Responses to Reviewers

We thank the reviewers for their thoughtful and constructive comments. This document merges our previous responses to Reviewer 1 (Chris Weaver), Reviewer 2 (Rebecca Priestley), and Reviewer 3. Please also see our responses to the community comments by Robert Kopp, Jeremy Bassis, Judy Lawrence, Marjolijn Haasnoot, and Robert Lampert.

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 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 text includes some guidance on how practitioners can appropriately incorporate low-confidence sea-level projections in planning.

The rest of this document includes our responses to the reviewers. Responses are in blue font.

Reviewer 1 (Chris Weaver):

I appreciate the opportunity to comment, since I would like to point out, and help correct, an error in the paper.

Specifically, on page 6, the authors have made the following statement about the influence of DeConto and Pollard (2016) on the U.S. interagency sea level rise scenarios report of Sweet et al. (2017) (my emphasis):

“To be consistent with “recent updates to the peer-reviewed scientific literature”, they issued an “Extreme” global mean sea-level projection of 2.5 m by 2100 for RCP8.5, exceeding the previous upper bound of 2.0 m based on Pfeffer et al. (2008). Their Extreme projection relied on a large AIS contribution, **based primarily on DP16**. [Footnote 4]

[Footnote 4] The 2.0 m upper bound of Pfeffer et al. (2008) assumed large contributions from both ice sheets: 0.54 m from the GrIS and 0.62 m from the AIS. In AR5, Church et al. (2013) estimated a likely upper bound of just 0.21 m for the GrIS, since process models do not support “the order of magnitude increase in flow” in Pfeffer et al. (2008). To reach an upper bound of 2.0 m or more, S17 therefore needed an increased AIS contribution of ~1.0 m or more. To support this increase, they cited DP16 along with the expert-judgment assessment of Bamber and Aspinall (2013). However, the latter study gave a high-end (95th percentile) estimate of 0.84 m SLR from the two ice sheets, less than Pfeffer et al. (2008). Other studies cited by S17 did not give independent evidence of a large AIS contribution. Thus, both the “High” projection of 2.0 m and the “Extreme” projection of 2.5 m in S17 relied on DP16’s claim that the AIS could contribute at least a meter of SLR by 2100."

The highlighted statement is a misstatement of fact. The accompanying footnote is also erroneous, as well as the part of the statement, on page 7, that references DeConto and Pollard (2016) in the context of Sweet et al. (2017), i.e., “(2) S17 and other reports that were published in 2016–2020 **and relied on DP16 for the AIS contribution**.”

Here is the relevant paragraph from page 14 in Sweet et al. (2017):

“The growing evidence of accelerated ice loss from Antarctica and Greenland only strengthens an argument for considering worst-case scenarios in coastal risk management. Miller et al. (2013) and Kopp et al. (2014) discuss several lines of arguments that support a plausible worst-case GMSL rise scenario in the range of 2.0 m to 2.7 m by 2100: (1) The Pfeffer et al. (2008) worst-case scenario assumes a 30-cm GMSL contribution from thermal expansion. However, Sriver et al. (2012) find a physically plausible upper bound from thermal expansion exceeding 50 cm (an additional ~20-cm increase). (2) The ~60 cm maximum contribution by 2100 from Antarctica in Pfeffer et al. (2008) could be exceeded by ~30 cm, assuming the 95th percentile for Antarctic melt rate (~22 mm/year) of the Bamber and Aspinall (2013) expert elicitation study is achieved by 2100 through a linear growth in melt rate. (3) The Pfeffer et al. (2008) study did not include the possibility of a net decrease in land-water storage due to groundwater withdrawal; Church et al. (2013) find a likely land-water storage contribution to 21st century GMSL rise of -1cm to +11 cm. Thus, to ensure consistency with the growing number of studies supporting upper GMSL bounds exceeding Pfeffer et al. (2008)’s estimate of 2.0 m by 2100 (Sriver et al., 2012; Bamber and Aspinall, 2013; Miller et al., 2013; Rohling et al., 2013; Jevrejeva et al., 2014; Grinsted et al., 2015; Jackson and Jevrejeva, 2016; Kopp et al., 2014) and the potential for continued acceleration of mass loss and associated additional rise contributions now being modeled for Antarctica (e.g., DeConto and Pollard, 2016), this report recommends a revised worst-case (Extreme) GMSL rise scenario of 2.5 m by 2100.”

In developing the report, we wished to provide a number of scenarios that could be used to fully bracket the evidence base for the physically possible 21st century sea level rise, as well as providing expert judgment about the central tendency/best guess trajectory. These ranged from 0.3 m at the lowest of the lower bounds to 2.5 m at the uppermost of the upper bounds. Briefly, the motivation was to support as wide a possible range of decision contexts as existed at the time in coastal risk planning and management (e.g., see Hinkel et al., 2015, Nature Climate Change, and many others), including long-term adaptation pathways approaches and "stress test" type applications, both of which often use a "not-to-be-exceeded," upper bound metric of performance.

As described in the quoted paragraph from Sweet et al. (2017), above, we arrived at the 2.5 m upper bound by synthesizing a number of lines of evidence from numerous studies, as well as the IPCC AR5, to individually interrogate the physically possible ranges of the contributing components to global-mean sea level rise. This was new evidence, and/or new synthesis of that evidence, since Pfeffer et al. (2008), the study that helped define the physically possible upper bound for a preceding U.S. interagency sea level rise scenarios report (Parris et al., 2012).

All of these studies predated the publication of DeConto and Pollard (2016); we had already decided on the 2.5 m upper bound, and completed most of the work of developing the global and regional scenarios, before that paper was published. As just one example, Kopp et al. (2014) estimated 2.45 m as the 99.9 percentile outcome for global-mean sea level rise in 2100 under RCP8.5. Once DeConto and Pollard (2016) was published, we added it to our citation list as another piece of evidence, but the conclusions of that paper had no influence on our choice of 2.5 m as the upper bounding scenario. The successor report to Sweet et al. (2017), i.e., Sweet et al. (2022), stated this clearly, as well (e.g., see page 11): "In Sweet et al. (2017), these scenarios were developed to span a range of 21st-century GMSL rise from 0.3 m to 2.5 m. Sweet et al. (2017) built these scenarios upon the probabilistic emissions scenario–driven projections of Kopp et al. (2014)."

The bottom line is that, if DeConto and Pollard (2016) had never been published, we would have written exactly the same report at the time that we wrote it.

We thank the reviewer for clarifying the reasoning that led to the 2.5 m "Extreme" projection in Sweet et al. (2017; hereafter S17). As he requested, we removed language "stating or implying a reliance of Sweet et al. (2017) on DeConto and Pollard (2016)". We removed the footnote and rewrote the paragraph on S17 as follows:

> Publications aimed at coastal adaptation planners highlighted the DP16 projections. Perhaps the most influential was Sweet et al. (2017; hereafter S17) a multi-agency U.S. government report on future SLR. S17 sought to "support a wide range of assessment, planning, and decision-making processes," signaling the aim to influence practitioners. To be consistent with "recent updates to the peer-reviewed scientific literature," they issued an "Extreme" GMSL projection of 2.5 m by 2100 for RCP8.5, exceeding the previous upper bound of 2.0 m based on Pfeffer et al. (2008). To support this projection, S17 cited several studies, most prominently Kopp et al. (2014; hereafter K14) and DP16.

Based on process modeling and expert assessments and elicitation, K14 presented a very likely (90% probability) range of 0.5–1.2 m GMSL rise by 2100 under RCP8.5. By fitting a log-normal distribution to AR5 results and the BA13 Antarctic projections, they estimated a 0.1% probability of GMSL exceeding 2.45 m. This value was the source of the Extreme scenario. S17 stated further that the processes modeled by DP16 could "significantly increase the probability of the Intermediate-High, High, and Extreme scenarios"—i.e., that the likelihood of 2.5 m GMSL rise by 2100 might be much greater than 0.1%, because of MICI. By our criterion, the Extreme scenario was not actionable, since it was based on probabilities extrapolated from BA13 without reference to physical processes that were understood with at least medium confidence.

We would also like to respond to the statement that "if DeConto and Pollard (2016) had never been published, we would have written exactly the same report at the time that we wrote it". We think that DP16 plays an important role in the main arguments of S17, even if it was not the source of the 2.5 m projection. S17 cited DP16 nine times by our count, often in support of important claims. For example:

- S17 cited DP16 (p. 3) to support the following statement: "Sea level science has advanced significantly over the last few years, especially improving understanding of the complex behaviors of the large, land-based ice sheets in Greenland and Antarctica under global warming, and the correspondingly larger range of possible 21st century rise in GMSL than previously thought." The Extreme projection relies on the long tail in Kopp et al. (2014; hereafter K14), which was based on the expert elicitation study of Bamber and Aspinall (2013; hereafter BA13) but did not specify mechanisms that could make the long tail physically possible. By exploring the mechanisms of hydrofracture and MICI, DP16 aimed to provide a physical foundation for a large Antarctic sea-level contribution. To the extent that the mechanisms described in DP16 were thought to be plausible, the Extreme projection was more credible to practitioners than would have been the case based on BA13 and K14 alone.
- S17 stated (p. 13) that "additional GMSL rise upwards of 0.6–1.1 m to median estimates under RCP8.5 are possible by 2100 (DeConto and Pollard, 2016), potentially raising median GMSL projections for RCP8.5 of Kopp et al. (2014) as high as 1.9 m by 2100." Without DP16, a practitioner reading S17 might have minimized the relevance of the 2.5 m projection for decision-making, given the low 0.1% probability. However, the addition of ~1 m to median estimates would suggest that under RCP8.5, the Extreme scenario is much more likely than 0.1% and therefore should be taken seriously in planning.
- The last sentence in the paragraph quoted by CW (p. 14) cites DP16—with its "potential for continued acceleration of mass loss and associated additional rise contributions now being modeled for Antarctica" —in support of the Extreme scenario.
- S17 stated (p. 21) that "as discussed in Section 3, new evidence regarding the Antarctic ice sheet, if sustained, may significantly increase the probability of the Intermediate-High, High, and Extreme scenarios, particularly for RCP8.5 projections based upon Kopp et al. (2014)." The context indicates that "new evidence" refers to DP16.

- Figure 8 (p. 22) shows the study's six representative SLR scenarios in relation to historical GMSL reconstructions. The Extreme scenario is shown with a red curve that reaches 2.5 m in 2100. To the right side of the graph, three boxes illustrate the 5th–95th percentile ranges of RCP-based GMSL projections from recent studies. The range is 0.5–1.3 m for the RCP8.5 scenario, shown in red. Appended to these boxes are dashed lines described as "the difference between the median Antarctic contribution of the Kopp et al. (2014) probabilistic GMSL/RSL study and the median Antarctic projections of DeConto and Pollard (2016)". For RCP8.5, the red dashed line representing the DP16 contribution extends from 1.3 m to about 2.4 m, i.e. nearly to the top of the Extreme curve. Thus, the figure suggests that the DP16 Antarctic projections for RCP8.5 are able to bridge the gap between the 95th percentile RCP8.5 projection and the Extreme scenario, lifting the probability of the Extreme scenario from 0.1% (the value from K14) to a value many times greater, perhaps ~5%.

Thus, S17 without DP16 would not have been "exactly the same report" and likely would not have been as influential for adaptation planners. As our manuscript states, the reports developed by practitioners and practitioner advisors in the science community (e.g., Boston Research Advisory Group, 2016; California OPC, 2018; and the Griggs et al. "Rising Seas in California" 2017 report underpinning the OPC report) refer to DP16 as a key driver (perhaps *the* key driver) of high-end projections recommended for planning. We would submit that the signal sent by the multiple citations of DP16 in S17 was highly influential.

For example, Griggs et al. (2017) highlighted DP16 as the most important driver of the 3.1 m high-end estimate later adopted in California OPC (2018), while also pointing out the many unanswered questions associated with DP16 in an appendix substantially devoted to these uncertainties. This focus on DP16 became concrete when the OPC (2018) report instructed practitioners to use the 3.1 m "H++" projection in planning for any project that "would have considerable public health, public safety, or environmental impacts should this level of sea-level rise occur." A core goal of our Commentary is to advocate for a stronger underpinning for actionable science, as regulators and practitioners struggle to adjust to the realization that the high-end estimates in OPC (2018) were based on what is now understood to be low-confidence science.

In closing, I wanted to note that, on the initiative of one of the authors of this brief communication (DB), he and a number of others of us (including myself and Kopp, as well as DeConto) spent substantial time in productive discussions of the very points I have just summarized, and related topics, in the broader context of the nuances of using cutting-edge sea level rise science to support decision-making. These extensive discussions following the publication of Sweet et al. (2017) resulted in an AGU presentation in December 2017 by DB (see https://par.nsf.gov/servlets/purl/10066643), and a written summary of our engagement (see https://acwi.gov/climate_wkg/minutes/nal_agu_consensus_statement_probabilisitic_projections_dec_2017.pdf), both of which reflected a useful integration of our diversity of perspectives as scientists and practitioners.

DB confirms that he led a group process including himself, CW, Robert Kopp, Rob DeConto, representatives from the US Army Corps, and others. We suggest that interested readers access this document at https://www.wucaonline.org/assets/pdf/pubs-sfpuc-agu-consensus-statement.pdf. (Neither location provided by CW appears to link to this document.)

The outcome of this process, "Consensus Statements: Planning for Sea Level Rise: An AGU Talk in the Form of a Co-Production Experiment Exploring Recent Science" (Behar et al., 2017; hereafter Consensus Statements) reports two goals for the process. The first was to address the increased appearance of Bayesian probabilistic projections intended for practitioner use, particularly K14, in documents intended for use by practitioners developing plans to address rising seas. Research led by DB indicated confusion among practitioners about the nature and meaning of Bayseian probabilistic projections. The authors of the Consensus Statements observed that these estimates "in many instances. . . are arriving on the desks of planners, engineers, and decision makers who have little background in the methodologies used…and do not provide sufficient guidance on how to use them in planning, decision making, or adaptation design context." While the Consensus Statements list a number of opportunities and limitations associated with Bayesian probabilities, it is worth repeating one that relates to the conversation here (emphasis added): "*There is no consensus on how to meaningfully assign quantitative probabilities for the upper extreme range of potential future global SLR*; therefore, a given set of Bayesian probabilistic projects may underestimate or overestimate the SLR contributions due to rapid ice sheet loss after 2050." The Consensus Statements go on to recommend that, to properly represent uncertainty, multiple analyses and PDFs, rather than a single Bayesian PDF, should underpin adaptation planning.

We think it was unfortunate that the California OPC (2018) guidance included the following statement: "Probabilistic projections represent consensus on the best available science for sea-level rise projections through 2150." This statement is neither true nor consistent with the Consensus Statements drafted by DB, CW, and Dr. Kopp, among others.

The second goal of this group was to address DP16 which, according to the Consensus Statements, "suggested the potential for significantly higher upper end projections for Antarctic ice sheet melt, which increase both global and regional SLR above most previously assumed upper limits." However, the group did not achieve this goal. "The group did not completely fulfill one of its two objectives," the Consensus Statements said, "the consideration of how DeConto and Pollard (2016), as a defining example of cutting-edge science leading to new upper end SLR estimates, can or should be incorporated into planning." Considering the chaos and confusion prevalent in the uptake of high-end projections into planning, as reported in our submitted manuscript and other sources (e.g., Stammer et al., 2019; Boyle et al 2022; van de Wal et al., 2022; Hirschfeld et al., 2023; Hirschfeld et al., 2024), DB wishes to express his regret that he failed to lead the authors of the Consensus Statements into this next round of conversations in 2018. Perhaps the authors, working together, could have helped mitigate the confusion that persists today about high-end projections, including how to treat modeling studies that project catastrophic Antarctic ice melt on an adaptation time scale but are not widely accepted in the science community (including DP16 and DeConto et al., 2021).

The authors should remove language stating or implying a reliance of Sweet et al. (2017) on DeConto and Pollard (2016). That would be a good first step in helping the paper be considered for publication.

We rewrote this paragraph, as described above.

Note that I do not, in any way, have any objection to the authors disagreeing with the decision in Sweet et al. (2017) to use 2.5 m globally by 2100 as the top-end, bounding scenario on other grounds. Such a disagreement would simply have to be justified in terms of the totality of references and lines of evidence summarized above, absent any reliance on DeConto and Pollard, as well as the stated purpose of the use of a limiting upper-bound scenario in that report - in other words, the choice to include 2.5 m not because it is at all likely, but precisely because it is very, very unlikely.

We do, in fact, disagree with the decision in S17 to use 2.5 m as the top-end global scenario. To support our disagreement, we will apply our actionable science criterion to the other studies cited in S17, not including DP16. S17 cited eight papers as among the "growing number of studies supporting upper GMSL bounds exceeding Pfeffer et al. (2008)'s estimate of 2.0 m by 2100": BA13, K14, Sriver et al. (2012), Rohling et al. (2013), Jevrejeva et al. (2014), Grinsted et al. (2015), Jackson and Jevrejeva (2016), and Miller et al. (2013). We will comment on each study, starting with BA13 and K14.

Table A1.1 in Sweet et al. (2022; hereafter S22) states that the Antarctic projections in S17 are based on the "*likely* range from IPCC AR5", with the "shape of tails" for high-end projections based on the structured expert judgment (SEJ) study of BA13, as interpreted by K14. BA13 gave a 95th percentile estimate of 0.84 m for the Greenland and Antarctic ice sheets together. K14 combined BA13 with independent estimates for glaciers and ice caps, thermal expansion, and land water storage to obtain a 95% upper bound of 1.21 m. This upper bound was based mainly on processes that were understood at the time with at least medium confidence. To derive their 99.5% and 99.9% upper bounds (1.76 m and 2.45 m, respectively), K14 had to assume a mathematical form for the tail probabilities. The Supporting Information in K14 states: "To reconcile the AR5 and BA13 projections of ice sheet mass loss, we first fit log-normal distributions to the rates of ice mass change in 2100 for AR5 and BA13." They created hybrid curves (see their Fig. S1) which were scaled to match the median and likely ranges of AR5, with tails based on a log-normal fit to BA13. They did not try to justify the tails in terms of physical processes.

We do not think the statistical analysis in K14 was robust enough to underpin decision-making for adaptation planners. We refer to the quotations above from the Consensus Statements, in particular the statement that "there is no consensus on how to meaningfully assign quantitative probabilities for the upper extreme range of potential future global SLR". We think that more robust efforts to display the significance and sources underpinning deep tails in Bayesian probabilistic projections would improve clarity for practitioners who are considering these outputs for adaptation planning.

Next, we will comment on the other six studies.

Sriver et al. (2012) proposed an upper bound of 0.55 m for the thermal expansion (TE) contribution to GMSL. This estimate was based on a perturbed physics ensemble applied to an Earth system model of intermediate complexity (the UVic model) with a coarse-resolution ocean component. AR5 cited this paper but gave a likely range of 0.21–0.33 m for TE under RCP8.5, adding that "we have *high confidence* in the projections of thermal expansion using AOGCMs" (p. 1151; emphasis in original). Similarly, AR6 gave an upper bound of 0.36 m for TE. We conclude that the value of 0.55 m from Sriver et al. (2012) was an outlier based on a single model. This high projection was discounted by the AR5 authors prior to S17 and thus was not appropriate for use in adaptation planning.

Rohling et al. (2013) used the geologic record to inform projections of future SLR. They concluded that the geologic context supports SLR of up to 1.8 m by 2100 at 95% confidence. This estimate was based on Monte Carlo–style sampling of the distributions of three parameters in a logistic equation (their Eq. 1). They cautioned that their high-end estimate requires SLR rates approaching 4 m/century, similar to those associated with Meltwater Pulse 1a during the collapse of large Northern Hemisphere ice sheets about 14,000 years ago. This collapse might not be an appropriate analog for the future (since these ice sheets no longer exist) and in any case does not yield a projection as large as 2.5 m.

Jevrejeva et al. (2014), like K14, took the AR5 likely range as a starting point and used BA13 to estimate the additional ice-sheet contribution. They obtained a 95% upper bound of 1.8 m GMSL rise by 2100—well below 2.5 m. They noted that "large uncertainties remain due to the lack of scenario-dependent projections from ice sheet dynamical models, particularly for mass loss from marine-based fast flowing outlet glaciers in Antarctica. This leads to an intrinsically hard to quantify fat tail in the probability distribution for global mean sea level rise." The studies of Grinsted et al. (2015) and Jackson and Jevrejeva (2016) used similar methods and were broadly consistent with Jevrejeva et al. (2014). None of these studies supports a 2.5 m projection. Moreover, these studies do not provide evidence independent of K14. Like K14, they rely on BA13, but they make different statistical inferences about the high end.

The only one of these six studies with a high-end GMSL projection exceeding 2.0 m is Miller et al. (2013). Starting from a projection of 2.0 m based on Pfeffer et al. (2008), these authors argued for additions of 0.1 m for land water storage (which Pfeffer et al. (2008) neglected), 0.25 m for TE based on Sriver et al. (2012), and 0.3 m for the Antarctic ice sheet based on BA13. We would challenge this projection on the following grounds:

- Pfeffer et al. (2008), which was taken as a starting point, assumed unrealistically high GrIS discharge. AR5 (which appeared after Miller et al. (2013) was submitted for publication) is a better starting point since it includes the land water storage term and has a much lower GrIS contribution.
- Sriver et al. (2012), as discussed above, is an outlier. It does not provide robust evidence for increasing the thermal expansion estimate.

- Miller et al. (2013) assumed an Antarctic contribution of 22 mm/yr by 2100 based on BA13. This is larger than the 95th percentile upper bound of 17.6 mm/yr in BA13 for the GrIS and AIS *combined*. To obtain a much higher Antarctic value than BA13, Miller et al. (2013) assumed perfect correlation of the estimated 95th percentile contributions from East and West Antarctica, without explaining why this assumption was justified. In our view, BA13 does not support a 30-cm increase (from 0.62 m to 0.94 m) for the AIS relative to Pfeffer et al. (2008).

We have proposed that actionable science should rest on multiple, consistent lines of high-quality evidence, resulting in medium or high confidence as evaluated by a group of experts in a transparent process. Collectively, the eight studies above do not meet this criterion. Several of them (Miller et al., 2013; K14, Jevrejeva et al., 2014; Grinsted et al., 2015; Jackson and Jevrejeva, 2016) depend on BA13. In agreement with AR6, we would argue that SEJ studies like BA13 and Bamber et al. (2019) do not meet a medium-confidence threshold, since the expert surveys can incorporate low-confidence processes in a non-transparent way. Sriver et al. (2012) is an independent line of evidence, but as early as 2013, this evidence was assessed as not being of high quality. Rohling et al. (2013) added evidence from the geologic record but did not support a projection above 2 m. Thus, the Extreme scenario did not meet our actionable science standard at the time S17 was published.

The withdrawal of the Extreme scenario in S22 supports our argument for greater caution in presenting low-confidence science. Furthermore, our actionable science criterion suggests that the High scenario of 2.0 m by 2100 presented in S22 should not be regarded as actionable by practitioners, since it relies in a non-transparent way on low-confidence studies.

We would also like to reply to CW's statement that the Extreme scenario of 2.5 m was included "not because it is at all likely, but precisely because it is very, very unlikely". We do not object to presenting unlikely scenarios *per se*. Rather, we object to presenting a single set of misleadingly precise probabilities that lack a physical underpinning and draw from low-confidence analyses.

Finally, while my main concern is helping the authors correct this particular error, I do also largely agree with the criticisms outlined in Community Comment 1 (CC1: '"Actionable" for whom, in what decision context?', Robert Kopp, 15 Mar 2024). It would be good to see the authors respond to and/or address those in their revision.

We have responded to CC1 in a separate document.

I appreciate the authors spending the time and effort to grapple with these issues in the literature. I continue to be very supportive of having these types of issues and ideas discussed, and I believe the continuation of the dialogue through this paper is valuable.

We thank Dr. Weaver for joining us in grappling with these issues.

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 mass 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 value 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 create laws and regulations.”

On l. 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 l. 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 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:

> When practitioners learn about climate research through media reports, their attention may 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 this 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 the manuscript.