

***Interactive comment on “Estimate of climate sensitivity from carbonate microfossils dated near the Eocene-Oligocene global cooling” by M. W. Asten***

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Received and published: 3 January 2013

2 referee reports and 4 short comments have been posted. The Cenozoic climate sensitivity seems to be a hot topic, and the present study brings new elements to the discussion, although it must be improved. Globally, I recommend a major revision of this contribution. In my opinion, the main questions are the following, and they should be addressed clearly in the revised version:

1) Almost all the comments suggest that the number of sites for reconstructing deep water temperatures is insufficient (one site only). All the discussion in the paper is based on the DSDP 744 data. If, as the author mention, the data from DSDP 522 sup-

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port their analysis, this should be clearly demonstrated in the revised version, as suggested by the anonymous reviewer. Although this would strengthen the conclusions, it must be emphasized that this study is only a first step towards the understanding of the post EO climatic sensitivity.

2) The link between deep ocean temperature and global climatic evolution is a key point. It is certainly not obvious that deep ocean temperature at one site track the global temperature. One referee report and one comment display model results showing that temperature may indeed decrease at some deep ocean site, while the global climate is warming. This is particularly the case for the EO transition, as shown by M. Huber. But the EO transition is marked by a severe drop in temperature, allowing the Antarctic ice sheet to grow, which promotes a major reorganization of the oceanic circulation. As the contribution of Asten is dealing with the post-EO transition, there might be a chance that the temperature at DSDP 744 roughly reflects the global mean temperature, especially since the timescale is long enough (well above the oceanic mixing time) and the climate change not to severe. But only a slight chance. If DSDP 522 confirms the same temperature trend, as it seems to be the case, this should be clearly shown in the revised version. Also, the author should take advantage of the PALEOSENS contribution by citing it in the revised version. I also agree with M. Huber that the figs can be largely improved.

3) Although all comments point at a too restricted number of temperature data point, the number of CO<sub>2</sub> data point is rather low too (4 data points from Pearson et al., 2009). Given the large uncertainties in CO<sub>2</sub> reconstruction, I agree with both reviewers that the Pagani et al. (2011) data must be included in the discussion, as done in the answer to the reviewers. I do not see why the Pagani's data (hole 511) should be less reliable than the Pearson's data.

4) Regarding the conclusions of the paper: I think the Asten contribution demonstrates two things: (1) reconstructing climate sensitivity from past data is subjected to major uncertainties, and (2) that the past CS can be largely different than present day CS (but

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this conclusion is pending on the analysis of more data), for mostly unknown reasons. The Earth system is possibly too complex to use geological climates to constrain future CS. The conclusion section must be expanded accordingly.

5) A Monte-Carlo statistical analysis should be included in the revised version, as suggested by Ed Hawkins, since it is more robust and seems to confirm the results of Asten. Also, it is not clear why a 66% confidence interval is used instead of a 95% (anonymous reviewer). A discussion about this point must be included in the revised version.

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Interactive comment on Clim. Past Discuss., 8, 4923, 2012.

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