Climate
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Discussions

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Interactive comment on "A seasonality trigger for carbon injection at the Paleocene-Eocene thermal maximum" *by* J. S. Eldrett et al.

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Thankyou for the review and good suggestions for improving the manuscript. I believe we can incorporate many of the suggestions and address the remainder with more transparent discussion of the data.

A response to specific points:

1) Position of the CIE. We believe the stratigraphic position of the PETM is well constrained, however if the evidence and our reasoning is not transparent and presented clearly we shall endeavour to do this in the next version. The identification of the PETM is based on biostratigraphy and isotope stratigraphy. The marker species Apectodinium augustum is diagnostic of the PETM. To make this even clearer, besides reworked

specimens, this species has never been recorded in strata other than the PETM, and this is particularly well-established in the North Sea, and calibrated with carbon isotope stratigraphy on multiple sedimentary phases (e.g., Bujak & Brinkhuis, 1998; Schmitz et al. 2004; Sluijs et al., 2007; Egger et al. 2009; Sluijs and Brinkhuis, 2009; Kender et al. 2012). The occurrence and abundance of A. augustum and the associated globally recorded Apectodinium acme in core 22/11-N1 constrains the stratigraphic position to the PETM. In addition, the position of the uppermost Paleocene is constrained by the last occurrence of Areoligera gippingensis at the top of the Lista Fm, which is a regional late Paleocene marker and rules out the possibility of the A. augustum peaks down section corresponding with older Paleocene Apectodinium spp. peaks, consistent with numerous regional stratigraphic reports (e.g., summarized in Payne et al. 2005). From a biostratigraphic point of view, assigning the studied record to the PETM is unequivocal.

a) The shape of the CIE may be anomalous compared to deep marine records but is common for shelf sections. For example, Cui et al. (2011) interpret their smeared-out onset of the CIE in a section on Spitsbergen as a 20,000 year injection time of 13C-depleted carbon at the onset of the PETM. This was later questioned and argued to reflect the mixing of Paleocene and Eocene organic carbon (Sluijs et al. 2012). With the current dataset we cannot confidently argue whether the slow decrease in d13C represents extreme sedimentation rates or a mixing line of Paleocene and Eocene organic matter. If the CIE initiation is 10-20Kyrs this would indeed mean a very high sedimentation rate (2.5m-5m/kyr), something which is fully supported in similar depositional environments today, such as i) Brazos-Trinity system with sedimentation rates as high as 60m/kyr (Primez et al., 2012) and ii) Amazon fan, with sedimentation rates up to 10m/kyr (see Piper & Deptuck, 1997). Even with de-watering and compaction/lithification these rates are comparable. Sedimentation rates below the CIE interval are also high, with the deposition of the Andrew fan system, however this older interval is not cored. Even if the drop in d13C represents a mixing line between Paleocene and Eocene organic matter, the shear vertical scale of the excursion indicates a

very expanded section.

b) Isotopic signal and source of organic matter. We acknowledge in the text that the shape and magnitude of the isotope signature is likely impacted by the changing contributions of various types of organic matter (lines 83-85). One of the authors of our paper published a quite extensive paper on this (Sluijs and Dickens, 2012) in which such factors are quantified. This work shows that, in theory, it is possible to generate a negative CIE of about 4‰ during the early Paleogene if there is a switch from 100% terrestrial to 100% marine organic matter. Although the relative amount of marine kerogen increased during the PETM, it might account for maximally 1 ‰ of the CIE, which, in fact, brings the magnitude of the CIE as recorded close to what is generally considered to be the CIE in the global exogenic carbon pool. The material in the Central North Sea basin is too mature for confident biomarker analyses, including our samples. The CIE has, however been recorded in maceral in the Central North Sea basin (Kender et al. 2012) and specific biomarkers in a marginal marine North Sea record in Denmark (Schoon et al. 2013), and at all these sites the CIE occurs concomitant with the occurrence of Apectodinium augustum. In other words, the combined information of our TOC carbon isotope record and regional and global biostratigraphic correlations, is diagnostic of the CIE across the PETM. For various reasons, but not really for stratigraphic purposes as we explain above, it would have been nice to expand the d13C record but unfortunately the data we present here is from the cored interval and even though the well penetrates deeper, only cuttings material is available which we consider unreliable for such analyses. We will expand the text on this topic.

2) Statistical analyses. We shall include significance t-test results in the next version as suggested. A t-test is generally not appropriate for testing time series (e.g., Nicholich and Weinstein, 1981), such as testing for changes in temperature or other climate variables between 2 intervals of time. However, a t-test is possible if you do not treat the data as a time series but by comparing two populations of data. So compare pre-CIE to CIE for example, which you can do within the hour in software such as excel.

You'll need a more mathematically complex test to identify the exact level of a transition but that is not required here.

3) MAT, MAP no change across PETM. We agree that a minimal change in MAT across the CIE is surprising, although the method did yield shifts at the right places in the record, just that these were well inside the possible range of temperature for each sample, i.e. the signal to noise ratio was too high. Annual precipitation appears insensitive for our method at high precipitation (i.e., MAP ∼120cm/yr) as plants appear to show threshold responses to MAP (i.e., a series of steps reflecting major biome shifts) rather than a linear response in this type of analysis (e.g., Eldrett et al., 2009; Pross et al., 2012). Recent papers have also highlighted concerns over some sources of climate range data for nearest living relatives used in NLR analysis, recommending use of regional datasets such as the recently published data for Chinese plants (Fang et al., 2011) and the pre-existing North American data set we had already employed (Grimm and Denk, 2012; Thompson et al., 2012). Grimm and Denk (2012) further recommended that NLR-type analyses employ methods such as those used by Eldrett et al. (2009) that analytically remove outliers, i.e. the method used in our present analysis. Thompson et al. (2012) tested the NLR method for modern North American vegetation sites, using a number of analytical approaches grouped under the term Mutual Climate Range technique, and recommended using 11 or more NLRs. Accordingly, we have removed a small number of samples that did not meet this criterion, although this change did not change our results.

As we now have access to the Chinese modern plant climate range data (e.g., taxa in our samples such as Englehardia and Platycarya) we have undertaken a re-analysis of our spore-pollen sum record using those data, but also employing the Mutual Climate Ranch technique of Thompson et al. (2012). This alternative analytical NLR approach yields a clear warming at the CIE as well as showing the seasonal shifts we have already presented, and will be presented in the revised paper as well as a revision of our original bioclimatic method analysis employing the Chinese data. A recent paper on the

Wilkes Land Eocene Antarctic record that employed NLR analysis of terrestrial spore-pollen sums from marine sediment cores (Pross et al., 2012) similarly presented both bioclimatic analysis (e.g., Eldrett et al., 2009 and this study) and MCR-type analysis of those data.

4) We will make the changes as suggested by the reviewer on the manuscript pdf.

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