

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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This is very interesting manuscript about the CIE, a dramatic event where temperatures worldwide increased by ~3-5 Celsius in less than 10,000 years. This event has received a large amount of attention as it could represent an excellent analogue for modern climate warming. Eldrett et al present the isotopic and palynological results of a core drilled in the North Sea that contain the CIE. They analyzed the isotopic d13C record of the core to identify the stratigraphic position of the CIE and then did an excellent palynological study of the pollen/spores/algae and dinoflagellates present in the core. Based on the palynological content, they infer climatic conditions (temperature, precipitation) and type of forests, across the CIE. However, I think the manuscript needs to improve certain aspects before it can be published, including: 1. CIE stratigraphic

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position. The manuscript makes a number of major assumptions about the position of the CIE. That most of the body and recovery CIE is missing, and that instead there is an expanded (~50 meters) initiation CIE interval. Many recent papers have shown that the initiation phase was very fast, around 10-20Kys. Then, it would mean that the core has 50 meters that accumulated in 10 Ky. That would imply a very fast sedimentation rate that is very hard to accept. Can you support such a fast sedimentation rate? How was the sedimentation rate in the strata below? Otherwise, several alternative explanations could be proposed, eg. that the CIE is restricted to the 2225-2217 strong negative excursion and there is an unconformity bounding the CIE above and below (as it is shown in supplementary figure S1). Apectodinium peaks from down section could correspond to Paleocene Apectodinium peaks. Additionally, how the d13C values correlates with the type of organic matter? It has been shown that gymnosperms vs angiosperms vs pediastrum vs dinoflagellates produce different d13C values. It would be useful to present a correlation of dC13 versus 1) amount of angiosperm pollen, 2) amount of gymnosperm pollen 3) algae+dinoflagellates. If there is a correlation, that influence needs to be removed from the d13C values (e.g. using the residuals of the correlation rather than the actual values, see Wing papers for instance). Could the increase in *Carya* pollen, for example, produce the isotopic shift seen in the d13C values? I think that biomarker d13C MUST to be presented together with bulk d13C. How else could it be shown that the d13C signal is not due to a change in the type of organic matter? A key element of this manuscript is the stratigraphic position of the CIE, and at least in the data that is presented, that is not clear at all where it is. It would be useful if a longer d13C sequence is presented, e.g. many more meters above the 2205 level, and below the 2280 level. That could show more clearly where the CIE could be.

2. Lack of statistical analysis. Almost all interpretations along the manuscript (see PDF with my comments for which ones) require a statistical test to support the significance of the differences. e.g. a t-test showing if the abundance differences of a given plant group within the CIE versus the intervals above or below are significant.

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3. MAT and MAP. Neither MAP or MAT seemed to change across the CIE. That is an unusual result that requires a much longer explanation, as the CIE has been characterized by strong changes in those two parameters elsewhere.

4. There are also specific comments as notes in the PDF manuscript.

This manuscript has the potential to become an excellent paper, but still requires additional work. Hope to see it published soon.

Carlos Jaramillo

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/9/C2609/2013/cpd-9-C2609-2013-supplement.pdf>

Interactive comment on Clim. Past Discuss., 9, 5837, 2013.