insights into its methodological approach. The points outlined below are intended to help the authors improve the overall coherence and impact of the study.

1 Introduction

While the authors have effectively situated their study within the existing literature and clearly articulated the research gap, I have a few suggestions that could further enhance the clarity of the manuscript. The introduction argues that trust in science is increasingly being publicly challenged (p. 3, l. 49). However, current survey data show that this decline in trust is not observed in Germany (Wissenschaft im Dialog 2025; Reif et al. 2025), where most of the scientists surveyed are affiliated. Nevertheless, particularly in the case of climate change, the authors could briefly address climate change skepticism online in order to describe the environment of scientists’ public online engagement in more detail. (e.g., Treen et al., 2020) (p. 3, ll. 58-59). For the context described as “socio-ecological crises”, it might be useful to mention that these issues often intertwine with political issues, described in the literature as “science communication as political communication” (Scheufele, 2014), which might pose challenges to the communicating scientists addressing these issues.

Moreover, I was wondering whether it is really the case that previous studies often solely focus on the public perception of scientists’ engagement (p. 3, ll. 62-63), since there are several studies that deal with perceptions of public engagement of scientists in general (Calice et al., 2023, Kessler et al., 2022) and climate scientists (van Eck, 2023; Cologna et al., 2021). I suggest that the authors more clearly state their focus on perceptions of insults and reputational threat emphasizing the research gap they are addressing with their study.

It does not become entirely clear to me how the paper can contribute to position scientists as environmental actors within socio-ecological systems (p. 3, ll. 87-90). This should be explained in more detail.

For the theoretical background, it might be useful that when the authors refer to populism when introducing the concept of discursive repression (p. 4, ll. 107-112), to mention the concept of science-related populism (Mede & Schäfer, 2020). With regard to discursive repression, I think the manuscript would benefit from a clearer definition and more detailed description of the concept from the beginning. Furthermore, reading the article also raises the question for me of whether it is possible to make statements about discursive repression (macro level) based on the

study design. The suitability of the concept should be more justified and how it is operationalized in the study (2 Methods).

When referring to the “chilling effect” (p. 5, ll. 149-155) in the context of discursive repression, it would be worthwhile to consider the findings of Peters (2025) and Nölleke et al. (2023).

The authors could also go into more detail in the theoretical background on the new possibilities offered by social media for scientists to directly engage with the public on these platforms (e.g., Biermann et al., 2023), as the study differentiate between public engagement experiences in traditional media and social media (1. 3 The Role of Media in Science Communication).

Hypotheses

While the overall research design is suited to address the overall research question. The hypotheses could be more precise. It is questionable whether the study is permissible to make statements about the association of the politicization of public discourse with increased self-3

censorship (Hypotheses 2, p. 6, ll. 205-207). I would recommend to use in the formulation of the hypotheses, the constructs actually measured.

o better contextualize the results of the exploratory multiple regression analysis (ll. 11, p. 369) and to make the procedure transparent, it can be useful to formulate an explicit exploratory research question in addition to stating hypotheses (p. 6).

2 Methods

With regard to the sampling procedure, the institutions through which participants were recruited need to be described in greater detail (p. 6, ll. 210-213). Furthermore, the selection criteria should be more thoroughly justified. This also includes to define the term “scientists” precisely, since apparently not only researchers from research institutions were included but a very broad definition was applied.

The scale for the frequency of interviews and social media posts is ambiguous, as 0 = never and 1 = once a year or less was used for frequency in the last 12 months (p. 7, ll. 245-248). Less than once a year therefore means never in the last 12 months.

With regard to willingness to give interviews/posts on social media (p. 7, lines 292–294), I wondered why willingness was not calculated separately for interviews and social media, since negative experiences on social media may only lead to people no longer posting on social media, but not necessarily to them no longer giving interviews. Reasons for this decision should be given and it might be necessary to state this operationalization of willingness as a limitation of the study.

Moreover, I would recommend the authors to share the full questionnaire for transparency reasons on OSF or in their supplemental material.

3 Results

The analysis strategy and the structure of the result section is suitable for addressing the hypotheses. As outlined, I would suggest to introduce an exploratory research question for the further exploratory results. In my opinion, this would make it even easier to follow the structure of the results section.

The findings that participants reported expressing opinions more frequently on social media than in traditional media (p. 9, ll. 308-312) aligns with previous research on (visible) scientists' social media communications. I think it could be worthwhile to discuss how the 4

expression of opinions on these platforms might make scientists more vulnerable to attacks on social media in the discussion section (see also Zhang, 2025).

I also stumbled a little over the presentation of the results, in that Figure 1 combines willingness to engage in interviews in traditional media and post on social media as one (see my comment below).

The explanations regarding organisational support remain somewhat unclear, as they refer to qualitative results whose analysis have not been described (p. 11, ll. 362-368). I would like to see the procedure explained in more detail in the methods section. How many respondents gave those qualitatively insights, are they based on the 19.2% of the respondents reported receiving active support from their organizations (p. 11, ll. 361). Furthermore, it should be discussed whether the low level of support is also related to the fact that negative experiences were generally hardly perceived by the respondents and with a large standard deviation between them (p. 11, ll. 315-320).

4 Discussion

In the discussion section, it might be useful to emphasize in more detail what can be learned from this study for further contexts, as the study is rather a case study in nature. It should be made clearer that the results are limited to a very specific group of scientists. Moreover, due to the relevance of "socio-ecological crises", scientists communicating in this context might be more exposed to negative experiences than scientists who communicate about less controversial topics.

I think some of results can also connected more with existing research in the discussion section (e. g., Mede & Volk, 2025). For example that negative experience not systematically correspond with a reduced willingness to engage publicly (p. 11, ll. 333-335) aligns with the findings of Nölleke et al. (2023) in the context of the Covid-19 pandemic.

Moreover, the reference to the false balance literature seems somewhat out of context to me and, in my opinion, needs to be better linked to the actual results of the study (p. 14, ll. 435-438).

In addition, I would recommend to state in the limitations that no further distinction was made between online and offline in terms of willingness (see comments below). 5

Moreover, it should be discussed to what extent the results are transferable due to the very specific group of scientists, since only scientists in the narrow sense were surveyed.

Open science

The authors mention that the study was not pre-registered because it was conducted as exploratory research. With regard to the formulation of hypotheses, it is questionable to what extent the results are actually purely exploratory in nature.

While the authors state in the manuscript that ‘all elements and data are available upon request’ (p. 8, line 298), they have in fact made the data available on OSF. The availability statement should therefore be amended.

Minor points

- It should be made clearer in the abstract that the surveyed scientists were rarely exposed to insults. (Abstract)
- I would recommend to use throughout the manuscript the terms “social media platforms” or “social media” instead of social platforms because these terms are more commonly used.
- When describing the operationalization of frequency of interviews/posts, there is missing “post” before social (media) platforms (p. 7, ll. 244-245).

I hope the points outlined above will help the authors to improve their paper further.

References

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<https://doi.org/10.1080/1369118X.2025.2565313>

Comments by Editors

Thank you again for submitting your manuscript for publication in EPO! We believe that your research is timely and can be an important contribution to the field. This notion is also reflected in the reviewers’ comments. However, we are in agreement with the reviewers’ suggestion of revisions before publication. We ask you to consider every comment and justify when you do not consider a comment for your revisions. In the reviews, the referenced lines in the manuscript are based on the numbering of the anonymized version of your submission. We attached this file for you. Below, we stress some specific comments from the reviews and add some additional comments:

1. Thank you for generally following our manuscript template. However, a German version of the abstract, keywords, and impact statement is currently missing. Please add them in the revised submission. In addition, you currently have six keywords, while the limit is five. Please revise them accordingly.
2. There is a slight inconsistency or inaccuracy between the abstract and the impact statement about the role of institutional support. In the abstract you clearly refer to a mediation effect, while the wording in the impact statement could also be interpreted as a main effect of institutional support. Please align abstract and impact statement in that regard.
3. We agree with the reviewers that the introduction could be more concise in some parts (e.g., the first paragraph or in the third paragraph) and would benefit from discussing more of the recent literature on the topic. Especially reviewers A and C provide some literature suggestions that could be helpful.
4. In your open science statement, you frame your analyses as exploratory, yet you formulate hypotheses in the introduction. Please explain why this is the case in Section 1.4. It might be helpful to frame the hypotheses as exploratory or formulate competing hypotheses.
5. In Section 2.1, we are missing a rationale for your sample size or any post-hoc power assessments. Why are 138 participants sufficient for your study? What drove your decision to stop sampling?
6. You repeatedly use Cronbach’s α as a measure for internal consistency for two-item scales. Please use Spearman-Brown coefficients instead. In general, it would be good to not only present Cronbach’s α but also McDonald’s ω .
7. The mean of the frequency of interviews in Table 1 (1.57) does not match the mean presented in the text (1.56); the same applies to positive experiences. In addition, we were wondering whether the correlations of psychosomatic complaints with insults, physical attacks, and reputational harm are really not significant as indicated in Table 1? Please also use italics for abbreviations of statistic parameters such as M , SD , n , and p throughout the manuscript, and check for which statistics zeros before the decimals are used and when there are no zeros before the decimals (e.g., unstandardized vs. standardized regression coefficients).
8. Why are the variables positive experience with traditional media and with social media not included in Table 1?

9. As mentioned by the reviewers, please add Figure notes (e.g., explaining error bars). Also try to use consistent variable names (e.g., the variable names in Figure 1 are not consistent with Table 1).
10. In line with the reviewers, we have some concerns about the explanation and presentation of factor analyses and the structural equation model that should be addressed in a revision of the manuscript. First, it would be helpful to present factor analyses for all multi-item scales (for the established scales confirmatory factor analyses would be appropriate). Second, the factor analyses and the mediation model need more explanation (e.g., parallel analysis approach; what estimation did you use for the EFAs? Maximum Likelihood? How did you treat missing values in these analyses? How did you treat latent factors in the mediation analysis? Did you use a Structural Equation Model?). Third, you currently do not report any fit indices. Please add some indicators like CFI, RMSEA, R^2 . Fourth, did you control for socio-demographic variables in your mediation model? If yes, please report the corresponding regression coefficients. If no, please do the corresponding analyses; you could report them in the Supplementary Materials.
11. Please be clearer about the aggregation of the items on experiences with interviews and/or comments on social platforms (Section 2.2.4). Do you use mean scores or sum scores for the following analyses? This question is related to whether you assume positive and negative experiences to be reflective or formative constructs (i.e., whether the items reflect an underlying latent person characteristic that causes the answers to the items or whether the items collectively define and cause the construct). Depending on your assumption different aggregations would be more appropriate.
12. It might be helpful for the reader to visualize your mediation model in addition to Table 2.
13. We appreciate the analyses presented in Table 3. However, please also present the standardized regression coefficients, confidence intervals, and some measure of model fit (e.g., R^2) parallel to Table 2 (where fit indices are also missing; e.g., CFI, RMSEA, SRMR, R^2). We have two additional remarks for Table 3: (1) would it not also be interesting to investigate the interaction between reputational harm and tenure parallel to RH x OS? (2) The relation between socio-demographic variables with all variables in Table 1 would be interesting. You could use these analyses to identify vulnerable groups to discursive repression (e.g., based on gender, career stage, age). The corresponding table would probably be more appropriate to add to the Supplementary Materials.
14. We concur with the reviewers that the qualitative analyses need better explanation and should be added to the code on OSF.
15. Please add Table notes in the Supplementary Materials, including n , fit indices of the model, and the scale on which the items were measured (e.g., slider from 0-100). It would also be helpful to include item communalities and the extracted variance for each factor in the tables.

We hope that these comments are helpful. We are looking forward to your revisions of this important work. If you have any question about the reviews and our own comments, please contact us.

Authors' Response

Comments to the Editors

1. A German version of the abstract, keywords, and impact statement is currently missing. Please add them in the revised submission. In addition, you currently have six keywords, while the limit is five. Please revise them accordingly.

In the revised manuscript, we have added a German translation of the abstract, the impact statement, and the keywords, as requested. We have also reduced the number of keywords from six to five to comply with the journal's guidelines. The revised keywords are: science communication; public engagement; reputational risk; organisational support; socio-ecological crises.

2. There is a slight inconsistency or inaccuracy between the abstract and the impact statement about the role of institutional support. In the abstract you clearly refer to a mediation effect, while the wording in the impact statement could also be interpreted as a main effect of institutional support. Please align abstract and impact statement in that regard.

We have revised the wording of the impact statement to align it more closely with the empirical results reported in the abstract and results section. The revised formulation clarifies that institutional support should not be interpreted as exerting a simple main effect on engagement, but rather as a contextual factor that may mitigate reputational risks associated with hostile public reactions. This adjustment ensures that the impact statement remains consistent with the statistical interpretation presented in the manuscript.

3. We agree with the reviewers that the introduction could be more concise in some parts (e.g., the first paragraph or in the third paragraph) and would benefit from discussing more of the recent literature on the topic. Especially reviewers A and C provide some literature suggestions that could be helpful.

We have revised the introduction to improve conciseness, particularly in the opening paragraphs, and streamlined several passages to reduce redundancy. In addition, we have integrated several relevant references suggested by Reviewers A and C into the theoretical framing of the study in order to better situate our argument within the recent literature on science communication and negative reactions in public discourse. As research on these dynamics seems to have developed rapidly in recent years, some of the cited studies were published after the initial conceptualization of our study and have now been incorporated where relevant.

4. In your open science statement, you frame your analyses as exploratory, yet you formulate hypotheses in the introduction. Please explain why this is the case in Section 1.4. It might be helpful to frame the hypotheses as exploratory or formulate competing hypotheses.

We have addressed this issue by adding a clarifying statement at the beginning of Section 1.5. We now state explicitly that the study was guided by theory-derived directional hypotheses, but that, because it was not preregistered, the findings should not be interpreted in a strictly confirmatory manner. We reinforce this point in Section 2.1 by clarifying that the analyses were primarily intended to estimate associations and to assess whether the observed pattern was consistent with these expectations, rather than to constitute strict confirmatory tests.

5. In Section 2.1, we are missing a rationale for your sample size or any post-hoc power assessments. Why are 138 participants sufficient for your study? What drove your decision to stop sampling?

We have added the requested rationale and power assessment to Section 2.1 (Participants). Regarding the stopping rule, we now clarify that recruitment ended when the planned recruitment period (mid-April to mid-June 2024)

concluded and additional outreach through the targeted professional networks, including Scientists4Future, did not yield further participants. We further state that, although the study was guided by theory-derived expectations, it was not intended as a strict confirmatory test; rather, the primary aim was to estimate associations in this sample. With the final sample ($N = 138$), correlations of approximately $|r| \geq .17$ would reach statistical significance at $\alpha = .05$ (two-tailed). A sensitivity analysis conducted in G*Power (Faul et al., 2007) indicates about 81% power to detect a population correlation of $r = .24$ under these settings.

6. You repeatedly use Cronbach's α as a measure for internal consistency for two-item scales. Please use Spearman-Brown coefficients instead. In general, it would be good to not only present Cronbach's α but also McDonald's ω .

We have revised the manuscript accordingly. For all multi-item scales, we now report McDonald's ω as the primary measure of internal consistency. For two-item scales, Cronbach's α has been replaced with Spearman–Brown (SB) coefficients.

7. The mean of the frequency of interviews in Table 1 (1.57) does not match the mean presented in the text (1.56); the same applies to positive experiences. In addition, we were wondering whether the correlations of psychosomatic complaints with insults, physical attacks, and reputational harm are really not significant as indicated in Table 1?

We have cross-checked all statistical values in the revised manuscript to ensure internal consistency. The means for interview frequency and positive experiences are now reported consistently between the results section and Table 1. In addition, we verified the correlations involving psychosomatic complaints. These correlations with insults, physical threats/legal risks, and reputational harm are statistically significant, and Table 1 has been corrected to display the appropriate significance markers.

8. Please also use italics for abbreviations of statistic parameters such as M , SD , n , and p throughout the manuscript, and check for which statistics zeros before the decimals are used and when there are no zeros before the decimals (e.g., unstandardized vs. standardized regression coefficients).

We have revised the manuscript to ensure that abbreviations of statistical parameters are consistently italicized throughout the text and tables. In addition, the use of leading zeros has been standardized in accordance with APA guidelines. These conventions are now applied consistently throughout the manuscript.

9. Why are the variables positive experience with traditional media and with social media not included in Table 1?

These variables were indeed missing from the original version of Table 1 and have now been added in the revised manuscript. They appear as “Experiences with Traditional Media Overall” (item 6) and “Experiences with Social Media Overall” (item 7). As described in Section 2.2.3, these variables represent single-item global ratings of media experiences. The multi-item scale “Positive Experiences” (row 10 in Table 1) remains a general measure of positive experiences across media contexts.

10. As mentioned by the reviewers, please add Figure notes (e.g., explaining error bars). Also try to use consistent variable names (e.g., the variable names in Figure 1 are not consistent with Table 1).

We have revised the figures and tables to ensure consistent variable naming throughout the manuscript. For example, the label previously used in Figure 1 has been aligned with the terminology used in Table 1 and the results section. In addition, figure notes have been added to explain graphical elements such as the meaning of the error bars.

11. Concerning factor analyses and structural equation models: First, it would be helpful to present factor analyses for all multi-item scales (for the established scales confirmatory factor analyses would be appropriate). Second, the factor analyses and the mediation model need more explanation (e.g., parallel analysis approach; what estimation did you use for the EFAs? Maximum Likelihood? How did you treat missing values in these analyses? How did you treat latent factors in the mediation analysis? Did you use a Structural Equation Model?). Third, you currently do not report any fit indices. Please add some indicators like CFI, RMSEA, R². Fourth, did you control for socio-demographic variables in your mediation model? If yes, please report the corresponding regression coefficients. If no, please do the corresponding analyses; you could report them in the Supplementary Materials.

We have revised the manuscript accordingly. First, we now report factor analyses for all multi-item scales. Exploratory factor analyses (EFA) are presented for the positive experiences, negative experiences, and reputational harm scales (Sections 2.2.4 and 2.2.6), while a confirmatory factor analysis (CFA) is reported for the established PANAS negative affectivity scale (Section 2.2.7). Second, we have expanded the methodological description of the factor analyses and the mediation model. For the EFAs, we now specify the extraction method (principal axis factoring), the factor retention procedure (parallel analysis), and the handling of missing data. We also clarify that the mediation analysis was estimated as a manifest-variable path model in lavaan (Section 2.3). Third, we now report model fit indices for the CFA (Section 2.2.7) and provide R² values for the regression and mediation models (Table 2 and Section 3.2). As the mediation model is just-identified (df = 0), global fit indices (e.g., CFI, RMSEA, SRMR) are not informative and are therefore not reported. Finally, we have added control variables (gender, tenure, and media activity frequency) to the regression and mediation models. The corresponding coefficients for the regression models are reported in Table 2, and the mediation model including these controls is visualized in Figure 2, with detailed coefficients provided in Table S5 of the Supplementary Materials. A version of the mediation model without these covariates is provided in Table S6.

12. Please be clearer about the aggregation of the items on experiences with interviews and/or comments on social platforms (Section 2.2.4). Do you use mean scores or sum scores for the following analyses? This question is related to whether you assume positive and negative experiences to be reflective or formative constructs (i.e., whether the items reflect an underlying latent person characteristic that causes the answers to the items or whether the items collectively define and cause the construct). Depending on your assumption different aggregations would be more appropriate.

We have revised Section 2.2.4 to describe the aggregation procedure more explicitly. For the positive and negative experience scales, we treated the items as reflective indicators and computed unit-weighted mean scores across the respective items. Mean scores were used rather than sum scores in order to retain the original response metric of the 0–100% slider scale, which facilitates the interpretation of descriptive statistics and ensures comparability across the different experience dimensions.

13. It might be helpful for the reader to visualize your mediation model in addition to Table 2.

We have added Figure 2 (Section 3.2) to the revised manuscript to visually present the mediation model. The detailed statistical coefficients corresponding to this model are reported in Table S5 of the Supplementary Materials.

14. We appreciate the analyses presented in Table 3. However, please also present the standardized regression coefficients, confidence intervals, and some measure of model fit (e.g., R^2) parallel to Table 2 (where fit indices are also missing; e.g., CFI, RMSEA, SRMR, R^2). We have two additional remarks for Table 3: (1) would it not also be interesting to investigate the interaction between reputational harm and tenure parallel to RH x OS? (2) The relation between socio-demographic variables with all variables in Table 1 would be interesting. You could use these analyses to identify vulnerable groups to discursive repression (e.g., based on gender, career stage, age). The corresponding table would probably be more appropriate to add to the Supplementary Materials.

We have implemented several changes to address your points. First, we updated Table 2 in the main manuscript and the corresponding regression tables in the Supplementary Materials (Tables S7 and S8) to include standardized regression coefficients (β), 95% confidence intervals, and R^2 values. Second, regarding fit indices for the mediation model, we added a note explaining that the manifest-variable mediation model is just-identified ($df = 0$), meaning that global fit indices (e.g., CFI, RMSEA, SRMR) are not informative. Accordingly, model evaluation focuses on parameter estimates and explained variance. Fit indices are reported for the CFA of the PANAS scale (Section 2.2.7). Third, following your suggestion, we examined the interaction between reputational harm and tenure. This analysis is reported in Section 3.2 and detailed in Table S8 of the Supplementary Materials. The interaction was not statistically significant. Finally, socio-demographic variables are now included in Table 1 and were incorporated as covariates in the regression and mediation models.

15. We concur with the reviewers that the qualitative analyses need better explanation and should be added to the code on OSF.

In the course of revising the manuscript, we decided to remove the qualitative analysis of the open-ended survey responses. As the revised manuscript now focuses more clearly on the quantitative analyses and corresponding statistical models, this change improves the overall coherence and scope of the paper. In addition, the open-text responses were not collected as part of a dedicated qualitative research design and were not accompanied by a predefined analytic strategy. Retaining this material would have required a dedicated qualitative analytic framework, which was beyond the scope of the present study. Consequently, all references to the qualitative material have been removed from the main text and the Supplementary Materials.

16. Please add Table notes in the Supplementary Materials, including n , fit indices of the model, and the scale on which the items were measured (e.g., slider from 0-100). It would also be helpful to include item communalities and the extracted variance for each factor in the tables.

We have updated the Supplementary Materials to include detailed table notes with the requested information. Specifically, the notes now report the sample size (n), the measurement scale used for the items (e.g., the 0–100 slider scale), item communalities (h^2), and the extracted variance for each factor in the exploratory factor analysis

tables (Tables S1–S3). For the mediation models (Tables S5 and S6), we added notes explaining the model specification and reporting the relevant variance explained (R^2).

We are grateful for these thoughtful editorial comments. They helped us improve the manuscript in several important respects, including the precision of the framing, the transparency of the methods and analyses, the consistency of reporting, and the overall presentation of the paper.

Comments to Reviewer A

1. The qualitative analysis is underdeveloped. It is mentioned in 2.2.9 but there is no detail on how these open text responses were analysed, how many responses were received, the amount of detail etc. Later, it is mentioned again on line 362 to support the need for organisational backing directing the reader to the Supplementary Details, and again in the Discussion. The level of detail here is not sufficient to support the points. This is compounded by the fact that the open text data is not available publicly. If this is for reasons of anonymity, then I would suggest going through the data and anonymising the responses before sharing, or at the very least sharing more detail (e.g., themes with illustrative quotes if thematic analysis was used). Given the current level of detail regarding the open text responses and the analysis, this is not sufficient to support the claims. You could also include a couple of illustrative quotes if they provide a particularly good example of what participants were calling for, especially if it connects with some of the reasoning behind choosing to speak out or not (only if present of course).

We agree that the qualitative analysis of the open-ended responses was not sufficiently developed to support the claims made in the original version of the manuscript. In the course of revising the paper, we therefore decided to remove the qualitative analysis entirely. All references to the qualitative analysis and the corresponding table have therefore been removed from the main text and the Supplementary Materials. The revised manuscript now focuses exclusively on quantitative analyses.

2. The script used for running the analysis is also not present at this time. It is listed but cannot be downloaded. Please double-check and confirm that this is available for download. At present the data is available which is good, but the script is not accessible.

We have uploaded the complete analysis script and questionnaire to the OSF repository and confirmed that it is accessible for download. We also revised the script to improve clarity and reproducibility. The OSF materials now include both the dataset and the code required to reproduce the reported analyses. The script now specifies key preprocessing steps, variable derivations, reliability calculations (e.g., Spearman–Brown and McDonald's ω), and the main analyses (ANOVA, regression, mediation, and moderation), allowing the reported results to be reproduced.

3. Ethics? Who approved the study? This will need to be present in the final version.

We have revised Section 2.1 (Participants) to include an explicit ethics statement. The study consisted of an anonymous online survey of adult participants and did not involve any experimental intervention, deception, or the collection of identifiable personal data. Participation was voluntary, and informed consent was obtained at the beginning of the questionnaire. Because the study involved minimal-risk anonymous questionnaire data only, formal ethics review was not required. This is now stated explicitly in the manuscript to ensure transparency.

4. The authors suggest that relatively few studies have examined scientists' own experiences in engaging in public discourse. While this was largely accurate several years ago, there has been a growing body of work since 2021 that speaks directly to these issues. Alongside Dablander et al.'s work on factors motivating engagement, several studies have examined how scientific identity shapes scientists' emotional responses, perceived constraints, and sense of professional risk when communicating about climate change. This includes survey-based work on how core scientist identity values influence willingness to engage, as well as qualitative research that explores scientists' lived experiences of managing the perceived tension between scientific norms and public advocacy. Some of this research has also taken an ethnographic form, providing insight into the day-to-day strategies scientists adopt to reconcile these roles. This literature would help connect the author's discussion of discursive repression and psychological strain as this literature helps illuminate how scientists actively negotiate and mitigate some of the very pressures the authors describe. For example, recent survey-based work has examined how scientific identity shapes willingness to engage, highlighting the importance of particular scientist identity constructions for whether and how scientists choose to speak out (Finnerty et al., 2024b). It also, along with (Dablander et al., 2024) who you cite regarding the risk of advocacy politicising science, finds age to be positively associated with engagement, which supports your finding regarding tenured scientists feeling more able to speak out.

We have revised the introduction and discussion to better reflect the growing literature since 2021 on scientists' experiences with public engagement. While Dablander et al. (2024) were already cited in the original manuscript, we have now incorporated recent and forthcoming work by Finnerty et al. (2024; 2026) (Section 1.3). This literature helps contextualize the dynamics we describe as discursive repression by highlighting how scientists negotiate professional norms, perceived career risks, and public roles when engaging in contested debates.

Following your suggestion, we also relate our finding that tenure is positively associated with engagement (Section 3.2) to similar evidence reported by Dablander et al. (2024) (Discussion, pp. 23–24). At the same time, we clarify that in our data the buffering effect against reputational risks appears to be primarily linked to perceived organisational support rather than career stage (see also section 3.2).

5. In Section 1.2 Ivory Tower versus Activism Dilemma, given that the paper frames “ivory tower versus activism” as a central dilemma capturing the tension between choosing not to speak and being seen as detached versus speaking out and being seen as compromised, I think it is important to incorporate work that has examined this tension directly. This has been explored via interviews with scientists, setting out this dilemmatic framing, and documents the varied ways scientists choose to engage in public discourse while preserving their scientist identities and credibility (Finnerty et al., 2024a).

We have revised Section 1.3 (“Ivory Tower versus Activism Dilemma”) to incorporate the work by Finnerty et al. (2024) and Finnerty (2026), which examines this tension directly. Integrating these studies strengthens the theoretical framing of the dilemma by highlighting how scientists negotiate the perceived safety of the “ivory tower” and its associated norms of neutrality against the risk of being perceived as biased when engaging publicly. We also refer to this literature in the Discussion to contextualize our findings on reputational harm in relation to the professional tensions scientists may face when engaging in contested public debates.

6. I have included these suggestions because they fill specific conceptual gaps the authors themselves identify. In my view, incorporating this work would strengthen the manuscript's theoretical clarity and ensure it reflects the current state of research on scientists' experiences of public engagement and how scientist communication is situated within the broader literature. Feel free to use some or all of it as you see fit.

Regarding arguments for institutions supporting scientists engaging in public discourse see (Perrin et al., 2025) and (Gardner et al., 2021).

Regarding how early-career climate scientists see their roles as communicators (van Eck, 2023).

We have reviewed the recommended works and incorporated them where appropriate. Specifically, we have added references to Finnerty et al. (2024; 2026), van Eck (2023), Gardner et al. (2021), Mede & Volk (2025), Nölleke et al. (2023), and Peters (2024). These additions help situate our study more clearly within the growing literature on scientists' experiences with public engagement and its psychological and professional implications (p. 4). We also refer to this literature in the Discussion to contextualize our findings in relation to work on scientific identity, professional risk negotiation, and the role of institutional support structures (pp. 22–24).

7. More broadly, you speak to credibility and public trust in science. You may find the following recent review useful for these purposes Climate Advocacy And Activism By Scientists: A Narrative Review (Finnerty et al., 2025) as it synthesises the literature from the past few years on what scientists are doing regarding advocacy and activism, why they are doing this, and what the consequences are for public trust in science, how their peers see it, and what the consequences are for the scientists themselves.

We have integrated the narrative review by Finnerty et al. (2025) into the revised manuscript to better situate our study within the emerging literature on scientific advocacy and its consequences for scientists themselves. Specifically, we cite this work when discussing recent research on scientists' public engagement and its implications for professional roles and public trust in science. This synthesis provides an important broader context for our findings on reputational harm and psychosocial strain associated with engagement in contested public debates.

8. Recent qualitative and ethnographic work has also examined how these dilemmas are negotiated over time, offering insights into how scientists manage identity, credibility and risk across transitions into more visible forms of engagement (Finnerty, 2026).

We have incorporated the ethnographic work by Finnerty (2026) into the revised manuscript. Specifically, we cite this work in the section on the "Ivory Tower versus Activism Dilemma" to further illustrate how scientists negotiate tensions between professional norms, credibility, and public engagement over time (p. 7). We also refer to this research in the Discussion to contextualize our findings on reputational harm and withdrawal from contested public discourse.

9. Regarding whether scientists should or should not remain neutral, see this recent perspective (van Eck et al., 2024), which sits nicely alongside pieces by (Oreskes, 2020) and (Fossen, 2025). Van eck et al here challenge the assumption that scientists should avoid value expression in public communication which may be useful regarding expectations around neutrality.

We have revised Section 1.3 ("Ivory Tower versus Activism Dilemma") to incorporate the debate on scientific neutrality and value expression in public communication. Specifically, we now reference van Eck et al. (2024) and

Fossen (2025) to further contextualize the normative expectations surrounding scientists' public engagement and the tensions between neutrality and advocacy (p. 7). The work of Oreskes (2020), which is cited at several points throughout the manuscript, provides additional context for the broader discussion of scientists' responsibilities in public discourse.

10. Line 244. You mention frequency of interviews in traditional media or on social platforms. I assume you mean posts with regards to social media, needs correcting.

We have corrected the wording to clearly distinguish between giving interviews in traditional media and posting on social media platforms. The relevant passages now consistently refer to participants who “gave interviews in traditional media or posted on social media platforms” (e.g., p. 10; p. 12). The corresponding section headings have been adjusted accordingly, and Table 1 now clearly labels the social media variable as “Freq. of Posts.”

11. Table 1. You might consider standardising the variable means and standard deviations to a shared scale, as there are multiple scales present in the table making it difficult to parse at a glance without referring back to the methods sections. I understand that the main aim to is communicate the relationships, but this would help.

We considered this carefully. In the revised manuscript, we decided to keep the original response metrics in Table 1 because they preserve the substantive interpretability of the variables and remain closely tied to the wording and scaling of the underlying measures. At the same time, we improved the table notes and Method descriptions—including the addition of a 'Range' column in Table 1 (p. 14)—so that the scales are easier to understand without repeated cross-checking. We hope this strikes a better balance between comparability and substantive transparency. We greatly appreciate these insightful and constructive comments. They were especially valuable in helping us strengthen the theoretical framing, incorporate important recent literature, improve the transparency of our reporting, and sharpen the interpretation of our findings within the broader field.

Comments to Reviewer B

1. Overall, the introduction would benefit from a more focused and clearly articulated framing, especially in leading up to the variables assessed and tested in the study and the focus of the discussion.

We revised the introduction substantially to sharpen the framing, tighten the progression toward the focal variables, and improve the alignment between the conceptual background, the hypotheses, and the discussion. Specifically, we condensed several passages and integrated recent literature on scientific identity, neutrality, and advocacy—as also suggested by Reviewers A and C—to provide a more focused theoretical foundation. We believe these changes improve the lead-up to the key variables assessed, such as reputational harm and perceived organisational support.

2. Line 55/56: Could you please clarify what type of advocacy is being referred to here? While the example is interesting, it would benefit from a bit more background explaining its relevance and what it is intended to illustrate. Alternatively, you might consider omitting the example if it does not add essential information at this stage.

We followed your alternative proposal and omitted the specific example of Peter Frankopan to improve the conciseness and focus of the introduction. Instead, we revised the relevant passage to more clearly articulate the general tension between scientific neutrality and advocacy in the context of socio-ecological crises, explaining how scientists may face accusations of bias or activism when addressing these issues (p. 3).

3. Line 59: Please clarify what you mean by the “blurred” relationship in this context and how these examples illustrate this blurring?

We revised this part of the introduction to clarify what was meant by a “blurred” relationship and how the cited examples illustrate the overlap between scientific communication, advocacy, and political contestation in public discourse. In the revised manuscript, we replaced the term “blurred” with a more precise description of the tensions between scientific expertise, political positioning, and public legitimacy. We also clarify that these dynamics arise because scientific statements are increasingly scrutinized for perceived bias and reinterpreted through political lenses, regardless of the scientists’ intentions.

4. Line 73: The rationale for introducing COVID-19 at this point is unclear. It is not evident why the COVID-19 pandemic is repeatedly used as a comparison when the manuscript focuses on climate injustice. If the concept of discursive repression is primarily derived from prior research conducted on societal phenomenon during the COVID-19 pandemic, this should be stated explicitly. Alternatively, if COVID-19 is being framed as part of or analogous to climate injustice, this connection should be clarified.

We revised the introduction to clarify the role of COVID-19-related literature. In the revised manuscript, this literature is now framed more explicitly as a related empirical context in which communication backlash and chilling effects toward experts have been studied, rather than as a direct analogue to socio-ecological crises. We clarify that the pandemic represents a context in which polarization and crisis-related uncertainty intensified conflicts over scientific authority and legitimacy, providing a comparative backdrop for the dynamics examined in this study. Where necessary, we reduced or clarified references to avoid potential confusion.

5. Starting Line 123: This section would benefit from a bit more contextualization of the repression literature. You draw on relevant work from several fields of repression research. However, to improve clarity and conceptual grounding of your work, it would be helpful to include a few sentences situating discursive repression more explicitly within the broader repression literature, particularly given that in your Discussion you emphasize expanding our understanding of repression in democratic regimes.

We have expanded the theoretical framing of discursive repression in Section 1.2 to situate the concept more explicitly within the broader repression literature. Specifically, we clarify how our use of the concept relates to existing work on delegitimization, intimidation, chilling effects, and non-state forms of repression, while distinguishing it from more overt or state-driven forms of political repression. We also highlight that these processes are not limited to undemocratic regimes but can operate through informal and decentralized mechanisms in democratic societies, thereby increasing the reputational and psychological costs of participation. We believe these revisions provide a stronger conceptual grounding that aligns with our discussion of repression in democratic contexts (p. 25).

6. Line 171: Building on my previous point, you cite Marheinecke et al. (2024) in relation to scientists and discursive repression, although their study focuses on state-driven repression and does not specifically examine scientists. While I agree that the concepts are related and see the relevance of this work in the context of your study, it would be helpful to clarify that this literature comes from a related field and to explain more explicitly how insights from state-driven repression can be extended to scientists' experiences of discursive repression.

We revised the relevant passages to make clear that this literature comes from a related, but not identical, field. In the Introduction, Marheinecke et al. (2024) are cited as an example of covert forms of political repression to conceptually distinguish discursive repression from other types of repression (p. 6). In the Discussion, we refer to this work more narrowly as a point of reference for the psychological strain associated with delegitimization and intimidation. While their study does not focus on scientists or discursive contexts, this use is intended to situate our findings within a broader literature on the psychosocial consequences of hostile and delegitimizing environments. We also clarify that our study addresses a different mechanism and context—namely, scientists' experiences of delegitimization, intimidation, and reputational threat in public discourse rather than state repression (p. 25).

7. Line 249 / Line 271: Please clarify whether validated scales were used. If not, how were the positive statements and reputational harm statements developed? Additionally, please clarify whether items with factor loadings below 0.5 were dropped entirely from the analyses after the PCA.

In the revised manuscript, we now clarify in the Measures section (Sections 2.2.4 and 2.2.6) which scales were adapted from established instruments and which were developed for the present study. Specifically, the positive experience statements were primarily informed by reports summarized in Nogrady (2021), supplemented by one self-developed item. The reputational harm items were developed for this study to capture the professional risks scientists face in contested debates, informed by the authors' experiences and established behavioral taxonomies. Regarding the factor analysis, we expanded the description of our procedure in Section 2.2.4. We used Principal Axis Factoring with oblique rotation and applied a retention criterion of primary loadings $\geq .50$ and cross-loadings $< .25$. Items not meeting these criteria were excluded from scale construction and all subsequent analyses. The excluded items and their loadings are reported in Tables S1–S3 of the Supplementary Materials.

8. The Methods section is missing a description of the statistical analysis plan. Please clarify, at the end of the Methods section, which analyses were conducted and for what purpose, how missing data (if any) were handled, and which tests or diagnostics were used to assess model assumptions and fit. Additionally, please specify which analyses were confirmatory in order to test your hypotheses and which were exploratory or post-hoc.

We added a dedicated Statistical Analyses subsection (Section 2.3) that outlines the analytical workflow more transparently. We specify that we first summarized the data using descriptive statistics and zero-order associations (Spearman's ρ). To test our hypotheses, we conducted repeated-measures ANOVAs and multiple regression models predicting willingness to engage. We also estimated an exploratory manifest-variable mediation model (equivalent to PROCESS Model 4) in lavaan to examine indirect effects of negative experiences via reputational harm. Regarding missing data, we clarify that we used pairwise deletion for correlations and full-information maximum likelihood (FIML) for structural models. Model fit was assessed using χ^2 , CFI, TLI, SRMR, and RMSEA for the confirmatory factor analysis (Section 2.2.7), whereas the mediation and regression models—

estimated as manifest-variable models—were evaluated based on parameter estimates, bias-corrected bootstrap confidence intervals (5,000 resamples), and explained variance (R^2), as global fit indices are not informative for just-identified models. Finally, we specify that, as the study was not preregistered, all analyses are interpreted as theory-informed and exploratory rather than strictly confirmatory, with additional exploratory analyses clearly labeled as such.

9. Building on my previous point, the beginning of the Results section is somewhat confusing regarding when correlations are reported versus when inferential statistics are presented, and it is not always clear why some relationships are tested while others are not. While the correlation table is relevant, inferences such as one relationship being “higher than” another cannot be drawn from simple comparisons of correlations alone. You might consider first presenting descriptive statistics, followed and clearly separated by your hypotheses (and sub hypotheses) testings.

We have reorganised the Results section to clearly distinguish between descriptive summaries and inferential hypothesis testing. In the revised manuscript, Section 3.1 focuses on descriptive results and zero-order associations (Spearman’s ρ), while Section 3.2 is dedicated to the inferential testing of our hypotheses using regression, mediation, and moderation analyses. We also revised the wording throughout to ensure that comparative claims (e.g., one relationship being ‘higher than’) are only made when supported by appropriate inferential tests rather than simple comparisons of correlations. We believe this improves the transparency and structure of the Results section.

10. Figure 1: Consider using a higher resolution for this and all following figures. Furthermore, the figures are missing a note explaining the figure in more detail, as figures should be able to stand on their own. Please clarify, e.g., what comprises insults, positive experiences, ... and what a violin plot displays.

We revised the figures to improve clarity and export quality and added fuller figure notes so that they can stand on their own. Specifically, the note for Figure 1 (p. 16) now explains what information is conveyed by the violin plots (kernel density estimation), indicates that diamonds represent means and vertical bars standard deviations, and clarifies the underlying 0–100 response scale. We also added references to the Measures section (2.2.4) and Supplementary Materials (Tables S1–S2) for details on the specific item content.

11. In Line 318 you state that “these results support Hypothesis 1: A considerable proportion of scientists who take a public stance on socio-ecological crises report experiencing repressive responses.” However, the result you refer to appears to be based on the proportion of statements resulting in negative experiences rather than the proportion of scientists reporting such experiences. While this is an interesting finding, to adequately address your hypothesis, please also clarify the proportion of scientists who reported negative experiences, as opposed to the number of negative experiences reported overall.

We revised the Results section (Section 3.1) to distinguish explicitly between prevalence among scientists (the proportion of respondents reporting at least one such experience) and frequency per engagement event (the average percentage of statements followed by such experiences). Specifically, we now report that 78.4% of scientists experienced insults and 29.9% reported physical threats or legal risks (p. 15), and contrast these figures with the average frequency across communication episodes (20.8% and 2.27%, respectively; p. 15). We believe this distinction clarifies the empirical support for Hypothesis 1 and avoids conflating prevalence with frequency.

12. Line 326 second hypothesis: It appears that you are primarily reporting bivariate correlations, which are not sufficient to answer your hypothesis. To do so, you should consider using inferential statistics that control for intercorrelations and other relevant variables. However, if you intend to address the second hypothesis using your mediation analysis, then the mediation model should be used consistently to test (and report on) the hypothesis, and reporting correlations at this stage may not be necessary.

We addressed this by ensuring that the inferential evaluation of Hypothesis 2 is based on multivariable analyses rather than bivariate correlations alone. In the revised manuscript, we reorganised the Results section to clearly distinguish between zero-order associations (reported in Section 3.1 and Table 1 for descriptive transparency) and hypothesis testing. To test Hypothesis 2, we now rely on multiple regression models (Table 2) and the mediation model (Section 3.2 and Figure 2), which control for intercorrelations and relevant covariates such as gender, tenure, media activity frequency, and positive experiences. This approach shows that while direct negative experiences are not significantly associated with willingness to engage, there is a significant indirect effect via reputational harm, particularly for physical threats and legal risks. We now explicitly clarify in the manuscript that this indirect effect is specific to these more severe forms of negative experiences, whereas more common but less severe experiences (i.e., insults) did not show a statistically reliable indirect effect in the multivariable model.

13. Line 362: The reporting of the qualitative findings appears somewhat unexpected. It would be helpful to clarify in the Methods section how these qualitative findings are incorporated into the study design and results. Additionally, please explain whether and how these findings inform your suggestions and implications in the Discussion section.

We resolved this issue by removing the qualitative material from the study's empirical results and discussion. After reconsidering the role of the open-text responses, we concluded that focusing exclusively on the systematically analysed quantitative data provides a clearer and more coherent basis for the manuscript. As the open-text responses were not collected as part of a dedicated qualitative design, retaining them would have required a dedicated qualitative analytic framework that was beyond the scope of the present study. Consequently, all references to qualitative findings and related implications have been removed from the revised manuscript and the Supplementary Materials.

14. Figure 2: Please use a higher-quality version of the figure and adjust the font size. In addition, consider using boxplots rather than bar plots, as the figure appears to display means with standard deviations rather than frequencies; boxplots would better illustrate the underlying distributions.

We have revised all figures to ensure higher resolution and improved font sizes for better readability. Regarding the interaction plot (formerly Figure 2, now Figure 3), we replaced the bar plots with a visualization that displays predicted values and error bars alongside individual raw data points. We believe this format illustrates the underlying distribution more transparently than the previous bar charts or a standard boxplot. Additionally, as requested by the Editor, we added a new Figure 2 to provide a visual representation of the mediation model.

15. In the first paragraph, focus on reporting your results more straightforwardly before moving to interpretation (e.g. "These patterns suggest a complex landscape in which public engagement can be both rewarding and demanding for scientists (Nogrady, 2021; Das NETTZ et al., 2024)"), In addition,

the mediation findings seem to not be reported in this paragraph, even though they are highly relevant to your results.

We have revised the opening paragraph of the Discussion (Section 4) to provide a more straightforward summary of the results before moving to interpretation. As requested, we have integrated the mediation findings into this paragraph. We now clarify that the association between negative experiences and willingness to engage publicly is mediated by perceived reputational harm, which appears to carry more weight than the experiences themselves.

16. Line 420: “Although positive engagement outcomes were more frequent than negative ones among our respondents, it appears that negative experiences (especially those threatening one’s reputation) carry more psychological weight and disproportionate influence on future engagement decisions. Even a single serious negative incident can have a chilling effect that outweighs many positive aspects.”: I am not sure where this conclusion is coming from. You report a mediation analysis involving negative experiences, but I do not see a comparison with positive experiences with respect to future engagement decisions. Please clarify whether such analyses were conducted, or revise the wording to reflect only the conclusions that can be supported by the presented data.

We agree that our original wording overreached by implying a direct empirical comparison of the ‘psychological weight’ of positive versus negative experiences. We have revised the Discussion (Section 4) to more closely reflect the data. In the revised manuscript, we no longer claim that negative experiences generally outweigh positive ones. Instead, we focus on the finding that while positive experiences are a significant positive predictor of willingness, certain severe but rare negative experiences (physical threats and legal risks) show a significant indirect association with engagement via increased concerns about professional reputation. This interpretation is supported by our multivariable models (Table 2 and Figure 2), which include positive experiences as a covariate.

17. Line 455: Your finding that tenure is positively associated with public engagement supports this argument, consider adding this to strengthen your argument.

We have incorporated this point into the revised Discussion (Section 4, pp. 23–24) to strengthen our argument regarding the role of professional security in public engagement. We now state that tenured scientists report a higher willingness to engage publicly, aligning with international evidence suggesting that senior academics perceive greater professional stability. This addition contextualizes the role of career stage while emphasizing that institutional backing remains a critical buffer against reputational harm.

We are very grateful for these careful and constructive suggestions. They helped us refine the conceptual focus of the manuscript, improve the clarity and organisation of the Methods and Results sections, and present the empirical contribution of the paper in a more rigorous and coherent way.

Comments to Reviewer C

1. The introduction argues that trust in science is increasingly being publicly challenged (p. 3, l. 49). However, current survey data show that this decline in trust is not observed in Germany (Wissenschaft im Dialog 2025; Reif et al. 2025), where most of the scientists surveyed are affiliated.

We have revised the Introduction to provide a more precise characterization of the current trust landscape, particularly in the German-speaking context. We no longer imply a generalized decline in public trust in science

and acknowledge that trust levels remain comparatively high in Germany, Austria, and Switzerland. We frame the DACH region as a context in which high public trust coexists with strong expectations of scientific neutrality. As suggested, we have also incorporated work on online climate change skepticism (e.g., Treen et al., 2020) and on science communication as political communication to better capture the specific challenges and delegitimizing dynamics scientists encounter when engaging publicly on socio-ecological crises.

2. Nevertheless, particularly in the case of climate change, the authors could briefly address climate change skepticism online in order to describe the environment of scientists' public online engagement in more detail. (e.g., Treen et al., 2020) (p. 3, ll. 58-59).

In the revised manuscript, we address online climate change skepticism by incorporating the work of Treen et al. (2020) in Section 1.4. This addition allows us to more clearly describe the digital environment—particularly the algorithmic amplification of misinformation—in which scientists' public engagement takes place. We believe this provides a clearer backdrop for the discursive pressures examined while maintaining the conciseness of the introduction.

3. For the context described as “socio-ecological crises”, it might be useful to mention that these issues often intertwine with political issues, described in the literature as “science communication as political communication” (Scheufele, 2014), which might pose challenges to the communicating scientists addressing these issues.

We have incorporated the perspective that science communication in the context of socio-ecological crises often takes place in politically contested environments. Specifically, in Section 1 (p. 4), we now state that scientific statements are increasingly interpreted through political or ideological lenses, particularly in polarized public debates, rather than being received as neutral contributions to knowledge. This addition clarifies the discursive pressures scientists face when their expertise intersects with contested socio-political issues and strengthens the conceptual foundation for our analysis of reputational risks.

4. I was wondering whether it is really the case that previous studies often solely focus on the public perception of scientists' engagement (p. 3, ll. 62-63), since there are several studies that deal with perceptions of public engagement of scientists in general (Calice et al., 2023, Kessler et al., 2022) and climate scientists (van Eck, 2023; Cologna et al., 2021). I suggest that the authors more clearly state their focus on perceptions of insults and reputational threat emphasizing the research gap they are addressing with their study.

We have revised the statement of our research focus in the Introduction to be more precise. Rather than suggesting that prior work has primarily focused on public perceptions of scientists' engagement, we now acknowledge the broader literature on scientists' engagement and communication practices. We clarify that our specific contribution lies in examining scientists' own experiences of insults, threats, and reputational harm, as well as their implications for engagement decisions. By emphasizing these dynamics - particularly the role of perceived reputational harm - we more clearly delineate the specific focus and contribution of our study within the existing literature.

5. It does not become entirely clear to me how the paper can contribute to position scientists as environmental actors within socio-ecological systems (p. 3, ll. 87-90). This should be explained in more detail.

We have revised the Introduction to clarify the theoretical positioning of our study. We now frame discursive repression as a lens for describing the hostile and delegitimizing dynamics in which scientists operate. This framing allows us to understand scientists as participants in socio-ecological systems whose communicative decisions—to engage or to withdraw—may be shaped by these systemic pressures. By examining how discursive repression relates to the perceived professional costs of participation, we highlight that scientists' engagement can be a relevant factor in how these systems process and respond to scientific evidence.

6. For the theoretical background, it might be useful that when the authors refer to populism when introducing the concept of discursive repression (p. 4, ll. 107-112), to mention the concept of science-related populism (Mede & Schäfer, 2020).

We have incorporated the concept of science-related populism (Mede & Schäfer, 2020) into the revised Theoretical Background (Section 1.1, p. 5). We now describe how this framework characterizes science as part of an 'unvirtuous academic elite' and challenges its legitimacy in public decision-making. This addition helps to contextualize the anti-elitist dynamics that may contribute to discursive repression within the broader literature on populist communication.

7. With regard to discursive repression, I think the manuscript would benefit from a clearer definition and more detailed description of the concept from the beginning. Furthermore, reading the article also raises the question for me of whether it is possible to make statements about discursive repression (macro level) based on the study design. The suitability of the concept should be more justified and how it is operationalized in the study (2 Methods).

We have revised the manuscript to provide a clearer definition of the concept and to specify the scope of our study more explicitly. First, we now introduce a more detailed definition of discursive repression earlier in the manuscript (Section 1.2). We clarify that it refers to persistent, diffuse, and indirect processes of delegitimization that can function as a structural feature of public discourse rather than a centrally coordinated practice. Second, we address the concern regarding macro-level implications. In the Introduction we now more precisely frame the study as providing a lens for describing discourse environments. In addition, we explicitly state in the Limitations (Section 4.1) that our study does not directly assess discursive repression as a structural property of public discourse, but instead examines individual experiences that may reflect mechanisms consistent with this concept. Third, we clarify the operationalization of the concept in the Methods (Section 2.2.4) by linking it to perceived professional costs and related experiences. By framing the study as theory-informed and exploratory, we avoid overstating macro-level conclusions.

8. When referring to the “chilling effect” (p. 5, ll. 149-155) in the context of discursive repression, it would be worthwhile to consider the findings of Peters (2025) and Nölleke et al. (2023).

We have incorporated the work of Nölleke et al. (2023) and Peters (2024) into the revised manuscript to strengthen our theoretical framework and the interpretation of our findings. As requested, we have integrated these references into the discussion of the 'chilling effect' and self-censorship (Section 1.2). Beyond this section, we also refer to

these works in the Introduction and Discussion to further contextualize how hostile communication environments can increase the perceived professional costs of engagement. Drawing on these studies, we are able to more clearly situate our findings within the literature on chilling effects, suggesting that the patterns observed in our data may be linked to reputational mechanisms.

9. The authors could also go into more detail in the theoretical background on the new possibilities offered by social media for scientists to directly engage with the public on these platforms (e.g., Biermann et al., 2023), as the study differentiate between public engagement experiences in traditional media and social media (1. 3 The Role of Media in Science Communication).

In the revised manuscript, we have briefly noted the specific affordances of social media in Section 1.4. By acknowledging that these platforms enable more direct communication between scientists and broader audiences (e.g., Allgaier, 2023; Peters, 2024), we provide a clearer theoretical motivation for our empirical distinction between traditional and social media experiences. While we have kept this addition concise to maintain the focus on discursive risks, we believe it better situates scientists' engagement within the digital media landscape and clarifies the relevance of distinguishing between media types.

10. While the overall research design is suited to address the overall research question. The hypotheses could be more precise.

We have revised and sharpened the hypotheses in Section 1.5 to ensure they are more precise and directly aligned with the specific constructs measured in our study. Specifically, we now refer consistently to the operationalized variables (e.g., negative experiences and willingness to engage) rather than broader conceptual terms. Furthermore, we clarify that the hypotheses are theory-informed and exploratory. While the hypotheses now explicitly refer to both traditional and social media to reflect the study's scope, we added a limitation in Section 4.1 noting that willingness was ultimately analyzed as a combined composite measure. We believe these revisions improve the transparency and alignment between our theoretical expectations and the empirical analyses.

11. It is questionable whether the study is permissible to make statements about the association of the politicization of public discourse with increased self-censorship (Hypotheses 2, p. 6, ll. 205-207). I would recommend to use in the formulation of the hypotheses, the constructs actually measured.

We have revised the hypotheses in Section 1.5 to refer directly to the constructs measured in the study rather than broader conceptual processes that were not operationalized. Specifically, in Hypothesis 2, we replaced the terms 'politicization of public discourse' and 'self-censorship' with 'negative experiences' and 'willingness to continue public engagement'. This revision improves the alignment between our theoretical framework and the empirical analysis. We also clarify that these hypotheses are intended as theory-informed and exploratory directional expectations rather than strictly confirmatory tests.

12. Better contextualize the results of the exploratory multiple regression analysis (ll. 11, p. 369) and to make the procedure transparent, it can be useful to formulate an explicit exploratory research question in addition to stating hypotheses (p. 6).

We have addressed this by explicitly framing the study and its hypotheses as theory-informed and exploratory rather than strictly confirmatory in Section 1.5. Rather than adding a separate research question, we have clarified

the analytical structure by more clearly distinguishing between hypothesis testing and exploratory analyses. Specifically, the multiple regression models and the mediation analysis are now explicitly characterized as exploratory, and the additional moderation analysis involving tenure (Section 3.2) is described as a follow-up analysis conducted in response to reviewer suggestions. We acknowledge that the absence of an explicit exploratory research question in the original version may have reduced transparency, and we believe that the revised framing addresses this concern by making the exploratory nature of these analyses more explicit.

13. With regard to the sampling procedure, the institutions through which participants were recruited need to be described in greater detail (p. 6, ll. 210-213). Furthermore, the selection criteria should be more thoroughly justified. This also includes to define the term “scientists” precisely, since apparently not only researchers from research institutions were included but a very broad definition was applied.

We have expanded Section 2.1 to provide a more detailed description of the recruitment process and selection criteria. First, we specify the recruitment channels, noting that participants were reached through the German, Austrian, and Swiss channels of Scientists4Future (S4F), the German Psychologists for Future (Psy4F) Signal channel, and professional networks engaged in socio-ecological research. Second, we provide a clearer operational definition of ‘scientists’: eligibility required participants to self-report employment in scientific research and to earn at least part of their income from scientific work. We acknowledge that this definition is broad; however, 90.6% of participants indicated that research is a core part of their professional role. Third, we justify the inclusion of researchers from diverse institutional backgrounds. While the majority of the sample (82.9%) is employed at universities or research institutes, including scientists in applied roles (e.g., NGOs or advisory bodies) allows us to capture a broader spectrum of engagement on socio-ecological crises. Finally, we clarify the stopping rule, noting that recruitment ended when the planned period concluded and no further participants were reached through the targeted networks.

14. The scale for the frequency of interviews and social media posts is ambiguous, as 0 = never and 1 = once a year or less was used for frequency in the last 12 months (p. 7, ll. 245-248). Less than once a year therefore means never in the last 12 months.

We agree that the response options ‘0 = never’ and ‘1 = once a year or less’ were redundant, as both refer to the past 12 months. This resulted from a survey programming oversight. In the revised manuscript, we explicitly acknowledge this issue in the Limitations section (Section 4.1). We clarify that, although the variable was used descriptively and as a covariate in our models, this measurement ambiguity should be considered when interpreting the results. We believe that this transparency improves the clarity of our methodological reporting.

15. With regard to willingness to give interviews/posts on social media (p. 7, lines 292–294), I wondered why willingness was not calculated separately for interviews and social media, since negative experiences on social media may only lead to people no longer posting on social media, but not necessarily to them no longer giving interviews. Reasons for this decision should be given and it might be necessary to state this operationalization of willingness as a limitation of the study.

We addressed this point by providing the statistical rationale for our decision in the Measures section (Section 2.2.8) and by explicitly acknowledging this operationalization as a limitation in Section 4.1. We combined the two items because they were strongly correlated ($r = .66$) and showed adequate two-item reliability ($SB = .79$),

indicating substantial shared variance and supporting their interpretation as a broader construct of general public engagement readiness. However, we agree that platform-specific differences—such as selective withdrawal from social media while maintaining traditional media engagement—may be obscured by this aggregation. Accordingly, we added a note in the Limitations section that future research should distinguish these communication channels more explicitly.

16. Moreover, I would recommend the authors to share the full questionnaire for transparency reasons on OSF or in their supplemental material.

We agree that sharing the questionnaire improves the transparency of our study. We addressed this by providing all survey items in the Supplementary Materials (Tables S1–S4) and by describing them more explicitly in the Measures section (Section 2.2). In addition, we updated the Open Science statement (Section 5) to indicate that the study materials are publicly available via our OSF repository.

17. The analysis strategy and the structure of the result section is suitable for addressing the hypotheses. As outlined, I would suggest to introduce an exploratory research question for the further exploratory results. In my opinion, this would make it even easier to follow the structure of the results section.

We have addressed the need for greater transparency by sharpening the distinction between our focal hypotheses and the exploratory analyses. Rather than introducing a separate research question, we clarified in Section 1.5 that the hypotheses are theory-informed and exploratory, and we structured the Results accordingly. In addition, we explicitly characterize the mediation and moderation analyses in Section 3.2 as exploratory follow-up analyses. We believe this revised framing improves the transparency and readability of the Results section.

18. The findings that participants reported expressing opinions more frequently on social media than in traditional media (p. 9, ll. 308-312) aligns with previous research on (visible) scientists' social media communications. I think it could be worthwhile to discuss how the expression of opinions on these platforms might make scientists more vulnerable to attacks on social media in the discussion section (see also Zhang, 2025).

In the revised Discussion (Section 4), we further contextualize the finding that participants express opinions more frequently on social media than in traditional media. We explicitly connect this pattern to the direct, visible, and personally attributable nature of social media engagement, which may increase scientists' vulnerability to public attacks and reputational risks. By situating these experiences within the dynamics of digital platforms—such as amplification and emotionalization—we clarify how frequent online engagement may be associated with higher perceived professional and psychological costs.

19. I also stumbled a little over the presentation of the results, in that Figure 1 combines willingness to engage in interviews in traditional media and post on social media as one (see my comment below).

We revised the presentation of our results to more clearly distinguish between platform-specific experiences and the combined willingness measure, and standardized the variable labels across Figure 1, Table 1, and the text. We also clarify in Section 2.2.8 the rationale for combining the two willingness items into a composite score and acknowledge this operationalization as a limitation in Section 4.1, noting that it may obscure platform-specific differences.

20. The explanations regarding organisational support remain somewhat unclear, as they refer to qualitative results whose analysis have not been described (p. 11, ll. 362-368). I would like to see the procedure explained in more detail in the methods section. How many respondents gave those qualitative insights, are they based on the 19.2% of the respondents reported receiving active support from their organizations (p. 11, ll. 361).

We have addressed this concern by removing the qualitative material from the empirical results and discussion. After reconsidering the role of the open-text responses, we concluded that they were not collected as part of a dedicated qualitative research design and lacked a predefined analytic strategy. Retaining them would have required a dedicated qualitative analytic framework that was beyond the scope of the present study. Consequently, the revised manuscript focuses exclusively on the quantitative analyses, and findings regarding organisational support are reported solely on the basis of the quantitative survey measure and the corresponding statistical analyses. All references to the qualitative material have been removed from the main text and the Supplementary Materials to ensure methodological consistency.

21. It should be discussed whether the low level of support is also related to the fact that negative experiences were generally hardly perceived by the respondents and with a large standard deviation between them (p. 11, ll. 315-320).

In the revised manuscript, we report the descriptive statistics for negative experiences more precisely in Section 3.1 (Descriptive results and correlations) and Table 1, highlighting their relatively low average frequency and high variability (e.g., insults: $M = 20.8\%$, $SD = 26.43$). In the Discussion (Section 4 and Section 4.1), we added a passage noting that although direct negative experiences are reported relatively infrequently, perceived reputational harm is nevertheless associated with engagement decisions. We also note that the comparatively low level of reported organisational support may be related to the sporadic nature and variability of such experiences, which may limit the development of standardized institutional responses. Accordingly, we outline the potential relevance of more proactive support structures (e.g., clear communication policies and procedures) even for infrequent but impactful events.

22. In the discussion section, it might be useful to emphasize in more detail what can be learned from this study for further contexts, as the study is rather a case study in nature. It should be made clearer that the results are limited to a very specific group of scientists. Moreover, due to the relevance of “socio-ecological crises”, scientists communicating in this context might be more exposed to negative experiences than scientists who communicate about less controversial topics.

We have revised the Discussion (Section 4) and Limitations (Section 4.1) to more clearly emphasize the contextual specificity of our study. We now state explicitly that our findings are based on a non-representative sample of scientists engaged in the highly politicized field of socio-ecological crises. We also acknowledge that this context may be associated with higher exposure to negative experiences compared to less controversial scientific domains. Rather than claiming broad representativeness, we frame this group as a ‘frontline indicator’ of emerging communication risks in public discourse. These revisions clarify the scope of our findings while indicating how the observed patterns—particularly regarding reputational harm and organisational support—may be relevant for other contested areas of science communication.

23. I think some of results can also connected more with existing research in the discussion section (e. g., Mede & Volk, 2025). For example that negative experience not systematically correspond with a reduced willingness to engage publicly (p. 11, ll. 333-335) aligns with the findings of Nölleke et al. (2023) in the context of the Covid-19 pandemic.

In the revised Discussion (Section 4), we have integrated suggested literature to better contextualize our findings. Specifically, we now relate our finding—that negative experiences are not systematically associated with reduced willingness to engage—to similar patterns reported by Nölleke et al. (2023) in the context of the COVID-19 pandemic. We also incorporate Mede and Volk (2025) to further contextualize the role of public backlash and organisational support. These revisions situate our results more clearly within the existing empirical literature on science communication in contested contexts.

24. The reference to the false balance literature seems somewhat out of context to me and, in my opinion, needs to be better linked to the actual results of the study (p. 14, ll. 435-438).

We have revised the relevant passage in the Discussion (Section 4) to better integrate the ‘false balance’ literature with our empirical findings. Specifically, we now link these media practices to the discursive tensions identified in our results. While our findings indicate that scientists generally evaluate traditional media experiences more positively than those on social media, we clarify that practices such as ‘false balance’—where scientific consensus is framed as a controversy—may still contribute to a contested communication environment. By presenting minority viewpoints as equally valid, traditional media can inadvertently reinforce the politicization of science. This connection helps explain how such media dynamics may shape the professional risks and communication environments for scientists, even when their direct interactions with journalists are perceived as positive.

25. I would recommend to state in the limitations that no further distinction was made between online and offline in terms of willingness (see comments below).

We have incorporated this point into the Limitations section (Section 4.1). We now state that willingness to engage was operationalized as a combined measure across interviews in traditional media and posts on social media platforms. We acknowledge that this aggregation may obscure potential differences, as negative experiences on social media may influence willingness to post more strongly than willingness to give traditional interviews. Accordingly, we suggest that future research distinguish these communication channels more explicitly when examining engagement decisions.

26. It should be discussed to what extent the results are transferable due to the very specific group of scientists, since only scientists in the narrow sense were surveyed.

We have clarified in the revised Discussion (Section 4) and Limitations (Section 4.1) that our findings are based on a non-representative sample of scientists in the ‘narrow sense’ engaged in socio-ecological crises. We acknowledge that this context may be associated with higher exposure to negative reactions than less controversial fields. Rather than claiming generalizability, we frame this group as a ‘frontline indicator’ of emerging communication risks, with the identified patterns—particularly regarding reputational harm—providing exploratory insights for other contested areas of science communication.

27. The authors mention that the study was not pre-registered because it was conducted as exploratory research. With regard to the formulation of hypotheses, it is questionable to what extent the results are actually purely exploratory in nature.

We appreciate this critical reflection. We clarified in Section 1.5 that our approach is “theory-informed and exploratory” rather than strictly confirmatory. While we retain directional hypotheses to structure the analyses, we explicitly state that these are intended as tentative expectations rather than formal confirmatory tests, particularly given the lack of pre-registration. This revision clarifies the exploratory nature of our inferences while maintaining their theoretical grounding.

28. While the authors state in the manuscript that ‘all elements and data are available upon request’ (p. 8, line 298), they have in fact made the data available on OSF. The availability statement should therefore be amended.

We have corrected the availability statement in the revised manuscript to reflect that the data and materials are publicly available via our OSF repository.

29. It should be made clearer in the abstract that the surveyed scientists were rarely exposed to insults. (Abstract)

We have revised the Abstract to provide a more balanced representation of the empirical patterns. Specifically, we now clarify that most participants reported positive experiences overall and more clearly distinguish between the prevalence of negative experiences and their comparatively lower frequency. We also note that more severe incidents, such as physical or legal threats, were reported by a smaller proportion of participants.

30. I would recommend to use throughout the manuscript the terms “social media platforms” or “social media” instead of social platforms because these terms are more commonly used.

We have revised the terminology throughout the manuscript to use the more standard terms ‘social media’ or ‘social media platforms’ consistently. To preserve accuracy, we retain the term ‘social platforms’ only where it reflects the original wording of the questionnaire (e.g., in the Methods section and Supplementary Materials).

31. When describing the operationalization of frequency of interviews/posts, there is missing “post” before social (media) platforms (p. 7, ll. 244-245).

We have corrected the wording in Section 2.2.3 to refer to participants who ‘gave interviews in traditional media or posted on social media platforms.’ This clarifies the operationalization.

We sincerely appreciate these highly valuable comments. They helped us sharpen the conceptual positioning of the study, clarify the scope and limitations of our claims, and situate the findings more precisely within the relevant literature and the specific context of scientists’ public engagement on socio-ecological crises.

Review Round 2

Comments by Reviewer A

Dear author(s),

The authors have substantially revised the manuscript, and it is much improved. They have engaged thoughtfully and in detail with the comments from all reviewers and the editors. In this instance, removing the qualitative analysis was the right call (though some of that language remains in the discussion, see my comment below). I commend the care taken across the revision and am happy to recommend publication.

I have one point for the authors to address. The opening of the Discussion (p. 22) states that scientists "reported benefits such as increased visibility and greater confidence in communicating" and that "many participants described negative experiences". This phrasing reads as a summary of open-ended responses, but the qualitative analysis has been removed from the manuscript. I would suggest rewording these sentences so that the benefits and negative experiences are attributed explicitly to the relevant quantitative measures and removing any wording that implies participant description of open-text material. Each should correspond to a measured item.

This is a small wording matter, and I see no obstacle to publication once it is resolved.

Comments by Reviewer B

Great job incorporating the reviewers's feedback! The manuscript has gained immensely in structure, flow and clarity, I have nothing more to add.

Comments by Reviewer C

Thank you very much for the revised version of your manuscript and for your thorough consideration of the comments from the other reviewers as well as my own remarks. It is understandable that the qualitative analysis is therefore no longer included; however, this is somewhat unfortunate given its potential contribution. Taken together, in my opinion, the revision has refined and improved your manuscript. However, I still have minor comments that I believe should be taken into account.

- In the abstract, it would be important to specify which researchers were surveyed, since you rightly point out the specific of this group of scientists, which you describe as "frontline indicators of emerging communication risks in public discourse." That said, I'm not sure if the term "frontline indicator" is entirely appropriate, since researchers who do not conduct research in these fields are not in a position to serve as such.

- Overall, the introduction is well done, and the description of the contributions makes it clearer from the outset how the paper is positioned. Yet, in the introduction, the following sentence seems somewhat vague. What is meant by “conservative political spheres in Western societies”? Does this refer to individuals with conservative political attitudes or to the country level? “This development is more pronounced in conservative political spheres in Western societies, where scepticism towards scientific expertise tends to be more pronounced (Chinn et al., 2020; Cologna et al., 2025; Falkenberg et al., 2022; Mede et al., 2025; Mede & Schäfer, 2020; Resende & Reinke de Buitrago, 2023)” (ll. 97-98).
- The following sentence is somewhat clumsy; you might consider splitting it into several sentence: “These dynamics do not silence scientific voices outright (e.g., Cologna et al., 2025; van Eck, 2023), but they can systematically narrow the conditions under which public engagement is perceived as viable or worthwhile (e.g., Mede & Volk, 2025; Peters, 2024), particularly when they are reinforced by polarised discourse environments 205 (e.g., Chinn et al., 2020; Falkenberg et al., 2022) and digitally mediated 206 communication practices (e.g., Earl et al., 2022; Pascale, 2019)” (ll. 202-207).
- I enjoyed reading the introduction of discursive repression as an analytical framework in your literature review and was wondering whether your theoretical contribution, particularly its application to individual actors, could be revisited in the discussion section.
- I think the hypotheses are well introduced. However, Hypothesis 3 is still not entirely clear; it remains somewhat ambiguous whether it refers specifically to support following negative experiences or whether the relationship is discussed more generally.
- Regarding the measurement, what are the surveyed topics based on?
- It remains unclear how the poor fit of the PANAS scale was addressed.
- It becomes clear from the reported numbers that there were substantial fluctuations in sample size; this should be discussed in more detail as well (see, e.g., “Pairwise deletion was used for missing data ($n = 50-129$)”).
- Overall, it would also be desirable to report how many participants indicated that they had given interviews or posted on social media: “Among scientists who had given interviews or posted on social media, all reported at least one positive experience (100%) (l. 477)
- In the results section, or at least in the discussion, it should also be addressed that the sample size varies considerably because not all participants had relevant experiences. For example, the regression model is based on only 69 cases. The transparent reporting of the statistics makes the limitations related to the sample size more apparent.
- At first glance, the variation in N/n between the two mediation model, one including and one excluding demographic covariates, is not immediately clear; what accounts for this difference (p. 19)
- Are the scientists examined in the study truly highly engaged in public discourse, based on their self-reported levels of public engagement? (ll. 773-774)
- Overall, it should also be made more explicit in the appendix how many cases were missing in each instance (“Missing data were handled using full-information maximum likelihood (FIML) for endogenous variables (exogenous variables treated as fixed)”).

Comments by Editors

Thank you again for submitting the revised, and much improved manuscript for publication in EPO! We have finally received all three reviews and would like to inform you that two reviewers demand only minor revisions and one reviewer suggests acceptance. We concur with the reviewers and would like to request minor revisions. If you appropriately address the reviewers' points or why you do not include the comments in the manuscript, the likelihood is high that your manuscript will be accepted and published in EPO.

In the reviews, the referenced lines in the manuscript are based on the numbering of the anonymized version of your submission. We attached this file for you. Below, we stress some specific comments from the reviews and add some additional comments:

1. Two of the cited manuscripts/preprints (Finnerty et al., 2025; Finnerty, 2025) have either been published (Finnerty, 2026) or updated/will be published on 17 June 2026 (Finnerty et al., 2026). Below you can find the updated references.
2. Please refine the wording in the abstract on the role of organisational support ("Perceived organisational support was positively associated with willingness to engage and appeared to buffer the negative association between reputational harm and willingness to engage"). The positive association only holds in the bivariate correlation, but disappears when controlling for other variables. The presentation of the relation in the abstract is therefore somewhat under complex.
3. In Figures S1 and S2, please add the corresponding numbers above each bar.
4. Please check again all tables regarding the formatting of statistical abbreviations and values. Especially in the Supplementary Materials, there are inconsistencies between tables whether b's and betas are reported with or without a zero before the decimals (b's with 0 and betas without 0). Also in some CIs, beta boundaries > 1 are reported although betas cannot go beyond 1.
5. For the PANAS scale, you could compute modification indices for the CFA and potentially add error covariances between items where sensible to improve model fit.

We hope that these comments are helpful. We are looking forward to your revisions of this important work. If you have any question about the reviews and our own comments, please contact us.

References

- Finnerty, S. (2026). Scientists as activists: An ethnography of the 'critical moments' in scientists' transition to climate activism. *PLOS Climate*, 5(2), e0000828. <https://doi.org/10.1371/journal.pclm.0000828>
- Finnerty, S., Sachisthal, M., Cologna, V., Dablander, F., Messling, L., & van Eck, C. (2026). *Climate Advocacy and Activism by Scientists: A Narrative Review*. OSF. https://doi.org/https://osf.io/preprints/psyarxiv/6c24e_v2

Authors' Response

Comments by the editors

Thank you again for submitting the revised, and much improved manuscript for publication in EPO! We have finally received all three reviews and would like to inform you that two reviewers demand only minor revisions and one reviewer suggests acceptance. We concur with the reviewers and would like to request minor revisions. If you

appropriately address the reviewers' points or why you do not include the comments in the manuscript, the likelihood is high that your manuscript will be accepted and published in EPO.

Thank you very much for the positive feedback. We are pleased and grateful for your comments; we have carefully incorporated the remaining suggestions and have thoroughly reviewed the manuscript once again, making both formal corrections and linguistic revisions. All changes beyond spelling and grammar are tracked in the attached document. Below, we address the remaining points in detail.

In the reviews, the referenced lines in the manuscript are based on the numbering of the anonymized version of your submission. We attached this file for you. Below, we stress some specific comments from the reviews and add some additional comments:

6. Two of the cited manuscripts/preprints (Finnerty et al., 2025; Finnerty, 2025) have either been published (Finnerty, 2026) or updated/will be published on 17 June 2026 (Finnerty et al., 2026). Below you can find the updated references.

Thank you for providing the updated references. We replaced the earlier preprint citations with the 2026 versions throughout the manuscript and reference list. We also updated the reference of Mede & Volk.

7. Please refine the wording in the abstract on the role of organisational support ("Perceived organisational support was positively associated with willingness to engage and appeared to buffer the negative association between reputational harm and willingness to engage"). The positive association only holds in the bivariate correlation, but disappears when controlling for other variables. The presentation of the relation in the abstract is therefore somewhat under complex.

Thank you for pointing this out. We revised the abstract to distinguish the bivariate association from the results of the adjusted moderation model. The revised wording now makes clear that perceived organisational support did not show a significant main effect on willingness to engage, whereas the interaction with reputational harm remained significant. Because this clarification increased the length of the abstract, we also streamlined the remaining text to comply with the 200-word limit without changing its substantive content. We made the corresponding clarification and adjustment in the German abstract as well with the same consequences for any further changes due to the character limit.

8. In Figures S1 and S2, please add the corresponding numbers above each bar.

Thank you for your careful reading. We added them.

9. Please check again all tables regarding the formatting of statistical abbreviations and values. Especially in the Supplementary Materials, there are inconsistencies between tables whether b's and betas are reported with or without a zero before the decimals (b's with 0 and betas without 0). Also in some CIs, beta boundaries > 1 are reported although betas cannot go beyond 1.

Thank you for noting these inconsistencies. We checked all tables and harmonized the formatting of statistical abbreviations and values. We standardized the use of leading zeros across tables: leading zeros are used for coefficients, standard errors, test statistics, and confidence interval bounds, whereas p values are reported without leading zeros. Regarding the β confidence intervals exceeding |1| in Tables S5 and S6, we note that standardized regression/path coefficients are not mathematically bounded by -1 and +1, unlike correlations. Such values can occur in multiple regression or path models, particularly when predictors are correlated or suppression effects are present

(Bring, 1994; Deegan, 1978). We therefore retained these estimates where they resulted from the fitted models, while harmonizing the formatting across all tables.

Bring, J. (1994). How to standardize regression coefficients. *The American Statistician*, 48(3), 209–213.

<https://doi.org/10.1080/00031305.1994.1047605>

Deegan, J. (1978). On the occurrence of standardized regression coefficients greater than one. *Educational and Psychological Measurement*, 38(4), 873–888. <https://doi.org/10.1177/001316447803800404>

10. For the PANAS scale, you could compute modification indices for the CFA and potentially add error covariances between items where sensible to improve model fit.

We followed your advice and inspected modification indices for the one-factor PANAS model. Modification indices of at least 10 suggested three residual covariances: PANAS4 with PANAS5, PANAS5 with PANAS7, and PANAS4 with PANAS6. Freely estimating these covariances improved model fit, scaled $\chi^2(11) = 17.89$, $p = .084$, robust CFI = .973, robust TLI = .949, SRMR = .049, and robust RMSEA = .114, 90% CI [.000, .206]. The revised model fit significantly better than the original model, $\Delta\chi^2(3) = 11.07$, $p = .011$, and no remaining modification indices exceeded 10. Because the residual covariances were added post hoc, the respecified model should be interpreted cautiously.

We hope that these comments are helpful. We are looking forward to your revisions of this important work. If you have any question about the reviews and our own comments, please contact us.

Reviewer A

Dear author(s),

The authors have substantially revised the manuscript, and it is much improved. They have engaged thoughtfully and in detail with the comments from all reviewers and the editors. In this instance, removing the qualitative analysis was the right call (though some of that language remains in the discussion, see my comment below). I commend the care taken across the revision and am happy to recommend publication.

I have one point for the authors to address. The opening of the Discussion (p. 22) states that scientists "reported benefits such as increased visibility and greater confidence in communicating" and that "many participants described negative experiences". This phrasing reads as a summary of open-ended responses, but the qualitative analysis has been removed from the manuscript. I would suggest rewording these sentences so that the benefits and negative experiences are attributed explicitly to the relevant quantitative measures and removing any wording that implies participant description of open-text material. Each should correspond to a measured item.

This is a small wording matter, and I see no obstacle to publication once it is resolved.

Thank you very much for your positive assessment of the revised manuscript. We also appreciate your careful reading and the remaining wording suggestion. We revised the opening of the Discussion so that the findings are clearly attributed to the reported constructs.

Reviewer B

Great job incorporating the reviewers' feedback! The manuscript has gained immensely in structure, flow and clarity, I have nothing more to add.

Thank you very much!

Reviewer C

Thank you very much for the revised version of your manuscript and for your thorough consideration of the comments from the other reviewers as well as my own remarks. It is understandable that the qualitative analysis is therefore no longer included; however, this is somewhat unfortunate given its potential contribution. Taken together, in my opinion, the revision has refined and improved your manuscript. However, I still have minor comments that I believe should be taken into account.

- In the abstract, it would be important to specify which researchers were surveyed, since you rightly point out the specific of this group of scientists, which you describe as “frontline indicators of emerging communication risks in public discourse.” That said, I’m not sure if the term “frontline indicator” is entirely appropriate, since researchers who do not conduct research in these fields are not in a position to serve as such.

Thank you for this clarification. We revised the abstract to describe the sample more precisely as scientists in German-speaking countries with varying levels of public engagement who were recruited through professional networks such as Scientists4Future.

We also agreed that the term “frontline indicator” could be misleading. We therefore replaced it with the more cautious formulation “exploratory indicator of emerging communication risks in public discourse” in the Limitations section.

- Overall, the introduction is well done, and the description of the contributions makes it clearer from the outset how the paper is positioned. Yet, in the introduction, the following sentence seems somewhat vague. What is meant by “conservative political spheres in Western societies”? Does this refer to individuals with conservative political attitudes or to the country level? “This development is more pronounced in conservative political spheres in Western societies, where scepticism towards scientific expertise tends to be more pronounced (Chinn et al., 2020; Cologna et al., 2025; Falkenberg et al., 2022; Mede et al., 2025; Mede & Schäfer, 2020; Resende & Reinke de Buitrago, 2023)” (ll. 97-98).

Thank you for pointing out this ambiguity. We intended to refer to politically conservative milieus within Western societies rather than to differences between countries. We therefore clarified the level of analysis and narrowed the statement to politicised domains such as climate change.

- The following sentence is somewhat clumsy; you might consider splitting it into several sentence: “These dynamics do not silence scientific voices outright (e.g., Cologna et al., 2025; van Eck, 2023), but they can systematically narrow the conditions under which public engagement is perceived as viable or worthwhile (e.g., Mede & Volk, 2025; Peters, 2024), particularly when they are reinforced by polarised discourse environments (e.g., Chinn et al., 2020; Falkenberg et al., 2022) and digitally mediated communication practices (e.g., Earl et al., 2022; Pascale, 2019)” (ll. 202-207).

Thank you for this suggestion. We agree that the original sentence was overly complex. We therefore divided it into shorter sentences.

- I enjoyed reading the introduction of discursive repression as an analytical framework in your literature review and was wondering whether your theoretical contribution, particularly its application to individual actors, could be revisited in the discussion section.

Thank you for this valuable suggestion. We now make more explicit how a structural process may become consequential for individual scientists, without reducing the concept to individual experiences. First, we clarify that discursive repression in democratic public discourse may operate through reputational mechanisms that increase the perceived professional risks of public engagement and thereby affect individual scientists' engagement decisions. Second, we added an illustrative case from Germany showing how repeated ideological and personalising media frames can transform a technical policy debate into a partisan conflict and thus may create conditions in which public engagement by experts appears increasingly risky. The revised passages in the Discussion section now read:

"From this perspective, discursive repression in democratic public discourse appears to operate primarily through reputational mechanisms that increase the perceived professional risks of public engagement (e.g., Murunga et al., 2024) and may thereby become consequential for individual scientists' engagement decisions."

and

"The 2023 controversy over Germany's Buildings Energy Act illustrates how re-peated ideological and personalising frames can transform a technical policy debate into a partisan conflict (Loschke et al., 2026). From a discursive-repression per-spective, such framing dynamics may make public engagement appear increasingly risky to experts."

- I think the hypotheses are well introduced. However, Hypothesis 3 is still not entirely clear; it remains somewhat ambiguous whether it refers specifically to support following negative experiences or whether the relationship is discussed more generally.

We clarified that we refer to organizational support after negative experiences in public discourse.

- Regarding the measurement, what are the surveyed topics based on?

Thank you for asking us to clarify how the topic categories were selected. The item was designed as a descriptive multiple-response checklist rather than as a psychometric scale. Accordingly, our goal was to ensure content-valid coverage of salient domains of public scientific engagement in socio-ecological and politically contested debates, not to create an exhaustive or mutually exclusive taxonomy. The predefined categories were developed through expert judgment within the author team, which is an established source of content-validity evidence when item content is evaluated against the intended construct domain (Haynes et al., 1995). The list included core ecological crisis domains reflected in the planetary-boundaries literature, such as climate change, biodiversity loss, and planetary boundaries (Richardson et al., 2023), as well as societal issues in which scientific expertise often enters politically contested public discourse, such as migration, social justice, war, human rights, and gender (Scheufele, 2014). To reduce the risk of construct underrepresentation, respondents could select multiple categories and could additionally choose "other topics" and specify them in an open-text field. We have clarified this rationale in the Methods section.

Haynes, S. N., Richard, D. C. S., & Kubany, E. S. (1995). Content validity in psychological assessment: A functional approach to concepts and methods. *Psychological Assessment*, 7(3), 238–247.

<https://doi.org/10.1037/1040-3590.7.3.238>

Richardson, K., Steffen, W., Lucht, W., Bendtsen, J., Cornell, S. E., Donges, J. F., Drüke, M., Fetzner, I., Bala, G., von Bloh, W., Feulner, G., Fiedler, S., Gerten, D., Gleeson, T., Hofmann, M., Huiskamp, W., Kummu, M., Mohan, C., Nogués-Bravo, D., ... Rockström, J. (2023). Earth beyond six of nine planetary boundaries. *Science Advances*, 9(37), eadh2458. <https://doi.org/10.1126/sciadv.adh2458>

Scheufele, D. A. (2014). Science communication as political communication. *Proceedings of the National Academy of Sciences*, 111(Supplement 4), 13585–13592. <https://doi.org/10.1073/pnas.1317516111>

- It remains unclear how the poor fit of the PANAS scale was addressed.

Please note our response #5 to the editor above. We inspected modification indices for the one-factor PANAS model. Modification indices of at least 10 suggested three residual covariances: PANAS4 with PANAS5, PANAS5 with PANAS7, and PANAS4 with PANAS6. Freely estimating these covariances improved model fit, scaled $\chi^2(11) = 17.89$, $p = .084$, robust CFI = .973, robust TLI = .949, SRMR = .049, and robust RMSEA = .114, 90% CI [.000, .206]. The revised model fit significantly better than the original model, $\Delta\chi^2(3) = 11.07$, $p = .011$, and no remaining modification indices exceeded 10.

- It becomes clear from the reported numbers that there were substantial fluctuations in sample size; this should be discussed in more detail as well (see, e.g., “Pairwise deletion was used for missing data ($n = 50–129$)”).

Thank you for highlighting this issue. We have added a brief clarification to the Limitations section explaining that sample sizes varied across analyses because some measures applied only to participants with relevant media or negative experiences and because of item nonresponse. We also now state explicitly that findings based on smaller analytic samples should be interpreted particularly cautiously.

- Overall, it would also be desirable to report how many participants indicated that they had given interviews or posted on social media: “Among scientists who had given interviews or posted on social media, all reported at least one positive experience (100%) (l. 477)

We added the number in the text: Among scientists who had given interviews or posted on social media ($n = 105$; 76.09%), all reported at least one positive experience (100%).

- In the results section, or at least in the discussion, it should also be addressed that the sample size varies considerably because not all participants had relevant experiences. For example, the regression model is based on only 69 cases. The transparent reporting of the statistics makes the limitations related to the sample size more apparent.

Thank you for this comment. The analysis-specific sample sizes are reported in the Results section and the corresponding tables, including the sample size of $n = 69$ for the second regression model. To make the implications of these varying sample sizes more explicit, we have now added a clarification to the Limitations section. Please also see our response to the preceding comment.

- At first glance, the variation in N/n between the two mediation model, one including and one excluding demographic covariates, is not immediately clear; what accounts for this difference (p. 19)

The difference in N between the two mediation models reflects model-specific FIML estimation. The data set contained 138 participants. In the model without covariates, 98 participants had usable information on the variables included in that model. In the model with covariates, additional covariates, including gender and tenure, were included and were available for more participants; therefore, lavaan retained 138 participants under missing = "fiml.x". Thus, the reported N s refer to the number of cases used by lavaan for each model, not to the number of complete cases on all model variables.

- Are the scientists examined in the study truly highly engaged in public discourse, based on their self-reported levels of public engagement? (ll. 773-774)

Thank you for raising this point. We agree that the term "highly engaged" was too strong given the self-reported frequency of public engagement. We therefore revised the wording to avoid overstating the level of engagement in the sample.

- Overall, it should also be made more explicit in the appendix how many cases were missing in each instance ("Missing data were handled using full-information maximum likelihood (FIML) for endogenous variables (exogenous variables treated as fixed)")

We revised the Supplementary Materials accordingly. We now distinguish between the full data set size, the model-specific N used by lavaan, and the variable-specific numbers of observed and missing cases. The full data set comprised $N = 138$ participants. lavaan used $N = 138$ cases for the mediation model with covariates and $N = 98$ cases for the mediation model without covariates. We added an additional table to the Supplementary Materials, Table S7, which provides a variable-level missing-data overview, including N observed, N missing, and the percentage of missing values for each variable included in the mediation models. The table notes now clarify that FIML uses all available information, so participants did not need complete data on all model variables to contribute to model estimation.

Review Round 3

Comments by Editors

Dear Myriam N. Bechtoldt, Felix Peter, Phillip Gutberlet,

we are delighted to let you know that your manuscript "Between Public Engagement and Discursive Repression: Media-Related Communication Risks for Scientists in the Context of Socio-Ecological Crises" has been accepted for publication in *Environmental Psychology Open*. Congratulations!

Please note that our decision is contingent on adherence to all requirements of the production process. Before we will send you the proofs, we ask you to make some final changes to the main text and supplementary materials. Please find our comments below (the lines correspond to the tracked changes version of the manuscript):

Main Text

- Line 298ff: Please split the sentence in multiple sentences: "Traditional news outlets (e.g., newspapers, television) continue to have a comparatively high level of credibility (Fotopoulos, 2023; Chinn et al., 2020), but often frame scientific issues in ways that create false balance, thereby lending disproportionate visibility and legitimacy to marginal or dissenting viewpoints (Brüggemann & Engesser, 2017; Chinn et al., 2020; Logemann et al., 2025), with such frames potentially diffusing into broader political discourse and contributing to shifts in policy trajectories (Loschke et al., 2026)."
- Line 466: There is a dot missing: "Negative affectivity was assessed using seven items from the Positive and Negative Affect Schedule (PANAS; Krohne et al., 1996; McDonald's $\omega = .93$, e.g., "annoyed," "frightened," "irritated"), following the instruction, "How did these negative consequences make you feel?" Respondents answered using a slider bar ranging from 0 to 100."
- The abbreviations for the PANAS items (e.g., PANAS4) are not used anywhere else in the main text or supplementary materials. Please add them in the corresponding SM table, so that readers know which items you are referring to.
- Line 482: It should probably read fitted? "The revised model fit significantly better than the original model"
- Figure 3: Just a suggestion: You could also color the dots corresponding to the two organizational support levels.

Supplementary Materials

- In the overview, you use different font sizes and types between the titles and the numbers. Please standardize.
- Also minuses are formatted differently between and within tables. Please standardize.
- Table S1: You don't need an "i" after the lambda as you only have one factor. Please also check in the later tables with only one factor
- Table S1: Please add dots after each item.

- Table S1 Note: Please round percentages to one decimal (i.e., 39.4%). Please check this in the whole SM document.
- Table S2: Change the i's after the lambdas to 1 and 2. Also there seems to be something off with the rotated sums of squared loadings. 4.891 and 3.655 (round to two decimals please) should not equal such different variance shares. Please check.
- Table S3: You present only one factor, but the communalities don't equal the squared factor loadings. Please explain this discrepancy in the note. Please also add dots at the end of each item.
- Table S4: Please add lines above and below "Psychosomatic Complaints" and the first item is missing a dot at the end.
- Table S5 Note: N should be in italics.
- Table S8 Note: n should be in italics

Thank you for choosing Environmental Psychology Open and we look forward to publishing your article.

Best wishes,
Daniel Hanss and Lukas Engel
for Environmental Psychology Open

Authors' Response

Dear Editors,

We sincerely thank you for the conditional acceptance of our paper and for the careful and constructive comments on the revised manuscript and Supplementary Materials. We are very grateful for the opportunity to make these final improvements. Below, we address each comment individually and explain the corresponding revision or, in the few cases in which we respectfully retained the original presentation, the reason for doing so.

1. Reviewer comment: Line 298ff: Please split the sentence into multiple sentences.

We agreed that the original sentence combined several conceptually distinct claims and was unnecessarily difficult to process. We therefore divided it into three sentences.

2. Reviewer comment: Line 466: There is a dot missing in the sentence describing the PANAS instruction.

Thank you for drawing our attention to the punctuation. We carefully rechecked the sentence. The quoted instruction is a complete question: "How did these negative consequences make you feel?" The question mark already functions as the sentence-final punctuation. Adding a full stop after the closing quotation mark would therefore result in double terminal punctuation. We consequently retained the original punctuation and did not add an additional full stop.

3. Reviewer comment: The abbreviations for the PANAS items (e.g., PANAS4) are not used anywhere else in the main text or Supplementary Materials. Please add them in the corresponding SM table.

Thank you for this useful request. Table S4 now identifies the measure in its first line as “Negative Affectivity (PANAS; Krohne et al., 1996).”

4. Reviewer comment: Line 482: It should probably read “fitted” rather than “fit.”

Thank you. Both “fit” and “fitted” occur as past-tense forms, with some variation across varieties of English and disciplinary usage. To avoid this stylistic ambiguity altogether, we revised the sentence to read: “The revised model showed significantly better fit than the original model.” This wording is unambiguous and directly conveys the result of the model comparison.

5. Reviewer comment: Figure 3: You could also colour the dots corresponding to the two organisational support levels.

Thank you for this thoughtful graphical suggestion. We decided to retain the points in neutral grey. Organisational support was analysed as a continuous variable, whereas the two coloured lines show model-implied values at two selected conditional levels, namely -1 SD and $+1$ SD. The observed points represent respondents across the full continuous range of organisational support and therefore do not belong uniquely to either of these two levels. Colouring each point as though it represented either “low” or “high” support could imply that the moderator had been dichotomised or that respondents formed two discrete groups, which would not reflect the analysis. The neutral point colour also helps distinguish the observed data from the two model-based conditional effects. We therefore did not change the colours of the points.

6. Reviewer comment: In the overview of the Supplementary Materials, different font sizes and types are used for the titles and page numbers. Please standardise them.

Thank you. We standardised the typeface and font size used for the table and figure titles and their corresponding page numbers in the overview. The overview now uses a consistent typographic format throughout.

7. Reviewer comment: Minus signs are formatted differently between and within tables. Please standardise them.

Thank you. We reviewed the numerical entries and confidence intervals throughout the Supplementary Materials and standardised the presentation of negative signs within and across the tables.

8. Reviewer comment: Table S1: You do not need an “i” after lambda because there is only one factor. Please also check the later tables with only one factor.

Thank you for raising this point. We respectfully retained the notation λ_i in the one-factor tables because the subscript i indexes the item or indicator, not the factor. Thus, λ_i denotes the loading of item i on the single extracted factor. In a one-factor solution, the factor index can indeed be omitted, but the item index remains informative because the table reports a distinct loading for every item. Writing only λ would denote a generic loading without explicitly identifying the item-specific parameter. The table note defines λ_i as the item loading. We hope that this clarification makes the indexing convention transparent.

9. Reviewer comment: Table S1: Please add full stops after each item.

Thank you. We added full stops to the end of all item statements in Table S1.

10. Reviewer comment: Table S1 note: Please round percentages to one decimal place and check this throughout the Supplementary Materials.

Thank you. We rounded the reported EFA variance percentages to one decimal place throughout the Supplementary Materials. For example, the percentage in the Table S1 note is now reported as 39.4%, and the corresponding percentage in Table S3 is reported as 38.8%.

11. Reviewer comment: Table S2: Change the i's after lambda to 1 and 2. The rotated sums of squared loadings and the reported variance shares also appear inconsistent. Please check and round 4.891 and 3.655 to two decimals.

Thank you for identifying this issue. We rechecked the output and corrected the reporting. The values 4.891 and 3.655 are the rotated sums of squared loadings, whereas the previously reported percentages of 53.722% and 12.469% refer to the extraction sums of squared loadings before rotation. Pairing the rotated sums with those percentages was therefore misleading. The revised note reports the rotated sums as 4.89 and 3.66 and does not attach factor-specific percentages to them. It also clarifies that, because direct oblimin is an oblique rotation and the factors may correlate, the rotated sums are not additive and unique factor-specific percentages of variance cannot be assigned.

We respectfully retained the notation λ_{i1} and λ_{i2} . In this notation, i indexes the item, whereas 1 and 2 index the two factors. Accordingly, λ_{i1} denotes the loading of item i on Factor 1, and λ_{i2} denotes the loading of the same item on Factor 2. This two-index notation distinguishes both the indicator and the factor and is consistent with the notation λ_i used in the one-factor tables. Using only λ_1 and λ_2 would omit the item index and could be interpreted as referring to one generic loading for each factor. We hope that this explanation clarifies why we retained the item index.

12. Reviewer comment: Table S3: Only one factor is presented, but the communalities do not equal the squared factor loadings. Please explain this discrepancy in the note and add full stops after each item.

Thank you for pointing this out. We clarified the source of the apparent discrepancy. Because the number of factors was not fixed in advance, the principal axis factor analysis initially extracted two factors. Based on our factor-retention decision, however, only the first factor was retained for substantive interpretation and scale construction and is therefore displayed in Table S3. The b^2 values reported in the table are the extraction communalities from the complete two-factor exploratory solution. They reflect the common variance accounted for jointly by both extracted factors and therefore do not necessarily equal the squared loading on the single retained factor displayed in the table. In addition, because an oblique rotation was used, the communalities incorporate the loadings on both factors as well as the correlation between the factors. We have clarified this distinction in the table note and added full stops to the end of all item statements.

13. Reviewer comment: Table S4: Please add lines above and below "Psychosomatic Complaints," and add the missing full stop to the first item.

We added horizontal lines above and below the "Psychosomatic Complaints" heading to separate the two groups of stress items more clearly. We also added the missing full stop to the first psychosomatic-complaints item.

14. Reviewer comment: Table S5 note: N should be italicised.

The statistical symbol *N* in the Table S5 note is now italicised.

15. Reviewer comment: Table S8 note: n should be italicised.

The statistical symbol *n* in the Table S8 note is now italicised.

We again sincerely thank you for the conditional acceptance and for these careful final comments. The revisions have improved the readability, consistency, and transparency of both the manuscript and the Supplementary Materials.

Kind regards,

Myriam Bechtoldt, Felix Peter, and Phillip Gutberlet