Estimate of climate sensitivity from carbon microfossils dated near the Eocene-Oligocene global cooling

reviewed by Matthew Huber

Estimate of climate sensitivity from carbonate microfossils dated near the Eocene-Oligocene global cooling

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Precís

Asten uses the Pearson et al., 2009 boron-derived atmospheric CO_2 record across the Eocene-Oligocene Transition (EOT) and a benthic oxygen isotope record from DSDP hole 744 (Kerguelen Plateau) to derive estimates of climate sensitivity. The manuscript is riddled with unjustified assumptions and is generally not up to the standards of current scholarship.

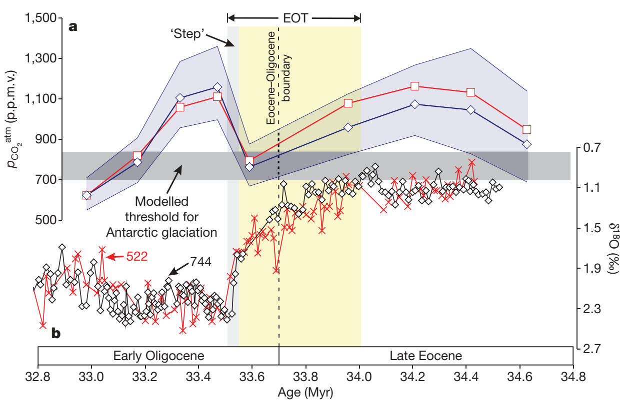
Within the piled-high overburden of sloppy science in this manuscript is a nugget of often-overlooked potential truth: the substantial increase in CO_2 reconstructed by Pearson et al. post-EOT in the absence of similarly significant deep ocean warming is presumably telling us that something is wrong with these paleo-proxy records or their interpretation or that climate sensitivity during this interval was weak. A better manuscript would carefully sift through the evidence and present a compelling case for one of those options. This manuscript does not.

In this review I will address the strengths and weaknesses in Asten's arguments about EOT sensitivity. It is also crucial to understand the great conceptual failure in any argument that suggests that EOT sensitivity tell us anything directly about modern climate sensitivity. While such comparisons do provide useful constraints and insights they are not directly relevant to modern and future sensitivity discussions and this distinction needs to be drawn more clearly in this manuscript and many others. I am recommending that this manuscript, in its current form be rejected, but a more complete and better executed manuscript on the same subject is certainly publishable in my view.

Review

The 'big picture' of Asten's argument

One can fault Asten's manuscript for weak graphical presentation, poor description of how the data are handled and the details of the statistical treatment, and a failure to address the literature properly. These are all valid criticisms in my mind and by themselves suggest to me that a completely re-written manuscript is in order (I will flesh out these criticisms below). If you have a good point, why not write a good paper? But, it would be inaccurate to simply gloss over Asten's main point. It is obvious, even upon a cursory glance over Pearson's CO_2 record and the benthic oxygen isotope compilations of Zachos et al. (2008) or Cramer et al. (2009), that these show little evidence of warming in the face of a large increase in carbon dioxide. After that, the calculation of sensitivity is but a detail—the modal value will be low, even if the distribution has a fat tail. I reproduce Figure 3a,b of the Pearson et al paper here to guide the discussion.



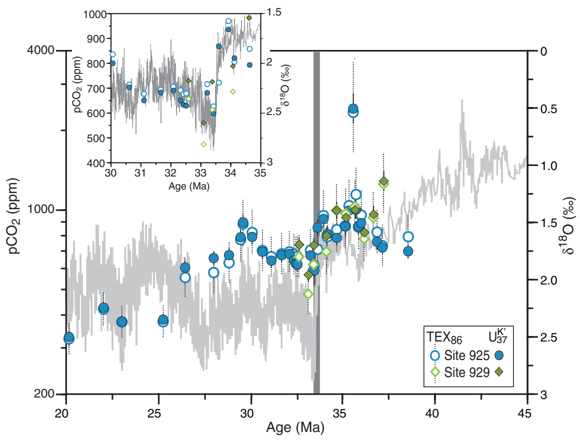
I think the figure makes it clear that, while one might fault Asten for some of the details in the calculation or its presentation, the pattern being described is easily visible and a valid target for investigation.

The problem, as I see it, is that the author does not fully grasp the potential errors in the proxies themselves and especially the uncertainties in their interpretation. Several flawed assumptions are made that, by themselves, completely account for Asten's results. His main justification is that others have made the same flawed assumptions. I cannot disagree with the fact that others have made the same flawed assumptions and written papers suggesting very high values of climate sensitivity, but I am not reviewing those papers here. In the past, I have actually reviewed such papers or handled them as Editor, and as the interested reader can see, I have rejected them as flawed (http://www.earth-syst-dynam-discuss.net/2/211/2011/esdd-2-211-2011-discussion.html).

Summary of the main weaknesses

- <u>Weakness One, and it's a big one (and probably not one that Pearson et al. really want to advocate) is that the CO_2 record of Pearson et al. (2009) may not be entirely accurate.</u>

The gold standard in paleoclimate is multiple, independent reconstructions. Even then, any proxy record interpretation is not guaranteed to be accurate, but at least it is more likely to be correct than one reconstruction. The 'new' boron paleo-CO_2 reconstruction technique used in Pearson et al. (2009) is a large improvement over prior work, but it still has its weaknesses and comparison with other methods is informative. New paleo-CO_2 reconstructions across the EOT in Pagani et al. (2011) utilizing an independent method, alkenones, looks very similar to the boron record in terms of the fall across the EOT, but not in terms of the post-EOT rise. See the figure here from that analysis (their Figure 4).



No CO_2 rise corresponding to that in the Pearson et al. record. Given the divergence of these two proxy records during this interval, they cannot both be correct. If we calculated sensitivity using the values in Pagani et al. and the temperature change Asten uses from 744, perhaps sensitivity would be very large? I do not know, as Asten ignored the Pagani et al. (2011) study.

The main point is, in paleoclimate one should never believe or use just one proxy or one proxy record. Instead, the modern approach to paleoclimate is to use multiple, independent proxies and investigate the robust signals arising from multi-proxy convergence and to explore the remaining uncertainties imposed by multi-proxy divergence. That is not done in the current manuscript.

Asten has cherry picked a CO_2 record that ensures a weak sensitivity.

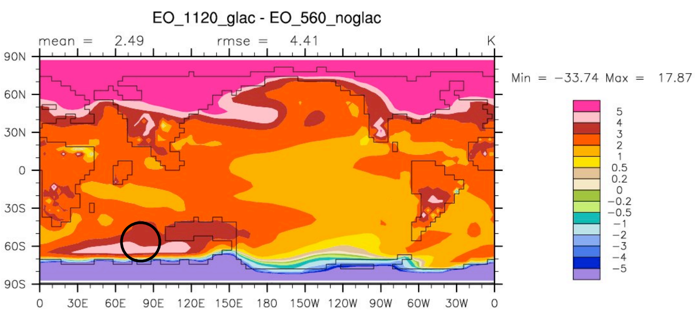
- <u>Weakness Two is another big one: you cannot estimate global mean surface temperature changes from one location and its even more impossible if that is a record from the deep ocean, not the surface.</u>

Imagine if someone tried to publish a paper establishing the global mean surface temperature trend of half a degree over the past century using one site in the Southern Indian Ocean, would that be considered credible? Not likely. How about if that estimate was 35 million years ago and based on isotopes in ocean sediments? Even less likely. The only comprehensive effort to establish, using multiple proxies, the temperature change at many locations across the EOT is that of Liu et al. (2008). The showed large cooling in high southern latitudes at the surface. Notably, there is no comparable cooling in the deep ocean seen in Mg/Ca (there is obviously a large signal in oxygen isotopes that reflects some mixture of ice volume and temperature). There is no obvious immediate post-EOT warming in the records presented in that paper, or in other papers such as Eldrett et al. 2009. This does not necessarily mean that warm did not occur, but simply that it is actually very hard to establish with any accuracy anything but the largest temperature trends across these intervals. The signal is quite 'noisy' and as such one record is likely to reflect either local temperature conditions or may not even record temperature at all.

Asten's argument is that Hansen and Sato (2012) and Kohler et al., (2010) make similar assumptions and therefore it is correct. Again, I cannot be responsible for the review process on other people's papers, only this one. The manuscript implicitly assumes that vertical ocean stratification (the vertical temperature gradient) did not change during the EOT or post-EOT. This would be very surprising given that a major climate change occurred during these intervals, an ice sheet was emplaced with associated wind and sea ice feedbacks, and finally major changes in ocean gateways were ongoing through this interval. Such issues are discussed at length in section 2.2.4 of Gasson et al (2012) and are totally ignored in this study.

Additionally, Asten makes the following unjustified and incorrect assumption, "Since global temperatures in the post EOT time under discussion are approximately equivalent to, or may be a degree or so warmer than, peak interglacial temperatures (with a unipolar ice-cap as inferred by ZQS), a linear 25 relation between deep ocean and global temperatures is a reasonable assumption for the post EOT." I'm not sure how he reaches the conclusion that post EOT temperatures were equivalent to peak interglacial temperatures. It is clear that not only is he assuming that vertical stratification did not change much during the EOT but did not change much from modern interglacials to the early Oligocene, which flies in the face of most scholarship (see Toggweiler and Bjornsson, 2000; Nong et al., 2000; Cramer et al., 2009; Sijp et al., 2011; Gasson et al., 2012). Perhaps Asten makes the circular assumption that he can use the benthic oxygen isotope record to infer EOT temperatures because he infers from benthic isotopes that EOT temperatures were near modern values? The argument is tortured at best and is not even made in any clear fashion Global mean temperature at the end of the Eocene is a debatable quantity, but estimates are that it was >7°C warmer than modern, not (the equivalent of) ~1°C warmer than modern. Asten appears to have missed most of the literature on surface temperatures and vertical temperature gradients in the late Eocene-early Oligocene and consequently sees the climate as a small perturbation from modern, and his assumptions reflect this.

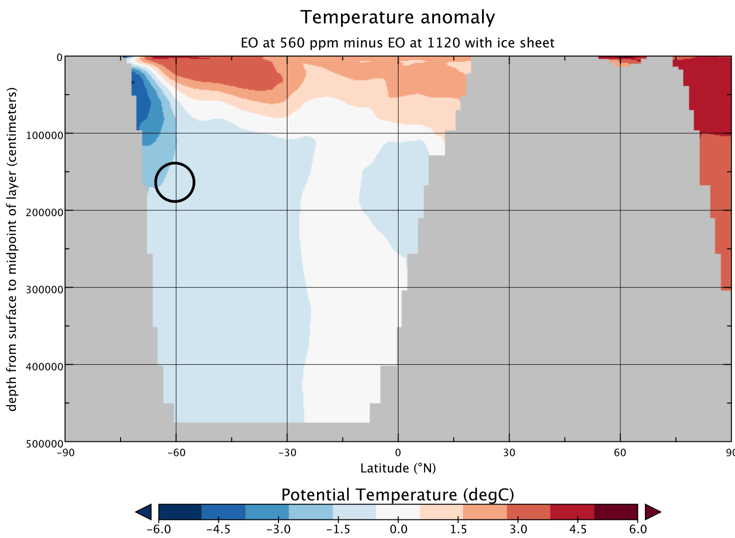
I prefer to not simply guess what the relationship between temperature at a site (say, DSDP site 744) and global mean temperature is (site indicated by circle on figure). Or the how the vertical (surface to benthic) temperature gradient might have changed. Guesses tend to have infinite error bars and zero falsifiability. In place of a guess, I show unpublished results from fully coupled ocean-atmosphere model simulations (using the NCAR CESM1 model) for late Eocene-Oligocene conditions. I show a comparison of annual mean surface temperatures from a simulation with 560 ppm CO_2 and no ice Antarctic ice sheet with one with 1120 ppm CO_2 and a near-modern sized ice sheet.



The former state corresponds to the low CO_2 state that immediately preceded ice sheet initiation and the latter state corresponds to the situation in which the ice sheet remains but CO_2 has risen as in the Pearson et al. (2009) reconstruction. Much of the results shown here is very similar to that in Goldner et al. (2012) (http://www.clim-past-discuss.net/8/2645/2012/cpd-8-2645-2012-discussion.html) although these are fully coupled results.

The figure shows the annual mean surface temperature anomaly. As can be clearly seen, most of the is much warmer because of higher CO_2 (global mean temperature 2.49°C anomaly). But it is also clear that the presence of ice on Antarctica cools locally and has an impact on the Southern Ocean. Site 744 is located at 60°S latitude and 77° longitude, right at the heart of a warm SST anomaly in the model results. What about at depth?

Next, I show a slice through the ocean model results at that longitude. The plot shows temperature anomaly in a latitude-depth transect. Positive values, as before correspond to the higher CO_2 case being warmer, and negative values reflect cooling due to the higher ice sheet in that case. The paleodepth of site 744 was approximately 1800m. The model results indicate local cooling at depth even though the simulation is more than two degrees warmer in global mean at the surface. More careful analysis reveals that changes in winter temperatures, wind fields, and sea ice along Antarctic in the case with significant Antarctic ice volume and enhanced CO_2 results in the interesting situation of a warmer surface climate and a cooler deep ocean, i.e. a significant change in vertical ocean circulation (about 4°C on average).



One does not have to believe these model results or any model results to acknowledge the fact that assuming constant ocean stratification during some of the largest climate changes in Earth's history is difficult to justify. At the least it introduces large sources of uncertainty (error). Asten's paper makes just this assumption although it is not acknowledged as such. The results shown here, or for that matter in a number of the prior mentioned studies support large changes in vertical stratification during this interval.

This is absolutely the worst period to choose using a benthic temperature as a good estimator of surface temperature change.

- <u>Weakness Three is more nuanced but just as important: sensitivity is likely to be state dependent, so this is all not directly relevant to many of the other sensitivity estimates compared by Asten.</u>

The manuscript actually acknowledges this possibility in several places, but does not do it justice.

I refer the interested reader to the recent discussion of this kind of issue in a recent Nature paper, (PALEOSENS, 2012). The key point is that one should be very careful to distinguish between paleo-estimates of sensitivity and modern estimates. It is only through finding models that agree with paleo-estimates of sensitivity and then using those models to project into the future that one can find a good application of these estimates as prognostic tools. One cannot simply project into the future with a paleo-sensitivity estimate because the climate system was in a different state in the past. Comparing modern and E-O estimates in the cavalier way done in this manuscript is misleading.

Concrete recommendations for a new manuscript

- Try to actually estimate surface temperature changes in many places, not benthic temperature changes at one point.
- Try to utilize multiple CO_2 proxies (boron, alkenones, and stomata)
- Try to perform a proper Monte Carlo type error propagation considering the joint errors in both the surface temperature and CO_2 estimates from above.
- Articulate clearly how EOT sensitivity estimates are quantitatively relatable to other, modern estimates.
- Consider the fact that many processes are operating concurrently, some of which may involve non-CO_2 forcing, such as ocean gateway changes.