

## ***Interactive comment on “Climate sensitivity in the Anthropocene” by M. Previdi et al.***

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Review of “Climate sensitivity in the Anthropocene” by Previdi et al

Summary: Previdi et al. present a discussion of various ways to characterize the sensitivity of the climate system on various timescales, and argue that the climate sensitivity of the Anthropocene is likely to be bigger than previously estimated. They propose to include other feedbacks into the concept of climate sensitivity.

Recommendation: This is an interesting manuscript which I very much enjoyed reading. What is written in the manuscript is nice and clearly presented. It summarizes part of what we know about climate sensitivity and how we might think differently about it. The authors no doubt have a lot of expertise in the topic and present some valid and interesting arguments. I do however struggle a bit with the format of the manuscript. I would not consider this as a normal research article, since it presents no new material

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or scientific insight beyond what was published before e.g. on the slow feedbacks by Jim Hansen (e.g. [http://www.columbia.edu/~jeh1/2008/TargetCO2\\_20080407.pdf](http://www.columbia.edu/~jeh1/2008/TargetCO2_20080407.pdf)), in the various references given in the paper, and also briefly in the review by Knutti and Hegerl Nat. Geo. 2008. The manuscript is also not really a review because it lacks a discussion of much of the evidence we have about climate sensitivity, e.g. evidence from climate models, 20th century warming and energy imbalance, understanding of processes, Pinatubo, etc. So if anything I would say it is a perspective or opinion piece that has some elements of a review (but only covering some aspects) but a strong conceptual character and a clear personal viewpoint of the authors. There is nothing wrong with that, and there is certainly a lot of interesting material, but I am unclear as to what type of article this should be, and how it fits into the journal. There are also a number of points where the manuscript is too vague (see below), and where the authors argue for something different but don't say how this could be achieved. Once those are addressed I think the manuscript may be published as a conceptual or opinion piece, but in this case I would recommend to shorten it a bit. I don't see it being published as a normal research article because it does not provide new material and it is not quantitative and explicit in how a different sensitivity framework should be defined.

## 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

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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.

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.

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 CO<sub>2</sub> 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?

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

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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 CO<sub>2</sub> 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 CO<sub>2</sub> doubling, the sensitivity for 1W/m<sup>2</sup>, 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).

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

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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.

In summary, I think the authors need to actually spell out how (in a quantitative way) they would want to see climate sensitivity defined, say how it could be constrained and if it's based on paleo evidence how it would transfer to future projections. I can see many of the conceptual points, but I don't see them easily introduced in a quantitative framework.

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