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Environmental Psychology Open

Peer review process documentation

of the article

Validation of the Inventory of Climate Emotions (ICE) in a German sample

by

Dorothea Metzen, Paula Blumenschein, Felix Peter, Myriam N. Bechtoldt, Alexandra Lina Frisch, Mira Tschorn & Stephan Heinzel

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Environmental Psychology Open

Review Round 1

Comments by Reviewer A:

Thank you for the opportunity to review the manuscript "Validation of the Inventory of Climate Emotions (ICE) in a German sample". The manuscript presents the translation and validation of the *Inventory of Climate Emotions* (ICE) into German and examines its associations with a range of psychological and behavioral outcomes in a large, quota-sampled representative sample (regarding age, gender, federal state). The topic is highly relevant and timely, given the urgency of climate change and the growing psychological research on climate emotions. The study contributes to both methodological development and applied understanding. The translation process is exemplary: the authors employ the TRAPD approach and include a pilot study with qualitative feedback, which strengthens the validity of the German version. The large, representative sample adds robustness and external validity, and the psychometric analyses are thorough, including CFA, internal consistency checks, and convergent/discriminant validity assessments with appropriate multiple-testing corrections. The introduction and discussion are well-situated within the existing literature, integrating prior validation studies across languages and cultures. Despite these strengths, I have concerns about whether the current results convincingly support the claim that the ICE measures eight *distinct* emotions, as well as about the interpretive specificity of the reported associations. Please find my detailed comments below.

1. **Distinctness of ICE subscales, incremental validity, and predictive utility**
   The manuscript asserts that the ICE captures eight distinct climate emotions, yet the evidence suggests substantial conceptual and statistical overlap among most subscales. Several pairs of subscales show very high correlations (e.g., anger–sorrow r = .74; sorrow–anxiety r = .75; anger–anxiety r = .70), and nearly all subscales (except *contempt* and, to a lesser extent, *enthusiasm*) exhibit highly similar association patterns with external variables. The convergent/discriminant validity assessment relies on the Fornell–Larcker criterion, which—while useful—does not establish discriminant validity in the broader sense, as no external constructs were included that would be expected to relate differently to the subscales. As such, the evidence presented does not convincingly demonstrate that the subscales are separable in practice or that they offer predictive or explanatory value beyond what could be obtained from a general negative climate emotion factor. To more directly assess whether the eight-factor ICE structure offers meaningful added value beyond a more parsimonious representation, I ask that the authors: 1) **Test a higher-order factor model** in which the eight first-order emotion factors load onto a single overarching "general climate emotion/distress" factor. This model would allow a direct comparison with the current correlated eight-factor solution and provide a clear indication of whether the ICE primarily reflects a common higher-order construct. Model fit indices (CFI, TLI, RMSEA, SRMR, scaled χ^2, AIC/BIC) should be reported alongside those from the current model. 2) **Test the predictive value of subscales** by entering all eight subscale scores simultaneously into multiple regression models predicting key external outcomes (e.g., collective climate action, policy support, depression/anxiety symptoms). The R code I reviewed already had some of these analyses in it, but based on what was described in the manuscript, it seemed like the authors only presented bivariate correlations there. This will help determine whether any subscale explains variance above and beyond the others. Given the high intercorrelations among subscales, collinearity diagnostics (e.g., VIFs) should

be reported; if collinearity is severe, this itself would be informative and would support the conclusion that the subscales are not sufficiently distinct for simultaneous predictive use.

2. **Interpretation of association patterns**
   The analytic approach involves examining the associations between each ICE subscale and a large set of external variables. While this is informative, the high intercorrelations among subscales and the conceptual overlap among many external measures mean that significant associations are, to a large extent, expected. As table 4, almost every single emotion subscale correlates with every outcome, and often scales that were not hypothesized to correlate with a certain outcome correlate more strongly with it, than those scales the authors hypothesized would correlate with them. Likewise, some scales show distinctly lower correlations with measures they were hypothesized to correlate with than with other measures. I ask that the authors frame these findings with greater caution and explicitly discuss as a limitation the potential for redundancy across subscales and the limits this places on interpreting the results as evidence for distinct emotion–outcome relationships and discriminant validity.

3. **Theoretical rationale for choice of validation constructs**
   While the manuscript describes the set of external measures used for validation, the introduction does not provide a clear theoretical rationale for their selection. Given the breadth of variables included—ranging from self-efficacy and resilience to dietary CO_2 emissions—it would strengthen the paper to explain explicitly *why* each was chosen, what aspect of validity it was intended to test (e.g., convergent vs. discriminant), and how these choices follow from theory on climate emotions. This would help readers understand the logic of the validation strategy and how the expected patterns of associations were derived.

4. **Sample description and representativeness**
   The authors state that the sample was representative for the German population, but the main manuscript includes only very little additional information. The current manuscript points to the full description of the sample in a markdown on OSF. From the pre-registration document, I was able to discern that the sample is representative with regard to age, gender and federal state. This information should be added to the manuscript and the authors should detail, how gender identity was assessed (only binary or additional options?). Additionally, I would ask the authors to include at least a short description of the sample composition. This is particularly important given that sample representativeness may be limited on other key variables, such as ethnicity/migration background or education level, which all showed substantial associations with climate attitudes, knowledge, and emotions in prior work. I recommend including these details in the participants section and discussing any potential limitations in representativeness and how this may have affected the results in the discussion.

5. **Preregistration detail and deviations**
   The study was preregistered, which is commendable, but the preregistration lacks sufficient detail to be considered a full registration of methods and analyses. For example, the measurement model for the CFA is not specified (e.g., factor loadings, cross-loadings, error covariances, estimation method, fit criteria), leaving it unclear whether the CFA was confirmatory in the strict sense. Also, no indices were defined for all the questionnaires included. Furthermore, some data exclusion criteria (e.g., use of the longstring response filter) were not preregistered. These may be reasonable, but they represent post hoc decisions that should be described transparently in the main text, including their potential impact on results. I would ask the authors to highlight deviations from the preregistration in the main text

(currently the authors refer to an external document, which is difficult to consult alongside reading the main manuscript). Moreover, I had some difficulty navigating the R markdown that was provided, as it seemed to contain many more analyses than the ones presented in the manuscript. A clearer structure as to what was confirmatory and exploratory in the script would help remedy this.

6. **CFA**
   For transparency and to aid interpretation, I recommend including a figure of the CFA model with all factor loadings, factor covariances, and error variances labeled. This would allow readers to directly inspect the model parameters and more fully evaluate the measurement structure.

7. **Tempering of claims and expansion of limitations**
   The Discussion repeatedly uses strong language to assert the distinctness and importance of the eight climate emotions (e.g., "This indicates that the different climate emotions are mostly distinct from each other"; "We thereby confirm the importance of investigating the effects of distinct climate emotions"). Given the high intercorrelations and similar association patterns reported, these statements are likely overstated. I recommend tempering such claims to reflect the data more accurately and expanding the limitations section to explicitly address the possibility that the emotions may not, in fact, be strongly distinct in this dataset.

8. **Critical engagement with the original ICE validation**
   The manuscript does not engage critically enough with the limitations of the original ICE validation work. The Polish validation study (Marczak et al., 2023) included only 319 adults, which is a relatively small sample for an eight-factor CFA, and the validation was limited in scope. Moreover, as the authors themselves note, even in the original validation the "distinct emotions were similarly related to external variables," suggesting that the subscales did not predict substantially different outcomes. This is a central issue for the interpretation of the German validation, and it should be addressed more explicitly in both the Introduction and the Discussion. A more critical review of prior work would help contextualize the present findings, especially when considering whether the ICE subscales in the German version truly capture distinct constructs or largely reflect a common underlying factor.

9. **Minor points**

- Section 2.4.2. should be labelled "convergent" validity

Conclusion

This study provides a German translation of the ICE, using a rigorous translation process and a large, representative sample. The paper is relevant, methodologically competent in many respects, and well-embedded in the literature. Addressing the issues noted above—particularly clarifying the distinctness and incremental predictive value of the subscales, providing a stronger theoretical rationale for validation constructs, reporting key sample characteristics in the main text, and tempering claims in line with the data—would substantially strengthen the manuscript and provide a firmer basis for the substantive conclusions.

Recommendation: Revisions Required

Comments by Reviewer B:

The paper "Validation of the Inventory of Climate Emotions (ICE) 1 in a German sample" describes the results of a systematic validation of the ICE for the German language. The validation replicated the original structure of the ICE and produced acceptable to good consistencies.

The paper presents the current state-of-the-art with regard to measuring climate emotions well. Also, the condensed presentation of the status quo in research on relation between climate emotions and pro-environmental behaviour is well-presented. The study presented was pre-registered and the hypotheses presented in table 1 are convincing based on the literature review. Inspecting the deviation from the pre-registration presented in the supplementary material, the changes are marginal and correct smaller errors/improve the quality.

The implementation of the validation study is of high methodological rigour (including translation of the items). Excluding participants from the final sample is described well and is documented sufficiently. The statistical analyses are adequate (and sophisticated).

The results of the pilot study are interesting as they might point to weaknesses of the original ICE scales. The main analysis is solid and conclusive, the discussion gives some helpful indication to develop the ICE further.

Overall, this is a very high-quality paper that describes a well implemented study with conclusive results. I recommend accepting the paper as it is (which is not a usual thing to do for me), probably with some minor clarifications:

- When the included measurements are introduced in the method section, some of them appear to be general although they are related to climate change (risk perception, efficacy, social influence) whereas others actually are general (e.g., wellbeing, resilience, ...). Please include "climate-change" in the title of the ones that are specific.
- In line 591ff, the authors claim that no studies on interventions to alleviate negative health effects of climate emotions have been conducted, which is not completely correct. There are some few studies that implement exactly the interventions that are described here. One such study has just been published by Eklöf & Klöckner (2025): Full article: Helping young people cope with eco-anxiety: a qualitative intervention study
- Line 640: "CO2" -> "CO_2"
- Lines 650ff: The non-correlations of the CO_2 footprint with the ICE scales points to a (somewhat concerning) reoccurring finding: While psychological variables are often very capable of predicting other psychological variables (like intentions or self-reported behaviours), the relation to independently measured impacts is often close to 0. Maybe this should make us environmental psychologists reconsider what we are focussing on?

Recommendation: Accept Submission

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Authors' Response to the Reviewers Round 1

Authors' Response to the Editor:

We would like to thank Daniel Hanss for the chance to revise our manuscript and the valuable feedback. We have reworked the formatting regarding capitalization in and italicization. Furthermore, we added confidence intervals to Tables 2, 3 and 4. Please find our responses to Reviewer A and Reviewer B below.

Authors' Response to Reviewer A:

Thank you for the opportunity to review the manuscript "Validation of the Inventory of Climate Emotions (ICE) in a German sample". The manuscript presents the translation and validation of the *Inventory of Climate Emotions* (ICE) into German and examines its associations with a range of psychological and behavioral outcomes in a large, quota-sampled representative sample (regarding age, gender, federal state). The topic is highly relevant and timely, given the urgency of climate change and the growing psychological research on climate emotions. The study contributes to both methodological development and applied understanding. The translation process is exemplary: the authors employ the TRAPD approach and include a pilot study with qualitative feedback, which strengthens the validity of the German version. The large, representative sample adds robustness and external validity, and the psychometric analyses are thorough, including CFA, internal consistency checks, and convergent/discriminant validity assessments with appropriate multiple-testing corrections. The introduction and discussion are well-situated within the existing literature, integrating prior validation studies across languages and cultures. Despite these strengths, I have concerns about whether the current results convincingly support the claim that the ICE measures eight *distinct* emotions, as well as about the interpretive specificity of the reported associations. Please find my detailed comments below.

Reviewer Comment 1: Distinctness of ICE subscales, incremental validity, and predictive utility
The manuscript asserts that the ICE captures eight distinct climate emotions, yet the evidence suggests substantial conceptual and statistical overlap among most subscales. Several pairs of subscales show very high correlations (e.g., anger–sorrow r = .74; sorrow–anxiety r = .75; anger–anxiety r = .70), and nearly all subscales (except *contempt* and, to a lesser extent, *enthusiasm*) exhibit highly similar association patterns with external variables. The convergent/discriminant validity assessment relies on the Fornell–Larcker criterion, which—while useful—does not establish discriminant validity in the broader sense, as no external constructs were included that would be expected to relate differently to the subscales. As such, the evidence presented does not convincingly demonstrate that the subscales are separable in practice or that they offer predictive or explanatory value beyond what could be obtained from a general negative climate emotion factor. To more directly assess whether the eight-factor ICE structure offers meaningful added value beyond a more parsimonious representation, I ask that the authors: 1) **Test a higher-order factor model** in which the eight first-order emotion factors load onto a single overarching "general climate emotion/distress" factor. This model would allow a direct comparison with the current correlated eight-factor solution and provide a clear indication of whether the ICE primarily reflects a common higher-order construct. Model fit indices (CFI, TLI, RMSEA, SRMR, scaled χ^2, AIC/BIC) should be reported alongside those from the current model. 2) **Test the predictive value of subscales** by entering all eight subscale scores simultaneously into multiple regression models predicting key external outcomes (e.g., collective climate action, policy support, depression/anxiety symptoms). The R code I reviewed already had some of these analyses in it, but based on what was described in

the manuscript, it seemed like the authors only presented bivariate correlations there. This will help determine whether any subscale explains variance above and beyond the others. Given the high intercorrelations among subscales, collinearity diagnostics (e.g., VIFs) should be reported; if collinearity is severe, this itself would be informative and would support the conclusion that the subscales are not sufficiently distinct for simultaneous predictive use.

***Authors' Response 1**:*

Before getting into the comment, we would like to thank Reviewer A for their very thorough and constructive feedback and for really taking the time to critically examine our manuscript. We believe that incorporating their suggestions into the manuscript improved its quality greatly and we hope that these revisions address Reviewer A's concerns adequately.

(1) We agree that the overlap and distinction of different climate emotions is a highly important topic and one of the biggest limitations of the ICE scale as it currently is. As recommended by Reviewer A, we conducted a second-order factor CFA. Comparing the fit-indices of both models, we found that the first-order model fits the data better than the second-order model. Thus, a broad general "climate emotion" factor seems unlikely. The results are discussed in the manuscript in the following section:

2.4.3. Additional Analysis:

"Like reported in previous studies (Marczak et al., 2024; Marczak et al., 2023), climate emotions correlated highly with each other (see Table 2) and showed similar association patterns with external variables (see Table 4). To investigate the possibility that all climate emotions load onto one higher order factor, we calculated a second CFA. Here, all eight first-order climate emotions loaded onto a single overarching "climate emotion" factor."

3.3. Confirmatory Factor Analysis

"To test for an overarching second-order "climate emotion" factor, we conducted a second CFA. Results can be found in the analysis report under "5.3. Second Order Factor Model". Fit indices indicated worse fit than the first-order model (scaled $\chi 2(456) = 1648.955$, $p < .001$, SRMR = 0.077, scaled RMSEA = 0.052, scaled TLI = 0.91, scaled CFI = 0.92) and Chi-Square Difference Test (Schermelleh-Engel et al., 2003) indicated that the second-order model fit the data significantly worse than the first-order model ($\chi 2diff = 526.57$, $dfdiff = 20$, $p < 0.001$). The inferior fit of the second-order model suggests that ICE emotions are not adequately represented by a single higher-order construct."

(2) We also completely agree with Reviewer A that it is important to investigate the predictive value of the subscales simultaneously. This research question deserves more attention than we can give it in the scope of the current manuscript. Since the submission of the current manuscript, we have written a second manuscript using the same data set to investigate the multidimensional understanding of climate emotions and their predictive power concerning pro-environmental behavior in great detail. This manuscript includes regression analyses with all eight climate emotions and hierarchical regression analyses to investigate the role of climate emotions together with further predictors of pro-environmental as postulated by the Social Identity Model of Pro-Environmental Action (SIMPEA). Multicollinearity analysis revealed variance inflation factors (VIF) below 5, indicating no critical level of multicollinearity. Furthermore, it includes exploratory mediation analysis. Taken together, we show that the association between climate emotions and PEB is much more complex than previously thought and that the interplay between different emotions is highly relevant and under-researched.

*The regression paper is also currently under review – due to the double-blind review I unfortunately cannot send you the link to the preprint. However, this is the anonymized pre-registration (*https://osf.io/5ay3e/?view_only=31f3b9bd58ef491280f36f8fcdc420b6*) and this is the current abstract:*

"Climate emotions are widespread affective responses to the progressing climate crisis. While mainly positive associations between pro-environmental behavior and selected emotions, such as climate anxiety or climate anger, have been observed, research following a multidimensional understanding of climate emotions is scarce. To better understand the psychological predictors of pro-environmental behaviors, we examined eight climate emotions (1) as unique predictors and (2) in combination with other predictors of the Social Identity Model of Pro-Environmental Action (SIMPEA). We recruited a German sample representative in terms of age, gender, and federal state using an online survey (N = 966) to investigate the role of our predictors for collective climate action, social influence behavior and diet-related CO2 emissions. Regression analyses revealed climate isolation, guilt, enthusiasm, and surprisingly contempt as positive predictors and climate powerlessness as a negative predictor of collective climate action and social influence behavior. Social identity and social norms emerged as further positive predictors. Meanwhile, climate anger, climate sorrow and social norms predicted less diet-related CO2 emissions while climate guilt, contempt and powerlessness showed positive associations. Climate anxiety did not predict any of the outcomes when other predictors were considered. Climate emotions outperforming established SIMPEA predictors suggest giving them a prominent role within theoretical frameworks on pro-environmental behavior as proximal predictors. Specific emotions such as climate isolation and enthusiasm are currently overlooked and should be given more attention in future research."

We have added a section to the discussion concerning the results of the regression manuscript and what they mean for the ICE:

"However, it is conceivable that distinct emotions like anger, sorrow and anxiety as different reactions to different aspects of the underlying threat still have distinct effects on important behaviors, which is information that is lost when summarizing them into one factor. This is underlined by our first-order model outperforming the second-order model. Thus, it is unlikely that all climate emotions load onto one common factor. Using the same data set as in this study, our group investigated the unique predictive value of climate emotions for collective climate action and social influence using multiple regression analyses in a recent preprint (xxxx, 2025). We found that climate contempt, enthusiasm, powerlessness, guilt, isolation and anxiety explained unique variance in both PEB outcomes. [...] This finding highlights that different climate emotions are mostly distinct from each other and that an individual may experience them in different intensities at different points in time. However, the conceptual overlap and simultaneous predictive use of climate anxiety, anger and sorrow is in need of further investigation."

Reviewer Comment 2: Interpretation of association patterns

The analytic approach involves examining the associations between each ICE subscale and a large set of external variables. While this is informative, the high intercorrelations among subscales and the conceptual overlap among many external measures mean that significant associations are, to a large extent, expected. As table 4, almost every single emotion subscale correlates with every outcome, and often scales that were not hypothesized to correlate with a certain outcome correlate more strongly with it, than those scales the authors hypothesized would correlate with them. Likewise, some scales show distinctly lower correlations with measures they were hypothesized to correlate with than with other measures. I ask that the authors frame these findings with greater caution and explicitly discuss as a limitation the potential for redundancy across subscales and the limits this places on interpreting the results as evidence for distinct emotion–outcome relationships and discriminant validity.

***Authors' Response 2**: As already discussed in Author Response 1, redundancy between emotions might be an issue, but there is also empirical evidence that supports a unique attribution of climate emotions with PEB. However, for most outcomes such research is missing, so we agree with the reviewer and added the following block to the limitation section:*

"Furthermore, while most of our hypotheses were supported by the data, the results indicate that many climate emotions are associated with external variables, even in cases where we did not formulate hypotheses about the existence or direction of such associations. In addition, some emotions showed weaker correlations with the measures they were hypothesized to relate to than with other measures (e.g., climate contempt). While this partly reflects the limited literature on which our hypotheses could be based, it is important to consider when interpreting the results. Conceptual overlap between emotions (e.g. climate anxiety, sorrow, anger), emotions and external variables (e.g. climate anxiety and social identity (xxxx. et al (2025)) or between external variables (e.g. wellbeing and depression and anxiety) may lead to redundancy. Future research would therefore benefit from examining the unique associations between climate emotions and outcomes, as well as their interplay in predicting these outcomes."

Reviewer Comment 3: Theoretical rationale for choice of validation constructs

While the manuscript describes the set of external measures used for validation, the introduction does not provide a clear theoretical rationale for their selection. Given the breadth of variables included—ranging from self-efficacy and resilience to dietary CO_2 emissions—it would strengthen the paper to explain explicitly *why* each was chosen, what aspect of validity it was intended to test (e.g., convergent vs. discriminant), and how these choices follow from theory on climate emotions. This would help readers understand the logic of the validation strategy and how the expected patterns of associations were derived.

***Authors' Response 3:** We added a section to 1.1. The current study explaining our reasoning for variable selection:*

"Furthermore, we wanted to investigate the association of the distinct climate emotions with pro-environmental behavior and other important variables and test construct validity. We wanted to investigate convergent validity by investigating the association between climate emotions and the established HEAS subscales (Hogg et al. 2021). Moreover, personal risk perception, self-efficacy and collective-efficacy were chosen to test discriminant validity as they are found alongside climate emotions in theoretical models explaining different forms of pro-environmental behavior (Fritsche et al. 2018, Lehrer et al. 2024). As presented in the introduction, climate emotions have been of great interest when trying to explain pro-environmental behavior and mental health indicators. Thus, we investigated the association of climate emotions and pro-environmental social influence, dietary CO_2 emissions, collective climate action, policy support, wellbeing, depression and anxiety and resilience to test for predictive validity."

Reviewer Comment 4: Sample description and representativeness

The authors state that the sample was representative for the German population, but the main manuscript includes only very little additional information. The current manuscript points to the full description of the sample in a markdown on OSF. From the pre-registration document, I was able to discern that the sample is representative with regard to age, gender and federal state. This information should be added to the manuscript and the authors should detail, how gender identity was assessed (only binary or additional options?). Additionally, I would ask the authors to include at least a short description of the sample composition. This is particularly important given that sample representativeness may be limited on other key variables, such as ethnicity/migration background or education level, which all showed substantial associations with climate attitudes, knowledge, and emotions in prior work. I recommend including these details in the participants

section and discussing any potential limitations in representativeness and how this may have affected the results in the discussion.

Authors' Response 4: *Thank you for pointing out the missing information about what variables we sampled for! We added the information. We also added how gender was assessed. We added the following information to Supplementary Table 3: annual household income (after tax), country of birth, highest level of school education and highest professional education. We also added a brief overview about these variables to the text. The participants section now reads:*

"For the main study, we quota sampled 1156 participants via the service provider Bilendi (Bilendi GmbH - Uhlandstr. 47, 10719 Berlin – Germany) in an online-survey presented in Unipark (QuestBack GmbH, Oslo, Norway). We acquired this large representative sample (for age, gender and federal state) for multiple research questions (https://osf.io/xtw2p/?view_only=1d10ffb4c85b4b9ca9a2227eb74cd1eb). Gender was assessed by asking participants "How would you describe your gender?". Possible answers were "male", "female", and "diverse"."

"Based on this, 56 participants were removed, totaling a sample of 966 individuals (51% women, 0% diverse, M_{age} = 49.88 years, SD_{age} = 16.15 years, age range = 18 – 80 years). Demographics (age, gender, federal state, household income, education, country of birth) of the participants are found in Supplementary Table 3. The majority of participants (≈57%) reported an annual household income be-tween €24,000 and €80,000 after tax, while 10% chose not to disclose their income. Almost all participants were born in Germany (96%). With respect to educational background, just over half had completed Abitur (12-13 years, 52%), followed by Realschule (10 years, 34%) and Hauptschule (9-10 years, 12%). Regarding professional qualifications, the most common were apprenticeships (31%), vocational or business school degrees (18%), and university degrees (Bachelor: 13%, Master: 15%, PhD: 2%). All participants gave informed consent."

Reviewer Comment 5: Preregistration detail and deviations

The study was preregistered, which is commendable, but the preregistration lacks sufficient detail to be considered a full registration of methods and analyses. For example, the measurement model for the CFA is not specified (e.g., factor loadings, cross-loadings, error covariances, estimation method, fit criteria), leaving it unclear whether the CFA was confirmatory in the strict sense. Also, no indices were defined for all the questionnaires included. Furthermore, some data exclusion criteria (e.g., use of the longstring response filter) were not preregistered. These may be reasonable, but they represent post hoc decisions that should be described transparently in the main text, including their potential impact on results. I would ask the authors to highlight deviations from the preregistration in the main text (currently the authors refer to an external document, which is difficult to consult alongside reading the main manuscript). Moreover, I had some difficulty navigating the R markdown that was provided, as it seemed to contain many more analyses than the ones presented in the manuscript. A clearer structure as to what was confirmatory and exploratory in the script would help remedy this.

Authors' Response 5: *As suggested, we have added all information from Supplementary Table 1 to the main text. I also restructured the R markdown file, hopefully it is now easier to navigate. The corresponding section in the text read:*

2. Methods

"In the preregistration, the variable "CO2 footprint (diet)" was labeled "eco-friendly diet" and expected effects were therefore reversed compared to Table 1. As we have converted the answer of participants into CO2 emissions (see 2.3. Measures), we made these changes to make the results easier to interpret."

2.2. Participants

"Due to this, we additionally filtered the data for conspicuous response styles. This exclusion criterion was not pre-registered, but seemed important to the authors for meaningful data interpretation. We used the longstring function from the R careless package (version 1.2.2.) to identify the longest string of identical answers for every participant."

2.4.2. Convergent Validity

"To investigate convergent validity, we calculated the average variance extracted (AVE), which is the amount of variance captured by a construct compared to the amount of variance that is attributed to the measurement error. This step was not preregistered, but recommended after preregistration by an expert in the field. "

Reviewer Comment 6: CFA
For transparency and to aid interpretation, I recommend including a figure of the CFA model with all factor loadings, factor covariances, and error variances labeled. This would allow readers to directly inspect the model parameters and more fully evaluate the measurement structure.

Authors' Response 6: *The requested Figure was added to the analysis report (3. Confirmatory Factor Analysis), as the maximum number of figures and tables that the manuscript can have is already reached.*

Reviewer Comment 7: Tempering of claims and expansion of limitations
The Discussion repeatedly uses strong language to assert the distinctness and importance of the eight climate emotions (e.g., "This indicates that the different climate emotions are mostly distinct from each other"; "We thereby confirm the importance of investigating the effects of distinct climate emotions"). Given the high intercorrelations and similar association patterns reported, these statements are likely overstated. I recommend tempering such claims to reflect the data more accurately and expanding the limitations section to explicitly address the possibility that the emotions may not, in fact, be strongly distinct in this dataset.

Authors' Response 7*: see Author response 1.*

Reviewer Comment 8: Critical engagement with the original ICE validation
The manuscript does not engage critically enough with the limitations of the original ICE validation work. The Polish validation study (Marczak et al., 2023) included only 319 adults, which is a relatively small sample for an eight-factor CFA, and the validation was limited in scope. Moreover, as the authors themselves note, even in the original validation the "distinct emotions were similarly related to external variables," suggesting that the subscales did not predict substantially different outcomes. This is a central issue for the interpretation of the German validation, and it should be addressed more explicitly in both the Introduction and the Discussion. A more critical review of prior work would help contextualize the present findings, especially when considering whether the ICE subscales in the German version truly capture distinct constructs or largely reflect a common underlying factor.

Authors' Response 8*: We thank the reviewer for the suggestion. We added the following paragraphs to the introduction where the ICE is discussed.*

"The original Polish version was validated via confirmatory factor analysis in a representative sample of 319 adults (Marczak et al., 2023). While this is a represents a relatively small sample for an eight-factor analysis, the English and Norwegian versions were validated in a representative sample of 485 Irish, 659 Australian and 491 Norwegian adults, respectively (Marczak et al., 2024; Rice et al., 2025)."

"These studies indicate that the ICE is a valid instrument for assessing climate emotions and has the potential to explain variance in important outcomes such as policy support. However, they also highlight several limitations. First, the climate powerlessness scale shows low internal consistency and limited convergent validity (Marczak et al., 2023, Marczak et al., 2024; Rice et al., 2025). Second, although the eight-factor model demonstrated good fit across studies, climate sorrow did not emerge as a factor in the exploratory factor analysis on which the ICE was built. Instead, the original analysis suggested a seven-factor solution, and climate sorrow was added on theoretical grounds. Finally, all validation studies reported high correlations between climate anxiety, climate anger, and climate sorrow (ranging from r = 0.65 to r = 0.79; Marczak et al., 2023; Rice et al., 2025), raising questions about the conceptual distinctiveness of these three subscales."

For changes in the discussion and limitation section, see Authors Response 1.

Reviewer Comment 9: Minor points

- Section 2.4.2. should be labelled "convergent" validity

Authors' Response 9: *The heading has been changed.*

Authors' Response to Reviewer B:

The paper "Validation of the Inventory of Climate Emotions (ICE) 1 in a German sample" describes the results of a systematic validation of the ICE for the German language. The validation replicated the original structure of the ICE and produced acceptable to good consistencies.

The paper presents the current state-of-the-art with regard to measuring climate emotions well. Also, the condensed presentation of the status quo in research on relation between climate emotions and pro-environmental behaviour is well-presented. The study presented was pre-registered and the hypotheses presented in table 1 are convincing based on the literature review. Inspecting the deviation from the pre-registration presented in the supplementary material, the changes are marginal and correct smaller errors/improve the quality.

The implementation of the validation study is of high methodological rigour (including translation of the items). Excluding participants from the final sample is described well and is documented sufficiently. The statistical analyses are adequate (and sophisticated).

The results of the pilot study are interesting as they might point to weaknesses of the original ICE scales. The main analysis is solid and conclusive, the discussion gives some helpful indication to develop the ICE further.

Overall, this is a very high-quality paper that describes a well implemented study with conclusive results. I recommend accepting the paper as it is (which is not a usual thing to do for me), probably with some minor clarifications:

Reviewer Comment 1: When the included measurements are introduced in the method section, some of them appear to be general although they are related to climate change (risk perception, efficacy, social influence) whereas others actually are general (e.g., wellbeing, resilience, ...). Please include "climate-change" in the title of the ones that are specific

Authors' Response 1: *Before getting into the comments, we want to thank Reviewer B for their positive evaluation and their decision to accept the manuscript.*

As suggested, we have marked which of the measures are climate change specific in the headings.

- *Personal Risk Perception → Personal Climate Change Risk Perception*
- *Efficacy → Climate Change Efficacy*
- *Social Influence → Pro-environmental Social Influence*

Reviewer Comment 2: In line 591ff, the authors claim that no studies on interventions to alleviate negative health effects of climate emotions have been conducted, which is not completely correct. There are some few studies that implement exactly the interventions that are described here. One such study has just been published by Eklöf & Klöckner (2025)

Authors' Response 2: *Thank you for the reference! We changed the section accordingly, it now reads:*

"Reoccurring content of possible interventions is accepting and validating climate emotions as a healthy response, while also encouraging individuals to take action and helping them to build up resilience and social connections. However, the body of literature empirically testing said interventions is small. Lindhe et al. (2023) tested an internet-delivered cognitive behavioral therapy for adult 30 participants experiencing psychological distress related to the climate crisis in Sweden. In comparison to the control group, mental health outcomes improved in the experimental group. In an interview study, emotion regulation techniques and the social context were identified as effective means by which an after-school intervention program supported young people in coping with climate anxiety (Eklöf & Klöckner, 2025). Considering the accumulated evidence for a negative association between mental health and climate emotions, advancing the empirical evaluation of these suggested intervention approaches represents an important next step."

Reviewer Comment 3:

- Line 640: "CO2" -> "CO_2"
- Lines 650ff: The non-correlations of the CO_2 footprint with the ICE scales points to a (somewhat concerning) reoccurring finding: While psychological variables are often very capable of predicting other psychological variables (like intentions or self-reported behaviours), the relation to independently measured impacts is often close to 0. Maybe this should make us environmental psychologists reconsider what we are focussing on?

Authors' Response 3*: We fully agree with Reviewer B's second point. Accurately measuring CO_2 impact has been a persistent methodological challenge for our and many other research groups. Given the considerable discrepancy between perceived and actual CO_2 emissions, conventional private-sphere pro-environmental behavior questionnaires likely capture pro-environmental attitudes rather than actual behavioral impact. At the same time, it is not clear whether more direct or objective measures of CO_2 emissions would provide a satisfactory solution. Such an approach risks reinforcing the narrative that responsibility for emission reduction lies primarily with individual consumers, thereby supporting forms of "greenwashing," while underemphasizing systemic and structural drivers of emissions. An alternative strategy may be to focus on indicators of collective climate action aimed at structural change, rather than relying exclusively on individual-level CO_2 footprints. While the present manuscript may not be the ideal place for an in-depth discussion of this issue, it is a topic that is increasingly debated within the field*

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Review Round 2

Comments by Reviewer A:

I thank the authors for their responsiveness to my earlier comments. The manuscript has improved in clarity, structure, and transparency. The revised version reads well and demonstrates that the authors took the review process seriously and implemented most of the requested changes in a thoughtful way. The addition of the higher-order factor analysis was particularly helpful, as it directly responds to the concern about the potential presence of a general "climate emotion" factor. The results show that the eight-factor model fits the data somewhat better than the higher-order model (ΔCFI ≈ .03, ΔTLI ≈ .04). However, given the substantial number of additional parameters in the first-order model, it is important to acknowledge that the second-order model still demonstrated acceptable overall fit (CFI = .92, TLI = .91, RMSEA = .052). This means the data are generally compatible with both interpretations, eight correlated but partly distinct emotions, or one dominant underlying "general climate emotion" factor with some residual specificity. It would therefore be useful if the discussion reflected this nuance more clearly and avoided implying that the one-factor solution can be entirely ruled out on empirical grounds.

The added discussion of high intercorrelations among the negative emotions and the phrasing of the interpretation now strike a better balance between recognition of distinctness and acknowledgment of overlap. The manuscript also benefits from the newly added rationale for the choice of validation measures. Likewise, the expanded discussion of previous ICE validation studies now provides a more nuanced and critical overview of the existing evidence base. The improved limitations section captures many of the interpretive constraints I had highlighted, including redundancy among emotions and measures.

That said, in my eyes one central issue remains. While the authors ran the requested second-order CFA, they did not include an analysis testing whether the eight subscales have incremental predictive value beyond one another. Instead, they refer to a separate manuscript using the same data set. I fully understand wanting to treat this as a separate paper and would not expect these analyses to be added here. However, it is important that the discussion section makes clear that the present study does not establish whether the subscales predict distinct variance in external outcomes. The authors may therefore wish to slightly strengthen the relevant paragraphs in the discussion to emphasize that, although the eight-factor structure fits somewhat better than a higher-order model, this does not in itself demonstrate distinct predictive utility. For instance, the sentence "Thus, it is unlikely that all climate emotions load onto one common factor." seems an overreach given the fit they found for the one factor solution. A brief clarification that this question remains open and will be addressed in subsequent work or referencing the results reported in their print in much more detail is needed to clarify this. With either added, I would consider the manuscript fully responsive to my earlier concerns.

Comments by Reviewer B:

Dear authors, as I was already convinced about the manuscripts value in the first round, it comes as no surprise that I still consider this a strong manuscript which I recommend to accept. The answers to reviewer A and the corresponding edits in the manuscript have made it even better with some interesting additional analyses and information added.

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Authors' Response to the Reviewers Round 2

Authors' Response to Reviewer A:

We thank the reviewer for their overall positive evaluation of our revision. We rewrote the respective part of the discussion. First, as requested the sentence "*Thus, it is unlikely that all climate emotions load onto one common factor*" was removed. The section discussing the interpretation of factorial structure now read like this:

"However, it is conceivable that distinct emotions like anger, sorrow and anxiety as different reactions to different aspects of the underlying threat still have distinct effects on important behaviors, which is in-formation that is lost when summarizing them into one factor. While our first-order model outperformed the second-order model, the overall fit of the second-order model was acceptable. The data could thereby be interpreted within both factorial solutions. Thus, the potential existence of an overarching climate emotion factor remains an open question that will need to be addressed in subsequent work."