Reply to Referee#2 Mathew Huber

1 On the use of peer-reviewed data

Before discussing my paper I point out there is a fundamental flaw in Mathew Huber's handling of peer-reviewed science. An author is entitled to quote peer-reviewed literature and build on it Isaac Newton observed, as scientists we see further by "standing on the shoulders of giants"). That does not imply we accept unquestioningly the conclusions of prior work but it is dubious practice to discard prior data without careful argument. Mathew Huber offers the comment "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".

I too have spent 25 years reviewing and editing scientific papers and I suggest that it is not for a reviewer to discard new papers because he believes prior peer-reviewed material is faulty. Rather the reviewer should author rebuttal and submit to the journals which published the papers in question.

Mathew Huber criticizes my literature review, data, methodology and conclusions with considerable energy, however precision is missing in his own arguments.

2 On Weakness One

Mathew Huber states of my paper "Weakness One, and it's a big one (and probably not one that Pearson et al. really want to advocate) is that the CO2 record of Pearson et al. (2009) may not be entirely accurate." I suggest it is unsound to begin a discussion by surmising that a dataset published by a highly regarded team in a top journal is not accurate. It is of course open to question their methodology and possible biases and errors in the data if new facts or insights are at hand, but until such argument is made by a person competent with details of such methodology, it is reasonable for a subsequent author such as myself to take that dataset and use it for further scientific study. It is not reasonable to use a position as a referee of one paper to provide fact-free criticism of another paper.

The referee then quotes with approval his own co-authored work Pagani et al (2011): "New paleo-CO2 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." However the referee appears to be unaware of the (lack of) time resolution in his own work , in that in the main paper the two boreholes 925 and 929 having the most credible results have only two data points each within the time window of interest in my paper (33.5-33.1 Ma). So yes, the alkenone method shows the pCO2 fall post-EOT, but it is a weak argument to claim lack of evidence of a post-EOT rise when that lack of evidence is actually a lack of measurements.
(refer to the table of data for Pagani et al 2011, online at
http://people.earth.yale.edu/profile/mp364/content/data-files).

Mathew Huber concludes the section of discussion with the comment "Asten has cherry picked a CO2 record that ensures a weak sensitivity." It is an insulting comment, and I find it offensive. However combined with his earlier use of the descriptive comment "piled-high overburden of sloppy science" and in the context of his lack of familiarity with his own work I wonder to whom these comments are really addressed.

3 On Weakness Two

Mathew Huber states "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."

I suggest that in fact it is easier to estimate from a deep ocean record, when looking at time slices of order10ky, than from sea surface temperatures. Mathew Huber's analogy of measurement over half a century is a straw-man argument; we concern ourselves in my paper with 10ky slices as provided by Zachos et al (1996). Over such spans of time (much greater than the period of thermohaline currents) we can expect deep ocean temperature variations to reflect global temperature variations.

The referee states:
"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."

The major climate change at the EOT is acknowledged in my paper, but it is not the subject of the paper. My paper makes the assumption that vertical ocean stratification did not change during the short interval of 33.5 to 33.1 Ma. Over longer periods of time it probably would change because yes the continents were moving and as I acknowledge in the paper the South America-Antarctica (Drake passage) separation was taking place in the mid-Eocene to early Oligocene.

More generally the referee appears to wish to disagree with the prior work of Zachos et al (1996), Kohler et al (2010), Hansen and Sato (2012) for purposes of criticising my paper. As stated previously this approach to peer review is unsound.

The recent paper Palaeosens (2012) appears to agree in principle with the above references. It states:
"Based on these data, the estimated average surface warming is 5-6K. On average, deep ocean temperatures increased by the same magnitude. The increase in Arctic and deep ocean temperatures was of the same magnitude as in the tropics."

This is a useful observation supportive of my paper because it deals with the PETM (55Ma) whereas the Kohler, and Hansen and Sato, references I use are limited to Pleistocene ages. Thus the principle of using a proportional relationship between variations in deep sea temperature and variations in global mean temperature is supported, and at a geological time when continental positions and ocean currents were different from the Pleistocene.

Mathew Huber should be aware of this paper in Nature; he is listed as a co-author.

The referee states:
"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."

This is another straw-man argument. I think he has failed to recognise my paper focuses on a narrow time interval 33.5-33.1 Ma, not the EOT itself (33.7Ma), when global mean temperatures were up to 5 deg warmer than at the end of the Eocene (see Zachos at al, 1996, fig. 8). At 33.5 Ma there was a unipolar icecap estimated at 40% of the current Antarctic icecap. It certainly was not 7 deg warmer than modern, because that hot condition corresponded to Eocene ice-free greenhouse conditions. If the Eemian was a degree or so warmer than modern, and 33.5 Ma post-EOT was in my words "may be a degree or so warmer than, peak interglacial temperatures", then the approximate arithmetical relationship (familiar to most geoscientists) of 1+1+5=7 is close enough for the purpose of the discussion without need for introducing yet another straw man argument suggesting I used circular reasoning based on numerical deltaO18 values.

Far from "flying in the face of modern scholarship", my statement as to the cool post-EOT time under discussion is supported by the recent paper Palaeosens 2012, which contains the statement (Supplement section B3):
"The Eocene-Oligocene transition (34Ma)reflects a major step in the Cenozoic global climate change from a warm *greenhouse* climate to a cold *icehouse* climate."
Again we have a peer-reviewed statement co-authored by Mathew Huber which appears to contradict the position he takes in reviewing my paper.

4 Weakness Three

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

The referee is in effect saying that my statements are correct but he would have phrased it differently.

The referee comments "One cannot simply project into the future with a paleo-sensitivity estimate because the climate system was in a different state in the past." I agree, and I don't. What is the purpose of the comment?

I tabulate estimates of CS from various sources, for the purpose of comparison and discussion. The referee calls this "cavalier"; I call it objective discussion. As the Palaeosens paper in Nature 2012 states, "Clarifying the dependence of feedbacks, and therefore climate sensitivity, on the background climate state is a top priority, because it is central to the utility of past climate sensitivity estimates in assessing the credibility of future climate projections".

A part of such assessment will surely include CS estimates based on current climate and satellite records, and all will play a part. I recognise and I state in the paper that differences between transient and equilibrium CS must be recognised.

I thank the referee for drawing my attention to the Palaeosens paper (it was not available at the time of submission of my manuscript).

5 Estimation of CS from alkenones

Both referees suggest these be included in my paper .

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 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.

Only one hole of the Pagani et al datset (of 7 holes in total) has pCO2 estimates spaced in time close enough to be of interest in the time range 33.5-33.1Ma is hole 511. The values are

Hole	time Ma	maxpCO2	pCO2	min pCO2
511	33.22	2862.96	2187.21	1694.34
511	33.31	3558.97	2573.53	1913.34
511	33.49	3838.31	2720.89	1994.98
511	33.53	2582.39	1966.66	1517.34

(the selected pCO2 values here are a subset using ε_f =25‰).

The pCO2 values are offset to high values and their validity and cause of the offset are subject of some discussion in Pagani et al. Given the uncertainties recognised by those authors I do not regard it as appropriate to use the numbers for a peer-reviewed paper. However given that both referees have asked about use of the Pagani data, I point out in the informal spirit of a “departmental seminar” that we can average the first and last values to get a base value, average the middle two values to get a higher value, then take the ratio and compute a “climate sensitivity” identical to what was done using the Pearson et al boron-based data set in my paper.

Using Equation 6 of my paper, and making an a priori assumption that offsets in pCO2 estimates for hole 511 are a multiplicative factor not an arithmetic shift from more realistic estimates, we have

$$CS_{2xCO2} = \Delta T_{global} / \log_2 (1+ \Delta pCO2/ Base\_pCO2)$$
$$CS_{2xCO2} = 0.594/ \log_2 (2647/2077)= 1.7\ °C$$

I emphasise that given the uncertainties ascribed to the data set by Pagani et al, this is not claimed as a useful number, but it is provided as an answer to the referees’ question. While this value is higher than the estimate of CS in my paper (which was 1.1°C, and subject to scaling factors S1 and S2) the value so obtained for hole 511 is not outside the uncertainty range given in my paper. We may make the qualitative point that the hole 511 value also points to a CS estimate on the low side of previously published results (see Table 2 of my paper), but scientists specializing in 13C and alkenone geochemistry will need to be more confident about the accuracy of such data sets before I would be confident in progressing such a result from a “blackboard” to a journal article.