Responses to Community Comments

We thank Robert Kopp, Jeremy Bassis, Judy Lawrence, Marjolijn Haasnoot, and Robert Lempert for their thoughtful and detailed comments. Our replies are below (in blue font). Line references are to the revised manuscript.

Reply to Robert Kopp

In my mind, the core of the authors' response is the following bold statement:

> "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."

These are bold statements that appear to reject the entire field of decision making under deep uncertainty (DMDU). Scholarly practice would suggest that such a bold claim be made head on – that is, if the authors are to dismiss the entire DMDU literature, they need to cite and engage with that literature in a manner that justifies rejecting decades of scholarly work (and of practice), a relatively recent synthesis of which is provided by

> Marchau, V. A., Walker, W. E., Bloemen, P. J., & Popper, S. W. (2019). Decision making under deep uncertainty: from theory to practice. Springer Nature, 405 pp. https://doi.org/10.1007/978-3-030-05252-2

Contrary to this literature, the authors argue that the only appropriate decision-making practice regarding those elements of sea-level projection characterized by deep uncertainty (or low confidence) is to ignore them. I strongly disagree with this position, but would welcome an argument that makes it while engaging with this robust body of literature.

We regret the appearance that we are rejecting the large body of work on DMDU, including the excellent synthesis of Marchau et al., (2019). The revised manuscript describes DMDU and discusses its value. The Introduction now includes a brief overview of DMDU (ll. 40–47):

> Researchers and practitioners have developed frameworks for decision making under deep uncertainty (DMDU; Marchau et al., 2019a). Deep uncertainty arises when experts are unable to "specify the appropriate models to describe interactions among the system's variables, select the probability distributions to represent uncertainty about key parameters in the models, and/or value the desirability of alternative outcomes" (Lempert et al., 2003; Marchau et al., 2019b). DMDU frameworks support a shift from a "predict then act" paradigm to a "monitor and adapt" paradigm (Marchau et al., 2019b). In the

> new paradigm, the focus is on exploring a wide range of plausible futures and committing to short-term actions, while keeping open long-term options that might be triggered by new evidence. Ideally, costly actions are deferred until they are necessary.

We are comfortable with the use of low-confidence science in a monitor-and-adapt paradigm, provided it is not prematurely held to be actionable. The revised Introduction also clarifies what we mean by "actionable" (ll. 33–38):

> Here, we will say that a claim is actionable when it is sufficiently accepted to justify adaptation action in the near term (assuming that other requirements for actionability, such as salience and legitimacy, have also been met). Near-term actions—for example, physical measures such as building seawalls and levees, as well as financial investments such as acquiring land—may be needed not only to address short-term vulnerability but also to prepare for long-term climate impacts. Thus, uncertainties about the rate of climate change in the next several decades (to 2100 and sometimes beyond) must be factored into near-term decisions.

This language clarifies that we do not think low-confidence science should be ignored. It can be useful for dynamic adaptive planning even when it is not sufficient to justify near-term action.

The authors may also find it helpful to engage with the new manuscript: Lempert, R., Lawrence, J., Kopp, R., Haasnoot, M., Reisinger, A., Grubb, M., & Pasqualino, R. The Use of Decision Making Under Deep Uncertainty in the IPCC. Frontiers in Climate, 6, 1380054, https://doi.org/10.3389/fclim.2024.1380054).

Thank you for pointing out this paper, which is cited in the revised manuscript (ll. 241–243):

> Lempert et al. (2024) noted that AR5 and AR6 "opened the aperture" to provide a wider range of possible futures by discussing low-confidence science in detail. Consistent with van de Wal et al. (2022), we argue that practitioners should embrace widening uncertainty in designing adaptation action only when justified by scientific confidence.

In other words, we suggest that the aperture be opened cautiously. If low-confidence claims are included in the range of possible futures, then the level of confidence should be communicated clearly to practitioners.

It is certainly the case that there has been – and often remains – broad disagreement among experts regarding technological and policy development processes that would justify applying the label of "low confidence." As Moss et al. (2010, https://doi.org/10.1038/nature08823) noted in laying out the RCP/SSP framework, "An underlying key issue [and, thus, point of low agreement and therefore low confidence] is whether probabilities can be usefully associated with scenarios or different levels of radiative forcing; for example, the probability that concentrations will stabilize above or below a specified level."

Do the authors think that the issue highlighted by Moss et al. must be answered affirmatively -- that is, we must show that probabilities can be usefully associated with emissions scenarios -- for climate projections driven by those scenarios to be actionable? If experts cannot agree on probability distributions regarding policy and technological development, does that moot the actionability of any climate projections -- which is to say, any climate projections beyond about a 20-30 year time horizon – that exhibit substantially sensitivity to these deeply uncertain processes? If so, that somewhat moots the discussion of low-confidence ice-sheet processes, which we have little reason to think might manifest at significant levels until late in the century.

No, we do not think that probabilities must be assigned to emissions scenarios in order for the climate projections driven by these scenarios to be actionable. More generally, the lack of a pdf does not imply low confidence. According to Mastrandrea et al. (2011), "In most cases, the author team should have high or very high confidence in a finding characterized probabilistically." We think that medium-confidence projections can be actionable, although in many cases they cannot be characterized probabilistically. This is why our actionable science criterion ("*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")* does not mention probability.

We would not describe the CMIP emissions scenarios as having low confidence or being insufficiently vetted by the community. The ScenarioMIP activity leading to the CMIP6 scenarios (O'Neill et al., 2016) is a good example of a diverse group of experts engaging in an open, transparent process. Although high-end scenarios such as SSP5-8.5 are not described probabilistically, they have been judged to be plausible based on a great deal of physical and socioeconomic evidence. The scenarios themselves and the process producing the scenarios are updated based on experience and new evidence (O'Neill et al., 2020). This is very different from the rapid adoption of high-end sea-level projections described in our article.

I continue to believe that the authors' actual objection is not to the use of deeply uncertain information in decision making, but the use of such information with decision frameworks that are designed for probabilistic information.

It's true that we don't object to the use of deeply uncertain information in decision making, but only to inappropriate use. The revised manuscript clarifies how we think this information can be used appropriately.

Reply to Jeremy Bassis

This short paper describes a framework for actionable science. I am writing this comment as a glaciologist who also works broadly in the field of adaptation and science usability. I would really like to encourage the authors and editors to expand the paper to full length to give service to the ideas because in the short format currently there is much ambiguity and with significant room for mischief and harm.

We think the Brief Communication format is appropriate for our arguments and intended audience. We regret that there was ambiguity in the initial submission as a result of some omissions and lack of clarity. We hope the revised manuscript will resolve the ambiguity while staying within the short format.

To start, the study of how knowledge systems are applied to decision making often goes under the name "science usability", but I have also seen it referred to as "actionable science". There is substantial literature about the process of knowledge creation for planning and decision making. Foundational to this field, is the concept that for knowledge to be applied in a decision making or planning concept requires credibility, salience and legitimacy (see, e.g., Cash et al., 2003). To quote directly from Cash et al., (2003) ". . . . *credibility* involves the scientific adequacy of the technical evidence and arguments. *Salience* deals with the relevance of the assessment to the needs of decision makers. *Legitimacy* reflects the perception that the production of information and technology has been respectful of stakeholders' divergent values and beliefs, unbiased in its conduct, and fair in its treatment of opposing views and interests."

The definition of "actionable" that the authors present only includes what would be traditionally called "credibility", omitting salience and legitimacy completely. This is a fatal omission from a usability standpoint because there is abundant, credible literature that demonstrates the importance of salience and legitimacy (e.g., Cash, 2003). To give two examples of salience, communities adapt to local sea level not global sea level and thus global sea level rise might have low salience and thus low usability for many decision makers. Or century and longer sea level rise projections have little salience for, say 30 year home mortgages. Legitimacy is also important as it impacts the process and values of science creation and there are numerous examples where low legitimacy jeopardized science usability.

Clearly usability depends on the specific decisions and the community making decisions and, as noted by another commenter, usability cannot be divorces from this context: actionable in one decision context does not mean actionable in a different decision context. We also know that the way to increase the usability of knowledge is through co-generation, but that this is inefficient. Usually it is knowledge brokers and boundary organizations that play the role of interpreters and translators of knowledge to users. There is, again, abundant literature in this field and it is a shame to have no dialogue with the many previous studies that identify criteria AND the process of usable knowledge production.

My view is that the present study has *much* more value if this study is reframed more specifically around credibility rather than more broadly about usability/actionable. I would even recommend switching out the term "actionable" for "credible" to better reflect existing terminology.

These comments are based on a misunderstanding. It was never our intention to argue that credibility (or acceptance) is a sufficient condition for actionability, only that it is a necessary condition. Other writers, including Cash, have addressed the importance of salience and legitimacy. We chose to focus on credibility/acceptance because there has been confusion among practitioners about what counts as "sufficiently accepted".

The revised manuscript includes the following revised text in the Introduction (ll. 29–34, with new text in italics):

> The WUCA definition hints at the kind of knowledge appropriate for driving adaptation action ("sufficiently accepted"), but does not guide practitioners in identifying this knowledge. *Similarly, Cash et al. (2003) argued that scientific information can motivate action to the extent it is seen as credible, salient, and legitimate, but they did not offer criteria for credibility.*
>
> *Here, we will say that a claim is actionable when it is sufficiently accepted to justify adaptation action in the near term (assuming that other requirements for actionability, such as salience and legitimacy, have also been met).*

This text clarifies that we are not discounting salience and legitimacy.

Even here, I would have really liked the authors to examine the role of values and tradeoffs in defining credibility, especially recognizing that scientific uncertainty can often be weaponized to delay action.

We agree that scientific uncertainty can be, and has been, weaponized to delay action on climate change. But we think it is important to distinguish between claims which are sufficiently accepted to justify near-term action and those which are not. Our case study illustrates some problems that can arise from a standard that is too weak. We think a high-confidence threshold would be too strict, in part because of the danger JB notes. In proposing a medium-confidence threshold, we are trying to find a workable middle ground.

Determining if a study has multiple lines of evidence to support it is, as we can see from the comments, subjective and the authors have not demonstrated that their definition improves decision making.

Minimizing subjectively (i.e., maximizing objectivity) in decision-making is one of our core goals. It should be uncontroversial that greater objectivity will improve decision-making, even if perfect objectivity is an ideal that can only be approached asymptotically.

If science is social knowledge, as argued by Longino (1990), then objectivity is maximized when new ideas are examined critically by a diverse group of experts, as in the IPCC process. Objectivity can be compromised, on the other hand, if the unit of knowledge is taken to be a single peer-reviewed paper, or a report written by a small group of scientists who do not provide detailed, traceable accounts of their reasoning. The DP16 case study shows what can happen when scientific claims are communicated to practitioners without a vetting process designed to maximize objectivity. We think our criterion would have improved decision-making if it had been applied to recent sea-level adaptation guidance in California and elsewhere. We acknowledge that a more general demonstration of our criterion's fitness for decision-making is beyond the scope of a short article.

As the current paper reads, it seems more like it is specifically designed as a refutal of DeConto and Pollard's (2016) projections and the impact they have had as a high-end scenario in planning and adaptation decisions rather than as a general framework. If that is the goal then the study should, perhaps be framed as such.

Our aims are both specific and general, with the case study of DP16 offering support for a general framework. To acknowledge the role of this case study in supporting our arguments, we modified the last sentence in the first paragraph of the Recommendations as follows (ll. 277–278, with new text in italics): "This criterion is informed by IPCC practices, by philosophical arguments that scientific knowledge is social knowledge, *and by the DP16 case study*."

To give some additional examples, the explosive disintegration of the Larsen B ice shelf and subsequent acceleration of tributary was not anticipated by any models nor was the sudden acceleration and retreat of Jakobshavn, Pine Island or Thwaites Glacier. The disintegration of Conger ice shelf was also a surprise, although perhaps shouldn't have been. Going beyond glaciology, the pre-eminent physicists of the time Lord Kelvin famously said that "Heavier than air flying machines are impossible" a mere 8 years before the first airplane successfully flew (Shoemaker, 1995). The Harvard Economic Society announced that "A severe depression like that of 1920-1921 is outside the range of probability" on November 16, 1929 (Shoemaker, 1995). I could go on with other examples in which real world events failed to adhere to the academic consensus. Here the point isn't that scientists can be wrong (of course we are!), but that how communities, stakeholders and decision makers choose to incorporate information depends heavily on values, resources and objectives. Would we be better off stress testing, strategizing or planning to deal with low probability, high impact events? I don't know and I would be loath for scientists to insist on being on the arbiter of these value laden decisions. Actionable doesn't always mean infrastructure and it might mean longer term strategic thinking to be better positioned to incorporate new information as it becomes available. Perhaps one useful framing for the authors is that their approach might be most suited to mega infrastructure investments that are expensive and take decades to plan and build. Different criteria would be appropriate for different scales of intervention.

We generally agree with these statements. In the revised manuscript, we endorse stress-testing and strategizing within DMDU frameworks as an appropriate way to deal with low-confidence, high-impact processes. Also, we clarify that we use the term "actionable" to refer to near-term action, including large infrastructure investments. We agree that longer-term strategic thinking can incorporate low-confidence information—appropriately differentiated from information that meets our criteria—and that different criteria are appropriate for different physical scales and time scales of intervention.

We also agree that “how communities, stakeholders and decision makers choose to incorporate information depends heavily on values, resources and objectives”. At the same time, it is not impinging on the values of stakeholders and decision-makers to tell them that certain scientific claims are characterized by low confidence or deep uncertainty. For example, practitioners are routinely told not to use temperature projections from a single GCM in planning. Similarly, we are suggesting that practitioners should not use projections from a single paper without confirmation from the broader scientific community.

My penultimate point illustrates the degree of mischief that is possible if terminology and definitions aren’t tightened up and illustrated with multiple case studies. The authors argue that DeConto and Pollard’s study of sea level rise isn’t credible (actionable in their words) because there isn’t multiple lines of evidence to support ice cliff instability. I suspect that the paleo-record results in more ambiguity than the authors concede here, but nonetheless this is a fair point. However, we can also say that there is zero evidence that the calving front will remain stationary and very little evidence to support *any* calving law used in ice sheet modeling. By the authors same logic, we might conclude that *none* of the projections of sea level rise are credible (actionable in the authors words). We can apply this same reasoning to other processes. Should we doubt the entire enterprise of climate modeling because of the quasi-empirical treatment of clouds, precipitation and aerosols? Clearly, this is not what the authors intend, but it might be an unintentional side effect if not clarified. To this end, the IPCC, NOAA, NASA Sea level team and multiple organizations are involved in summarizing literature and play a well documented role in determining “credibility” and these organizations form one step in a chain of boundary organizations. Here there needs to be dialogue with the role played by these existing structures and organizations and the interplay with other boundary organizations.

Our argument does not imply that “none of the projections of sea level rise are credible”. In particular, we view the IPCC medium-confidence sea-level projections as credible enough to be actionable, if other criteria for actionability are also met. The same would be true of other projections associated with medium (or high) confidence.

We have worked to tighten and clarify our terminology and definitions. Additional case studies are beyond the scope of this work, but we agree that case studies from other fields would be valuable. We agree about the importance of dialogue, as emphasized in the Recommendations.

My final point relates to the definition of “action”. As noted by Knaggard (2014), one of the common actions that stakeholders take is to decide that the scientific uncertainty is currently too large and devote resources to research to better quantify and hopefully reduce uncertainties. By this definition, the action associated with high impact/low probability impacts might be more research (potentially coupled with adaptive decision making).
However, the most common action that decision makers take, however, is simply to do what is currently politically and technically feasible (Knaggard, 2014). These decisions, based on political expediency and co-benefits depend more on the solution space than scientific uncertainty and so establishing credibility is less important than salience and legitimacy.

We agree that adaptive decision making and additional research are appropriate ways to deal with the possibility of high impact/low probability events. We have clarified these points in the revised manuscript, for example in the discussion of DMDU. We also agree that in some cases, credibility may be less central than salience and legitimacy.

I'm very sympathetic to the authors goals, but I'm not sure that, to use the authors definition, their definition of "actionable" has multiple, independent lines of evidence to support its use and adoption. I think it would be hard to demonstrate this in the limited space available, but I think it would be a great addition if the authors built their case through a larger number of case studies and literature review and applied their own criteria of multiple lines of evidence to their own proposed definition of actionable science.

While we have clarified what we mean by "actionable", our main goal is not to define every aspect of actionable science. Rather, we have proposed a practical criterion for identifying scientific claims that are sufficiently accepted to be actionable. To support this criterion, we have offered multiple lines of argument and evidence drawn from IPCC practice, the philosophy of science, and the scientific case study. But we would be the first to say that our article is not the last word on the subject.

Reply to Judy Lawrence, Marjolijn Haasnoot, and Robert Lempert

We are researchers and science brokers who work with SLR science in the context of decision making under uncertainty (DMDU). We recognize the challenge of dealing with multiple scenarios that are frequently updated as new understanding emerges. However, the proposal set out by LBM runs the risk that we wait until there is certainty before taking adaptation action. This would also not meet the precautionary test in the UNFCCC which underpins the Paris Agreement, and lead to adaptation decision delay.

We thank these researchers for their thoughtful comments and also for their contributions to the DMDU literature, which have been helpful in sharpening our ideas. Some omissions and lack of clarity in our initial submission have created the appearance of disagreement, when in fact we support the use of DMDU methods in adaptation planning.

We suggest that medium confidence is a lower and more appropriate standard for adaptation action than "certainty". Our goal is not to delay adaptation decisions, but rather to help practitioners take robust near-term actions with limited information, including deep uncertainty. We think that if a particular action (e.g., building costly infrastructure to protect against 3 m of SLR by 2100) is likely to be unnecessary, then it is consistent with the precautionary principle to delay action and wait for more information. At the same time, it is prudent to make dynamic plans that could trigger such actions in the future, if justified by new evidence.

There are approaches that have emerged and assessed in IPCC AR6 for considering the dilemma that decision makers find themselves in as new science continues to emerge and decisions have to be made under uncertainty over the trajectory and pace of change. These DMDU approaches and methods are able to deal with large uncertainties and updated (climate) information. The LBM paper mentions adaptive planning and integrating SLR information into planning but does not elaborate what this means or how these approaches can address the problem of over and under investment in adaptation. This omission leads to a very directive solution that would not be decision relevant (salient nor legitimate).

We regret that our initial proposals came across as too directive. While we want to reduce overinvestment in adaptation, we also don't want to push practitioners toward underinvestment. We agree that DMDU methods are good tools for dealing with large uncertainties and updated information. The revised manuscript describes how DMDU can be used to reduce the risks of both overinvestment and underinvestment.

Furthermore, we agree with Bassis's comment that the LBM paper only focuses on the matter of credibility and that salience and legitimacy are critical for decision making under uncertainty to enable local context to inform decisions. The response from LBM that the paper is not about **how** the science is used, misses the point that science does not sit in a vacuum outside how it may be used. In fact the authors have defined their problem with an example of how the projections were 'misused' in the San Francisco example. Science and its use are inextricably linked.

As stated in the reply to JB, we agree that salience and legitimacy are important for decision making, although they are not the focus of our article.

We agree that science and its use are inextricably linked. We did not mean to imply that we are unconcerned with how science is used. To the contrary, our goal is to ensure that the use of science in adaptation planning is consistent with the evidence supporting that science.

One cannot set a fixed criteria/standard for a changing context (both science and societal values-which decision makers weigh up). The question of the standard of the science cannot be divorced from a discussion about how sea-level rise projections are used. Using DMDU methods such as Dynamic Adaptive Pathways Planning (DAPP; Haasnoot et al. 2019) or Robust Decision Making (Lempert et al. 2019) to stress test adaptation options against a range of scenarios gives decision makers an idea of how sensitive each strategy is and then this information can be weighed against the other considerations that the decisionmakers must take into account as representatives of their communities, now and in the future. Given that surprises around polar ice processes and feedback are occurring, even high-end/low confidence scenarios are useful to remind decision makers that one cannot rule out such outcomes, as also stated in IPCC AR6. A DAPP approach enables adaptation to be broken down into near-term actions and mid-to long-term options to avoid lock-in of investments that are costly to adjust in the future and which shift the adaptation costs to future generations. It helps to prepare and keep options open or create them through innovation and planning, allowing further adaptation if necessary. DAPP thus enables more robust decisions to be made and contingency actions to be ready.

We support the use of DMDU methods such as DAPP and RDM, with accompanying stress tests and sensitivity studies, and we agree that it is essential to separate near-term actions from mid- to long-term options. We hope our revisions have clarified this agreement.

We would also note that most communities planning adaptation action do not use DMDU approaches today. We think that communities without the experience or resources to use these more advanced tools could benefit from the approach we propose.

Given the time it takes for long lived infrastructure projects to be designed and implemented, especially for coastal adaptation, to high and rapid sea level rise, a precautionary approach has merit. Not considering such scenarios can run the risk of being too late or invest in the wrong measures resulting in high sunk costs and transfer costs. Considering them on the other hand gives decision makers greater confidence in a changing situation (science and societal values).

The nature of the decision process is lightly addressed in the paper beyond one example. A range of approaches as to how SLR projections can be used should be proffered. These can be found in the literature.

For example, in the Netherlands a group of experts (the Signal Group) advise the government on relevant new research which is then assessed for its potential implication and need for further research or actions (Haasnoot et al. 2018). The follow-up research from Deltares and KNMI led to further assessment on the need to reassess the adaptation strategy. It also raised awareness of the long-term higher sea levels, even if they were in low global warming scenarios, and that the current adaptive plan would not be sufficient, and that transformative adaptation would be needed. While near-term actions were not changed immediately, it was recognised that preparations were needed to be able to further adapt, if such an accelerated SLR became a reality.

In New Zealand, revised national coastal hazards and climate change guidance (Ministry for the Environment 2024) has adopted a tailored approach for different types of decisions, using DAPP to stress test adaptation options with downscaled global scenarios that enable polar ice responses and vertical land movement to be incorporated into SLR which vary around the coast. Periodic revisions are undertaken to reflect new science and a precautionary approach is adopted considering at least a 100 year timeframe to account for change and uncertainty. Specific infrastructure guidance is also available (Lawrence and Allison, 2024).

Adaptive pathways planning and monitoring for signals is also done in practice in the Thames Estuary plan (Environment Agency, 2012; Ranger et al 2013) and New York (Blake et al., 2019; Rozenzweig). They also plan regular ongoing assessment and evaluation of changes (e.g. ongoing and every 6 six years in New York and in the Netherlands).

Thank you for sharing these examples. We agree that it is helpful to give examples of approaches (including DMDU) that have worked well. We added the following paragraph at the end of Section 4 (ll. 261–272):

Finally, we would like to give two examples of practitioner guidance that avoided the pitfalls described above. First, the most recent sea-level guidance from the Met Office Hadley Centre (Palmer et al. 2018) retained high-end projections adopted in 2009. Citing DP16, the authors noted that “marine ice cliff instability has been proposed as an important potential feedback” but added that “further research is required to strengthen the observational evidence for, and prevalence of, this mechanism”. Second, the 2017 New Zealand coastal adaptation guidance (Ministry for the Environment, 2017) proposed a high-end (“H+”) scenario of 1.05 m GMSL by 2100, based on AR5, as part of a dynamic adaptive pathways planning (DAPP) strategy. The updated guidance (Ministry for the Environment, 2024) retained the DAPP approach and used the AR6 medium-confidence projections for SSP5-8.5 to design an H+ scenario with 1.1 m GMSL by 2100. The new projections were described as a “plausible upper range” for SLR and were recommended for “high-end stress testing of adaptation options and pathways”. The AR6 low-confidence projections were assigned a limited role for “further stress testing” related to long-lived coastal development and managed-retreat options. Since the core recommendations in the New Zealand guidance have not relied on low-confidence science, there has been no whiplash.

Again, we appreciate the many thoughtful comments from the community. We conclude our response with one note. None of the above comments suggests a boundary between scientific claims that are accepted or credible enough for near-term adaptation action and those that are not. Perhaps the authors of these comments think that all peer-reviewed claims (or claims from some subset of journals) are actionable, but they do not offer epistemic arguments for a peer-review criterion. We continue to believe that adaptation planning would benefit from differentiating between actionable science and science that is peer-reviewed but not actionable. Without this differentiation, we have seen in recent years and believe we will continue to see confusion, whiplash, and adaptation planning inefficiencies.

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