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Earth System Dynamics Discussions

Interactive comment on "Climate sensitivity in the Anthropocene" *by* M. Previdi et al.

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We thank the reviewer for his very thoughtful review, which we found to be quite helpful for clarifying several points that were raised in the manuscript. Below are detailed responses to each of the reviewer's specific comments.

Specific points: 1) The authors argue that the sensitivity of the Anthropocene is likely to be larger traditionally been assumed. That point is interesting, but not new. I understand the paleo evidence, but what exactly is the argument that the number derived from paleo data can be transferred to the present? The sensitivity for LGM may well have been 6_C if slow feedbacks are included, but why should this be the same today? First, the high sensitivity from the LGM is likely to be dominated by the ice sheets, but in terms of area there is not much ice left to melt today. Second, while I agree that ice sheets may respond faster than previously assumed, I doubt that their area (and that

is what determines the albedo) will change significantly on the timescale of decades to centuries (as argued on page 536 middle). At least I don't see any evidence given in the article. The text often refers to slow feedbacks, but is not quantitative on the timescale. Would we expect the 6_C sensitivity to be realized on the timescale of a century, a thousand years, or ten thousand? And what is the quantitative evidence for it? The fact that something was observed ages ago in a very different world is not sufficient to claim that it will be the same today or in the future.

Response: We agree with the reviewer's assertion that climate sensitivity will depend on the climate state, notably the amount of ice on the planet. The slow feedback sensitivity of 6°C for doubled CO_2 that is cited in the manuscript is the average sensitivity for the range of climate states between glacial conditions and ice-free Antarctica (Hansen et al., 2008). A 6°C sensitivity would be relevant in the Anthropocene if complete loss of the Greenland and Antarctic ice sheets occurs. For the case of only partial ice sheet loss, the sensitivity would likely be somewhat smaller than 6°C (e.g., Lunt et al. (2010) estimate a slow feedback sensitivity of 4-4.5°C for the mid-Pliocene warm period), but still significantly higher than the fast feedback sensitivity. This point will be clarified in the revised manuscript.

The reviewer also expresses doubt that significant ice sheet response will occur on timescales of decades-to-centuries as stated in the manuscript. We agree that a timescale of decades is likely too short to expect a significant response, at least in terms of changes in ice sheet area, which (as the reviewer notes) is what is relevant for the albedo. However, we contend that paleo-sea level changes (Thompson and Goldstein, 2005; Hearty et al., 2007) and present-day observations of Greenland and Antarctic ice loss (Tedesco, 2007; Rignot and Jacobs, 2002; Zwally et al., 2002; Chen et al., 2006) are supportive of the idea that significant ice sheet response can occur on centennial timescales (see also Alley, 2010). We will therefore modify the manuscript to characterize the ice sheet/vegetation response time as "centuries or longer". This being said, we would like to note that ice sheets may contribute substantially to sea level rise this century, long before their area changes significantly.

Finally, the reviewer asks how long it would take for a 6°C sensitivity to be realized. Hansen et al. (2008) show using a coupled atmosphere-ocean model with fixed ice sheets and vegetation that the full surface temperature response to a doubling of atmospheric CO2 occurs in about a millennium. This model has a (fast feedback) climate sensitivity of 3°C for doubled CO2. However, the time required for the climate to reach a new equilibrium in response to an applied forcing is known to be a strong function of climate sensitivity (Hansen et al., 1985). Thus, if we consider the slow feedback sensitivity of 6°C (which includes ice sheet response), we can expect that it will take much longer to reach equilibrium, at least several millennia.

2) In section 4, the authors discuss methane clathrates, pointing to the PETM. Again, what is the evidence that methane clathrates will be important on a century timescale for warming of a few degrees? It's interesting to list all those potential feedbacks, but to be of any use we need to put some number on it, and PETM doesn't seem useful for that.

Response: The reviewer questions whether methane release from clathrates will be important on a century timescale. Based on current knowledge, it appears very unlikely that there will be an abrupt release of methane to the atmosphere as a result of anthropogenic climate change over the next century (Brook et al., 2008). However, ongoing climate change is likely to increase the background rate of chronic methane release from clathrates on these timescales. This point will be noted in the revised manuscript.

3) The authors suggest that the climate sensitivity should be redefined. However, besides a sketch of a figure they do not provide a definition for it. In my opinion it will be difficult to define this in a meaningful way, because the feedbacks are time dependent, and likely state dependent. For the carbon cycle, it's not even clear whether the feedbacks are linear (Zickfeld, J. Climate 2011). One of the nice properties of the Charney

sensitivity is that it is a reasonably state independent number with a clearly defined timescale, so this number can be used to compare different models for example, or in probabilistic studies with energy balance models. If the new definition depends on timescales and the state of the carbon cycle and climate, and is different for each scenario and time period, it is unlikely to be of much use for anything. For example, we could define a LGM-Holocene Earth system sensitivity by relating the warming and CO2 emissions or concentrations before and after the deglaciation. But then how do we use that number to tell us something about the future, or about whether a 20th century simulation of a model has the right feedbacks?

Response: First of all, we share the reviewer's opinion that defining climate sensitivity in practice to include some of the additional slow feedbacks we have described (e.g., related to the carbon cycle) will be a formidable challenge. Doing so is beyond the scope of this paper. We have simply provided here a conceptual discussion of some of the main issues, in the hopes that it will promote further dialogue and ultimately more applied studies that actually quantify climate sensitivity including the slow feedbacks. A critical point (which we will emphasize more in the revised manuscript) is that there is no single definition of climate sensitivity that is appropriate in all cases. The reviewer notes a few of the attractive properties of the Charney sensitivity, which we recognize as well. Historically in modeling studies this has clearly been the relevant sensitivity to consider, since models lacked interactive ice sheets and carbon cycles. This is changing, though, with the advent of Earth system models. Additionally, the appropriate definition of climate sensitivity to consider depends on the nature of the forcing. For short-lived forcings such as volcanic aerosols, which remain in the atmosphere for a few years, the Charney sensitivity is the logical choice. However, the aim of the current paper is to discuss the definition of climate sensitivity that is most appropriate for the Anthropocene, in which the dominant forcing is long-lived greenhouse gases which remain in the atmosphere for centuries. On such timescales, slow feedbacks associated with changes in ice sheets, vegetation and the carbon cycle have the potential to become significant. We argue, therefore, that in this case, it is not only appropriate, but also necessary, to consider a definition of climate sensitivity that includes these slow feedbacks.

4) Carbon feedbacks: The discussion here is interesting and the processes are well described. But the authors should be careful here to not mix things up. Climate sensitivity in the classical definition is defined as the response to a given concentration or forcing. By definition it therefore does not depend on the carbon cycle because the concentration is fixed. There are no doubt feedbacks in the carbon cycle, but if those are strong then that would reduce the allowed emissions that lead to the specified forcing or concentration, but it doesn't affect climate sensitivity. So if the argument is that the carbon cycle feedbacks are part of climate sensitivity, then it's no longer a sensitivity to a given forcing but rather a sensitivity to emissions, which a very different thing (which has also been tried, see e.g. Matthews et al. Nature 2009 for the carbon climate response CCR). One could argue (and some have) that sensitivity is something like the response to the emissions that (in the absence of carbon cycle feedbacks) would have led to a CO2 doubling, but that gets overly complicated. I think it's fine to discuss these carbon and methane processes, they are relevant, I would just be careful to not mix up the different meanings of climate sensitivity. There is already a confusion in that people treat the climate sensitivity for CO2 doubling, the sensitivity for 1W/m2, the slab model sensitivity, the effective sensitivity determined from a transient simulation, and the Earth system sensitivity all as the same and compare numbers even though they mean different things. I think the community would benefit from a clear separation by giving it different names, i.e. keep climate sensitivity as the original quantity in the Charney sense and as used by IPCC, and define Earth sensitivity for the long timescales. Carbon feedbacks in my view can be discussed but might be better treated as leading to a different forcing. Or otherwise make it very clear that whatever this thing is called, it is not a response to a given forcing/concentration because the concentration can change (see also below a similar comment about humans).

Response: The reviewer makes an excellent point here about being careful not to

confuse the different meanings of climate sensitivity. We agree that this is an important issue. For example, although we begin in the Introduction to the manuscript by defining climate sensitivity in a very general way (as "the equilibrium change in global annual mean surface temperature that occurs in response to a radiative forcing"), we then go on to discuss more specific forms of the sensitivity: i) the fast feedback (i.e., Charney) sensitivity, ii) the slow feedback (i.e., Earth system) sensitivity, and iii) other forms of the sensitivity which may additionally include feedbacks due to changes in the carbon cycle and/or human behavior. Each of these more specific forms of the climate sensitivity is still consistent with the general definition given in the Introduction, but it is critical to be clear that what is classified as forcing versus feedback changes in each case. For instance, a climate sensitivity that explicitly includes carbon cycle feedbacks could be defined such that the forcing is the change in atmospheric CO2 concentration that would result from the anthropogenic emissions in the absence of any changes to the natural carbon sinks. The (carbon cycle) feedback, in this case, would then be the difference between this forcing and the total (actual) atmospheric CO2 change. (Of course, as the reviewer notes, defining climate sensitivity in such a way, in practice, is far from a trivial task.) Rather than introducing additional nomenclature, we will continue to use 'climate sensitivity' when discussing the specific cases i)-iii) above. However, because we do strongly concur with the reviewer's point here, we will revise the manuscript to make it clear that: there are several definitions for climate sensitivity; it is important to be clear about which definition is being considered (e.g., when comparing different studies); and there is no single definition that is appropriate in all cases (see response to previous comment above).

5) I have a bit of a problem with the suggestion at the end of humans being part of climate sensitivity. Of course the evolution of climate will affect our decisions, but if that is the argument then one might as well argue that there are no forcings, and everything since the Big Bang is feedbacks. Our decisions would then be a feedback of some neural process in our brain caused by some perception of the outside etc. and there would be no freedom for decisions. I like to think about the concept of feedbacks as

being determined entirely by the laws of nature, on whatever timescale, i.e. a series of responses that would be identical (apart from a bit of noise and chaos) in a repeated experiment. That applies to basically everything discussed in the paper except for humans. It would seem more natural to me to keep humans on the forcing side, but have an arrow in the figure from sensitivity back to humans. There is no doubt that the climate outcome will affect our decisions, but I think it is more logical to think about it as the feedbacks and responses interacting with the forcing to some degree, but keep the feedbacks as the part that is determined by how the Earth system works internally, i.e. the laws of nature. If we include human responses as a feedback, I would argue it's impossible to determine the sensitivity, because human decisions are not rational, and can't be described with any law. From a conceptual point of view, it's also difficult to think about humans being both on the forcing and the feedback side I think.

Response: The reviewer argues against the idea of considering humans as a feedback in the new framework for climate sensitivity. While we appreciate his arguments, we can also see at least a couple of reasons why humans could, in principle, be regarded as a feedback. First of all, humans are in fact an internal component of the Earth system, just like water vapor, clouds, sea ice, vegetation, etc. Thus, we can be impacted by Earth system change like these other components, even if the changes in humans that come about are not governed by the same laws of nature (as the reviewer notes). When viewed in this light, the only true external forcing of climate is the Sun. Secondly, one of the state-of-the-art activities in model development is to couple physical climate models with integrated assessment models (e.g., see http://www.iiasa.ac.at/Research/ENE/IAMC/iamc2010/iESM_2010-10-28v3-1.pdf), with the latter representing various aspects of human behavior (e.g., energy use, land use). In this next generation of models, humans will be internal (i.e., interactive) in the model world as well, and thus one could argue that it will no longer be appropriate to regard them as an external forcing. In summary, while we recognize the valid arguments presented by the reviewer against regarding humans as a feedback, we feel that this issue is at least worthy of further discussion based on the reasons

stated above. Thus, rather than declaring definitively that humans should be regarded as a feedback (as in the original version of the manuscript), we will instead pose this as a question at the conclusion of the manuscript: Should the human response to ongoing and anticipated climate change be regarded as an internal feedback contributing to climate sensitivity? How would this fundamentally alter the way we think about climate sensitivity?

References Cited:

Alley, R. B.: The role of warming in melting ice and sea-level rise, and the possibility of abrupt climate changes, Testimony Before US House Subcommittee on Energy and Environment, Washington, DC, 2010.

Brook, E., et al.: Abrupt changes in atmospheric methane, in: Abrupt Climate Change, U.S. Geological Survey, Reston, VA, 2008.

Chen, J. L., et al.: Satellite gravity measurements confirm accelerated melting of Greenland Ice Sheet, Science, 313, 1958-1960, 2006.

Hansen, J., et al.: Climate response times: dependence on climate sensitivity and ocean mixing, Science, 229, 857-859, 1985.

Hansen, J., et al.: Target atmospheric CO2: where should humanity aim?, Open Atmos. Sci. J., 2, 217-231, 2008.

Hearty, P. J., et al.: Global sea-level fluctuations during the last interglaciation (MIS 5e), Quat. Sci. Rev., 26, 2090-2112, 2007.

Lunt, D. J., et al.: Earth system sensitivity inferred from Pliocene modelling and data, Nature Geoscience, 3, 60-64, 2010.

Rignot, E. and Jacobs, S. S.: Rapid bottom melting widespread near Antarctic ice sheet grounding lines, Science, 296, 2020-2023, 2002.

Tedesco, M.: Snowmelt detection over the Greenland ice sheet from SSM/I brightness temperature daily variations, Geophys. Res. Lett., 34, L02504, doi:10.1029/2006GL028466, 2007.

Thompson, W. G. and Goldstein, S. L.: Open-system coral ages reveal persistent sub-orbital sea-level cycles, Science, 308, 401-404, 2005.

Zwally, H. J., et al.: Surface melt-induced acceleration of Greenland ice-sheet flow, Science, 297, 218-222, 2002.

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