

Responses to Reviewers 2 and 3

We thank the reviewers for their thoughtful and constructive comments. Here, we will respond to the comments from Reviewer 2 (Rebecca Priestley) and Reviewer 3. We responded previously to Reviewer 1 (Chris Weaver).

A few introductory remarks: We have prepared a revised version of the manuscript with several significant changes in response to the reviews and community comments:

- In the Introduction, we added a discussion of decision-making under deep uncertainty (DMDU), and the use of low-confidence science within DMDU. We have clarified that when we say a scientific claim is actionable, we mean that it justifies adaptation actions (i.e., physical measures and financial investments) in the near term. If a claim is not actionable based on current evidence, it can still be used fruitfully to explore future options in DMDU frameworks, but it should be differentiated from claims backed by more robust evidence.
- Accordingly, we reworded the criterion for actionable science in Section 2. The new criterion reads, *“A scientific claim is sufficiently accepted to justify adaptation action (i.e., near-term physical measures and financial investments) when it is supported by multiple, consistent independent lines of high-quality evidence leading to high or medium confidence, as determined by a diverse group of experts in an open, transparent process.”*
- Based on Chris Weaver’s comments, we clarified that the source of the 2.5 m Extreme Scenario in Sweet et al. (2017) was the probabilistic projection in Kopp et al. (2014), using the Antarctic projections of Bamber & Aspinall (2013). The modified text describes a narrower role for DeConto & Pollard (2016) in S17.
- We divided the previous Section 3 into two sections. The new Section 3 focuses on scientific projections (mainly DP16), and the new Section 4 discusses the communication of these projections to practitioners.
- The revised Section 4 includes some guidance on how practitioners might work with low-confidence sea-level projections in the future, given that there is not always a clear distinction between near-term adaptation actions and long-term contingency plans.

The rest of this document includes our responses to Reviewers 2 and 3. Responses are in blue font.

Reviewer 2 (Rebecca Priestley):

I found this paper very interesting. I am familiar with DeConto and Pollard’s 2016 paper, and the subsequent media coverage, but was not aware of the extent to which these projections were taken on by policymakers and practitioners. This case study aspect of the paper is very interesting and valuable (though I note the corrections advised by Chris P. Weaver in the

interactive review). I also found the comments about disciplinary journals vs high impact journals (lines 54-64) particularly valuable.

This has potential to be an important paper, so my feedback is quite detailed with much of it focused on precision of language, to ensure clear and purposeful communication of the argument of the paper.

We appreciate the reviewer's attention to precise language.

Specific comments on section 2 of the paper

I have specific comments about language use in section 2 of the paper, firstly around use of the word hypothesis (eg, line 68: 'transform novel hypotheses into accepted knowledge', line 99: 'A scientific hypothesis is sufficiently accepted for use in decision-making when it is supported by ... etc' and line 102: 'peer-reviewed hypotheses must be scrutinized by a diverse group of scientists etc'). I was surprised by the focus on the word 'hypothesis' here. Not all science starts with a hypothesis, and even when it does, this word is usually used to describe what comes at the start of a study, not the end. I would have thought that it's not the 'peer-reviewed hypothesis' that is the 'actionable' (or not) part of a research project, or the resulting paper. Rather, it's the peer reviewed conclusions, claims, findings, or theories. Or, as the quote from Behar says, the 'data, analysis and forecasts' (line 25).

We agree with the reviewer. In several instances, including ll. 24, 68, 99, 102, 204 and footnote 2 of the original manuscript, we changed "hypotheses" to "claims". We retained one instance of "hypotheses" when discussing Longino's work, since she herself uses that word. We think "claims" is more exact than "theories" (which has a broader meaning than what we want to convey) or "findings" (which does not as clearly connote the presence of uncertainty).

In the same section, I also suggest a review of the words 'viewpoints', 'opinion', and 'assumptions'. Scientists do, of course, have viewpoints, opinions, and assumptions, but this paper is focused on peer-reviewed published research which (we hope!) relies on evidence and observations that lead to claims and conclusions (even if it doesn't meet the criteria for actionable research). At the moment the paper could imply that scientists make claims in their published research, or IPCC authors make decisions, based only on opinions and assumptions (which could feed into politically motivated narratives seeking to undermine climate science).

I realise that different disciplines have different norms about language use, but with an interdisciplinary paper like this it's important that the meaning of this language is accessible to a broad readership. I suggest therefore that language use is reviewed, especially around the words I've mentioned here.

We thank the reviewer for this suggestion. We removed "viewpoints" and "opinions" on l. 74. We kept "assumptions" on ll. 47 and 76, since the process of critical scientific review to challenge implicit background assumptions is central to Longino's analysis.

Specific comments on section 3 of the paper

The first paragraph of section 3 is important, but is not communicating as clearly as it could. In lines 108-110 I suggest removing reference to 'land ice'. At the moment, the AIS is listed as an example of 'land ice' in one sentence, then the next sentence says it 'contains marine-grounded ice'. To avoid confusion, but not take away any meaning, the reference to 'land ice' could be removed and the more standard separation of SLR contributors into thermal expansion, mountain glaciers, the Greenland Ice Sheet and AIS used (as has been done in line 180). Then, in line 110, which says 'if melted, this ice could raise sea level by several meters', it needs to be explicit what ice is being referred to here.

We agree, and we revised the paragraph as follows:

Global mean sea level (GMSL) is rising by about 3.7 mm/yr, mainly because of ocean thermal expansion and the loss of ice from the Greenland and Antarctic ice sheets (GrIS and AIS) and mountain glaciers (Fox-Kemper et al., 2021). Uncertainty in long-term sea-level projections is dominated by the AIS, which contains a large volume of ice that is grounded below sea level and is vulnerable to retreat under climate warming. If melted, this Antarctic ice could raise sea level by several meters.

In line 140 it would be useful to provide the figures for the De Conto 2021 lowered 21st century SLR contribution, to allow comparison with the DP16 figure.

Thanks for the suggestion. We revised the text to read, "In a follow-up to DP16, DeConto et al. (2021) revised the atmospheric forcing, delaying hydrofracture and lowering the projected 21st century AIS sea-level contribution to 0.5 m or less, even if MICI is active." This figure comes from Table 1 of that study, which gives a median Antarctic contribution of 0.34 m and a range of 0.20–0.53 m under RCP8.5. Thus, the high-end Antarctic contribution in DeConto et al. (2021) is reduced by about 0.5 m compared to DP16.

Line 71: states that IPCC assessments 'are directed mainly to policymakers but are read by practitioners' – I suggest that the difference between policymakers and practitioners is teased out in this paper, and more emphasis given to the role of policymakers. The publications referred to in section 3 seem to be interpretations by policymakers, that were then actioned by practitioners. In other parts of the paper, though, the emphasis on practitioners suggests that they are actioning science without this layer of interpretation by policymakers. For example in line 213 is it primarily practitioners or policymakers who need to 'view novel peer-reviewed claims with caution'? In line 225, is it 'scientists and practitioners' who need to work more closely together, or scientists and policymakers?

We regret the confusion. We think of policymakers as the people who make laws and regulations, such as limits on greenhouse gas emissions. In general, policymakers would have more official power than practitioners and would be less involved in on-the-ground planning and implementation. (A similar distinction can be made between "decision-makers" and practitioners.) Both policymakers and practitioners lie on the receiving end of scientific communication; policymakers generally would not serve as intermediaries between scientists

and practitioners. The publications in Section 3 were written mainly by scientists from universities and government labs, with some representatives from the practitioner community.

We added the following text in footnote 1: “We think of practitioners as distinct from policymakers: the legislators and other government officials who make laws and regulations.”

On I. 213, it is primarily practitioners who should view novel peer-reviewed claims with caution. (We would encourage policymakers to exercise caution also, but they are not our main audience.) On I. 225, we think that scientists and practitioners should work more closely together.

Technical corrections and points of clarification

Line 27: says the term ‘actionable science’ (which I was not familiar with) has been ‘widely adopted’ but there’s only one citation here. More citations here would strengthen this claim.

We added the following citations, which refer to “actionable climate science” and “actionable climate information”, respectively:

Executive Office of the President, 2013. The President’s Climate Action Plan. Available: <https://obamawhitehouse.archives.gov/sites/default/files/image/president27sclimateactionplan.pdf>

WCRP Joint Scientific Committee (JSC), 2019. World Climate Research Programme Strategic Plan 2019–2028. WCRP Publication 1/2019. Available: https://www.wcrp-climate.org/images/documents/WCRP_Strategic_Plan_2019/WCRP-Strategic-Plan-2019-2028-FINAL-c.pdf

Others are available, including the USGCRP 2012-21 Strategic Plan, two Presidential Executive Orders, and a recent Biden-Harris administration press release regarding the Fifth National Climate Assessment, but we are mindful that the article already includes more citations than are standard for a Brief Communication.

Line 30: ‘Our goal is to offer guidance ...’ who to? Is this guidance for scientists, practitioners, or both?

We have clarified that the guidance is for both scientists and practitioners.

Line 38: As a science historian I have to note that the discipline is decades on from the ‘lone genius’ approach, as is much popular science history. This is perhaps a traditional approach, or a twentieth century approach, but I’m not sure it’s right to say ‘often’ when referring to current work.

Our sense is that while historians and philosophers of science have moved on from this approach, popular accounts in climate science and other fields have been slower to catch up. But we agree there has been progress, so we changed “often” to “sometimes”.

Lines 66, 67: Mentions first 'press releases' and then 'media accounts'. It would be good to explicitly make the connection between the press releases and the media accounts – while the press releases might cast the work in dramatic light, the media stories often go further, and the headlines (which are not written by the journalists) even further than that, with attention seeking headlines.

This is an excellent suggestion. Citing the study by Perga et al. (2023), we rewrote the first two sentences of this paragraph as follows:

To the extent practitioners learn about climate research through media reports, their attention will likely be drawn to a small number of studies in high-impact journals, focused on 21st century global-scale threats (Perga et al., 2023). Press releases from journals and universities often cast the work in a dramatic light, and media stories with attention-seeking headlines heighten the drama.

We took the liberty of using “attention-seeking headlines” from your suggestion.

Do you have a citation for the statement that ‘practitioners typically learn of scientific advances through media coverage’? (line 65)

DB has the experience of frequently receiving inquiries from fellow practitioners about media reports on new studies; these practitioners want to know whether the new studies change our basic understanding of SLR. However, we were not able to find a study that focuses on practitioner information-gathering, so we replaced this sentence with the wording above.

Line 90: citation and page number needed for this quote

The citation is Mastrandrea et al. (2010), the guidance note cited in the previous paragraph. We changed “this guidance” to “this guidance note” to make the reference more clear. In general, we have given page numbers when quoting from books (i.e., Longino) but not articles, but we can add page numbers (this one is on p. 2) if the editor thinks it would be helpful for readers.

Line 95: Makes an important point, but is it also worth noting that opting for ‘higher ground’ is not necessarily guarding against ‘unknown risks’, it could alternatively (or also) be seen as choosing an option with a longer lifespan, given that sea level rise will continue beyond 2100.

We agree that higher ground could extend the lifespan, but this perhaps is a distraction from the main point. We have rewritten the example to describe a levee with an expected lifetime of 75 years (i.e., until 2100). In this case, a decision to expand the levee footprint to potentially accommodate the greater SLR projected by a low-confidence study would not be justified by our criterion, unless the expansion was inexpensive.

Line 186: what does ‘community’ mean in this context? The scientific community?

Yes, we changed this to “scientific community”.

Line 213: Is 'contradict' the right word here, or would 'challenge' be more appropriate?

Thanks, we changed this to "challenge".

I look forward to your response. As I said at the start, this is a very interesting paper.

Thank you again for your helpful comments.

Reviewer 3:

In this manuscript, Lipscomb et al. discuss the challenges of providing 'actionable' scientific research in the context of climate adaptation. In the manuscript, the authors emphasize the importance of distinguishing between novel hypotheses/claims and actionable science that can be used for decision-making. The authors discuss this (also) within the context of a recent high-impact study projecting rapid sea level rise from the Antarctic ice sheet due to a low-confidence process. Overall, Lipscomb et al. propose (1) an epistemic criterion for determining when scientific claims are actionable, based on multiple lines of high-quality evidence and evaluated by a diverse group of experts, (2) recommendations for scientists and practitioners to improve the use of actionable science in decision-making.

This manuscript has clearly attracted lots of interest and sparked productive discussions (see comment section and follow-up AGU presentation highlighted by Chris Weaver, CC2). The authors replied already quite extensively to the main comments raised by Robert Kopp (CC1) and Chris Weaver (CC2). If the authors are willing to include their reply to CC1 and CC2 in the revised version of the manuscript (in particular, the misstatement about the relevance of DP16 on the Sweet et al. (2017) report), I will consider it ready for publication as is; the manuscript is in general very well written and clearly an excellent fit for The Cryosphere (Brief Communication).

Yes, the revised manuscript responds to these comments and states that Kopp et al. (2014) was the source of the Extreme (2.5 m) scenario in Sweet et al. (2017). We stand by our statement that the citations of DP16 in S17 served to bolster support for this scenario, even though K14 was the original source.

I do have a couple of minor comments to add, which the authors can see as a suggestion for the revised manuscript. I do not consider these (minor) comments as strictly required for publication - I rather hope they can contribute to the discussion.

1) In general, I agree with the authors on how the epistemic criterion for actionable science is formulated, and with the recommendations to scientists, journalists, and practitioners laid out in Section 4. Both the criterion and the recommendations largely rely on IPCC reports or meta analyses/community assessment. While this makes sense to me, I'm skeptical about how the criterion and recommendations could be applied in practice, as IPCC reports (or other community assessments) are published at much longer timescales than individual studies, and

media are typically very quick to pick up high-impact claims (often at the same time a new study comes out for high-impact journals). Even assuming improved awareness and communication between scientists, journalists, and practitioners in the future (which is one recommendation made by the authors and is certainly something we should aspire to), it looks to me that for a case similar to the one presented by the authors to not happen again much would be left to individual choices (for instance: being cautious when making/dealing with new claims). I am fine if the main goal of the authors is to start a discussion on the topics presented, rather than proposing some examples of practical solutions to implement their recommendations. However, I think it would be of great help to see some (more) critical reflection on the latter. For instance: should it become part of the peer-review process to have reviewers providing some level of confidence and/or rating how much a study can be considered reliable or even suitable for media coverage (using for example a formal rating system similar to the one used to evaluate originality, quality, etc.)?

This is a good point, which the three of us have talked about. The idea of adding a rating of confidence or reliability rating is intriguing, but we decided not to recommend changes in the peer-review process. In part, this is because we are uncertain that reviewers are well positioned to assess confidence, given that confidence can arise from the convergence of multiple research findings across disciplines, not all of which a single reviewer would necessarily be familiar with. We think confidence is better assessed by multidisciplinary groups as in the IPCC process.

We hope that by reading our paper, scientists and practitioners will become more aware of publishing and media incentives that might result in misinterpretation of scientific claims during adaptation planning. We think that discussions between scientists and practitioners are an important step toward finding lasting solutions. Finally, our recommendation that projections deemed by IPCC to be of low confidence are not actionable provides criteria we believe practitioners, climate service providers, and researchers alike can apply.

2) Line 65: 'practitioners typically learn of scientific advances through media coverage'. I think this is quite an important point of focus in the manuscript - the link between scientific results/media coverage/practitioners. I think however that this sentence is a bit too vague, and it would be good to have reference(s) backing it up. If there aren't, maybe it could make sense to make the example for the DP16 study, but to avoid generalizing ('typically learn').

We did not find a direct citation for this claim, so we reworded the sentence as shown above, with a citation of Perga et al. (2023).

Thank you for sharing your suggestions on our manuscript.