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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 her very insightful review, which will help to strengthen the manuscript in several regards. Below are detailed responses to each of the reviewer's specific comments.

Comments on the paper by Previdi 1) I think this paper has some important points and after revision can be a useful contribution, but had a very hard time with its present form because of the many things that are left undefined, or defined in a non-standard manner. I think these problems can be dealt with but major editing is needed. More care needs to be taken in defining and discussing fast and slow feedbacks, and the distinctions between them. This is key to the paper and needs a clear paragraph explicitly devoted to it, near the front of the paper. Clouds and water vapor respond within days or less to changes in surface temperature. But sea ice changes can take

place over several years. Changes in mixed layer ocean temperatures can occur within a decade or so, but the deep ocean requires many hundreds of years to warm up (and changes in ocean temperatures can in turn affect cloud distributions). In the abstract and elsewhere, it is not correct to say that only cloud and water vapor changes are included in current estimates of climate sensitivity and that these are 'fast'. Climate sensitivity includes slower responses of the ocean (decades to centuries). Please correct the abstract (and elsewhere e.g. page 534, lines 4-5), decide what you want to call 'fast' and say how fast is fast (days? Years? Decades? Century?), and be clear about what the models do regarding the ocean.

Response: We thank the reviewer for this comment, and agree that it is necessary to clarify what is meant by 'fast' and 'slow' feedback climate sensitivity. Fast feedbacks refer to changes in atmospheric lapse rate, water vapor, clouds, sea ice, snow cover, and natural (i.e., non-anthropogenic) aerosols, while slow feedbacks are changes in continental ice sheets and vegetation. The reviewer correctly notes that climate sensitivity additionally includes the response of the ocean. In models, of course, this oceanic response is represented to varying degrees. For example, models that have typically been used to estimate (equilibrium) climate sensitivity include only a representation of the oceanic mixed layer, neglecting the response of the deep ocean. While these responses of the different components of the ocean are indeed included in climate sensitivity, they are not typically regarded as feedbacks per se (e.g., we don't usually speak of a mixed layer feedback). However, the fast and slow feedbacks given above (which are changes in atmosphere and surface properties that affect the TOA radiation) will all clearly be influenced by how the ocean responds to an applied forcing. With regard to timescales, we will continue to use a characteristic timescale of decades or less for the fast feedbacks. For the slow feedbacks, we will use a characteristic timescale of centuries or longer. (Note that in the original version of the manuscript, we used decades or longer for the slow feedbacks.) An important point is that the characteristic timescales for the fast and slow feedbacks refer to the time required to establish the feedback process following a change in surface temperature. This is not the same as the time required for the full feedback effect to be realized, since the latter is set by the slow response time of the ocean. For example, a portion of the water vapor feedback will occur rapidly (within days) following an initial change in surface temperature, but the full feedback will occur over centuries or longer as the surface temperature continues to evolve on these timescales. Thus, the ‘fast’ and ‘slow’ feedback climate sensitivity are distinguished by the characteristic timescales of the feedbacks, and not by the time required for the surface temperature to reach a new equilibrium following an imposed forcing. Hansen et al. (2008) analyzed a coupled atmosphere-ocean GCM with a fast feedback climate sensitivity of 3°C for doubled CO2, and found that the equilibrium temperature response was achieved in about a millennium. For a slow feedback climate sensitivity of 6°C for doubled CO2, the equilibrium temperature response would be expected to take much longer (at least several millennia), since this response has been shown to be a strong function of climate sensitivity (Hansen et al., 1985). The above points will all be clarified in the revised manuscript.

2) What you want to call ‘slow’ are processes involving the distribution of vegetation and of the great ice sheets, which may take multiple millennia to evolve; please state this up front. I don’t think this quantity should be called the climate sensitivity, since sensitivity has a clear meaning: it is the climate response for a doubling of CO2 concentration. It is confusing and unhelpful to communication of climate change to incorporate CO2 concentration changes due to climate system feedbacks into this definition. The quantity you are discussing could be called ‘earth system sensitivity’ or something similar; please choose a name.

Response: The reviewer suggests here that it may be inappropriate to use ‘climate sensitivity’ when ice sheet, vegetation, and carbon cycle feedbacks are included, since this term is traditionally reserved for the climate response for a doubling of CO2 concentration (with only fast feedbacks included). In the Introduction to the manuscript, we define 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. Rather than introducing additional nomenclature, we will continue to use ‘climate sensitivity’ when discussing the specific cases i)-iii) above. However, we will revise the manuscript to make it clear that there are several definitions for climate sensitivity, and that it is important to be clear about which definition is being considered, which we will do throughout the manuscript.

3) The paper should introduce the concept of efficacy on lines 18-24 of page 532. Alternative definitions of common terms will confuse the non-expert reader. Please stick to standard definitions throughout and then say what you want to do differently, why you think a different definition is useful, and then provide a new name.

Response: We will introduce the concept of efficacy here as the reviewer suggests. Please see our response to the previous comment regarding the definition of climate sensitivity.

4) page 533, line 10-11. There is little evidence that ice sheet changes are important for climate sensitivity on timescales of decades during interglacials, nor is it clear that vegetation changes are. Please provide a more balanced statement.

Response: We agree that it is probably unrealistic to expect significant ice sheet changes to occur on timescales of decades. 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). Similarly, both observation (proxy) based studies (e.g., Peteet et al., 1994; Mann et al., 2002; Bos et al., 2005; Birks and Birks, 2008) and modeling studies (e.g., Jones et al., 2009) suggest that significant vegetation response can occur on centennial timescales, if not shorter. We will therefore modify the manuscript to characterize the ice sheet/vegetation response time as "centuries or longer".

5) page 533, line 15. Please make clear that the changes in ice sheets and vegetation are expected to be important on timescales of millennia – but may not be so on shorter time scales.

Response: Please see response to previous comment.

6) page 536, line 1. The reason it has not received greater consideration may not simply be that the models can't handle it, but rather that it is expected to occur very slowly. Please correct. Also, please be clear that the paleo data cannot be used to imply a rapid response – those data tell you what happened but it doesn't tell you how fast it happened.

Response: We agree with the reviewer that another reason the ice sheet feedback hasn't received greater consideration is that it is expected to occur very slowly. For example, on page 536, lines 6-9, we state that this feedback has been neglected "based on the long-standing notion that ice sheet changes occur so slowly (over several millennia) as to make them largely irrelevant to anthropogenic climate change occurring on timescales of decades to centuries". We argue that estimated rates of paleo-sea level change do provide meaningful information on the timescales of ice sheet response.

7) Evidence for meter per century sea level rise is largely restricted to transitions from

cold to warm climate states, when a great deal of ice is available to melt (much of which is at low elevation); the current state is quite different. Please make appropriate changes to explain this, in several places on page 536.

Response: We thank the reviewer for this comment. On page 536, lines 18-19, we indicate that there is a need to be able to "better constrain the range of possible magnitudes and the time dependence of the slow climate feedbacks". Thus, we recognize that the ice sheet feedback (and hence the slow feedback climate sensitivity) will depend on the climate state, notably the amount of ice that is available to melt. The slow feedback sensitivity of 6°C for doubled CO2 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. With regard to the timescale of ice sheet response, we recognize that a climate state with more ice than at present (much of which is at low elevation) would tend to favor a more rapid response. However, this would be compensated for to some extent (perhaps entirely) by the fact that the present rate and magnitude of anthropogenic forcing greatly exceed the forcing that drove glacial-interglacial transitions. To reiterate, though, we do believe that it will be necessary to better constrain the magnitudes and timescales of the slow climate feedbacks, and how these depend on the climate state, forcing characteristics, etc. We will stress this more in the revised manuscript.

8) The high end of carbon feedbacks is associated with a great deal of warming. Please make this clear on page 537.

Response: We will make this clear. For example, on page 537, line 20, we indicate the additional twenty-first century warming (0.1-1.5°C) due to climate-carbon cycle feedbacks as simulated by current models. However, the total (actual) warming in these simulations is substantially greater than this, particularly for the high end of carbon cycle feedbacks. We will make this point clear. Additionally, we will note here other carbon cycle processes not represented in the models which could potentially lead to still greater warming.

9) The description of the carbon cycle is good. It would be helpful to further discuss the very long lifetime of a portion of the CO2 (Archer's work). This would clarify that the carbon can last long enough to produce the earth system response, even if the climate system feedbacks (e.g., ice sheet change) take a long time.

Response: This is an excellent suggestion which we will follow.

10) The paper is a review. That's ok, but it is a little limited on the number and balance in the references. I'd like to see some more extensive review, e.g. on issues such as timescales of the decay of the ice sheets, where there is a range of views.

Response: We will make every effort in the revised manuscript to provide a more balanced view of controversial issues such as ice sheet response time.

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