

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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I thank the Referee for his/her constructive comments on this paper. In the following, referee comments are in quotes with my answers in normal type.

1) "Atmospheric CO₂ estimates. I know that Pagani has recently updated its own CO₂ estimates, why does the author ignore this datasets? Is it due to the time resolution?"

Pagani et al (2010) provides data sets for pCO₂ in the Pliocene, and has provided estimates of CS accordingly. I don't ignore his data sets; I quote his result. Pagani et al (2011) in Science gave pCO₂ from carbon isotopes in alkenones for 7 holes in the Oligocene but the credible data was limited to holes 925 and 929, both of which

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have only 2 data points in the time interval 33.5 to 33.1 Ma. In these holes the time spacing between pCO₂ estimates is in the range 0.3 to 0.6My I do not regard this time resolution as adequate for the purpose of comparison with the higher-resolution boron-based pCO₂ estimates of Pearson et al used in my paper. I make some further comment on this topic in my reply to Mathew Huber.

"In a more general sense, to be fair, the author should discuss about the accuracy of the CO₂ proxies. For example, are the other CO₂ proxies telling us the same story during the E-O time interval? "

Pearson et al provide formal confidence limits on their estimates and I have no reason to doubt these. I am not aware of other CO₂ proxies with the necessary resolution; it is not sufficient to find pCO₂ estimates across the E-O time interval; the point of my paper is to focus on the post-EOT CO₂ short pulse of duration of about 150ky because this (like the pulse at the PETM) has a CO₂ injection into the atmosphere of a similar order of magnitude to that occurring in modern times.

"Why is the author using a 66 % confidence interval? From the comment of Pearson et al., it seems that the author would find a larger Earth sensitivity value with 95 % confident interval. It is a very important."

I use 66% confidence intervals in order to be consistent with the majority of studies since 2007 (including the IPCC2007, and Hansen and Sato, 2012). I am dubious about the use of 95% confidence intervals when the detail of probability distribution functions of pCO₂ and ΔT estimates are not known. Hawkins and Kennedy (CPD online discussion on my paper) provide a more rigorous analysis of confidence limits which I hope will be published along with my paper as a formal comment in CP. See my reply to their discussion; while their work provides useful additional rigor in the statistics of uncertainty, I believe they do not alter the conclusions of my paper.

2) "Meaning of benthic d180 as a global temperature record. I did not understand why the author has not used the DSDP 522 record from its paper. Indeed, the record is

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shown but the author states that the number of points for each time interval is small. Ok, but do we see the same trend? And why do you show this record if it is not for using it (what is the mathematical basis to reject it)."

Perhaps I was overly cautious in not using hole 522 for my analysis. The small number of data points in each of two data sets (Gyroidinoides and Cibicidoides forams) gives high uncertainty, the two data sets are not as continuous as that for hole 744, and the two data sets give answers differing by 30%.

Given comments of the referee and of Hawkins and Kennedy it now seems preferable to recognise we have two locations, holes 744 and 522. The latter is subject to greater uncertainties and I avoided using it for my conclusions in the paper but in fact it does provide support for the conclusions from hole 744. The temperature shift for hole 522 using Gyroidinoides forams lies within the 1-sigma range of values obtained for hole 744. The temperature shift for hole 522 using Cibicidoides forams lies below the 2-sigma range of values obtained for hole 744. In the revision of my paper I plan to use hole 522, although with some care given the limited quality of the data for hole 522 and the fact that I am reluctant to attempt putting confidence limits on the ΔT estimates from hole 522.

Certainly in a qualitative sense it is possible to state that hole 522 supports the result of a estimate of a CS based on hole 744, namely that the CS for this post-EOT event is at the low end of values shown in Table 2.

3) "I would like to see at the end of the paper a paragraph stating that the very low Earth sensitivity value found by the author may also be the result of the uncertainties existing for both geochemical datasets."

This will be done in the revised version of the paper. I agree it is important because as T. Schneider von Deimling points out in his CPD discussion on this paper, the low end of the CS range derived in this study are hard to reconcile with current knowledge on CS.

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

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