Clim. Past Discuss., 11, C1187–C1192, 2015
www.clim-past-discuss.net/11/C1187/2015/

Climate of the Past Discussions

Interactive comment on "Frequency, magnitude and character of hyperthermal events at the onset of the Early Eocene Climatic Optimum" *by* V. Lauretano et al.

V. Lauretano et al.

v.lauretano@uu.nl

Received and published: 28 July 2015

We are grateful to Dr. Sexton for his constructive suggestions for improving our manuscript. We have addressed his comments below (reviewer comments: "RC"; author comments: "AC"), and have revised our manuscript accordingly.

RC: "The authors present valuable new benthic foraminifer stable isotope data across the lead-in to the peak warmth of the early Eocene. The data appear to be of high quality, are presented versus an astronomical age model and they consider the relationships between d13C and d18O across a series of 'hyperthermals' in an attempt to gauge the consistency in carbon source across these events. The manuscript is well-

written with few errors and the figures are clear. My main broad suggestion relates to the authors' comparison of the slopes of d18O vs d13C across different events. For the 'paired' events of H1/H2 and I1/I2, the d18O vs d13C slope for the second event of each pair is slightly different from the equivalent slope for the first event of each pair. The authors suggest that this may relate to different carbon sources driving the two events within each pair, with the second event of each pair being driven by a source of carbon with an isotopically heavier signature (they also mention other possible explanations for the discrepancy in slopes). For the first event they suggest a d13C composition of carbon that may have been methane at approx. -60 per mil, with the second event marked by a carbon source likely to have been organic carbon at approx. -25 per mil d13C. However, it would be good to test whether these respective d13C compositions, and thus the amount of carbon likely involved, make sense with the observed temperature changes. After all, the authors have deep-sea temperature changes in the shape of their benthic d18O data, and they can calculate the amount of carbon involved for each event by using the size of the d13C excursion and the assumed d13C composition of that carbon. From all this they could make a rough estimate of the climate sensitivity across each event and thus gauge whether their hypotheses of methane vs organic carbon are reasonable."

AC: We thank the reviewer for his comment, in agreement with the comment by reviewer #1, Prof. Lee Kump. As in our response to Prof. Kump, we follow the reviewers' advice and add some speculations and basic calculations.

We applied the "back of the envelope" approach, as suggested by Prof. Kump following model results by Panchuck et al. (2008) for the different sources. Based on the observed CIEs for our six events, we calculated the estimated amounts of carbon involved for each event (550 Gt C for a -60‰ source or 1700 Gt for -22‰ required for an observed 1‰ change in δ13C). We applied a conversion factor of $\sim$2.12 Gt (or Pg) of C per 1 ppm of CO2 (Hansen et al., 2013). As baseline conditions, we assume a pCO2 of 750 ppm, as this is considered within the range of estimates for late Paleocene pCO2

(Panchuck et al., 2008). We then compared the observed temperature changes with the results obtained for different scenarios (1.5°–6°C) of climate sensitivity. Assuming climate sensitivity to be invariant on short-term timescales, the -22‰ source would produce the same temperature response we observe for H2 and I2 based on benthic δ18O for a 3°C change per doubling of pCO2, which is the canonical value for fast-feedback sensitivity (IPCC, 2007). In the case of the other events (ETM2, I1, J and ETM3) a mass of C with a -22‰ source would overestimate the temperature response while a methane source would underestimate it.

However, this kind of reasoning might be misleading if solely based on our data. Our calculations derive from assumptions concerning the initial baseline conditions, which were probably higher than 750 ppm after the PETM, based on the warming baseline observed in the long-term δ18O records (Littler et al., 2014). Also, it is not clear whether benthic foraminiferal isotope data alone can actually be used to support this sort of discussion, as climate sensitivity necessarily depends on atmosphere/surface temperature. Constraining the specific contributions of each source and/or climate sensitivity would be very difficult to defend with the existing data. We have added to the manuscript a similar sort of discussion as proposed in our comments above.

Other specific comments:

RC: "p. 1801, line 1 – for the tuning process, what is the justification for aligning maximum a* values with maximum eccentricity? (e.g. why not maximum a* values with minimum eccentricity, or some other phase of the cycle?)" AC: We interpret colour reflectance (a*) as a proxy for carbonate dissolution. The hyperthermal events are marked by layers distinguished by high values in colour reflectance, magnetic susceptibility and iron content. These values are indicative of strong carbonate dissolution, with a* as a measure of redness in the sediment, which largely reflects the concentration of iron (oxides) bearing minerals (Westerhold et al., 2007). The a* maxima directly correlate with the position of δ13C minima which we link to eccentricity maxima.

RC: "p. 1801, line 26 – using this jumbled mix of nomenclatures for these events is becoming really confusing, and a bit of a mess. For several events, we have the situation where three different labels exist for each event – e.g. the one at 54.1 Ma is known as H1 or ETM2 or Elmo, and the one at 52.8 Ma is known as either X or K or ETM3. To avoid this confusion, and for consistency, Kirtland-Turner et al. (2014) labelled these events within the context of the GPTS to provide a consistent naming scheme for the multitude of events being discovered. I would suggest the authors should at least mention this scheme and use these event labels in addition to the array of older labels." AC: We agree that the nomenclature for these events has become increasingly confusing and that too many labels are given to the same event. However, it is necessary to report them all to avoid misunderstandings. We appreciate the attempt by Kirtland Turner and coauthors (2014) to provide a clearer scheme with a more straightforward approach to the labeling and therefore we will include it in the revised manuscript. It is likely, however, that even this scheme will require further revision. We propose adding a table reporting all the different labels.

RC: "p. 1803, line 20 – should 'specular' read 'speculative'? (I presume it shouldn't read 'spectacular'?)" AC: The word was intended to be actually "specular" as in a "mirror- like" image with the post-EECO hyperthermals reflecting the pre-EECO events as in a mirror, with decreasing frequencies and increasing sizes in time. However this word seems to be unusually used in English and we have removed from the text.

RC: "p. 1803, line 24 – I would reference the Kirtland Turner et al. (2014) paper at the end of the following sentence "showing that episodes of carbon release continued throughout the EECO and the onset of the cooling trend" because at the moment it's ambiguous as to who made that finding. AC: Thanks for the correction. The reference has been added.

RC: "p. 1803, line 25 – expand on what these mechanisms are, as this relates to the later discussion where the authors discuss methane and organic carbon as sources. AC: Thanks for your comment. This section has been expanded.

RC: “p. 1804, line 22 – ‘statically’ = ‘statistically’? AC: Changed

RC: “p. 1805, line 15 – “Evidently, the a* values, representative of redness and hence carbonate dissolution”. This is an assumption. The potential controls on % CaCO3 are dissolution, but also dilution and CaCO3 productivity. How can the authors rule out at least a partial contribution from dilution by clays or a reduction in top-down CaCO3 delivery from biological productivity? AC: This is partly an assumption. However, in the case of ETM2 and H2 the amount of carbonate dissolution has been closely constrained for the Walvis Ridge sites (Stap et al., 2009). The total CaCO3 content was determined to be 96% when all material is preserved and 93% on average for the studied interval, with Site 1263 showing the highest sedimentation rates. This implies that the terrigenous content averages 4% to 7% of the total sediment, while the estimated terrigenous flux is about 0.12 cm/kyr and the average sedimentation rate 1.76 cm/kyr. We therefore exclude any significant dilution. Similarly, biological productivity may have varied, but we do not observe any significant evidence of a top-down reduction.

References:

Gibbs, S. J., Bown, P. R., Murphy, B. H., Sluijs, A., Edgar, K. M., Pälike, H., Bolton, C. T. and Zachos, J. C.: Scaled biotic disruption during early Eocene global warming events, Biogeosciences, 9(11), 4679–4688, doi:10.5194/bg-9-4679-2012, 2012.

Hansen, J, Sato, M, Russell, G, Kharecha, P.: Climate sensitivity, sea level and atmospheric carbon dioxide. Phil Trans R Soc A 371: 20120294. http://dx.doi.org/10.1098/rsta.2012.0294, 2013.

Holbourn, A., Kuhnt,W., Schulz, M., Flores, J-A. & Andersen, N. Orbitally-paced climate evolution during the middleMiocene “Monterey” carbon isotope excursion. Earth Planet. Sci. Lett. 261, 534–550 2007.

Littler, K., Röhl, U., Westerhold, T. and Zachos, J. C.: A high-resolution benthic stable-isotope record for the South Atlantic: Implications for orbital-scale changes in Late

Paleocene-Early Eocene climate and carbon cycling, Earth Planet. Sci. Lett., 401, 18–30, doi:10.1016/j.epsl.2014.05.054, 2014.

Pälike, H., Norris, R.D., Herrle, J.O.,Wilson, P.A., Coxall, H.K., Lear, C.H., Shackleton, N.J., Tripati, A.K.,Wade, B.S.: The heart- beat of the Oligocene climate system, Science 314, 1894–1998, 2006.

Panchuk, K., Ridgwell, A. & Kump, L. R.: Sedimentary response to Paleocene–Eocene Thermal Maximum carbon release: A model-data comparison. Geology 36, 315–318, 2008.

Westerhold, T., Röhl, U., Laskar, J., Raffi, I., Bowles, J., Lourens, L. J. and Zachos, J. C.: On the duration of magnetochrons C24r and C25n and the timing of early Eocene global warming events: Implications from the Ocean Drilling Program Leg 208 Walvis Ridge depth transect, Paleoceanography, 22(2), doi:10.1029/2006PA001322, 2007.

Interactive comment on Clim. Past Discuss., 11, 1795, 2015.