

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 Ed Hawkins and John Kennedy for their extension of my work. Their study provides some numerical guidance as to uncertainties relating to ice-volume correction to $\delta^{18}\text{O}[\text{CO}_2]$ measurements (my factor S1) and to the relation between deep sea temperature changes and global mean surface temperature (my factor S2).

First to clarify what uncertainties have been assumed in S1 and S2. The ice-volume correction for the change in Eocene greenhouse conditions pre EOT, to the post-EOT condition of a (probable) unipolar icecap is given by Zachos et al , 1996, as roughly 0.5 % of the total 1.4% change, that is about 30% of the $\delta^{18}\text{O}$ change is ice volume,

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and 70% is associated with temperature change. That temperature change about 4 degC (Zachos et al, 1996).

The smaller change in global temperature associated with the post-EOT CO₂ pulse is estimated in my paper as 0.59 degC excluding an ice volume correction. Data is not available to give an independent estimate of ice-volume correction for the post-EOT. If the ice volume does not change significantly during the time span under consideration for the CO₂ pulse (33.5 to 33.1 Ma) then the delta18O changes will be 100% associated with temperature change, and the ice-volume correction (my factor S1) will be the factor 1. If the ice volume changes to the maximum, with ice-cap melting during the post-EOT CO₂ pulse and temperature increase, then the ice volume correction will be 0.7. The latter limit is highly improbable because the temperature curve through this timespan shows a visible 41ky periodicity which implies that the polar icecap remained in place, although it may well have varied in size. My hypothesis is that the relatively small temperature change under consideration (being about 0.59 degC compared with the total pre to post-EOT change of about 4degC) will cause relatively small ice volume change on the polar ice cap and the resulting ice volume correction are likely to be nearer 1.0 than 0.7. In the absence of data providing an estimate I retain the range for S1 of 1.0 to 0.7.

Likewise in the absence of data, I am happy to accept the rectangular PDF used by Hawkins and Kennedy for my factor S1, although I suspect it should be asymmetric towards the higher value, which I predict would have an end-result in the CS confidence limits of displacing the limits to slightly higher values of CS.

The uncertainty in factor S2 is also difficult to quantify. The factor represents the proportionality constant linking variations in deep sea temperature with variations in global mean surface temperature averaged over time spans of order 10,000 years. Two authors quoted give values S2=1.0 (Kohler et al, 2010) and 1.5 (Hansen and Sato, 2012). Both studies are dominated by northern hemisphere data and are based on records from Pleistocene rocks (excluding periods of deep glaciation such as the LGM and

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equivalents). I offer no hypothesis as to which value of S_2 is more likely, hence it is wise at this point to retain both values in the allowed range for S_2 . Again, I am happy to accept the rectangular PDF used by Hawkins and Kennedy.

Hawkins and Kennedy state that the PDF for CS is skewed. They are right in detail. In my simpler approach I took the 1-sigma errors for temperature and pCO₂ used the logarithm of pCO₂ and the quotient rule to estimate corresponding 1-sigma for CS. The exact values I obtained are indeed skewed as expected due to the log(pCO₂) term and are CS=1.0838, range 0.7227 to 1.4681 (1-sigma) However for the purposes of discussion and mindful of limitations of the data it seemed prudent to round off to the numbers provided being CS = 1.1 ± 0.4 (1-sigma)

The corresponding numbers produced by Hawkins and Kennedy are CS=1.15, range 0.87 to 1.57 (66% confidence). For practical purposes and in particular for the discussion of significance of these CS estimates in my paper, I do not regard the difference between these various estimates as being significant.

I do not agree that use of 5-95% confidence limits would be helpful, nor would plotting a PDF be useful where the assumption is that of a normal distribution. The majority of authors since 2007 listed in my Table 2 (including the IPCC and Hansen and Sato) use 66% limits. We simply do not know what the detailed shape of the PDF should be, and whether a 95% confidence limit is meaningful in such circumstances is dubious. I am sure the normal distribution is an inadequate representation of error distribution for the temperature data which I use, since the individual data points are clearly not randomly distributed about a mean but contain periodicities (in particular the Milankovich tilt periodicity 41 ky) as shown by Zachos et al.

Hawkins and Kennedy express doubts about the use of a single location for making an estimate of climate sensitivity. In fact 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

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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. I note one referee for this paper recommends the hole 522 data be included in the discussion in my paper; I plan to do so, 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.

While for reasons stated I am cautious about assumptions relating to the shape of PDFs for the various parameters used in this paper, I believe the computation of uncertainty ranges by Hawkins and Kennedy is a helpful addition in elucidating the nature of the uncertainties, and I propose to suggest to the Editor, if my paper is accepted for CP, that their calculations be considered as a formal peer-reviewed comment for CP.

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