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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.

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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 senstiviity across each event and thus gauge whether their hypotheses of methane vs organic carbon are reasonable.

I have some other specific comments:

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?)

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 evenbt – 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.

p. 1803, line 20 – should ‘specular’ read ‘speculative’? (I presume it shouldn’t read ‘spectacular’?)

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. 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.

p. 1804, line 22 – ‘statically’ = ‘statistically’?

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?

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