

Peer Review File

Positive, Global, and Health or Environment Framing Bolsters
Public Support for Climate Policies



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27th Apr 22

Dear Dr Dasandi,

Your manuscript titled "Which Climate Frames Matter? A Multi-Country Conjoint Study of Framing on Public Support for Climate Policy" has now been seen by 2 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in *Communications Earth & Environment*, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In particular, please ensure the revised manuscript meets the following editorial thresholds:

**** Present a compelling conjoint analysis of the influence of framing with respect to climate change messaging including a thorough rationale of your study design and a robust discussion of the study results ****

**** Clearly outline the limitations associated with your messaging and other aspect of your study design as specifically highlighted by both reviewers ****

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:

[link redacted]

**** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first ****

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at *Communications Earth & Environment* or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these

revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Joe Aslin

Senior Editor,
Communications Earth & Environment
<https://www.nature.com/commsenv/>
Twitter: @CommsEarth

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Please refer to our data policies at <http://www.nature.com/authors/policies/availability.html>.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you for the opportunity to review this manuscript. There is a lot to like about this study, including aspects of survey design and the cross-country comparison. It is a generally clearly written piece of work, and the authors do a good job of describing the methods and analysis in a lay-reader friendly way. Alongside the aspects I appreciate are two fairly substantial reservations that I hold about the study. In addition, are more specific points that are listed below. I'll describe the two substantial reservations ahead of the list of more specific points (there is some cross over between the substantial points and the list of additional points).

First, the study does not give space for expressing opposition and measures only support. Although the WTP measures are used as a check of the validity of the choice options, the proposition to participants is which is most likely to cause support. Given that opposition to climate policy is a significant issue, not just weak support, this means that there is a gap between the types of decisions respondents are being asked to make and the types of decisions they would make in a 'real world' setting. Much to their credit, the researchers are transparent about this in line with the rest of the generally clear and well-written manuscript. But, given the artificiality of this response option, it raises questions about the external validity of the study. Additionally, some of the description of results is phrased in terms of 'reduce' or 'increase' support, which is at odds with the actual measure used. I can see the potential to provide a rationale for this based on the inevitability of trading off the complexity of real world settings in order to administer a survey instrument, and to add caveats around the findings that speak to this issue.

Second, and in my view more important than the above issue is that the analyses disguise the nuance

between people by offering aggregate analyses at the national (and pooled) level. There is a vast literature base that demonstrates the variation between people's support for climate policy (and general climate attitudes) based on a number of variables, including several that demographic type variables that are measured in this study. The supplementary materials demonstrate that there are differences in the marginal means for these various groups. But, in the main analysis by aggregating together these differences are lost. It is good to see the differentiation between convinced v. unconvinced in the main text, though this is weakened by being side-by-side comparison of separate analyses. Articles such as the following speak to the importance of various factors shaping differences between people in levels of support for climate policy:

Hornsey, MJ, Harris, EA & Fielding, KS 2018, 'Relationships among conspiratorial beliefs, conservatism and climate scepticism across nations', *Nature Climate Change*, vol. 8, no. 7, pp. 614-620.

Hornsey, MJ, Harris, EA, Bain, PG & Fielding, KS 2016, 'Meta-analyses of the determinants and outcomes of belief in climate change', *Nature Climate Change*, vol. 6, no. pp. 622.

Much communications literature, too, based on the variation between people and in particular the fact that different identity groups find some messengers and messages (but not others) compelling, points toward the need for targeted rather than broad communications. Relevant to the current manuscript is the fact that climate policy support can be more or less assumed among certain parts of populations, but there might be sub-sets of populations that are disproportionately important (e.g. due to strong resistance among a particularly politically significant group). This means that aggregate messaging may have the effect of boosting support among people already generally supportive (so with minimal or no material impact), but not having an effect among those who are resistant. The current manuscript runs counter to this perspective, by highlighting components of messaging based on aggregate appeal. There is clearly an argument that can be developed for this (e.g., cohesive policy messaging around national policy that is perhaps accompanied by targeted messaging to other groups), but as this is absent in the current manuscript it comes across as an oversight.

It's clear that thought and time and care have been invested into the manuscript. I do think that a different approach to the manuscript can overcome these issues, not by casting aside the data that have already been collected but instead by revisiting the analysis in a way that incorporates additional variables into the analyses (like those that are explored individually in the SI – and which show there are differences between the variables in terms of the framing effects) and reconsiders the delivery of the paper and the results, to position the findings in the context of the 'forced support' which is a fairly artificial context for issues of this nature. It is always a struggle in a short article to engage with full complexity around context, but the points above really need to be covered – if the manuscript is not adjusted to incorporate these points then it at least needs to be adjusted to acknowledge them and position the current research in relation to those points. I also encourage the authors to defend these aspects of study and analytical design should they feel there are strong arguments in favour of the current approach. Beyond the review process, reasoning of this nature could be incorporated into the supporting materials document.

Best wishes to the authors for the process ahead.

Main text:

Page 4 line 9: “Participants were provided with pairs of these randomly generated messages and asked to select which of the two would make them more likely to support policies to tackle climate change.” This does not offer the option for respondents to indicate a general lack of support, or an opposition to climate mitigation policy. This is an important point of disjuncture between the study design and the real world context of policy contestation and debate. Respondents who are simply unsupportive of climate policy do not have the option of indicating this, instead they are forced into suggesting which statement is most likely to increase support. The WTP measures might be able to detect such respondents, i.e. with a low z-score for the WTP responses, but this is not incorporated into the primary analysis (instead used as a robustness check only).

Page 4 line 15: “There is also evidence of a growing divide between ‘convinced’ and ‘sceptics’, with reduced support among sceptics for policies framed around tackling climate change.” This statement needs revision – the sources cited are out of date and are overly generalised given the many differences between nations (e.g. see Hornsey, MJ, Harris, EA & Fielding, KS 2018, 'Relationships among conspiratorial beliefs, conservatism and climate scepticism across nations', *Nature Climate Change*, vol. 8, no. 7, pp. 614-620.)

Page 4 line 22: “The results of the pooled sample demonstrate that positive frames increase public support, while negative frames reduce support.” Related to the comment a couple above. Given the survey design – testing which statement is most likely to engender support – the phrasing of the discussion of results needs to be aligned with this design. I am not sure that the results can be taken to indicate ‘reduce support’, as there does not appear to be a measure of opposition. This is likely just a matter of going across the statements with a careful eye to ensure that the phrasing (which does a good job in providing lay-reader friendly understandings of the results) is true to what can actually be taken from the study.

Page 4 line 22: “The results of the pooled sample demonstrate that positive frames increase public support, while negative frames reduce support.” Additionally, given that the countries show quite different results, it does not seem useful to include the pooled sample – this gives a ‘headline’ finding that obscures the difference between the countries.

Page 8 line 5: “To address concerns with the external validity of the treatments that arise with any survey experiment, we ensured that our climate change messages reflected real world climate change communications.” An explanation for how needs to be provided – was this based on a systematic approach or was it more so that observations of common messaging in the public sphere was adopted to guide statement development? It is possible that if the researchers are based in a subset of the countries studied, that the statements might reflect what is common in some but not all countries.

Page 8 line 8: “We also include a discussion of the results from an earlier pilot we carried out in each of the five countries a month earlier, in August 2020. The sample size was smaller (n=4,935) and so our estimates are less precise, but the results were replicated increasing our confidence in the findings (see Methods and Supplementary Material).” This is a little unclear – it would be appropriate to flag the pilot earlier in the piece (e.g. around the discussion of developing the instrument). Additionally, the pilot does not seem to be sufficiently important to the findings to be introduced in this way. Instead, a 1 sentence note (that is not presented as if it is signposting a major

component of the discussion) could indicate that a pilot had been conducted and the results align generally, with more info in the SM.

Page 9 line 2: “It is notable that presenting the effects of climate change at the individual level, i.e. that it will personally effect you, makes the issue less salient not more. This aligns with other research that finds that ‘self-transcendent’ as opposed to self-enhancing values are positively associated with pro-environmental behaviors.” The link here between the message framing and the influence of values orientations is dubious and should be removed.

Page 9 line 3: effect should be affect.

Page 9 line 7: “Such differences underline the importance of nationally-focused communication.” This is a true point, though the discussion has earlier said that there are some unified elements across countries. Given these points are in conflict, there should be a more integrative discussion that highlights that there might be some consistent elements and some that are specific to country context, rather than making one claim at one point then a counter claim at a later point without the integration between them.

Page 9 line 9: “These findings provide key insights for governments seeking to build public support for stronger climate policies after COP26.” It may not be governments that are attempting to mobilise public support in order to enable climate policies. In many cases, governments can be a sticking point. This sentence should be updated accordingly.

Page 9 line 12: tackling should (I assume) be tackling.

Page 10 line 14: “In China and India, the samples were representative of urban populations in the two countries rather than nationally representative.” The rationale for this selection should be provided.

Page 12 line 11: statement should be statements.

Supplementary materials:

Page 3 ‘Outcome measures’: “While we do not report those results here, we used the quantitative ratings as a robustness check to see whether respondents’ choices matched their preferences as expressed through this alternative measure.” In the SM, these results are described in terms of the verification of choice patterns early on, but the table is right at the end. It would be helpful if the table was positioned nearer the relevant text.

Page 5 point 2 in list: “challenges they were personally care about” needs rephrasing.

Page 7 fig S2 caption: “Valence: the average number of times opportunity and threat were shown was not different than half, $t = 274.31$, $p\text{-value} < 2.2e\text{-}16$.” Does this not indicate that there was indeed a difference?

Page 7 fig S2 caption: Continuing the above. “Time: the $p\text{-value}$ of the test is 0.6967, which is greater than the significance level $\alpha = 0.05$. We can conclude that the observed proportions are not

significantly different from the expected proportions. Theme: p-value of the test is 0.38. Scale: p-value = 0.33.” These results are ordered in a confusing and unclear way. Theme, Scale and Time could be put together ahead of the interpretation of the p-value?

Page 8 fig S3 caption: It would be worth noting that these are not all the statements shown to avoid readers (as I did!) attempting to sum the values and then realising that holds no meaning due to it being 6 of 96(?) possible statements.

Figure S11: Were other age brackets tested? Generational groups would be of interest, particularly testing for greater difference with older people as has been found in other studies.

End of comments.

Reviewer #2 (Remarks to the Author):

This paper uses conjoint experimental methods to test the effectiveness of four different message attributes on public support for policies to address climate change. Attributes were derived from prior research on climate change communication and framing effects. Results show that messages that use a positive, opportunity-focused frame, that focus on health and environmental themes, that focus on the immediate time scale, and that focus on global impacts are most effective at eliciting support. The paper is well-written, and the results are clearly presented.

To my knowledge, this is the first conjoint study to examine this particular combination of message attributes and their effects on climate policy support. The results are useful and can help inform climate change communication and message design. A key strength of the study is that it includes data from five different high-emitting countries (the US, UK, India, China, and Germany), enabling cross-country comparisons. The authors also examine conditional effects among those less (vs. more) concerned about climate change (as well as conditional effects based on other demographic variables, like education and gender, which are reported in the supplementary files). The study's conclusions are bolstered by the replicability of the results with those from a pilot study conducted a month earlier, as well as by the consistency between results using a dichotomous choice measure and a continuous monetary measure.

I think the results will be of broad interest to researchers and practitioners in the area of climate change communication.

I have some minor suggestions and questions for the authors to consider:

*Did the survey include any attention checks or other quality controls (beyond the consistency check between the choice and monetary measures)? If so, were these used to screen out participants? This would be helpful to note.

*Was any pre-testing conducted of the specific language and examples that were used to illustrate each theme (i.e., health, environment, economy, migration), to ensure that these were, in fact, compelling illustrations of the respective theme, particularly across the different countries?

*It would be helpful if the authors included a sentence or two noting the limitations of the conjoint

method for message testing, i.e., that respondents compare messages side-by-side, which is not how people encounter information in the real world; that the choice outcome assesses people's perception of message effectiveness, as opposed to actual message effectiveness/persuasion; etc.

* Figure S3, which presents the most and least popular profiles (i.e., combined set of attributes), is interesting. I think it would be helpful to reference this in main text, so readers know to look for it in the supplementary files.

*While it is likely beyond the scope of this paper, I'd be interested to know if there are any interactions between attributes (e.g., does the effect of theme depend on valence?). While I don't expect the authors to present these analyses, they could at least recognize the possibility of such interactions in the discussion section.

*While the comparative focus is a strength of the paper, the authors do not reflect on the sociopolitical and cultural differences that might produce differences between countries. Including some brief discussion of this in the introduction and/or discussion sections would strengthen the paper (particularly given that some cross-country differences were observed, despite overall consistency).

*I appreciate the authors' point that respondents' preference for global over individual framing aligns with research linking self-transcendent values to pro-environmental behaviors. But I also wonder if this finding could be an artifact of the comparison-oriented structure of the conjoint design – i.e., seeing a message that emphasizes personal benefits/costs juxtaposed with one that emphasizes collective benefits/costs might create a social desirability bias that leads respondents to select the collective-oriented message, regardless of which one they found more compelling. A similar social desirability dynamic could contribute to the stronger performance of the health/environmental vs. economic themes (i.e., people may think that health and the environment are more socially desirable concerns than the economy, even if an economic message might persuade them on its own). As I already noted, it's important to recognize potential limitations of the conjoint design.

*In the discussion section, it would be helpful to include some theoretical interpretation of the results. In other words, are there existing theories/research that can help us better understand or contextualize the results? As I noted in my previous comment, the authors linked the individual vs. global framing result to research on self-transcendence and environmental behavior, but they didn't do this for any of the other key findings. For example, the preference for opportunity vs. threat-based framing may reflect the role of hope versus fear in driving climate engagement.

*This is perhaps just a pet-peeve of mine, but I don't like the use of the term "nationally representative" to describe non-probability samples. I encourage the authors to tweak their language to make clearer that they used quota sampling, based on age, gender, and region, to reflect the demographic characteristics of each country's population (or urban population, in the case of China and India).

Response memo for “Which climate frames matter? A multi-country conjoint study of framing on public support for climate policy”

We would like to thank the reviewers for their incredibly helpful and constructive comments on the paper. We have responded to, and sought to address, each of the issues raised by the reviewers. Below, we respond to each of the issues raised by the two reviewers in turn, explaining how we have sought to address the issues in the manuscript. We present the reviewers’ comments in blue, and our responses underneath in black. As our responses below indicate, we have made significant changes to the manuscript to address the issues raised by the reviewers, which we believe have strengthened the paper considerably.

Responses to Reviewer 1 comments

- Thank you for the opportunity to review this manuscript. There is a lot to like about this study, including aspects of survey design and the cross-country comparison. It is a generally clearly written piece of work, and the authors do a good job of describing the methods and analysis in a lay-reader friendly way. Alongside the aspects I appreciate are two fairly substantial reservations that I hold about the study. In addition, are more specific points that are listed below. I’ll describe the two substantial reservations ahead of the list of more specific points (there is some cross over between the substantial points and the list of additional points).

We thank the Reviewer for the positive feedback and the constructive engagement with the paper. As we discuss below, we have sought to address both the two substantial issues they have raised, as well as the list of more specific points. We believe this has significantly strengthened the paper.

- First, the study does not give space for expressing opposition and measures only support. Although the WTP measures are used as a check of the validity of the choice options, the proposition to participants is which is most likely to cause support. Given that opposition to climate policy is a significant issue, not just weak support, this means that there is a gap between the types of decisions respondents are being asked to make and the types of decisions they would make in a ‘real world’ setting. Much to their credit, the researchers are transparent about this in line with the rest of the generally clear and well-written manuscript. But, given the artificiality of this response option, it raises questions about the external validity of the study. Additionally, some of the description of

results is phrased in terms of 'reduce' or 'increase' support, which is at odds with the actual measure used. I can see the potential to provide a rationale for this based on the inevitability of trading off the complexity of real world settings in order to administer a survey instrument, and to add caveats around the findings that speak to this issue.

The Reviewer has highlighted an important issue that is a limitation of forced choice conjoint analysis. This is by far and away the most common design and is defended on the assumption that the no-choice option draws proportionately from the options presented, because of randomisation. We have revised the paper to address this issue in several ways:

- We have included a discussion of the limitations of the study in the 'discussion' section on p.17. In this we clearly explain that conjoint designs do not allow respondents to express opposition to policies, and instead only measures support due to the forced choice that respondents have to make as part of the conjoint experiment.
- To try to partially account for this issue of respondents being forced to choose an option that indicates support for climate policies, we have conducted additional tests in which those respondents that expressed on the WTP question that they would be unwilling to spend any amount of money to support climate policies (i.e., those that selected 0 for the WTP response) were removed from the sample. Our justification for this is that these respondents are the most likely to oppose climate policies and because of the randomisation the choices removed will also be at random. We present the findings of this additional test in the Supplementary Material (SM) on p.5 and Figure S23 (p.30). As we indicate in the discussion (SM, p.5), though there are slight differences in the results, overall the results are broadly robust to the exclusion of the non-payers. It is important to note that we are not arguing that this additional analysis (and the results) overcome the issues of the forced choice in the conjoint experiments – as we now clearly discuss in the paper this is a limitation of the approach; however, these results may help alleviate concerns about bias in our results due to those opposed to climate policy being unable to express this opposition.
- In addition to this specific limitation raised by the Reviewer, our discussion of the limitations of the approach (p.17) highlights other limitations of the conjoint design.
- We have changed the discussion of the results from being in terms of 'reducing' or 'increasing' support to increasing/reducing the likelihood of public support for climate policies (and similar statements) throughout the paper, in line with the Reviewer's comment.

- Second, and in my view more important that the above issue is that the analyses disguise the nuance between people by offering aggregate analyses at the national (and pooled) level. There is a vast literature base that demonstrates the variation between people's support for climate policy (and general climate attitudes) based on a number of variables, including several that demographic type variables that are measured in this study. The supplementary materials demonstrate that there are differences in the marginal means for these various groups. But, in the main analysis by aggregating together these differences are lost. It is good to see the differentiation between convinced v. unconvinced in the main text, though this is weakened by being side-by-side comparison of separate analyses. Articles such as the following speak to the importance of various factors shaping differences between people in levels of support for climate policy:

Hornsey, MJ, Harris, EA & Fielding, KS 2018, 'Relationships among conspiratorial beliefs, conservatism and climate scepticism across nations', *Nature Climate Change*, vol. 8, no. 7, pp. 614-620.

Hornsey, MJ, Harris, EA, Bain, PG & Fielding, KS 2016, 'Meta-analyses of the determinants and outcomes of belief in climate change', *Nature Climate Change*, vol. 6, no. pp. 622.

Much communications literature, too, based on the variation between people and in particular the fact that different identity groups find some messengers and messages (but not others) compelling, points toward the need for targeted rather than broad communications. Relevant to the current manuscript is the fact that climate policy support can be more or less assumed among certain parts of populations, but there might be sub-sets of populations that are disproportionately important (e.g. due to strong resistance among a particularly politically significant group). This means that aggregate messaging may have the effect of boosting support among people already generally supportive (so with minimal or no material impact), but not having an effect among those who are resistant. The current manuscript runs counter to this perspective, by highlighting components of messaging based on aggregate appeal. There is clearly an argument that can be developed for this (e.g., cohesive policy messaging around national policy that is perhaps accompanied by targeted messaging to other groups), but as this is absent in the current manuscript it comes across as an oversight.

We agree with the Reviewer's comment about the importance of considering variation between people's support for climate policy based on different variables. As the Reviewer notes, we had sought to address this issue with the inclusion of the analysis of those concerned with climate change vs those unconcerned with climate change. However, based on the Reviewer's comments,

we have made major changes throughout the paper to address this issue more thoroughly. The changes we have made are as follows:

- In the introduction, we include a discussion of the research showing that attitudes to climate change and climate policy vary across people based on different attributes. We include a reference to specific studies that engage with this issue, including the references that the Reviewer has highlight (pp.4-5).
- We also include a more detailed discussion of our focus on those that are concerned about climate change and those that are not concerned in the introduction, which includes further engagement with the relevant literature (and is also linked to an issue raised by the Reviewer below) (p.5).
- In the results section, we include a subsection that specifically focuses on the effect of the frames on support for climate policy across different subgroups (pp.8-12).
- In this section, we present our analysis of the effects of the frames across different attributes (gender, age, education, income, employment, concern for climate change – Figure 2). We were not entirely clear regarding the Reviewer’s comments about the side-by-side comparison of the concern vs unconcerned analysis – however, given the use of a conjoint design, this is the standard approach to subgroup analysis using conjoints (see e.g., Bansak, Hainmueller, Hangartner, 2016). We have, therefore, included an analysis of these different attributes/subgroups by comparing the differences in the effects of frames across the groups. For the main paper, we have presented results from the pooled sample of all respondents (across the five countries) for this analysis (see Figure 2, p.11). This is because it is not feasible to include figures of results for all these attributes across the five countries in the main paper – nor do we have the space to discuss all of the individual results from each country in the main text. We have included the analysis of these attribute for each country separately in the Supplementary Materials (Figures S4-S22), and we refer to these results in the main paper (on p.8 and p.9). As we state, while there are differences in the marginal means for these groups in the country-specific analysis presented in the SM, we would argue that the results are fairly consistent across different socio-demographic characteristics. The main exception to this is when we consider differences between those that are concerned about climate change and those that are unconcerned.
- We have expanded the analysis and discussion of the comparison of the effect of the different frames in increasing the likelihood of support for climate policies among those respondents that express concern about climate change vs those that are unconcerned by climate change. In addition to the pooled results presented in Figure 2, we also include

results of this comparison for each of the five countries in Figure 3 (p.13). We also provide a more detailed discussion of these results on p.12.

- We have revised the paper to refer to this analysis of different groups throughout.

In addition to the changes that we discuss above, we also looked into conducting the analysis with the inclusion of control variables (based on these different attributes) in the regression model. However, we found that this is not feasible with the use of a conjoint design as it violates independence assumptions. Instead, in looking into how this could be done we found that an approach suggested in the literature was based on the use of 'Bayesian Additive Regression Trees' (see <https://www.jstor.org/stable/27801587>). This is basically a machine learning algorithm that fits many, many models and then estimates an aggregate model that best fits the data. As such, we employed this approach and found the results to be very consistent with our results in Figure 3. We have not included this in the paper or Supplementary Material as we felt that it doesn't actually add much, and we're worried that the method distracts from the main empirical messages. Furthermore, we feel that to include we would have to provide a fair bit of additional discussion on the method and how we have employed it. We have pasted the results below just to demonstrate that it does produce similar findings to those presented in Figure 3. The main variable of interest in the table is the 'importance' variable. It confirms that concern (concern_fac) tends to be the most important covariate across different frames and largely mirrors the Figures we have included.

row	outcome	covar	importance	lower2.5	upper97.5
1	Opportunity	contest_no	-0.1486064	-0.4511982	0.15398541
2	Opportunity	concern_fac	58.633041	24.3217012	92.9443809
3	Opportunity	agecat_bin	0.58003018	0.3315805	0.82847986
4	Opportunity	gender_fac	-0.0168627	-0.1132293	0.07950395
5	Opportunity	educ_uni_fac	70.7420591	51.491268	89.9928501
6	Opportunity	income_mea	0.01845549	-0.1071782	0.14408914
7	Opportunity	work_status	4.33844138	3.10275198	5.57413078
8	Health	contest_no	-1.1090066	-1.7848347	-0.4331785
9	Health	concern_fac	47.578808	14.5133601	80.644256
10	Health	agecat_bin	2.1741304	1.68066361	2.66759719
11	Health	gender_fac	-0.2251053	-0.7607611	0.31055037
12	Health	educ_uni_fac	-0.031848	-0.5650545	0.50135857
13	Health	income_mea	0.05292218	-0.5870905	0.6929349
14	Health	work_status	2.92576512	1.38025633	4.47127391
15	Environment	contest_no	-0.2072353	-0.5984059	0.18393539
16	Environment	concern_fac	112.043555	37.2056228	186.881487
17	Environment	agecat_bin	0.4592561	-0.0202976	0.9388098
18	Environment	gender_fac	-0.0302296	-0.1804894	0.12003031
19	Environment	educ_uni_fac	0.02453645	-0.1130453	0.16211824
20	Environment	income_mea	0.00374393	-0.1478685	0.15535638
21	Environment	work_status	15.8297876	10.2744132	21.3851621
22	Migration	contest_no	-0.2460447	-0.5715889	0.07949947
23	Migration	concern_fac	80.2350542	22.4945406	137.975568
24	Migration	agecat_bin	0.52385193	0.32653467	0.72116918
25	Migration	gender_fac	-0.0183154	-0.1383531	0.10172229
26	Migration	educ_uni_fac	26.4050017	19.2845382	33.5254653
27	Migration	income_mea	0.00506671	-0.1361432	0.14627659
28	Migration	work_status	0.60933846	-0.3455832	1.56426008
29	Community	contest_no	-0.2958636	-0.7740735	0.18234632
30	Community	concern_fac	29.7139867	9.75887267	49.6691007
31	Community	agecat_bin	1.81790404	1.23563007	2.40017801
32	Community	gender_fac	0.04726705	-0.1597989	0.25433305
33	Community	educ_uni_fac	0.06573664	-0.1474679	0.2789412
34	Community	income_mea	9.05120362	6.65195881	11.4504484
35	Community	work_status	76.5097915	37.8720272	115.147556
36	World	contest_no	-0.3176193	-0.8240746	0.18883603
37	World	concern_fac	88.6529744	37.3331035	139.972845
38	World	agecat_bin	1.38393589	0.69293939	2.0749324
39	World	gender_fac	0.04145976	-0.1562914	0.23921091
40	World	educ_uni_fac	0.03252362	-0.2272106	0.29225779
41	World	income_mea	-0.0182928	-0.4468907	0.41030519
42	World	work_status	41.5301238	17.819898	65.2403496
43	Personal	contest_no	-0.4962389	-1.0271921	0.03471434
44	Personal	concern_fac	48.0395205	14.6687686	81.4102723
45	Personal	agecat_bin	5.18903532	4.01024273	6.36782791
46	Personal	gender_fac	0.03375659	-0.2141577	0.28167083
47	Personal	educ_uni_fac	0.40099848	0.15081116	0.65118581
48	Personal	income_mea	22.9294195	17.1461045	28.7127345
49	Personal	work_status	31.1166742	13.1798985	49.0534498
50	2030	contest_no	-0.0051893	-0.3137495	0.30337086
51	2030	concern_fac	55.9054612	24.1935881	87.6173344
52	2030	agecat_bin	5.01313454	1.97144867	8.05482041
53	2030	gender_fac	0.00280423	-0.0811868	0.08679527
54	2030	educ_uni_fac	0.01247498	-0.0698443	0.09479429
55	2030	income_mea	24.9971502	19.6440828	30.3502177
56	2030	work_status	60.9609772	25.5135951	96.4083594
57	Now	contest_no	-0.0056743	-0.2728749	0.26152618
58	Now	concern_fac	56.7496882	24.2037868	89.2955897
59	Now	agecat_bin	5.12464669	2.3201095	7.92918389
60	Now	gender_fac	0.00237531	-0.0784209	0.0831715
61	Now	educ_uni_fac	0.01161189	-0.0771074	0.1003312
62	Now	income_mea	26.5315017	20.7651624	32.2978411
63	Now	work_status	58.8253168	25.0104207	92.640213

- It's clear that thought and time and care have been invested into the manuscript. I do think that a different approach to the manuscript can overcome these issues, not by casting aside the data that have already been collected but instead by revisiting the analysis in a way that incorporates additional variables into the analyses (like those that are explored individually in the SI – and which show there are differences between the variables in terms of the framing effects) and reconsiders the delivery of the paper and the results, to position the findings in the context of the ‘forced support’ which is a fairly artificial context for issues of this nature. It is always a struggle in a short article to engage with full complexity around context, but the points above really need to be covered – if the

manuscript is not adjusted to incorporate these points then it at least needs to be adjusted to acknowledge them and position the current research in relation to those points. I also encourage the authors to defend these aspects of study and analytical design should they feel there are strong arguments in favour of the current approach. Beyond the review process, reasoning of this nature could be incorporated into the supporting materials document. Best wishes to the authors for the process ahead.

We thank the Reviewer for their incredibly helpful and constructive comments. As our responses above (and to the specific points below) indicate, we have sought to address all of the issues raised. This has included a greater focus on the variables that may shape differences in people's attitudes to climate change, and greater acknowledgement of the limitations of the study, particularly given the 'forced support'. We have also sought to strengthen the justification for, the approach taken in the study throughout, in line with the Reviewer's suggestion.

Specific points raised by Reviewer in the main text:

- Page 4 line 9: "Participants were provided with pairs of these randomly generated messages and asked to select which of the two would make them more likely to support policies to tackle climate change." This does not offer the option for respondents to indicate a general lack of support, or an opposition to climate mitigation policy. This is an important point of disjuncture between the study design and the real world context of policy contestation and debate. Respondents who are simply unsupportive of climate policy do not have the option of indicating this, instead they are forced into suggesting which statement is most likely to increase support. The WTP measures might be able to detect such respondents, i.e. with a low z-score for the WTP responses, but this is not incorporated into the primary analysis (instead used as a robustness check only).

As we note in our response to the Reviewer's comments above, we have sought to address this important issue in several ways:

- First, we have included a more explicit discussion of the limitations of the conjoint analysis approach used in the study (p.17), which highlights the issue of the conjoint survey not offering respondents the option to indicate a lack of support/opposition to mitigation policy.
- Second, we have sought to further address this issue – based on the Reviewer's suggestion – by making better use of the WTP responses and our findings on those that are not concerned about the impact of climate change. We argue that those that select '0' on the WTP response are least likely to support climate mitigation policies, and hence we reran the analysis in each country with those respondents excluded from the sample. As we have discussed above, though there are slight differences in the results, overall, the results are

broadly robust to the exclusion of the no-payers. As we have stated above, we certainly do not claim that this analysis fully addresses the limitation of respondents not being able to indicate a lack of support in forced choice analyses – however, the findings can help alleviate concerns about any potential bias in our results due to the issue. It is worth saying that we have not replaced the main analysis in the study with this additional analysis that excludes WTP=0 respondents. This is because we wanted to ensure our analysis was consistent with the standard approach to conducting conjoint experiments. However, we have included this additional analysis in the Supplementary Material (p.5 and p.30), and we include explicit references to this issue and this analysis in the main paper (pp.7-8, p.17).

- Page 4 line 15: “There is also evidence of a growing divide between ‘convinced’ and ‘sceptics’, with reduced support among sceptics for policies framed around tackling climate change.” This statement needs revision – the sources cited are out of date and are overly generalised given the many differences between nations (e.g. see Hornsey, MJ, Harris, EA & Fielding, KS 2018, 'Relationships among conspiratorial beliefs, conservatism and climate scepticism across nations', Nature Climate Change, vol. 8, no. 7, pp. 614-620.)

We agree with the Reviewer that this statement needs revision, and we thank them for highlighting the alternative study. We have altered the discussion on p.5 to reflect this issue, which includes reference to the article highlighted by the Reviewer.

Page 4 line 22: “The results of the pooled sample demonstrate that positive frames increase public support, while negative frames reduce support.” Related to the comment a couple above. Given the survey design – testing which statement is most likely to engender support – the phrasing of the discussion of results needs to be aligned with this design. I am not sure that the results can be taken to indicate ‘reduce support’, as there does not appear to be a measure of opposition. This is likely just a matter of going across the statements with a careful eye to ensure that the phrasing (which does a good job in providing lay-reader friendly understandings of the results) is true to what can actually be taken from the study.

Again, we agree with the Reviewer’s comment and thank them for highlighting this issue. We have altered the discussion of the results throughout the paper to refer to the effect on the likelihood of engendering support (and similar statements).

Page 4 line 22: “The results of the pooled sample demonstrate that positive frames increase public

support, while negative frames reduce support.” Additionally, given that the countries show quite different results, it does not seem useful to include the pooled sample – this gives a ‘headline’ finding that obscures the difference between the countries.

In line with the Reviewer’s comment, we have removed the pooled sample result from Figure 1 (and the related discussion).

- Page 8 line 5: “To address concerns with the external validity of the treatments that arise with any survey experiment, we ensured that our climate change messages reflected real world climate change communications.” An explanation for how needs to be provided – was this based on a systematic approach or was it more so that observations of common messaging in the public sphere was adopted to guide statement development? It is possible that if the researchers are based in a subset of the countries studied, that the statements might reflect what is common in some but not all countries.

We have addressed this issue by removing this statement from this part of the paper, and instead included a more detailed discussion of how we sought to ensure our climate statements reflected real world climate change communications as closely as possible in the Methods section (pp.21-22). As we note, we tried to do this in several ways. As the Reviewer suggests we used observations of common messaging in the public sphere to guide the statement development. We also showed the statements to individuals we know that work on global climate communications to ensure that the statements were broadly similar to the types of messaging that would be used in actual climate change messaging. As we note later in the Methods section, we also checked the translated surveys for China and Germany with native speakers with subject expertise – this included ensuring the climate statements reflected messaging in the two countries. We also used the pilots as a test of the messaging. Given the statements are fairly brief and do not refer to country-specific issues, and given our checks, we feel that the statements broadly reflect the types of messaging that may occur in all five countries.

- Page 8 line 8: “We also include a discussion of the results from an earlier pilot we carried out in each of the five countries a month earlier, in August 2020. The sample size was smaller (n=4,935) and so our estimates are less precise, but the results were replicated increasing our confidence in the findings (see Methods and Supplementary Material).” This is a little unclear – it would be appropriate to flag the pilot earlier in the piece (e.g. around the discussion of developing the instrument). Additionally, the pilot does not seem to be sufficiently important to the findings to be introduced in this way. Instead, a 1 sentence note (that is not presented as if it is signposting a major component

of the discussion) could indicate that a pilot had been conducted and the results align generally, with more info in the SM.

Again, we agree with the Reviewer's comment. We have removed the reference to the pilot study in the discussion. As per the Reviewer's suggestion, we have instead referred to the pilot studies earlier in the paper. In discussion the different frames and climate messages on p.4, we note that we tested the different messages in earlier pilots conducted in the five countries. In the results section, we include a paragraph that discusses additional analysis to demonstrate the robustness of our findings (pp.7-8). In this we state the present the results of the pilots in the SM, and that these are broadly consistent with the results of the main analysis.

- Page 9 line 2: "It is notable that presenting the effects of climate change at the individual level, i.e. that it will personally effect you, makes the issue less salient not more. This aligns with other research that finds that 'self-transcendent' as opposed to self-enhancing values are positively associated with pro-environmental behaviors." The link here between the message framing and the influence of values orientations is dubious and should be removed.

We agree with the Reviewer that this link is dubious, and so we have removed this link from the paper.

- Page 9 line 3: effect should be affect.

We have corrected this.

- Page 9 line 7: "Such differences underline the importance of nationally-focused communication." This is a true point, though the discussion has earlier said that there are some unified elements across countries. Given these points are in conflict, there should be a more integrative discussion that highlights that there might be some consistent elements and some that are specific to country context, rather than making one claim at one point then a counter claim at a later point without the integration between them.

We have revised the discussion section in line with the Reviewer's comment. For example, on p.14, we state that conducting the analysis in the different countries "enables us to identify common elements in the effects of these frames on public support for climate policies across these different contexts, as well as those that are specific to national contexts." Our expanded discussion of the findings (which better connects our results to existing research) avoids this conflict. Instead, it highlights the differences in the effects of the valence frames in different countries (p.14), and

points to the high level of consistency in the effects of the theme frames across the different country contexts. Hence, we have revised the discussion to avoid this conflict.

- Page 9 line 9: "These findings provide key insights for governments seeking to build public support for stronger climate policies after COP26." It may not be governments that are attempting to mobilise public support in order to enable climate policies. In many cases, governments can be a sticking point. This sentence should be updated accordingly.

Again, we agree with the Reviewer's comment. We have changed this sentence to refer to 'actors' rather than 'governments' (p.17, l.18).

- Page 9 line 12: tacking should (I assume) be tackling.

Yes, that's right – we've altered this.

- Page 10 line 14: "In China and India, the samples were representative of urban populations in the two countries rather than nationally representative." The rationale for this selection should be provided.

We have provided a rationale, explaining that as a result of the use of online panels in the two countries, participants were recruited from cities (pp.18-19). We have also changed the text to avoid referring to 'nationally representative' etc., and more clearly explained that quota sampling was used to ensure representative samples based on age, gender, and region to reflect the demographic characteristics of adult populations in the cases of Germany, UK, USA – and representative of urban populations in the cases of China and India.

- Page 12 line 11: statement should be statements.

We have corrected this.

Supplementary materials:

- Page 5 point 2 in list: "challenges they were personally care about" needs rephrasing.

We have removed this text.

- Page 7 fig S2 caption: "Valence: the average number of times opportunity and threat were shown was not different than half, $t = 274.31$, $p\text{-value} < 2.2e-16$." Does this not indicate that there was indeed a difference?

We thank the Reviewer for highlighting this issue. This is an error in the analysis testing whether the mean was 0 instead of 0.5, which is what we should have been testing. We have redone this analysis, and the results make much more sense. The text now reads: “Valence: the average number of times opportunity and threat were shown was not different than half, $t = 0.23351$, $p\text{-value} = 0.8154$.” (SM, Figure S2, p.8).

- Page 7 fig S2 caption: Continuing the above. “Time: the $p\text{-value}$ of the test is 0.6967, which is greater than the significance level $\alpha = 0.05$. We can conclude that the observed proportions are not significantly different from the expected proportions. Theme: $p\text{-value}$ of the test is 0.38. Scale: $p\text{-value} = 0.33$.” These results are ordered in a confusing and unclear way. Theme, Scale and Time could be put together ahead of the interpretation of the $p\text{-value}$?

We agree with the Reviewer. We have re-written this as “Time: the $p\text{-value}$ of the test is 0.6967, Theme: $p\text{-value}$ of the test is 0.38, and for Scale: $p\text{-value} = 0.33$. All of which are greater than the significance level $\alpha = 0.05$. We can conclude that the observed proportions are not significantly different from the expected proportions” (SM, Figure S2, p.8).

- Page 8 fig S3 caption: It would be worth noting that these are not all the statements shown to avoid readers (as I did!) attempting to sum the values and then realising that holds no meaning due to it being 6 of 96(?) possible statements.

We have made this point in the caption to Figure S3 (p.9), and we have also indicated that the full list of possible statements is presented in Table S2 of the SM.

- Figure S11: Were other age brackets tested? Generational groups would be of interest, particularly testing for greater difference with older people as has been found in other studies.

We have followed the Reviewer’s suggestion and tested additional age brackets based on generational groups. We include these results of this additional analysis of different generational groups in the Supplementary Material (Figures S13 and S14, pp.20-21.).

Response to Reviewer 2 comments

- This paper uses conjoint experimental methods to test the effectiveness of four different message attributes on public support for policies to address climate change. Attributes were derived from prior research on climate change communication and framing effects. Results show that messages that use a positive, opportunity-focused frame, that focus on health and environmental themes, that focus on the immediate time scale, and that focus on global impacts are most effective at eliciting support. The paper is well-written, and the results are clearly presented. To my knowledge, this is the first conjoint study to examine this particular combination of message attributes and their effects on climate policy support. The results are useful and can help inform climate change communication and message design. A key strength of the study is that it includes data from five different high-emitting countries (the US, UK, India, China, and Germany), enabling cross-country comparisons. The authors also examine conditional effects among those less (vs. more) concerned about climate change (as well as conditional effects based on other demographic variables, like education and gender, which are reported in the supplementary files). The study's conclusions are bolstered by the replicability of the results with those from a pilot study conducted a month earlier, as well as by the consistency between results using a dichotomous choice measure and a continuous monetary measure. I think the results will be of broad interest to researchers and practitioners in the area of climate change communication. I have some minor suggestions and questions for the authors to consider.

We thank the Reviewer for their positive and constructive feedback on the paper. As we explain below, in response to specific issues raised by the Reviewer, we have made significant changes to the paper to address the Reviewer's comments, which we feel have strengthened the paper.

- Did the survey include any attention checks or other quality controls (beyond the consistency check between the choice and monetary measures)? If so, were these used to screen out participants? This would be helpful to note.

We have checked this with the survey company (Deltapoll), who confirmed that attention checks and quality controls are part of the survey process. These are not in the public domain because they are proprietary. But they confirmed that these can be used to remove a small number of respondents post-hoc (typically < 5%). We have noted this in the Supplementary Materials (p.5).

- Was any pre-testing conducted of the specific language and examples that were used to illustrate each theme (i.e., health, environment, economy, migration), to ensure that these were, in fact, compelling illustrations of the respective theme, particularly across the different countries?

We sought to ensure that messaging was compelling and reflected real world climate change communications in several ways. We have included a discussion of this in the Methods section (pp.21-22). We used observations of common messaging in the public sphere to guide the statement development and the examples we used. We also showed the statements to individuals we know that work on global climate communications to ensure that the statements were broadly similar to the types of messaging that would be used in actual climate change messaging. As we note later in the Methods section, we also checked the translated surveys for China and Germany with native speakers with subject expertise – this included ensuring the climate statements reflected messaging in the two countries. We also used the pilots as a test of the messaging.

- It would be helpful if the authors included a sentence or two noting the limitations of the conjoint method for message testing, i.e., that respondents compare messages side-by-side, which is not how people encounter information in the real world; that the choice outcome assesses people's perception of message effectiveness, as opposed to actual message effectiveness/persuasion; etc.

We agree with the Reviewer's comment that it is important to note that limitations of the approach. We have incorporated a discussion of the limitations of the conjoint method on p.17 of the paper. In this, we state that respondents compare side-by-side messages unlike in the real world, and we also highlight the issue of the choice outcome assessing perception rather than actual message effectiveness.

- Figure S3, which presents the most and least popular profiles (i.e., combined set of attributes), is interesting. I think it would be helpful to reference this in main text, so readers know to look for it in the supplementary files.

Based on the Reviewer's suggestion, we have included a reference to the Figure S3 which presents the most and least popular profiles in the discussion (p.17).

- While it is likely beyond the scope of this paper, I'd be interested to know if there are any interactions between attributes (e.g., does the effect of theme depend on valence?). While I don't expect the authors to present these analyses, they could at least recognize the possibility of such interactions in the discussion section.

We did test for interactions between the attributes, and we found some significant results. For example, we found that the threat x environment had a significant impact, as too did the threat x health and the opportunity x health interactions. However, we have not included this in the paper

because there we were not able to identify any clear substantive interpretations to the results, and because of this it would require too much additional discussion to be able to explain and include here. However, this is something that we are interested in developing in our research. Therefore, as per the Reviewer's suggestion, we have included a reference to the possibility of such interactions in the discussion section. Specifically, we highlight such interactions as a fruitful area of future research (p.17).

- While the comparative focus is a strength of the paper, the authors do not reflect on the sociopolitical and cultural differences that might produce differences between countries. Including some brief discussion of this in the introduction and/or discussion sections would strengthen the paper (particularly given that some cross-country differences were observed, despite overall consistency).

We agree with the Reviewer on the need for more focus on the differences between the countries and how these may influence our findings. We have included a brief discussion of country differences in the introduction (p.3, p.5), the methods (p.18) and in the discussion section (pp.14-16). The latter is part of the revised discussion to address the Reviewer's subsequent comment on relating the findings back to existing research. For example, we highlight difference between Germany's history of public discourse on the threat of climate change and other countries in discussing the difference in the effect of the valence frames.

- I appreciate the authors' point that respondents' preference for global over individual framing aligns with research linking self-transcendent values to pro-environmental behaviors. But I also wonder if this finding could be an artifact of the comparison-oriented structure of the conjoint design – i.e., seeing a message that emphasizes personal benefits/costs juxtaposed with one that emphasizes collective benefits/costs might create a social desirability bias that leads respondents to select the collective-oriented message, regardless of which one they found more compelling. A similar social desirability dynamic could contribute to the stronger performance of the health/environmental vs. economic themes (i.e., people may think that health and the environment are more socially desirable concerns than the economy, even if an economic message might persuade them on its own). As I already noted, it's important to recognize potential limitations of the conjoint design.

The Reviewer raises an important issue, and one that we did consider in designing the survey instrument and messages. We specifically sought to use examples for the different frames that

broadly focused on social/societal costs and opportunities to minimise any potential risk of social desirability bias. Hence, the two economic examples included in the messaging (for the threat-economic and opportunity economic frames) focus on people's livelihoods and property, and job creation and reliable energy rather than focusing on issues such as wealth or economic growth, which may be more prone to social desirability bias. Our aim in using these examples was to ensure that the economic frame was comparable to the health and environmental frames. We have included text in the Supplementary Material where we provide all 96 statements (Table S2) explaining this.

- In the discussion section, it would be helpful to include some theoretical interpretation of the results. In other words, are there existing theories/research that can help us better understand or contextualize the results? As I noted in my previous comment, the authors linked the individual vs. global framing result to research on self-transcendence and environmental behavior, but they didn't do this for any of the other key findings. For example, the preference for opportunity vs. threat-based framing may reflect the role of hope versus fear in driving climate engagement.

We agree with the Reviewer's comments. We have revised the discussion section significantly to address this point. Specifically, on pages 13-15 we discuss our findings in more detail and relate these findings to existing research in order to contextualise these results. It is worth saying that based on Reviewer 1's comments, we have removed the reference to self-transcendence and environmental behaviour. However, we more have provided a discussion of how our results relate to existing research, which we believe significantly strengthens the paper.

- This is perhaps just a pet-peeve of mine, but I don't like the use of the term "nationally representative" to describe non-probability samples. I encourage the authors to tweak their language to make clearer that they used quota sampling, based on age, gender, and region, to reflect the demographic characteristics of each country's population (or urban population, in the case of China and India).

We agree with the Reviewer's issue with the use of the term 'nationally representative'. We have changed the text in line with the Reviewer's comments in the main paper (pp.3-4, p.18) and the Supplementary Material (p.2). In line with the Reviewer's comments, we remove the term 'nationally representative', and instead we explain that for Germany, UK, and USA, we used quota sampling to ensure representative samples based on age, gender, and region to reflect the demographic characteristics of each country's adult populations. For China and India, quotas were

based on age, gender, and region to ensure that samples were representative in terms of the demographic characteristics of urban populations.

We would, once more, like to thank both reviewers for their helpful comments on the paper, which as we have described here, we have sought to address throughout the paper.

25th Jul 22

Dear Dr Dasandi,

Your manuscript titled "Which Climate Frames Matter? A Multi-Country Conjoint Study of Framing on Public Support for Climate Policy" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you for the opportunity to review this revised manuscript. For the initial submission, I was reviewer 1. My thanks to the authors for submitting thoughtful and thorough responses to my comments (and those of the other reviewer). Their revisions to the manuscript are well made, and in the cases that the authors have explained not making a change (or explained why a change was made in a particular way), their justification is sound.

I spotted just one typo and a sentence that I felt could use a couple of words to be added to improve clarity of the link between findings and implications, noted below.

All my best wishes to the authors.

Page 15 line 1: emphasising to the threat – to should be removed

Page 16 line 7: regarding the potential effectiveness of health frames on those less concerned about CC, is this claim supported by the finding that health framing is appealing to the unconvinced in China and the US (in figure 3)? If so – it would be good to make this connection explicit.

Reviewer #2 (Remarks to the Author):

I found the first version of this manuscript compelling, and the revisions have made it stronger. In particular, the authors now more effectively highlight the limitations of the conjoint method and are more attentive to theory and cross-national differences/similarities in the discussion. I also think the

additional sub-group analyses strengthen the paper. The main and supplementary analyses are rigorous. The authors do an excellent job of presenting a large amount of data and analyses in a clear and accessible way. Overall, I find the work convincing and am happy to recommend publication.

I noted a few typos:

On p. 15, in the first line, the authors should delete “to” after “emphasizing.”

Also on p. 15, in line 16, the authors should delete “lead” before “elicit support.”

Response to Referees

We would like to thank the Reviewers for their positive and helpful feedback once more. We provide responses to the two Reviewers' comments below.

Reviewer #1 (Remarks to the Author):

Thank you for the opportunity to review this revised manuscript. For the initial submission, I was reviewer 1. My thanks to the authors for submitting thoughtful and thorough responses to my comments (and those of the other reviewer). Their revisions to the manuscript are well made, and in the cases that the authors have explained not making a change (or explained why a change was made in a particular way), their justification is sound. I spotted just one typo and a sentence that I felt could use a couple of words to be added to improve clarity of the link between findings and implications, noted below. All my best wishes to the authors.

We thank the Reviewer once more for their constructive comments on the paper and their positive feedback. We have addressed the remaining issues that the Reviewer raises, as we indicate below in response to the two comments.

Page 15 line 1: emphasising to the threat – to should be removed

We have removed the 'to' on page 15. We thank the Reviewer for alerting us to this typo.

Page 16 line 7: regarding the potential effectiveness of health frames on those less concerned about CC, is this claim supported by the finding that health framing is appealing to the unconvinced in China and the US (in figure 3)? If so – it would be good to make this connection explicit.

In line with the Reviewer's suggestion, we have made this connection explicit in the manuscript.

Reviewer #2 (Remarks to the Author):

I found the first version of this manuscript compelling, and the revisions have made it stronger. In particular, the authors now more effectively highlight the limitations of the conjoint method and are more attentive to theory and cross-national differences/similarities in the discussion. I also think the additional sub-group analyses strengthen the paper. The main and supplementary analyses are rigorous. The authors do an excellent job of presenting a large amount of data and analyses in a clear and accessible way. Overall, I find the work convincing and am happy to recommend publication.

We thank the Reviewer once more for their positive comments and their incredibly helpful comments on the manuscript.

I noted a few typos:

On p. 15, in the first line, the authors should delete "to" after "emphasizing."

Also on p. 15, in line 16, the authors should delete “lead” before “elicit support.”

We thank the Reviewer for alerting us to these typos. We have corrected both of these.