

Interactive comment on “Extreme warming, photic zone euxinia and sea level rise during the Paleocene/Eocene Thermal Maximum on the Gulf of Mexico Coastal Plain; connecting marginal marine biotic signals, nutrient cycling and ocean deoxygenation” by A. Sluijs et al.

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Thank you for the invitation to review this paper. This is a potentially important contribution to the PETM from some relatively underexploited localities in the southern US coastal plain. The authors use a number of geochemical proxies, micropaleontology and basic sedimentology to identify the carbon isotope excursion and temperature changes associated with the PETM from one locality in the southern coastal plain. As

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the title indicates, the authors argue that these sediments contain evidence of a sea level rise and bottom water anoxia. I may not concur with the authors on many of the interpretations or details, but I support the eventual publication of this paper if my concerns below can be satisfactorily addressed.

Overall, the work is a little discussion rich and data poor, in my opinion. There are a lot of interpretive aspects that we can agree or disagree over, but some of them are an overextension of the available data. Assertions, such as bottom water anoxia, are much more effectively tested by other means than those used here, and sea level rise is presented as if it is a foregone conclusion. Part of this is the result of a rather methodical attempt by the authors to paint a coherent global picture of the PETM, which is commendable. However, forcing observations to fit pre-existing interpretations by default means selective data use, a trap fallen into here (and in the past, e.g., Sluijs et al., 2007). The end-result is a literature that is a bit like a house of cards; for example, at least a third of the global sections interpreted to show a sea-level rise come from Sluijs et al. (2006, 2008, 2011). A review of those papers leaves me with the impression that the case for a sea level rise is not so compelling, indeed (see comments, below).

My other major comment is that for a “first identification” (6463:14) paper, it lacks a clear and careful discussion of the stratigraphic context of these results. I think that this needs to be emphasized with a figure that aligns the cored section with the overall bio- and chronostratigraphy of the southern US coastal plain. The authors present data that is potentially very important, but I had to do far too much digging through the literature to put this section into its depositional context and assess the author’s assignment of biozones, etc. In fact, the formation/member assignments and descriptions in the text are not completely consistent with what is presented in Figure 2 (e.g., Bashi Marl is a mb. or a fm.? If a Member, of which Formation, Hatchetigbee?). An additional figure with all the key observations, index taxa, etc. in relation to the observed CIE will make these arrangements more transparent (and likely make this contribution much more read/cited). There is a lot of discussion of regional biozones P5 and P9 including key

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stratigraphic identifications and relevant index fossils, but the correlation of these to the section represented by Fig. 2 from the Harrell core is an irritating omission. I cannot relate these important zones to any of the changes represented in Fig. 2 in a useful manner, or even make my own assessment of the duration of the interpreted hiatus at the base of the CIE.

As it stands, I would not feel confident citing this paper as good evidence for the CIE in the southern coastal plain without significant further work on my own part to check the stratigraphic arrangement. However, the authors can alleviate my concerns fairly readily by taking a stab at a graphical interpretation of the core-to-regional-to-global correlation. Stratigraphy and establishing a sequence based on superposition really are crucial, particularly when attempting to place geologically instantaneous events into an ultimately global context.

Carbon isotope excursion:

With all the noise in the organic carbon ^{13}C data, it is important for the isotope excursion to be documented in carbonates as well as organics (6466:15-25) in the case of a “first description” as this paper is being billed. It may be unwise to trust the organic carbon reservoir as representative of the magnitude, duration or stratigraphic position of the PETM isotope excursion. Bulk organics give inconsistent results at the PETM and are subject to all types of problems (assemblage, degree of photosynthetic fractionation, differential oxidation, etc.). The authors acknowledge this issue, and attempt to address it via stable isotopes on sulfur bound organic molecules. However, the presented record is pretty spotty. It is especially troublesome given the large supposed floral/ecological changes that happen in this section. Organic matter on the shelves can be several thousands of years older than its depositional age (e.g., Mollenhauer et al., 2005), and organic carbon is locked in mineral phase in other PETM sections (e.g., Schneider and Bowen, 2013). There is ample literature on carbonates from these

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sections – did I miss discussion of these? All I see are references to “mollusk geochemistry”, but there are both nannofossils and forams present through the interval as well. Even bulk carbonate would be better as a first cut – the only confusion is whether the signal is dominated by planktonic or benthic organisms (nannos and pf/bf), if diagenesis can be ruled out. It is much harder to bias the DIC reservoir from which carbonates grow because it is massive and relatively homogeneous. In contrast, there are a variety of sources for organic carbon on a shelf, and the reservoir is relatively small even when the catchment is considered. On the ^{13}C of organic carbon data alone I would not yet be convinced this was even the PETM CIE.

Overall, the CIE is very noisy, but if I fit a trend line between the two unconformities, it appears to continue to decrease, suggesting that the ^{13}C change had not reached its nadir. Interpreted another way, this observation is consistent with a highly expanded section where the observed excursion represents only the very beginning of the event. The $\sim 4\%$ positive excursion in the middle of the event is particularly troubling, and a good example of the sort of noise inherent to the organic carbon reservoir in an attempt to characterize a perturbation.

Also, where is the percent organic carbon record for this section? To argue that the bottom waters went euxinic and accumulated organic matter means we should be seeing an increase in the amount of organic carbon burial, but this record is not shown. I understand there is a lithologic change, but this data accompanies the measurement of bulk ^{13}C on organics, so should be a simple addition. Note that the literature is not terribly consistent in this regard either, but I am willing to accept at least pore-water disoxia based on the preservation of mangentic nano particles (Wang et al., 2012).

Interpretations of a sea level rise:

Regarding the interpretation of a sea level rise in a relatively ice-free world, what is the water source for such a eustatic increase? Some estimates have this rise at 75m – an

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ice volume equivalent to nearly all of modern east Antarctica. Where is all that water coming from?

I urge the authors to address this quantitatively if indeed a sea level rise is present. This speaks to the fundamental plausibility of a eustatic response. If it cannot be satisfactorily addressed, the authors are not justified in this interpretation. It is perhaps the single point that demands the most attention of the entire SL argument. Relative/local interpretations are fair, but global change on this timescale implies ice.

The authors seem to base their interpretation in part on other PETM sections. The best data suggesting a sea level rise in other sections is largely from examinations of benthic biofacies, in addition to some circumstantial sedimentologic evidence that really could go either way (e.g., Harris et al., 2010; Stassen et al., 2012a; Stassen et al., 2012b). However, benthic biofacies are of debatable relevance to shelf sites during the PETM, specifically because they are a model of ecological zonation, etc. which is derived from modern hydrography, temperature, and nutrient/food distributions at various depths/distances along the shelf. It is essentially a steady state model that has been repeatedly applied to a perturbation condition without any serious consideration for the hydrologic and nutrient supply changes that are likely to result from a major carbon cycle perturbation (e.g., Stassen et al 2012a, 2012b). This is especially egregious in light of data indicating an accelerated hydrologic cycle likely accompanied the event (Kopp et al., 2009), in which case none of the environmental parameters comprising biofacies zonation are expected to remain static.

It is these kinds of steady-state interpretive rules being applied to non-steady state perturbation conditions that result in major discrepancies between scenarios that attempt to account for the PETM. Perhaps these large changes in sea level are real, but my brief perusal of the literature suggests that this interpretation is more likely the result of no one wanting to upset the apple cart; the majority of the data can be equally interpreted in another context.

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Thus, with that piece of data effectively compromised (a steady state ecological model erroneously applied to a perturbation condition), I could argue that the observations made here are actually just as consistent with a highly expanded section that captures the onset in very high resolution, and where the majority of the CIE is truncated by the upper bounding unconformity. I invite the authors to address this.

There is very little data presented here (or in the literature on these sections that I am aware of) that is inconsistent with the uppermost Tusahoma Fm. having been deposited extremely rapidly. Rapid deposition has been interpreted elsewhere (Wright and Schaller, 2013), though it is contested and I readily admit to being bias in this regard. An interpretation of rapid sedimentation is consistent with the decrease in terrestrial palynoflora observed here simply via dilution and the nature sediments themselves. The bloom in Apectodinium, a highly freshwater tolerant taxa that enjoys eutrophic waters, seems to indicate an accelerated hydrologic cycle which would deliver plentiful nutrients and accompanying sediments to the shelf. These observations have made me extremely skeptical of the interpretation of slow, outer neritic deposition of many shelf sequences through the PETM. Many of these also have greater thicknesses through the isotope perturbation than are allowed by the time-averaged sedimentation rate of the macro-sections they are derived from (taking deep sea chronologies at face value), and are also demonstrably truncated with respect to the full history of the PETM. The authors even assemble data from the literature showing a rather global distribution of increased shelf sediment supply through the time interval.

I think this is the result of a predisposition to interpreting PETM sections as condensed, which then leads to the interpretation of a very large rise in sea level that nonetheless has very dubious origins (it is puzzling where the water for a ca. 75-100m eustatic rise in a nearly ice free world should be sourced?). I am happy with the interpretation of a local sea level change – regional/continental tectonics at this time were complicated.

Some other comments: 6468;1-11: this discussion needs a figure. I should not have to dig through the literature to assess whether these correlations are reasonable or

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even significant. This is crucial because their interpretation of the PETM onset could be entirely dependant on the duration of this hiatus, but this relationship is ambiguous at best from Figure. 2. I am willing to give the benefit of the doubt in this case, but just how much section is missing from an already very thin Earliest Eocene column?

6467:24-26: not sure if I understand how an increase in magnetic susceptibility implies an unconformity. Maybe I missed discussion of this?

6468:29: transported glauconite would not be consistent with slow sedimentation. . .

6469:15-18: What is the “mollusk geochemistry” being referred to here. . . ^{18}O ? Trace metals?

6470:25-6471:12: it is not clear to me why shelf localities ought to show temperature changes that are comparable to what is observed in the deep ocean. I would expect to see much larger temperature changes on the shelves.

6475:2-5: I'm not sure if I follow this. . . If the glauconite is brought in, how would this be consistent with a condensation of the interval? This seems like a bit of an over-interpretation. . . If we have learned anything from siliciclastic shelves, it is that there is almost always some glauconite.

6475:9-12: This interpretation is only consistent with a sea level rise if the section is first considered to be condensed, pretty circular reasoning. If the section were in fact highly expanded, the anomalous accumulation of apectodinium is easily explained by increased nutrient supply and lack of predation by a huge influx of freshwater. The low terrestrial pollen counts are exactly what are expected from a highly expanded section simply by dilution, and the thinness is easily explained by the truncation (none of the PETM recovery is observed).

6477:1-5: This is not consistent with the data just presented. If isorenieratane is at sub-detection limits below the PETM onset and measurable above it, one cannot state that photic zone euxinia developed; one can simply say that it was present during the

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PETM. There is no information about the pre-existing condition. This is the sort of “cart-before the horse” interpretations that are only marginally supported by the data presented.

6478:27: All the while we have the interpretation of “condensation” of shelf sections through the PETM?? This seems seriously inconsistent with an increase in terrestrial runoff.

6478: Taken at face value, I do find Sluijs et al’s results fairly consistent with a stagnation of ocean circulation, but it is important to note that this is unlikely to affect the shelves nearly as profoundly as it might the deep ocean. Also, there is little evidence for disoxia at 690.

6479-6480: I understand the temptation here, but this is overall a very weak argument full of speculation and conjecture. There is very little (if any) evidence for increased C burial on the shelves at this time, though there is substantial evidence for an increase in productivity. The authors do not show a % organic carbon record in support of these assertions either.

I would be happy to review a revised version of this paper.

Morgan Schaller

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