**Climate
of the Past**

Discussions

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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Dear Dr. Schaller,

Thank you for your thorough review. We will reply to your comments below, point by point.

Sincerely,

C3505

Also on behalf of all authors,

Appy Sluijs

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 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).

Reply: We address these 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

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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?).

Reply: Various authors have not used these names consistently in the past. We choose to use Tuscahoma Fm, and the Bashi Mb of the Hatchetigbee Fm and will do so consistently throughout the manuscript.

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 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.

Reply: We appreciate the reviewer's comment that stratigraphic context is important. The studied section lacks biogenic carbonate apart from a marine tongue within the Bashi member (6464-l19). This is why we simply cannot perform a primary correlation to calcareous microfossil zones in Figure 2 apart from indicating that the Bashi member is NP10. The data supporting this inference is included in the nannofossil data table

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in the supplement. Several reviewers have requested more information on the regional stratigraphy. We therefore will optimize and expand on the text on this and include a figure.

Carbon isotope excursion: With all the noise in the organic carbon 13C 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 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 13C of organic carbon data alone I would not yet be convinced this was even the PETM CIE.

Reply: The studied section lacks calcareous fossils apart from a marine incursion in the Bashi member (6464-l19). We attempted to generate carbonate isotope data but our

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analyses confirmed the absence of carbonate in the section (we will include this in the text). Because the TOC record was noisy (potential causes were discussed in section 4.1), we generated compound-specific isotope data to confirm the negative excursion. The compounds analyzed are sulfur-bound molecules, which cannot be derived from land since terrestrial biomarkers are not bound to S as sulfurization requires fresh functionalized molecules (e.g., Sinninghe Damsté et al., 1988). Such biomarkers have provided unambiguous evidence of CIEs in other sediment cores, without an apparent offset in time (e.g., Schoon et al., 2011). The reference of Mollenhauer is misleading in this sense as it reports the 14C contents of alkenones, which are laterally transported in some cases. There are numerous published records of the PETM where the CIE has been identified without carbonate isotope data (summarized in the review paper of McInerney and Wing, 2011). The combined information from dinocysts, pollen and the constraints based on nannofossil and planktonic foraminifer biostratigraphy (sections 2 and 4.1) does, in our view, unambiguously confirm the presence of the PETM.

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 13C 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 _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.

Reply: Because of the difficulties the reviewer mentions associated with bulk organic carbon isotope records, particularly in shallow shelf sections such as this one, we do not feel comfortable attempting to argue for the extremely expanded record he suggests. In the revised version, however, we will include light microscope photographs of thin sections that show that many of the glauconite grains are angular (Fig. R1), implying that they were formed in situ. It is estimated that glauconite requires a residence time at the sediment-water interface of 1000 to 10,000 years to develop (Prothero and

Schwab, 2004). This implies that sedimentation rates in this interval were very low rather than extremely high. On a side note, glauconite does not form in the absence of oxygen, further supporting the intermittent presence of oxygen on the sea floor. The nature of the positive carbon isotope values remains somewhat puzzling. There could be a diagenetic component; TOC values are very low (we will present the data in the revised version; see Figure R2) suggesting that oxidation may have caused the selective preservation of 13C-rich organic carbon in this comparatively coarse-grained layer. The palynological residue is somewhat anomalous relative to surrounding intervals; concentrations of palynomorphs are low and abundant mica is present. As indicated on page 6467 (l21-23), this could be a storm deposit; it could also mean that sediments were heavily winnowed, leading to a more coarse character (this is what was meant as an interval of non-deposition; we will make this point more explicit). The palynology does not show a large component of reworked specimens but it is possible that a large component of the organic matter is allochtonous. We will expand the discussion of this aspect of the data with this information but we cannot fully explain the positive peak in the δ13C-TOC record within the CIE.

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 13C 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 dysoxia based on the preservation of mangentic nano particles (Wang et al., 2012).

Reply: We do not pose the primary argument that bottom waters were euxinic. We show that the photic zone was at least intermittently euxinic and deduce that this most likely means that bottom waters were intermittently anoxic too. This is based on modern patterns of euxinic water columns (Yao and Millero, 1995); only in rare cases does photic zone euxinia develop with an oxic sea floor. We also do not argue that the sea

floor accumulated large amounts of organic carbon during the PETM at the study site. In fact, low TOC weight % (Fig. R2) and low sedimentation rates based on the observation that glauconite grains were produced in situ (both included in the revised version) suggests the opposite. This supports the hypothesis as postulated by Ellen Thomas in her review to this paper, that euxinic conditions were a seasonal phenomenon and that the low supply of siliciclastic material allowed for the oxidation of organic matter on the sea floor despite seasonal anoxia. This discussion will be included in the revised version.

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 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.

Reply: We do not state that sea level rose by 75 meters. Sluijs et al. (2008a) included a discussion on a potential maximum magnitude of sea level rise during the PETM of perhaps 30 meters if several independent mechanisms were combined, including tectonism, steric effects and a small ice sheet. In all of our sea level work including the present paper, we use proxies for proximity to the coast as well as sequence stratigraphy. These cannot provide accurate quantitative estimates of the magnitude of sea level changes. The depth gradient from coast to shelf-slope break was likely much less in the Eocene than at present due to the long-term absence of large variations in continental ice volume (Sømme et al., 2009). Therefore, the response in sedimentary data as compiled in figure 4a can likely be explained by a relatively small (perhaps 5-10m?) rise in global sea level. We will include a section in the revised manuscript to explain

these aspects.

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.

Reply: Indeed, one has to be careful with using benthic foraminifera for detailed sea level reconstructions. The papers cited by the reviewer are all from New Jersey. All published papers from this region that discuss sea level conclude that it rose during the PETM, based on work performed by three different groups using various independent proxies, including plankton rather than benthos (Cramer et al., 1999; John et al., 2008; Sluijs et al., 2008a; Harris et al., 2010; Stassen et al., 2012). These papers, in fact, discuss the issue brought up by the reviewer and still conclude that sea level rose (e.g., Stassen et al., 2012). We feel it is beyond the scope of this paper to discuss in detail all sea level proxy records for all sites included in our compilation but we refer to the individual papers that have described and discussed the evidence. Notably, sea level reconstructions at most sites in the compilation were determined independently of benthic biofacies (e.g., see, Sluijs et al., 2008a for 5 examples). In the revised version of the paper, we will indicate in the compilation that the data interpretation follows that

of the cited paper.

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. 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 Tuscahoma 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.

Reply: For clarity, we restate that our sea-level interpretations are not solely based upon benthic foram ecology, but on the trends in our proxies for proximity to the coastline in our studied section and a combination of various techniques from the published literature. As mentioned above, we have studied the glauconite in more detail and found that they were formed in situ. This compromises the hypothesis that the section was rapidly deposited. Moreover, an increase in the discharge of terrigenous sedimentary components relative to marine components should lead to an increase in the relative abundance of terrestrial components. The data in figure 2 instead suggest a decrease in the relative contribution of terrigenous organic matter.

The bloom in Apectodinium, a highly freshwater tolerant taxa that enjoys eutrophic

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waters, seems to indicate an accepelerated hydrologic cycle which would deliver plentiful nutrients and accompanying sediments to the shelf.

Reply: The palynological record contradicts this interpretation. We indeed showed that Apectodinium was tolerant to relatively low salinities. However, other taxa, notably those within the Senegalinium cpx were tolerant to even lower salinities (Brinkhuis et al., 2006; Sluijs and Brinkhuis, 2009; Sluijs et al., 2009; Harding et al., 2011), and these dominate assemblages prior to the PETM. This percentage decreases during the PETM (as described in bottom of P6474 to the top of P6475).

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.

Reply: We fully agree that the PETM is stratigraphically expanded relative to 'background' conditions in many marginal marine sediment sequences. Several of the authors of the present manuscript have argued for this very point in several papers (e.g., Sluijs et al., 2006; John et al., 2008; Sluijs et al., 2008b; Slotnick et al., 2012). Clearly, this is not the case for the PETM section described here.

Some other comments: 6468;1-11: this discussion needs a figure. I should not have

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to dig through the literature to assess whether these correlations are reasonable or 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?

Reply: we will include a regional stratigraphic scheme as a separate figure to substantiate correlations.

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

Reply: This was poorly explained in the first version. The change in magnetic susceptibility marks a significant lithological change interpreted to reflect an unconformity. This will be included in the revised manuscript.

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

Reply: As indicated above, we will present evidence in the revised version that shows the glauconite to have formed in situ.

6469:15-18: What is the "mollusk geochemistry" being referred to here: : : 18O? Trace metals?

Reply: δ18O. Revised sentence states "based on the oxygen isotopic composition of bivalve shells from the Bashi..."

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.

Reply: This is an interesting hypothesis but it is not necessarily supported by climate model simulations (Dunkley Jones et al., 2013) or data (Figure 4). It also shows the usefulness of Figure 4 if it triggers such questions.

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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 overinterpretation: : : If we have learned anything from siliciclastic shelves, it is that there is almost always some glauconite.

Reply: As indicated above, we have now studied the glauconite in thin sections. These analyses have shown that the glauconite grains are angular, implying that they were formed in situ. This will be presented as evidence to support low sedimentation rates.

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).

Reply: see above comment regarding the influx of marine components during the PETM.

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 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.

Reply: 'below detection limit' implies likely absent. Clearly, absence of evidence is not the same as evidence of absence, but given that organic matter preservation is better below than within the PETM (TOC content is higher in the Paleocene than in the PETM; Fig. R2), it was likely not produced at the site during the late Paleocene. We will include this sentence in the revision.

C3516

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.

Reply: this section regards our compilation of data presented in Figure 4. In contrast to the Harrell Core, we never argue for condensation of shelf sections through the PETM in other regions. The compilation shows expansion elsewhere, as the reviewer argues. In other words, we agree on this interpretation and we do not see how what is written here is inconsistent with this view.

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.

Reply: Based on the specific comments by Thomas, we will revise the text and data compilation regarding this issue, particularly regarding Site 690 (Table and Figure 4).

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.

Reply: The evidence for increased C burial on the shelves is described by Sluijs et al., (2008b) and John et al. (2008), as cited in P6480, l8 (only John et al. were cited in the original version; we will include the Sluijs et al. citation). More circumstantial evidence comes from Speijer and Wagner, (2002), and Gavrilov et al. (2003) and other papers cited in John et al. and Sluijs et al. We will include such citations to substantiate our point. The remainder of the section is indeed speculative and it is presented as a testable hypothesis.

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Interactive comment on Clim. Past Discuss., 9, 6459, 2013.

Fig. 1.
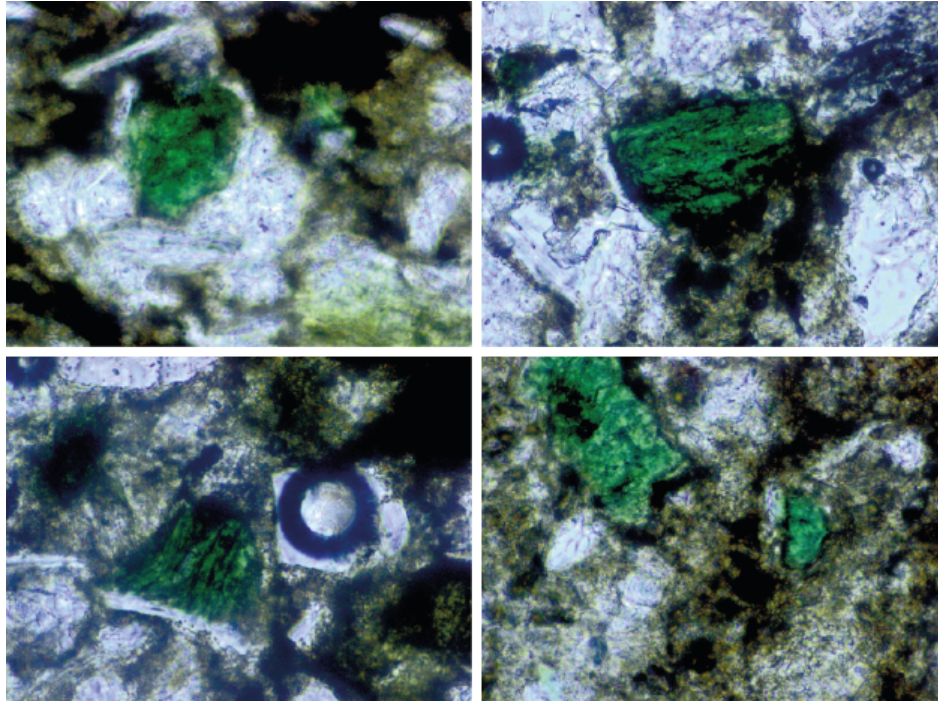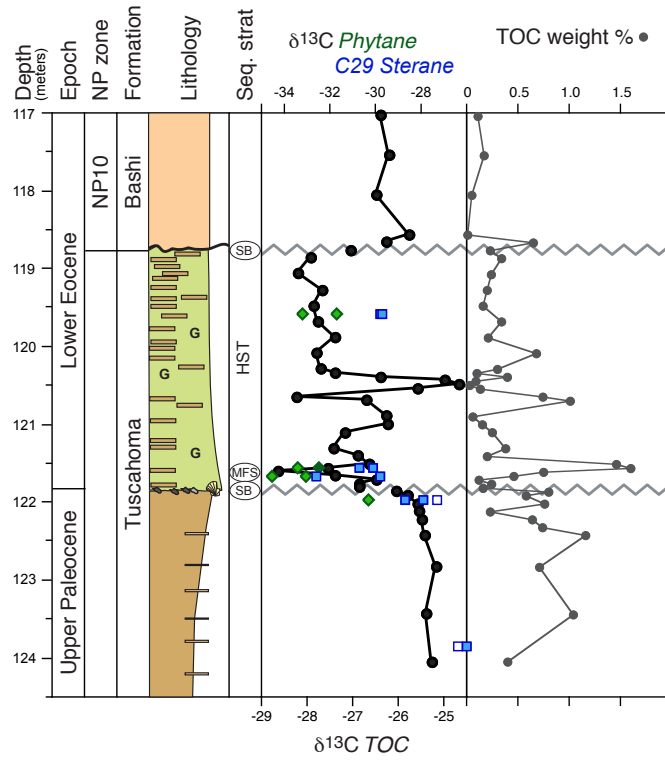
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Fig. 2.

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