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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.

Anonymous Referee #4

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This paper reviews a selected subset of temperature and CO2 estimates (those the authors consider more reliable) from the Cretaceous through the early Eocene, to infer values of earth-system sensitivity (ESS) over this time period. This sensitivity is expected to be higher than the so-called Charney sensitivity, which assumes ice sheets and atmospheric composition are fixed as climate changes. The Charney sensitivity has been the focus of many studies and is constrained to be at least 2C with a most likely value of 3-3.5C; this study finds a minimum ESS of 3C and a central value closer to 6C, consistent with previous studies. The main value of this study is in showing the relative consistency of ESS from various data sources and time periods, yielding some insight into the uncertainty in ESS obtained by this means. It gives a nice discussion of how to put temperature proxies in benthic ocean records, tropical surface and high latitude surface records onto equivalent footing in terms of global warmings (although there are significant uncertainties here as acknowledged by the authors).

I find this to be a useful review and would support publication. However I have concerns about some of the details and the conclusions, which I think the authors must address. After writing this review I read those by Abbott, Caballero, Caldeira and an anonymous reviewer and found that between them they had already made all the same points I came up with and I agree with the bulk of their comments. I also read RoyerÊijs responses and did not find them very satisfactory. Here is my own take on what is wrong:

1) The main added value of this paper is to begin to constrain the range of possible ESS values, by examining multiple time periods and proxies, rather than just quoting a single value. However I didnÊijt find the treatment of uncertainty very transparent or sufficiently thorough. In particular the authors put forward a "minimum" ESS, but do not cite a confidence interval that this goes with. More importantly they donÊijt really try to quantify all of the uncertainties (admittedly difficult), so (as noted by all the other reviewers) their "minimum" is not convincing. They mention uncertainties in T and CO2 but donÊijt say exactly what they take those to be. Moreover there are uncertainties associated with other possible forcings, such as dust, orbital changes or continental positions which are not discussed very thoroughly nor quantified.

Having said that, it is very useful (and a bit disturbing) to know that the central estimates are as high as they are. I cannot tell for sure from the brief description, but it appears that the ESS distribution in Fig. 2 is simply a sample distribution from their data, with no accounting of proxy or forcing uncertainties. This is a defensible way of showing the data, and I would keep the figure, but the caption should make it clear that the ESS curve is not a posterior pdf of a single parameter like the Hegerl et al. curve is. The true pdf of ESS could be much narrower than this, if the errors were independent over time and biases were small, but it could also be much broader if large biases that are

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consistent over the time period are possible (and I think they are).

The authors claim to have detected variations of ESS in time, but I see no evidence that these are not simply random variations due to changes in the neglected forcings or errors in the proxy data. The parsimonious approach would be to assume ESS is constant, unless there is evidence of changes that cannot be attributed to errors or missing forcings (a tall order in this case).

2) The treatment of forcings other than CO2 isnÊijt done right, as pointed out by Abbott. What the authors should do is obtain best estimates of the radiative forcing over time due to CO2, solar irradiance, and continental positions, and then ratio this to dT to get a sensitivity. What they have done instead is to make crude estimates of the warming from non-CO2 forcings and subtract these off the proxy dT, but these contributions are obviously proportional to the ESS itself which throws off the analysis.

Also, like some other reviewers I wonder how accurate their estimate of 2C for forcing due to Eocene-like continental rearrangement is. IsnÊijt is possible that reconfiguring continents could lead to disappearance of ice sheets at the same climate forcing value? This is not my area of expertise, but the burden of proof is on the authors to show that larger forcings are not possible (and a hand-waving argument isnÊijt enough).

Finally I didnÊijt pick up on this myself but agree with another reviewer that the presence of ice in the "control" climate also throws off the analysis in terms of non-glacial feedbacks. I think this is OK, especially if we are concerned with moving into an ice-free world from the one today—but it needs to be discussed.

3) The authors discuss previous sensitivity estimates from pre-Holocene times, and cite Hegerl et al. for recent evidence, but do not mention estimates from glacial cycles. Some comment should be made about these. In particular, there are suggestions that dust feedbacks may have amplified an otherwise low Charney sensitivity (Chylek and Lohmann 2008). Such dust feedbacks could perhaps be one of the reasons for ESS being higher than classical sensitivity.

4) Ken Caldeira notes a correlation vs. causation problem. I actually think one can make the argument that CO_2 is the only plausible "prime mover" for these long-term climate variations: we know tectonic processes can drive large CO_2 variations, and we have physically plausible models (BLAG) that account for the main signals in the proxy record on this basis, while other possible "prime movers" (the sun, or other gases like methane) seem unlikely for various reasons. However, causality is an assumption and needs to be stated and justified. If other drivers were significant, and if warmings produced more CO_2, this would indeed lead to a high bias in ESS. Ideally one would use Monte-Carlo simulations including T feedbacks on CO_2 to quantify how big this bias could be, using the data. In the absence of this the manuscript needs to be even more cautious.

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