Reply to Robert Kopp

We thank Dr. Kopp for sharing his perspective on our manuscript. Our responses are below, with his comments in black font and our replies in blue.

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I read this brief comment with interest, and found some core issues troubling.

Fundamentally, the authors discuss 'actionable' science, but they discuss it stripped of context. Actions are defined by the American Heritage Dictionary as 'organized activity to accomplish an objective'. Science cannot be judged to be actionable, or not, outside the context of an organized activity and an objective. It makes little sense to talk about something being 'actionable' in general, outside of a specific decision context.

We agree in part. In the context of adaptation planning, we acknowledge that there are many financial, socioeconomic, and political factors driving decisions.

Nonetheless, we think it is important to distinguish between scientific claims that are sufficiently established to underpin decisions with significant fiscal, sociopolitical, and community implications, and those claims that are not. Low-confidence sea-level projections are likely to be superseded within a few years by very different numbers. The volatility of these projections (for instance, the excursion in high-end SLR projections from 2.0 m to 2.5 m and back within less than a decade, as shown in our Fig. 1) makes them risky to use for *any* decision where public confidence and large sums of money are at stake.

The authors neglect the extensive literature on decision science and risk analysis relevant to using sea-level projections in adaptation decision making. For a relatively recent review, see Keller, K., Helgeson, C., & Srikrishnan, V. (2021). Climate risk management. Annual Review of Earth and Planetary Sciences, 49, 95-116, https://www.annualreviews.org/doi/abs/10.1146/annurev-earth-080320-055847.

In the specific context of communicating sea-level uncertainty and ambiguity, the authors should also see Kopp, R. E., Oppenheimer, M., O'Reilly, J. L., Drijfhout, S. S., Edwards, T. L., Fox-Kemper, B., ... & Xiao, C. (2023). Communicating future sea-level rise uncertainty and ambiguity to assessment users. Nature climate change, 13(7), 648-660, https://www.nature.com/articles/s41558-023-01691-8. Given the direct relevance, this latter omission is particularly surprising.

The manuscript includes more citations than are typical for a Brief Communication, so we tried to be sparing in the number of works cited. With the Editor's approval, we will add these two citations to the revised manuscript.

Why do the organized activity and the objective matter?

Broadly, high-end sea-level rise scenarios, including low-confidence processes, are valuable in flexible, adaptive decision-making.

First, we would like to clarify a misunderstanding that may have arisen from our omissions in the submitted manuscript. We fully support flexible, adaptive decision-making, and we will make this clear in the revised manuscript.

We disagree about the value of low-confidence processes in decision-making. We are comfortable with including *low-likelihood* processes, if the likelihoods are scientifically supported (i.e., the relevant processes are understood with at least medium confidence). We object, however, to including processes which are so poorly understood that it is not yet possible to make robust, quantitative projections. Giving premature credence to low-confidence processes can lead to misuse of scarce public and private resources and can damage the credibility of the climate science enterprise.

This is shown by a number of papers, but perhaps most clearly and directly for this context in a preprint by Feng et al. (https://doi.org/10.22541/essoar.170914510.03388005/v1).

Among other analyses, Feng et al. compare idealized protection schemes for Manhattan under (1) a static optimal approach, where a single sea wall elevation must be picked based on available knowledge today, and (2) a variety of dynamic approaches, where sea wall height can be periodically adjusted based on new information. (I focus particularly on the 'reinforcement learning' approach described therein).

They consider two cases where projects are planned under inaccurate sea-level rise projections: (A) where planning takes place under the SSP5-8.5 low-confidence projections but the reality corresponds to SSP2-4.5 medium-confidence projections, and (B) where planning takes place under the SSP2-4.5 medium-confidence projections by reality corresponds to the SSP5-8.5 low-confidence projections.

In the former case -- where high-end projections are used and reality underperforms -- the expected net present value cost is $2.3 billion, $1.0 billion more than with the correct (lower) distribution, if a static approach is taken. With a flexible approach, the expected net present value cost is $1.0 billion, just $0.1 billion more than if the correct distribution is chosen.

However, in the latter case -- where middle-of-the-road projections are used and reality overperforms -- the expected net present value cost is $15 billion, $12 billion more than with the correct (high-end) distribution if a static optimal approach is taken. With a flexible approach, the expected net present value cost is $3.9 billion, $0.9 billion more than if the high-end distribution had been used. *[Here, RK included a table.]*

Thus, with a dynamic approach, using high-end projections that capture low-confidence processes makes a lot of economic sense. Such an approach cuts off the tail risk at relatively small additional cost. (In fact, the cost of a static optimal approach using the correct distribution in a middle-of-the-road world is more than the cost of using a dynamic approach with the overestimated, high-end distribution.)

However, with a static approach, the costs of getting the distribution wrong are more substantial (though an order of magnitude larger if the distribution is underestimated than if it is overestimated).

We agree that a flexible, dynamic approach is better than a static approach. We also think that capturing low-likelihood (as opposed to low-confidence) processes makes economic sense. When the science suggests the possibility of a low-likelihood, high-impact event, this should be included in planning. In the case of sea level, an optimal approach might be to start with an intermediate projection of (say) 1 m SLR by 2100, with the option to revisit this decision later based on new scientific understanding and relevant events (e.g., revised carbon emission pledges).

As discussed in the manuscript, the policies adopted by California in the wake of DeConto and Pollard (2016; hereafter DP16) and Sweet et al. (2017; hereafter S17) did not incorporate flexible, adaptive planning. Griggs et al. (2017) recommended use of the high-end 2.5 m scenario from S17, adjusted higher (e.g., 3.1 m for San Francisco) based on regional factors. The subsequent policy guidance in California OPC (2018) stated that practitioners should apply the high-end estimate to any assets whose failure "would have considerable public health, public safety, or environmental impacts". This guidance made no exceptions for projects with adaptive capacity (e.g., assets that could be relocated at moderate cost if the most pessimistic SLR projections are borne out).

So while we agree that a static approach can drive up costs unnecessarily, we would argue that current static policies in some jurisdictions, including California, are much more likely to err on the side of costly overbuilding rather than underbuilding, as a result of overreliance on low-confidence science.

In truth, I think the concern the authors address is not one with scientists offering practitioners low-confidence, high-end projections as part of the domain of plausible futures. It is with how these projections are then used.

This is true only in part. Indeed, it matters how the projections are used, but we are also concerned about how they are communicated by scientists. We have already discussed (above, and in our reply to Christopher Weaver) the undesired impacts from the embrace of DP16 in the Rising Seas report (Griggs et al., 2017, co-authored by RK). Our manuscript endorses the AR6 approach to low-confidence, high-end sea-level projections (Fox-Kemper et al., 2021). AR6 clearly distinguishes high-end projections based on medium-to-high-confidence science from those based on low-confidence science. This separation is valuable for long-term adaptation planning.

Further, for each projection, AR6 quantifies the contribution from each major source: thermal expansion, the Greenland and Antarctic ice sheets, mountain glaciers, and land water storage. (See, e.g., Table 9.9 in AR6.) This makes it fairly straightforward to adjust the global projections if subsequent evidence suggests a higher or lower contribution from a specific source. In contrast, the High and Extreme scenarios in S17 do not itemize the contribution from each source or the fraction of each contribution linked to low-confidence processes. This makes the global projections less transparent and thus harder for practitioners to use.

As the Feng et al. analysis, and others, indicate, the most economic approach given substantial uncertainty and ambiguity is most often the dynamic one.

We agree, and we will clarify this point in the revised manuscript.

Where a static approach must be used, whether due to inability to undertake a dynamic approach or regulatory inflexibility, then benefit-cost theory tells us what needs to be taken into account in order to determine the best option. This includes:

1) The benefit in terms of reduced risk associated with choosing different adaptation levels
2) The cost in terms of additional adaptive expenditures in terms of choosing different adaptation levels
3) The discount rate used to tradeoff present adaptation costs and future harms
4) The risk aversion that determines how much weight is given to the high-end of the cost distribution
5) The ambiguity aversion that determines how much weight is given to different alternative probability distributions for sea level and thus for cost.

We agree with these statements when the science is understood well enough to quantify benefits and costs. However, we disagree with the underlying assumption that high-end projections based on low-confidence processes are sufficiently constrained to support quantitative risk assessments. See, for example, the quotations from the Consensus Statements (Behar et al., 2017) in our reply to Christopher Weaver.

Where the costs and benefits of adaptation are comparable, discomfort will arise if regulatory guidance specifies a single adaptation target stripped of context, because a user's risk and ambiguity aversion applies to both the costs and benefits of adaptation, not just the benefits.

We agree with this statement. As stated above, we think the guidance adopted in California and some other jurisdictions is inappropriate, in part because it specifies a single target (or a narrow range of high targets) stripped of context.

I suspect that the authors' concern with the actionability of projections incorporating low-confidence processes is misaimed. Given appropriately flexible decision frameworks, as Feng et al. show, we are better off incorporating such high-end projections.

We agree that flexible decision frameworks make it possible to live with uncertainty and adapt to advancing knowledge. Also, as our manuscript states, we have no objection to incorporating high-end projections, if supported by medium- or high-confidence science as evaluated in an appropriate community-based process.

We think our basic disagreement with RK is in how to decide which science is sufficiently accepted to underpin adaptation planning. We are trying to set forth a standard that is philosophically justified and practical to implement.

In his critique, RK implies that our standard is too strict, because it rules out science that (in his view) should be included in planning. However, he does not articulate a clear alternative.

We think some standard is necessary. Otherwise, any claims—no matter how outlandish—could be passed on to practitioners. We are not clear on the standard used by S17, who cited nine studies in support of the Extreme scenario. (Our response to Christopher Weaver explains why we think these studies were not actionable.) Each study appeared in a reputable peer-reviewed journal, which perhaps suggests an alternative standard.

However, we do not think this is the standard actually used by S17, because S17 omitted some relevant peer-reviewed science. Consider, for example, the study of Hansen et al. (2016), which received wide press coverage and was probably known to the S17 authors. This study claimed that mass loss from ice sheets “is better approximated as exponential than by a more linear response”. The authors suggested that a doubling time of 10 years is plausible, yielding 5 m of global mean SLR by 2100 (see their Fig. 5).

Had S17 cited this study, they could have argued for a high-end scenario much greater than 2.5 m. Why did they exclude it? We cannot say for sure, but we suspect that they did not find the study credible, perhaps because of criticism from other climate scientists (as highlighted by Revkin, 2015). Thus, they did not deem it suitable for planning in decision contexts. This suggests that S17 had an evidential standard, albeit with a lower threshold than ours. If so, then we disagree not on whether a threshold of evidence should exist, but on where it should be set.

We would like to quote from Rajashree Datta’s comments:

> Presumably, we would not present decision-makers with non-peer-reviewed SLR estimates and expect them to decide its merit based on the specific decision context. If we accept the current social production of science which is “peer review” (also imperfect), a higher standard for “actionable science” is simply a logical extension…. In another comment, Dr. Kopp suggests (in summary) that it is critical to present the long tail and leave room for a dynamic response, even where evidence is lacking, based on the extent of potential risk (and the associated benefit of more extensive adaptation). I see no meaningful contradiction between the need for a guideline presented by the authors and the presence of exceptions. In fact, the “exceptionality” here is still defined in reference to some guideline and underlying rationale, thus underlining the need for the guideline.

We agree that while it might not be possible to state guidelines that should apply without exception in all cases, evidence-based guidelines are still needed. We think that as a general rule, low-confidence science is not an adequate foundation for adaptation planning. The burden of argument then falls on those who think a low-confidence standard is justified in a specific context.

Both Stammer et al. (2019) and van de Wal et al. (2022) took on this question, with results that reinforce the value of separating medium- and high-confidence projections from low-confidence projections. Our effort seeks to amplify and deepen these perspectives. We suggest that those who seek different standards can propose their own criteria.

Regulations that rigidly prescribe the use of specific high-end projections in static contexts, however, run the risk of leading to sub-optimal outcomes.

We agree, and we are concerned that such regulations are still in place in the jurisdictions mentioned.

It may be appropriate for policy to set discount, risk aversion, and ambiguity aversion levels for specific contexts; this is a matter where different political philosophies will lead to different judgements. However, given these parameters, identifying the benefit-cost optimal outcome requires considering the net value of adaptation benefits and adaptation costs under these parameters. If costs and benefits are comparable, overly rigid targets might cut off the long tail of sea-level harms but create a long tail of adaptation cost overruns.

In short, the authors have chosen the wrong target. Scientists should strive to communicate not just projections that incorporate processes for which there is a high degree of evidence, but also processes that are of potentially great significance but less agreement and evidence -- as AR6 has done. It is, however, important that actions be guided by decision frameworks that correctly reflect the nature of the information provided.

We agree that there is value in communicating low-confidence projections, provided the contributions of low-confidence processes are clearly separated from those based on medium- and high-confidence processes. That is, we favor the approach of AR6 over that of S17 and its followup report, Sweet et al. (2022). We agree that practitioners should be aware of the current state of the science, including cutting-edge research like DP16. But in nearly all cases, we would discourage practitioners from using low-confidence projections for decision-making because of the risks discussed in the manuscript: confusion, maladaptation, and whiplash leading to loss of public confidence.

We hope these responses will clarify several points on which we agree with Dr. Kopp, along with some points of real disagreement.

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