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Earth System Dynamics Discussions

Interactive comment on "Geologic constraints on earth system sensitivity to CO_2 during the Cretaceous and early Paleogene" *by* D. L. Royer et al.

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Overview

This paper sets out to estimate the Earth system sensitivity (ESS) using a compilation of proxy temperature and CO2 records. This is a tough task, given the uncertainties in both data and mechanisms. The data compilation is useful in itself, and in this sense this paper can be a useful reference. I think the real scientific merit and "value added" in a paper like this, though, is in a careful discussion of uncertainties and/or new insights into concepts and feedback mechanisms, and I think the paper could be improved in that sense. I hope the section-by-section comments below will help lead to an improved manuscript.

1.1 Earth system sensitivity

My first problem is with the concept of ESS itself. The Charney sensitivity is a well-defined concept: the response of surface temperature to a fixed global-mean radiative perturbation. The key point is that the CO2 doubling in the Charney case is prescribed as an external forcing, and the climate system has no power to affect it. This allows for a clean separation between forcing and response which makes the Charney sensitivity useful in thinking about global warming. ESS is more problematic, since it explicitly brings in carbon cycle feedbacks. Thus an "external" CO2 perturbation (e.g. through increased volcanic outgassing) can cause a response in the system which changes the level of CO2 itself. This make the boundary between forcing and response blurry, and ESS a less useful concept. An extreme example of this problem occurs with glacial-interglacial transitions: in that case the CO2 perturbation is entirely a feedback and the external forcing is ... well, presumably the Milankovitch forcing, whose global-mean radiative perturbation is actually close to zero. So in this case, ESS is infinite. Clearly, ESS comes dangerously close to meaninglessness. I don't expect these problems to be solved in the present paper, but the discussion of ESS in the introduction should at least recognize and discuss these basic conceptual problems. Note that the "meat" of this paper is an estimate of ESS in which the proxy-estimated ancient-modern CO2 change is taken entirely as forcing; in light of the above discussion, this seems like an arbitrary choice. We don't even really know why CO2 declined after the EECO, for instance: it may have been due to increased weathering due to tectonic changes, but in that case what is the forcing? A shift in continental positions does not itself represent a radiative perturbation.

A more minor point is that throughout the paper it is assumed that a CO2 doubling gives a fixed 3.7 W/m2 radiative perturbation. This is not actually true: the "efficacy" of CO2 increases with increasing CO2 concentration (Hansen 2005, Colman and McAvaney 2009), so that by 4 doublings the forcing is more like 5 W/m2. Given the other

ESDD

2, C41–C44, 2011

uncertainties, this is not a big problem here, but it should at least be pointed out.

Colman and McAvaney (2009) Climate feedbacks under a very broad range of forcing GRL 36, L01702
Hansen, J., et al. (2005), Efficacy of climate forcings, J. Geophys. Res., 110, D18104

1.2 Prior estimates, 1.3 Goal of study

I find the discussion here confusing. First the authors cite previous work indicating ESS of 6C for the Pliocene, and attribute this high sensitivity to "cryogenic" feedbacks, which I take to mean surface albedo feedback due to changes in snow/ice. Then they state that looking at Cretaceous/early Cenozoic time is important since these are non-glacial times. However, the ESS estimates presented later in the paper mostly consider changes from present to ancient climates, i.e. from a glacial to an ice-free climate, and therefore the sensitivity estimates here also include "cryogenic" feedbacks. Please clarify the text to make these issues less ambiguous.

2 Method, 3 Results

My second major problem is with the approach to uncertainty in the ESS estimates. The authors proceed by picking a subjective "max CO2" line, dividing proxy temperatures by this max co2 value, and estimating uncertainties from the temperature variability in 10 Myr boxes. This leads to error bars in Fig. 1d which in my opinion are way too narrow and give a misleading impression of accuracy. In fact there are large uncertainties in both the temperature and CO2 estimates at each individual time slice. It would be much better to first give a more exhaustive discussion of how large these uncertainties may be for each time slice, and then estimate uncertainty bounds on ESS as Tmax/CO2min and Tmin/CO2max , where (Tmin, Tmax) is the error range for individual temperature estimates, and analogously for (CO2min, CO2max). This would give the reader a much more informative picture of how far the science has progressed to date in narrowing down these uncertainties.

ESDD
2, C41–C44, 2011

Relatedly, Fig. 2 compares uncertainties in modern Charney sensitivity from Hegerl et al with the ESS results of this study. The picture suggests that the uncertainties are comparable (the PDFs have similar variance). I think this impression is entirely misleading: we are most certainly not in a position to estimate Cretaceous/Cenozoic ESS with the same accuracy as Charney sensitivity over the instrumental record. I would strongly suggest removing Fig. 2, which in any case doesn't add much (or any) new information.

In my view, the most interesting result here is the attempt to estimate ESS within ice-free climates, bypassing comparison with modern. This gives a true estimate of ESS in the absence of cryogenic feedbacks, which is very useful new information. However, this is done very sketchily and with no error bounds at all. I would encourage the authors to expand and refine these calculations.

4 Discussion

This section addresses the issue of why the ESS found here is higher than the modern Charney sensitivity, and mostly revolves around a discussion of carbon cycle and more exotic cloud feedbacks. The discussion is fine in itself, though it doesn't add much to what is already known from previous work. There is no mention at all of "cryogenic" feedbacks, though most of the ESS results here result from a comparison of the modern glacial climate to pre-Oligocene, ice-free climates. Surely the loss of ice sheets and most (or all) sea ice/snow play a major role if lifting ancient temperatures above modern, and must be included in the discussion of elevated ESS here.

One feature that stands out in this compilation are the high temperatures around 90 Ma and associated high ESS. I would invite the authors to suggest some possible explanation for this, however speculative. This would inject some novelty into this section, which otherwise reads more like a literature review.

Interactive comment on Earth Syst. Dynam. Discuss., 2, 211, 2011.