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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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We thank Rodrigo for his helpful comments. He raises the important point that Charney sensitivity is different from Earth system sensitivity (ESS); in particular, for ESS part of the CO2 change can be a response (feedback), not necessarily all a forcing. Reviewer #1 (Abbot) raised a similar point. We will clarify this point in the Introduction of our revised paper, saying something along the lines of "Unlike the original formulation of Charney sensitivity, ESS does not require that CO2 be a climate forcing; climate feedbacks may drive some (or all) of the measured CO2 change. ESS reflects the long-term, equilibrium, whole-Earth coupling between doubled CO2 and global temperature.”

Rodrigo also points out that the radiative forcing associated with doubled CO2 (3.7 W m-2 for a 280 ppm baseline) changes as the baseline CO2 goes up. 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. We will make this point in the revised version. The only part of our manuscript where we assume a fixed 3.7 W m-2 value is when we account for solar evolution. But the radiative forcing of solar evolution in our time series is less than one CO2 doubling, and thus it is not necessary to accommodate for the potential of variable forcing.

Cryogenic feedbacks–Rodrigo is correct that most of our ESS calculations integrate across both glacial and non-glacial times. We discuss this point in the third paragraph of the results: “Third, because our results describe the mean ESS between the pre-industrial and a given geologic time slice, they could be biased by high ESS during glacial times (34-0 Ma). But even if glacial ESS was as high as 6 $^{\circ}$C (Table 1), non-glacial ESS still exceeded 3 $^{\circ}$C for some of the interval (orange lines in Fig. 1b-c).”

Rodrigo is concerned about the error bars in Figure 1d. We will emphasize in our revisions that these errors represent uncertainty in the *minimum* level of ESS, not uncertainty in the best-fit estimate of ESS. Related to this point, Rodrigo suggests that we try to place constraints on the upper bound of ESS (maximum temperature change, minimum CO2 change). Unfortunately, the CO2 and temperature data sets presently available are not adequate to do this: they sometimes lead to near-infinite ESS estimations. My sense is that this is telling us something about the ‘dark tail’ of the climate sensitivity probability distribution, but I would feel extremely uncomfortable trying to quantify this from fossil data; we simply don’t know enough (it’s tough enough for the present-day!). Given the behavior of climate sensitivity (i.e., much firmer lower boundary), it is far more prudent to use fossil data to establish reasonable lower bounds.

Rodrigo correctly points out the problems with Figure 2. As we discussed in our response to reviewer #1, we will remove the 'Hegerl et al.' probability density function (but retain our benthic-derived function).

Rodrigo suggests that we expand the section on calculating ESS from two fossil tie-points. We are reluctant to do this because, as we discuss in that section, the uncertainties stemming from the climate data are magnified ∼2-fold when making these types of ESS calculations. Along similar lines, Rodrigo suggests that we discuss in more detail what may be behind the very high ESS calculations ∼90 Ma. However, these ESS estimations are based on very sparse CO2 data and, as a result (and as stated in the manuscript), we don't place very high confidence in them.

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