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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 Dorian for his thorough and helpful review. We address here his four main comments in the order that they were presented.

1. Overstating the reliability of a 3 °C Earth System Sensitivity (ESS) minimum. Our intention was not to associate a minimum 3 °C ESS with the *entire* Cretaceous-Paleogene interval. For example, from the abstract: "We report ESS estimates of 3 °C or higher for *much* of the Cretaceous and early Paleogene" [emphasis added]. And from the first sentence of the discussion: "Our analyses indicate that ESS was at least 3 °C for *much* of the Cretaceous and early Paleogene" [emphasis added]. It is clear,

however, that in our revisions we need to clarify and emphasize our primary conclusion that ESS during *parts* of the Cretaceous and Paleogene exceeded 3 °C. This would seem to address Dorian's principal concern.

Dorian also questions our use of the CO_2 proxy data, especially the stomatal-based estimates. It is true that in a high CO_2 world (above 1000 ppm in most cases) the stomatal proxy only provides a lower constraint on paleo-CO_2. But at low-to-moderate levels of CO_2, the stomatal proxy is in the 'sweet spot' of its sensitivity to CO_2 and thus should be robust. Critically, most of the stomatal-based estimates in our study come from this 'sweet spot'. For estimates in our study from the less sensitive end of their respective calibrations, the upper errors are unbounded (vertical dashed lines in Figure 1a).

Related to this point, Dorian advocates for drawing the "max CO_2" line across the upper error limits of the individual estimates. We question this approach for two reasons. First, for those periods with a high density of CO_2 estimates, the upper confidence limit (for convenience, we'll say here 95% confidence) would not follow the upper limits of individual estimates, but would be closer to the mean estimates (where we drew the line). Second, if we assume the rather extreme view of a constant 2000 ppm CO_2 throughout the entire interval, our overall conclusion of a 3+ °C ESS for some of the interval remains valid (dashed lines in Figure 1b-c).

Dorian raises the possibility of other non-CO_2 greenhouse gases being important during our time interval. We agree completely, and a significant portion of our discussion is about this topic. The critical assumption that we are making, and that we will emphasize in our revision, is that any changes in the concentrations of these gases are part of the response to CO_2 change. In other words, while geography, plant evolution, and solar evolution impact global temperatures independent of CO_2, it is reasonable to assume that changes in, say, CH_4 are driven in part by changes in global temperature, which are in part driven by CO_2. Our assumption that these trace radiatively-active gases are purely a response to CO_2 is not entirely correct, but deconvolving what part is due to CO_2 is not possible at this time.

Finally, Dorian is troubled by Figure 2, in particular the juxtaposition of our probability density function (PDF) with a PDF based on recent climate records. This is an example of an "apples-to-oranges" comparison because the methods for the two approaches are very different. In particular, our PDF is based on comparing across time, while the Hegerl et al. study PDF is based on comparing across studies. In our revision, we will remove the Hegerl et al. PDF.

2. Treatment of solar forcing. Dorian proposes an alternative formulation for addressing solar evolution (his equation 1) that is in some ways more desirable than our formulation. We will adopt his approach in our revisions, which will lead to slightly higher ESS estimates.

3. Continental configuration. Dorian suggests that the radiative impact of continental configuration for the early Eocene is +3-5 °C (relative to pre-industrial conditions). The problem with this conclusion is that it is based on a study (Abbot et al., 2009) that prescribes ice sheets for the present-day simulation but no ice sheets for the early Eocene simulation. Thus, the 3-5 °C number combines both the impact of changing geography and ice sheet dynamics, unless one wishes to claim that the radiative impact of changing geography alone is sufficient to melt all ice. We discuss this issue at length in our manuscript and tentatively conclude that the impact of continental configuration is probably 2 °C or less. This is an area that needs further study.

4. Applicability to present-day climate change. Not all studies find no appreciable global cooling for tens of thousands of years once anthropogenic CO_2 emissions drop to zero (e.g., Solomon papers). Perhaps the difference of interpretation here is how we define "appreciable" cooling. Regardless, we prefer to preserve our more conservative language.

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