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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 #2

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This paper contains an island of possible fact in a sea of speculation. I would urge the authors to remove much of the extraneous material and write a much shorter paper dealing with their analysis of the data. The paper would sensibly start with section 2.

Much fuss is made over the fact that not all feedback mechanisms are operating in climate system models integrated over comparatively short times. Is this a surprise to anyone? (The attribution to Charney, as though it were a scientific paper, of the NRC committee report is both misleading and unfair. The report was written by a committee chaired by Charney, and the list of members includes some fairly fierce characters—e.g., Goody, Leith—who were more expert than he was, on feedback processes.) Cli-

mate sensitivity was originally defined as the *equilibrium* response to CO2 doubling over the modern world. The authors can redefine it, as many others have, to refer to a transient state, or to a different baseline (the glacial world), but they should be very specific that they are changing the definition. That the equilibrium state (if one can determine it) is different when integrated over 100 and 1 million years is again, no surprise. Why should "Charney" sensitivity be used as a reference? Hansen's models are constructed for the Pleistocene and have serious limitations. One would not know from this ms. that there are serious questions (e.g., Thompson et al., Nature, 2008) concerning the reliability of recent sea surface temperature records. Maybe the uncertainty is too small to be of concern?

The factual basis is the analysis of the proxies of 125-45Ma. The authors have taken the published error estimates at face value, and it would be a great service to review/analyze these numbers. E.g., is a Tex86 error estimate of +/-1.7 deg C likely to be realistic, factoring in all of the potential systematic errors? Are the minimum or maximum values of CO2 likely to be reliable? To within what percent? How is that determined? They make a very long list of assumptions: no ice; no benthic temperature gradient; a constant ESS value; a "known" shift in solar luminosity; no vegetation feedbacks; no continental displacements;... So how reliable is their minimum? Then there is the discussion of "missing feedbacks" which is further speculation. It's difficult for the reader to know from this paper what has really been found to be based on data, and what is dependent upon the long list of assumptions, stated and otherwise.

I would urge rewriting the paper dropping most of the discussion of the modern world GCMs, focussing on the quantitative estimates of the ESS and the accuracy with which their minima are likely to be determined, and reducing the discussion of missing feedbacks to a list.

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