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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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Paper: Geologic constraints on earth system sensitivity to CO_2 during the Cretaceous and early Paleogene

Authors: Royer et al.

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Overview: Given the massive uncertainties associated with proxy reconstructions of

temperature and CO_2 in deep time, attempting to establish climate sensitivity or Earth system sensitivity using these data might appear to be hopeless. Nevertheless, given the scientific and social importance of these issues, it is worthwhile to attempt to do so, as the authors have done. When we do this, however, we should be very careful to give an accurate picture of the certainty we can place on the statements we make. As I will elaborate below, I believe that the authors have substantially overestimated the certainty in some of their results. My opinion is that the paper would be greatly improved if a more serious effort were made to discuss and characterize the uncertainty in their methodology. I would also either back off of the claim to have established a "minimum" ESS, which I do not think is justified if the uncertainty of the methodology is seriously considered, or else change the methodology significantly so that a real "minimum" ESS can be established.

Comments:

1. **Claim to have established a "minimum" Earth system sensitivity of 3°C:** When the authors claim to have established a "minimum" Earth system sensitivity, it is unclear what they mean. For example, the ESS in Fig. 1d is about 2°C during the Eocene, which is the portion of the considered period for which the best data exist. How then can 3°C be a minimum? More generally, do the authors think the probability based on their data of an ESS less than is 3°C is 10%, 1%, 0.1% or something else? To answer questions like this a more rigorous statistical framework, accounting for errors in both temperature and CO_2 reconstruction would be needed. Such a study may be beyond the scope of the current work, but I would like to discuss some of the uncertainties involved to make the point that it is probably inaccurate to claim that 3°C is the minimum ESS supported by the data.

Although there is significant uncertainty associated with the reconstruction of global-mean temperature in deep time, for example the ad hoc scalings the authors use to obtain global-mean temperature from a few isolated measurements, the major source of uncertainty in ESS reconstruction probably comes from the proxy CO_2 data. The authors dismiss (with excellent justification) the proxy data that yield high CO_2 estimates. In a recently submitted paper, our Editor, Matt Huber, dismisses (also with excellent justification), at least during the early Eocene, the proxy data that yield low CO_2 estimates (*Huber and Caballero*, 2011). In particular, *Huber and Caballero* (2011) cite numerous problems with using the leaf stomata proxy for CO_2 levels, which is the main source of CO_2 data in this paper. *Huber and Caballero* (2011) argue that leaf stomata cannot be trusted when the CO_2 might be high, because they do not respond to CO_2 in this range (aside from other, more fundamental problems with the proxy). Where does this leave us? In my opinion a realistic assessment of the situation is that CO_2 is only very-poorly constrained on timescales when ice core data are not available (or as *Huber and Caballero* (2011) put it, CO_2 estimation is only "semi-quantitative" when the CO_2 might be high).

Another important issue is that the methodology the authors use does not properly account for non-CO_2 drivers of changes in global mean temperature. They try to account for changes in solar insolation, although I have a suggestion for improving this (comment 2). As far as I can tell, they do not account for changes in continental configuration, which could be significant (comment 3). Furthermore, as *Huber and Caballero* (2011) note, other greenhouse gases such as methane may have been much higher during these warm periods. Some of the warming that the authors attribute to CO_2 might actually be due to increased methane. All of these factors tend to bias the ESS estimate made in this paper high, and make it difficult to accept the notion that a "minimum" ESS has been established.

Returning to the issue of the CO_2 reconstruction, I think it is important to discuss the "max paleo-CO_2" curve. This curve is essential in order to establish the "minimum" ESS. I do not accept that it is a "max" CO_2 curve, however. Even if we accept the proxy CO_2 data that the authors choose and the error bars on this data, the "max" curve is

drawn through the middle of the higher data estimates, rather than through the top of the error bars. How then can we think of this as the "maximum" believable CO_2? At the very least this curve should be adjusted so it goes through the top of the error bars on the plotted data. Even then it should be carefully explained that this can only really be thought of as a "maximum" if you think the leaf stomata CO_2 proxy is reasonable, and all the potential problems given by *Huber and Caballero* (2011) should be cited.

Finally, I have serious problems with Figure 2, and think it should probably be cut or significantly altered. It is very troubling to me that the spread in the PDF of climate sensitivity is actually smaller for the reconstruction from $\sim$100 Myrs ago than for reconstructions over the past 1000 years. To me this implies that there's a serious problem with the error analysis. We actually know what the CO_2 was over the past 1000 years fairly well from ice cores, whereas, as discussed above, we have very little idea what it was $\sim$100 Myrs ago. Furthermore, even though you can argue with the temperature proxies for the past 1000 years, they're probably better than those from $\sim$100 Myrs ago, and at the very least the spatial coverage is much better, so a reconstruction of global temperature is more manageable. Can the authors explain this? What's going on here?

2. **Change in solar forcing:** The way the authors deal with the change in solar forcing as the sun evolves on the main sequence is unsatisfying. A more rigorous way to deal with this would be to assume that a W m^{-2} in longwave is worth a W m^{-2} in shortwave (not strictly true, but at least the assumption can be stated), then include the change in solar forcing in the total forcing function from which ESS is calculated. This can be written as

$$ESS = \frac{\Delta T(t) - \Delta T^*(t)}{\log_2\left(\frac{CO_2(t)}{(CO_2)_0}\right) - \frac{\Delta S(t)}{3.7Wm^{-2}}}, \tag{1}$$

where $\Delta S(t)$ is the change in global-mean insolation due to the change in solar luminosity, $\Delta T^*(t)$ is the change in global mean temperature due to changes in continental and non-CO_2 greenhouse gases, $\Delta T(t)$ is the reconstructed temperature time series, and $CO_2(t)$ is the reconstructed carbon dioxide time series. Notice that $\Delta S(t)$ is negative, so including this term increases the denominator. For example, using the formula from the citation the authors give, I calculate that at 125 Ma the solar luminosity was 1.1% weaker. Assuming a modern solar constant of 1365 W m^{-2} and a global-mean albedo of 0.3, this yields a reduction in solar forcing of 2.6 W m^{-2} (by the way, using the sensitivity of 0.8 W m^{-2} given, this yields 2.1°C, rather than the 1.5°C the authors give). If we use 3.7 W m^{-2} as the radiative forcing associated with doubling CO_2, then this is equivalent to 0.70 doublings of CO_2. This should be subtracted from the doublings of CO_2 calculated from the proxy data to give an adjusted "CO_2" before the ESS is calculated (and a similar thing done for all times). Notice that using this methodology it is not necessary to assume a climate sensitivity to calculate the climate sensitivity, which, as the authors note, is problematic.

3. **Continental configuration:** As far as I can tell, the authors do not include any changes in global mean temperature due to changes in continental configuration in their estimate of ESS (although they do discuss this issue). By comparing modern GCM simulations that I did to Eocene simulations that Huber did, we found that going to Eocene boundary conditions increased the global mean temperature by 3–5°C, depending on the CO_2. This is similar to what Donnadieu found for the Cretaceous, and is equivalent to 1–2 doublings of CO_2 in the model we used. These simulations are described in *Abbot et al.* (2009), although I don't think we discuss this effect in the paper. From Eq. (1), this would significantly reduce the estimated ESS, again making the claim to have established a "minimum" ESS questionable.

4. **Timescale of anthropocene:** A very minor point is that the authors write on page 214 that "global temperatures will not cool appreciably for many centuries, even if antropogenic greenhouse-gas emissions drop to zero." The timescale is actually tens of

thousands of years (see the title of the cited Archer paper, for example).

Concrete suggestions:

Here are my concrete suggestions to the authors to estimate a more believable "minimum" ESS without modifying their methodology too much. It is possible that the resulting minimum ESS will be near zero (or even negative) for some of the time period considered, but I think that's probably a realistic statement of how well we can constrain ESS over this time period. If this is unappealing to the authors, then maybe the methodology could be adjusted to give some sort of "best-guess" ESS, although the issues discussed here should still be included in this estimate.

1. Use Eq. (1) and properly account for changes in insolation.
2. Use a new "max"-CO_2 curve drawn through the top of the error bars on CO_2 proxy data. I also suggest discussing the limitations of the CO_2 data used more openly.
3. Pick a "maximum" global mean temperature offset for changes in continental configuration. This will vary with time, but let's say something like 4°C would be believable as the maximum value, so set $\Delta T^*(t)$=4°C in Eq. (1). Since we have no information on other GHGs, neglect their contribution to $\Delta T^*(t)$, but explicitly state this assumption.

References

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Abbot, D. S., M. Huber, G. Bousquet, and C. C. Walker (2009), High-CO_2 cloud radiative forcing feedback over both land and ocean in a global climate model, Geophys. Res. Lett., 36, L05702, doi:10.1029/2008GL036703.

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