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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.

**M. Huber (Editor)**

huberm@purdue.edu

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I think this paper is an attempt to make progress on a difficult problem and within a framework "Earth System Sensitivity" that is still immature, and the growing pains are showing. I hope the authors will endeavor to make the necessary changes to make this an important paper on a subject of great relevance.

The "Editor Main Points" below summarize my reading of the reviewer comments and also extend the reviews a bit to make further concrete suggestions.Since there were quite a few reviewers and they generally gave a thorough and highly critical analysis, I have highlighted some of the Reviewer Main Points that bear special attention.Do not



take that special emphasis as an indication that the other points raised by the reviewers are not important, because I think they are.

The reviewers had some important and accurate suggestions and I suggest listening to them.If the responses given to the reviewer comments already posted are an indication of what the final product will look like then that will not be suitably responsive. I encourage the authors to make the effort necessary to make this a great paper.

Editor Main Points (EMP)

EMP (1) One can not simply remove the non-CO2 temperature perturbation, i.e. the component due to tectonics.To be self-consistent, one must remove the radiative forcing due to the change.This is an important issue.The reasoning is straightforward; whereas one can use a model (often CCSM or FOAM, both low sensitivity models) to estimate that changing paleogeography only caused 2C global mean temperature change, it must be remembered that this a model result and depends in large part on the sensitivity of the model.If one has an insensitive model then one ascribes less of a temperature change to the paleogeography.This leads to a potentially large logical problem by which paleogeography is weighted by low sensitivity models, which artificially enhances the apparent ESS.The authors must consider the forcing in W/mˆ2 of non-CO2 boundary conditions, not their temperature response as estimated by models.

I am therefore concerned by Royer et al's response to (Reviewer 3), " Our methodology, for the most part, does not involve W m-2 units; we simply compare temperature to CO2 (delta T per CO2 doubling). As a result, this concern is not especially relevant. Certainly, if the reader wishes to convert CO2 doublings to W m-2, this effect is of some importance. "This is like a recipe for an omelette that does no break eggs.To estimate sensitivity, there is no way to avoid dealing with forcings in W/mˆ2, since that is part of the definition of sensitivity.The authors will need to address this problem head-on in a future revision.

EMP (2) All reviewers concur that treatment of errors and uncertainty were not handled properly or transparently in this paper.In a revised version, each relevant variable should have an associated and clearly stated uncertainty distribution (e.g. "flat", "normal", "log-normal", etc) with mean, max, min, and probably 1-sigma clearly stated.One might quibble about what distribution or bound was chosen, but regardless it should be clearly stated and then propagated formally throughout the entire analysis.Along with properly accounting for error/uncertainty this analysis should at some point be formally carried out in terms of forcing in W/mˆ2 and response.These issues matter, consider for example recent discussion between Roe and Zaliapin& Ghil (Reply to Roe and Baker's comment on "Another look at climate sensitivity" by Zaliapin and Ghil (2010)).

EMP (3)To properly put a minimum on ESS requires making choices that reduce systematic biases in the "too hot" direction (as the authors acknowledge), yet two of the main sources of temperature information appear to be systematically biased to 'high end' estimates.Benthic data will reflect polar temperatures and we know that even in a warmer world without ice, substantial polar amplification occurs, so benthic data are unlikely to lead to 'minimal' temperature changes.In the tropics, the Kim et al. 2008, calibration that is used is at the very hot end of existing calibrations, using the Liu et al., 2009 calibration would likely lead to lower tropical temperature estimates that are more in line with 'glassy' foraminiferal d18O (I have verified this for the Pearson et al data).Ordinarily I have no preference for which calibration is used, but in this case it is clear that a low-end temperature estimator is required.So it appears to me that the authors have systematically biased temperature changes to high values rather than the appropriate choice, which is to low values.This needs to be corrected.

EMP (4)The conclusions of the study appear flawed based on the simplest 'sniff test' imaginable.If the various assumptions made in this paper are correct then one should be able to take a course-grained estimate of sensitivity within the available data, by comparing the "max-CO2' and temperature estimates from one densely sampled Cretaceous/Eocene time period to another. Doing that for the Early Eocene, centered



around the max-CO2 peak at $\sim$50mya (which is multi-proxy) and around 90 Mya, would suggest a reduction of CO2 is associated with an increase in temperature (I'm eyeballing from Figure 1b and Figure 1c).This implies that the minimum ESS in this course-grained sense is negative.This can not be easily reconciled with the main points of this paper.This is not a cherry-picking issue, this is simply a quantitative statement of the often noted pattern that the Cretaceous appears to be warmer than early Eocene, combined with the authors own reconstructed difference in CO2 between Eocene and Cretaceous.While I realize that was not the approach taken by the authors, the point remains that this is a serious weakness.The approach the authors stakes seriously overestimates the signal to noise ratio of the available data by comparing arbitrarily with a modern reference point rather than by comparing with a reference point within the data they present.This choice adds a difficult-to-constrain component to ESS which is likely not related to any internal feedback (e.g. paleogeography) and thus artificially inflates the number.A better approach is to use the data presented to internally estimate ESS.If the authors do this, it is likely that mean ESS will be smaller and the PDF will include negative values.

My suggestions are as follows.To fully characterize the uncertainty in the data, I suggest resampling the time series into a variety of bins of various width, from temporally coarse-grained to fine-grained and recalculating PDFs of sensitivity for each combination of bins.If one were to assume there is no serial correlation in the data then each binned estimate is independent and thus for a given bin width one can make many more estimates of sensitivity than attempted in the current draft.Serial correlation almost certainly exists of course. As indicated by the sniff test above, I believe that this will substantially widen the PDF of sensitivity and even include small to negative values.There are various methods for doing this binned/moving averaging and analysis and I leave that to the authors to decide which is best, but it is important to establish the sensitivity of sensitivity to bin width and resample in ways that utilize all the degrees of freedom possible.It is feasible that different bin widths would have different errors that the authors would like to associate with them (for example on time scales long that $\sim$5 mya perhaps the non-CO2–paleogeographic–uncertainty bars on the forcing would be larger).

If the authors would like to include a PDF/ESS estimate using their existing methodology for comparison I think that would be fine as long as proper distinctions were made and the other suggested changes were made.

Highlighted Reviewer Main Points

Reviewer (1) MP 1) How then can 3C be a minimum? More generally, do the authors think the probability based on their data of an ESS less than is 3C 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 CO2 reconstruction would be needed.

Reviewer (1) Concrete Suggestions

1. Use Eq. (1) and properly account for changes in insolation. 2. Use a new "max"-CO2 curve drawn through the top of the error bars on CO2 proxy data. I also suggest discussing the limitations of the CO2 data used more openly.

Reviewer (1) (MP3). Pick a "maximum" global mean temperature offset for changes in continental con- figuration. This will vary with time, but let's say something like 4âŮęC would be be- lievable as the maximum value, so set ∆TâĹŮ(t)=4C in Eq. (1). Since we have no information on other GHGs, neglect their contribution to ∆TâĹŮ(t), but explicitly state this assumption.

Reviewer (2) MP

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?.... 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 feed- backs to a list.

Reviewer (3) MP 1) 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.

Reviewer (3) MP 2) The authors proceed by picking a subjective "max CO2" line, dividing proxy tempera- tures by this max co2 value, and estimating uncertainties from the temperature vari- ability 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.

Reviewer (4) MP 1) However I didn't 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 don't 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 don't 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.

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).

Reviewer (4) MP 2) The treatment of forcings other than CO2 isn't 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.

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