

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. Riboulleau, Thank you very much for your extensive review of our manuscript. We address all of your comments below and will incorporate several changes related to these comments in the revised version. Sincerely, Also on behalf of all co-authors, Appy Sluijs

This article presents a multidisciplinary study of a core section from the US Gulf Coastal

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Plains covering the Late Paleocene-Early Eocene interval. Through the use of magnetic susceptibility, isotopic analysis, organic geochemistry and palynology, the authors report the first identification of a sediment section recording part of the Paleocene-Eocene thermal maximum (PETM). A large part of this article is devoted to the different implications of this first report: the stratigraphic revision of the age of occurrence of the primate *Teilhardina* and the worldwide significance of developed shelfal anoxia during the PETM. These topics are of large interest and suited for publication in *Climate of the Past*. However, in the present state, there is disequilibrium between the different sections of the article: if the stratigraphic part appears relatively convincing, the paleoenvironmental/oxygenation part is more subject to critics. The last part of the paper discussing the implications of coastal anoxia, is an interesting piece of discussion based on bibliographic compilation, however it suffers from the abundant use of “should”, “would” or “may”, and from abundant self-citation suggesting that the authors neglected an important part of available data.

Reply: We thank the reviewer for her positive evaluation of the relevance of our work as well as the stratigraphy. We address the specific comments regarding the paleoenvironmental work below. Most notably, we will include and discuss %TOC in the context of organic matter supply to the study site and expand on the discussion regarding our oxygenation data. The final section is indeed speculative and we include it to pose a testable hypothesis regarding nutrient cycling during the PETM. We will attempt to make it more readable and critically reevaluate the references.

General comments While the paper describes the discovery of a sediment sequence that covers the PETM, the lithological description is very limited. It is important to determine if the so far absence of PETM sediment in the GCP results from an erosion, (in which case, why is it not eroded here?), or simply because of insufficient study. In particular, the glauconitic silts that are here described as corresponding to the PETM are apparently described in other sections (Red Hot Truck Stop for instance, Beard, 2008).

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Reply: It is probably both. The glauconite-rich section is not found everywhere, perhaps because it was eroded by post-PETM sea-level fall. We surmise that it represents the PETM where it is present, such as at the Red Hot Truck Stop (e.g., Beard, 2008) but this has not been determined yet. We will include a more thorough description of the regional stratigraphy with a separate figure and include the above rationale in the results and discussion section ‘stratigraphy’.

Similarly, the paleoenvironment, water depth and agitation of the water column are poorly considered. However, such parameters are important for the discussion of the geochemical results (see below). Though most results here described concern the organic matter content of the sediment, basic information on the organic matter is lacking. 1) A curve of the TOC content of the sediment is needed. Though not always linked to the oxygenation of the sediment and water column, the TOC content helps determining the paleoenvironmental conditions.

Reply: We will include a %TOC plot (Figure R1).

2) Despite the very detailed palynological study, an important information is missing: is there woody organic matter in this sediment? From the relatively nearshore environment, is it expected. However, nothing in the presented results allows to infer the presence or absence of woody material. This is a problem, because woody particles are transported to the sea by rivers, and not by the wind, and because, if present, woody particles may represent a much higher carbon mass than the abundant pollens (and algae) described, and therefore carry a large part of the 13CTOC.

Reply: There is microscopic woody material present in the samples but it represents only a minor component of the assemblage; amorphous organic matter dominates Paleocene palynological residues. We did not encounter larger fragments of wood. Most terrestrial palynomorphs in relatively humid settings such as the one described in the paper (e.g., Harrington and Kemp, 2001) as well as terrestrial GDGTs (Hopmans et al., 2004) are transported to the ocean by rivers, rather than wind, with the possible

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exception of saccate gymnosperm pollen (Moss et al., 2005). The high abundances of terrestrial palynomorphs and high BIT values, therefore, by themselves already indicate river transport. Regarding the impact of wood on the $\delta^{13}\text{C}$ of TOC. For palynology we did not perform total kerogen analyses but analyzed the palynological fraction larger than 15 micrometers (6465:11). Therefore, we cannot test the impact of woody material on the $\delta^{13}\text{C}$ of TOC with the present data. But, given the relatively small amounts of woody material in the palynological residue, its influence on the $\delta^{13}\text{C}$ of TOC is probably minor. This is supported by the absence of a correlation between $\delta^{13}\text{C}$ and C/N even though there are problems with using C/N ratios (see next point and Fig R2). We will include this information in the description of the palynological results.

3) Changes of the terrestrial/marine proportion of the organic matter can also be estimated by using the C/N ratio of the organic matter.

Reply: We did not include our C/N ratio data and explain here why; see also Figs R1 and R2. At face value, a decrease of uppermost Paleocene values averaging ~ 10 to PETM values averaging ~ 6 could be interpreted as consistent with a decrease in the relative abundance of terrestrial organic matter versus marine (Figure R1). However, we measure extremely low values of 2 to 3 in many samples. In addition, the linear regression between C and N crosses the %N axis at positive values. Both these observations compromise the interpretation of this data towards discriminating terrestrial and marine organic matter sourcing. With such low TOC content, there could be analytical issues (blank) as well as the influence of ammonium adhesion to clay particles and diagenetic issues. For these reasons, we consider the BIT index and the relative amounts of terrestrial and marine palynomorphs to be more reliable indicators for organic matter sourcing in this record.

Specific comments The title refers to “extreme warming”, however, the warming documented by the different proxies is in the range of values reported in the literature.

Reply: Clearly this is a qualitative statement. The recorded warming is indeed compa-

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able to PETM records at other locations and as such the result might not be surprising. Still, we consider a warming of the recorded magnitude is quite extreme. We will reconsider the title of the manuscript for the revised version to prevent such subjective discussions and also because it is currently quite long.

p6461 I15 “The recognition ... stratigraphic interpretation”: nowhere in the main text is mentioned a sequence stratigraphic division, nor a maximum flooding surface. Reply: Thank you for spotting this. We will make sure the revised abstract will specifically reflect our conclusion that sea level rose during the PETM and that it is bracketed by two unconformities that were driven by sea level change, consistent with the text.

p6462 I16: ref to Sluijs et al. 2008a. There are many other references to cite regarding the sea level rise. (also p6475 L16).

Reply: This is correct. The citation should have indicated (e.g., Sluijs et al. 2008a and references therein). The cited paper is in part a review that synthesizes sea level data from many locations. We will include the following citations here: (Speijer and Morsi, 2002; Harding et al., 2011).

p6462 I22 and following “One of the proposed...”: Complex sentence. Rephrase.

Reply: We will break this sentence into two separate sentences.

p6462 I25: decrease in oxygen content in deep settings. 1) the references cited do not refer to deep settings (600m maximum), 2) p6478 I1 is said that “deep sea experienced only a limited reduction” in oxygen content.

Reply: we will include reference to Chun et al. (2010) for the deep sea.

p6464 I4 and follow “The Tuscahoma...”: it would be much simpler to illustrate with a stratigraphic column.

Reply: also following a comment by Schaller, we will include a general description of the regional stratigraphy in a figure.

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p6464 I19 “lacks calcareous fossils”: do you mean macrofossils? The sentence is not consistent with the occurrence of nannofossils.

Reply: The stratigraphic section will be extensively revised based on the comments of the various reviewers, including a new figure and a better description of the regional stratigraphic context. There are marine tongues in both the Tusahoma and Bashi that contain marine micro- and macrofossils. Aside from these two marine incursions, the intervening sediments are essentially devoid of carbonate.

p6464 I15 and following: a lithological description of the studied rocks is needed. The only available description in Fig. 2 and its title is highly insufficient. What is the significance of the yellow horizontal rectangles in the “lithology” column? What is the significance of the grey little symbols (pebbles?) at the Paleocene-Eocene limit? A legend is needed. What are the “siliciclastics with carbonate” of the Bashi formation? This is not the name of a rock. Are there sedimentary features such as bioturbation or lamination or oblique lamination clearly visible in the rock? Such information is important for the discussion of the geochemical data. Concerning the sequence stratigraphic pattern shown on fig.2, on which argument(s) is an mfs placed a few decimeters above the base of the glauconitic interval, knowing that “detailed sedimentological analyses have not been performed” (p6477 I20)?

Reply: we will include a legend and expand the lithological description of the section as well as possible with the available data and expand the sequence stratigraphic rationale.

p6464 I27: which types of particles were considered for the calculation of the marine vs terrestrial ratio? The occurrence of woody particles is never described in the article.

Reply: This is the ratio of terrestrial palynomorphs versus marine palynomorphs as described in this sentence. Palynomorphs are distinct, identifiable units, and thus do not include fragments of larger components. In this study, the palynomorph association dominantly comprises pollen and spores from land and dinocysts. This information will

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be included.

p6466 I16 “Because this could... terrestrial organic matter”: Since no real lithological description is given before, at this point of the article nothing allows to infer that the organic matter content is more marine in the glauconitic interval. On the contrary, the lithological change from mudstone to siltstone rather suggests increased continental contribution and therefore more continental organic matter. From an isotopic point of view, marine and terrestrial organic matter show relatively similar values between -23 and -25 ‰ in the late Paleocene (Sluijs and Dickens, 2012 and references therein, but also Storme et al (2012) Terra Nova, Vol 24, No. 4, 310–317, and Manners et al. (2013) Earth and Planetary Science Letters 376 220–230), so that at first sight, the negative shift does not appear particularly related to a change in the origin or the organic matter. Do you have the value of the C/N ratio of the organic matter, this also would help determine to which extent the OM is of terrestrial or marine origin?

Reply: We completely agree with the reviewer that the negative shift is not related to changes in the origin of the organic matter. This section was intended only to test this particular aspect but our wording was apparently not very clear. We will revise the text to make this as clear as possible; the primary line of evidence being the biomarker-specific isotope data; see above for the discussion on the C/N data.

p6466 I19: I agree that sulfur-bound biomarkers are most likely of algal origin, however I have some concern regarding the origin of the biomarkers that were effectively analyzed isotopically. The method section indicates that desulfurization was performed on the total extract. If free phytane (or phytene) and free C₂₉ sterane (or sterene), which origin is more subject to discussion than the S-bound counterparts, were present in the total extract, these were combined to the compounds released by desulfurization/hydrogenation. From the -too scarce- lithological and environmental description available, I suspect that the depositional setting was relatively energetic. These conditions are not prone to organic matter sulfurization. Why not analysing other biomarkers present in a free state (e.g. long chain n-alkanes)? The description of data points in

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figure 2 is not clear. What are the “Light-colored and open symbols”? Do you refer to the 2 squares filled in white? Why not putting error bars on the figure?

Reply: Concentrations of free phytane, steranes and long chain n-alkanes were unfortunately insufficient to allow for isotope analyses. Therefore we generated isotope data on S-bound biomarkers. Crucially, however, in contrast to what was included in the original version of the paper, we performed such analyses only on the S-bound biomarkers liberated from the polar fraction, and not on the total lipid extract. As the reviewer indicates, these are most likely of marine origin. We will include this aspect in our discussion of the results. Light-colored and open symbols in the $\delta^{13}\text{C}$ values of sulfur-bound phytane, and sulfur-bound C29 sterane yield a relatively high uncertainty of 1 and 1.5‰ in isotope values, respectively, due to the low abundance of these compounds in the sediments. This is described in the figure caption.

p6467 I4 “extreme warming”: is this warming really extreme?

Reply: see reply above

p6467 I15 and following “this can be explained...terrestrial palynomorphs”: the increase of the BIT index and proportion of terrestrial palynomorphs indeed suggest that the ^{13}C increase is linked to an increased proportion of terrestrial organic matter (OM) however, to my opinion the isotopic data do not support this interpretation. The values around -25‰ in this positive spike are heavier than the value of the supposedly mostly terrestrial OM deposited at the end of the Paleocene. If the positive spike were simply related to an increased proportion of terrestrial OM, this would imply that terrestrial OM had a heavier isotopic signature during the PETM than during the Paleocene, which is of course not the case.

Reply: The reply to this point is combined with that of the next comment.

p6467 I19-23 is proposed that this peak in isotopic values and terrestrial OM could be related to degradation of the marine OM and/or to storm deposition. I agree with these

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interpretations, as the rock is more sandy and micaceous, but I do not support the “interval of non-deposition” hypothesis. Non-deposition rarely corresponds to sandy material, but rather to clayey material. A storm deposit appears more likely, however additional arguments could help: what is the aspect of the marine and terrestrial palynomorphs in these samples, are they corroded? Which type of terrestrial particles is present? Could they correspond to highly degraded/reworked organic particles (PM4 type particles in palynofacies analysis Whitaker M.F. (1984) - The usage of palynostratigraphy and palynofacies in definition of Troll Field geology. 6th Offshore Northern Seas Conference and Exhibition, Stavanger 1984, Paper G6). This might explain an anomalously heavy isotopic signature.

Reply: As indicated above, 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 these data in the revised version) suggesting that oxidation may have caused the selective preservation of ^{13}C -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 (121-23), 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 allochthonous. We will expand the discussion of this aspect of the data with this information but we cannot fully explain the positive peak in the $\delta^{13}\text{C}$ of TOC record within the CIE.

p6467 l24 “A \sim 10 cm ... unconformity”. Indeed, this level is indicated as an unconformity on figure 2, and the concentration of shells and sandy lithology is consistent with an unconformity. Are there additional arguments such as a hardground or erosive base? What is the meaning of the grey “nodules” in the lithological log?

Reply: We will include a complete legend and expand the lithological description of

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the section as well as possible with the available data and expand the sequence stratigraphic rationale. This will include better discussion on the magnetic susceptibility record as mentioned by Thomas.

p 6468 l8-14 “implying that ... is minor”: this sound reasonable, but in this very proximal setting, lateral facies variations are also possible, so that lithostratigraphic correlations are hasardous.

Reply: This is correct. The initial submission included an error - the Bells Landing marl is indeed present in the Harrell core, 239 m below the CIE. This has been corrected in the revised ms.

p 6468 l22 “a sea level drop in MS”: do you mean “a drop in MS”? p 6468 l 22 : “and to the Tuscahoma”

Reply: Thanks for spotting this. It should read, ‘as suggested by a drop in MS, coinciding with the Tuscahoma – Bashi contact’.

p 6468 l26 “the sediment were likely deposited”: What are the arguments?

Reply: The most important reason is that there are no signs of the recovery interval of the CIE as discussed in lines 24-26 just preceding this sentence; we will connect the argument to the conclusion explicitly in the revised text.

p 6468 L29 and following “glauconite... accumulation rates”: as a matter of fact, the origin of the glauconite is not trivial. Allochthonous glauconite has only little implications for the deposition rate.

Reply: we agree and we will change wording here.

p6469 l19-21 “in this interval ... underestimate” (also p 6470 l8-9): Why would the terrestrially derived GDGT lead to underestimation of the temperature? In this core, the low BIT samples are “warm” while the high BIT samples are “cold”, however, there is no reason to compare the two intervals as the sediments were not deposited synchro-

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neously. In order to estimate the influence of terrestrial GDGTs on the final TEX86 and paleotemperature values, you have to look for the terrestrial bias in sediments deposited contemporaneously. In Veijers et al (2007), the “temperature” determined from the African soils is higher than the temperature determined from the marine sediments of the Niger fan. This has to be confirmed by other studies, but it suggests that the presence of terrestrial GDGT leads to an overestimation of the temperatures.

Reply: We took exactly this approach; we tested the relation between TEX86 and BIT for the Paleocene and PETM intervals separately. In the text, we therefore explicitly indicate in line 19 that we discuss the relation between TEX86 and BIT ‘in this interval’. For optimal clarity, we will rephrase this to ‘in this upper Paleocene part of the section’. Note that it depends on the ‘TEX86’ value of GDGTs transported by rivers from the drainage basin whether there will be a cold or warm bias (cf. Schouten et al., 2013).

p6470 l11: “by _7-8 to 35_C”: not clear.

Rephrased to “by 7-8 °C to 35 °C”

p6470 l26-27 “Although records ... in temperature trends”: As pointed out by Dunkley-Jones et al (2013), the temperatures, and magnitude of the temperature anomaly, apparently highly depend on the proxy used. The different SST and MAAT values plotted on figure 4 therefore may not be comparable.

Reply: This is a good point although it only applies to TEX86 and only for those sites that are outside of the range of the modern calibration dataset. In essence, this comes down to two sites, New Jersey, where the magnitude of warming is also constrained by proxies other than TEX86, and Site 1172 and the Harrell Core. We will include a range of warming for these sites in the review.

p6471 l1: what do you mean by “modest”?

Reply: This section will be revised following the comment by Huber.

p6471 l3-6 “Although more estimates... anticipated.”. Complex sentence. Simplify.

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Reply: This section will be revised following the comment by Huber.

p6471 I6-8 “in their...below 30_C”. The model developed by Huber & Caballero (2011) is for the early Eocene climatic optimum, an interval that was warmer than the Paleocene. Comparing late Paleocene paleotemperatures obtained in this study with the temperatures modeled by Huber & Caballero (2011) has no real sense. However, the model of Huber & Caballero (2011) could be considered as a model for the PETM itself, as marine $\delta^{18}\text{O}$ values of the PETM are similar to those of the early Eocene climatic Optimum (cf Zachos et al. 2008). Interestingly, the model of Huber & Caballero (2011) indicates values between 30 and 35_C for the CGP while the average TEX86 paleotemperature for the PETM obtained here is 35_C (p6470, I9).

Reply: This section will be revised following the comment by Huber.

p6471 I8 “all available data”: add references.

Reply: This should have included a reference to the data in Figure 2. This section will be revised following the comment by Huber.

Part 4.3 : The presentation of the palynological results in the main text and in the supplement is somehow misleading and suggests that the total palynological study in new, which is not the case, as the palynological study of the Tuscahoma fm. has been previously published (Harrington et al., 2004 ; Harrington and Jaramillo, 2007). The text, figures, and supplement should more clearly refer to the previous publications regarding the data on the Tuscahoma formation. The present study focuses on the upper part of the Tuscahoma formation, above 124m. Is it useful to present in figure 3 and in the supplement the palynological data from below 124m?

Reply: We tried to make clear in the first version that the palynological study includes both previously published samples (e.g., Harrington et al., 2004) as well as new work on samples near the PETM but apparently we did not succeed very well. We will take care to make this clearer in the revised text and supplement.

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p6471 I14-20: this first paragraph of part 4.3 has no interest as it is a synthesis of all the data described afterwards. Either remove it or move it at the end of part 4.3.

Reply: We thank the reviewer for this suggestion and we will consider it for the revised text.

p6472 I1 “... throughout many lower Paleogene pollen assemblages”: give references.

Reply: We will refer here to the various papers by Harrington and colleagues cited elsewhere in this section.

p6473 I9 and following “The ordination ... particular sample”: this is a methodological description that should better be put in section 3.

Reply: We agree that these three sentences represent a brief methodological intermezzo. In section 3 it would require a separate paragraph that would be somewhat out of context, while this text is needed in the present discussion. Even though we agree that sensu stricto this is methodology, we chose to retain it here to optimize the clarity of the paper.

p6474 I10-11: Pediastrum is present, but is in low abundance.

Reply: This is correct. Pediastrum is not a major component of the assemblage but it is important. Qualitatively, its presence must imply the supply of terrigenous material. We will include this in the revised version of the text.

p6474 I12-14 “near shore environment”: OK but the description is insufficient. For a sedimentologist “near shore” is generally open and highly energetic and therefore implies the deposition of relatively coarse material such as sand. In the present case, the sediment is mostly clayey-silty which rather indicate a quiet environment such as a lagoon. Reply: We will change wording here accordingly.

p6474 I17-19 “Apectodinium was ... during the PETM”: this is a reverse reasoning. The abundance of Apectodinium in this subtropical setting during the Paleocene indicates

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that Apectodinium is a warm dwelling species. Even if temperature is not the sole parameter that governs the abundance of Apectodinium, its increase in abundance and poleward expansion during the PETM is consistent with general warming and poleward expansion of subtropical conditions.

Reply: We agree and do not see how the text disagrees with this. Please note, however, that temperature was unlikely the limiting factor for Apectodinium in mid latitude regions during the Paleocene (Sluijs et al., 2007; Sluijs et al., 2009)

p6474 l25-27 “Hence, whatever ...areas”: sentence not clear.

Reply: we will rephrase to: “Whatever environmental factors triggered the global spread of this taxon must have been common to both low and high latitude areas.”

p6475 l2-3 “The concomitant... condensation”: sentence not clear. What exactly is inferred to indicate condensation? Condensation means a very low sedimentation rate. In itself, a change from mudstone to silt does not indicate condensation. Don’t you rather mean a hiatus?

Reply: We cannot distinguish between very low sedimentation accumulation rates associated with winnowing of the clay fraction or a hiatus due to erosion. Very likely this is a hiatus and we consider it as such when we discuss potential amount of time missing.

p6475 l21-24 “The overlying Bashi ... PETM in the Harrell core”: this is repetitive.

Reply: We will include a stratigraphic figure, which should make this discussion clearer and we will adapt phrasing accordingly.

p6475 l24 26 “This explains ... GCP”: strange sentence where we start in the CGP, travel the world and finally get back to the GCP. Rephrase.

Reply: Thanks for spotting this; we will make this section spatially logical.

By the way, you do not clearly state the important fact: the sea level rise of the PETM was followed by a sea level fall that eroded the sediment deposited during all or part of

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the CIE in the GCP and in several other locations (e.g. Sluijs et al, 2008).

Reply: This is a great suggestion; we will rephrase accordingly.

p6476 I7 “it was suggested...”: give a reference.

Reply: we will cite Beard (2008) in the revised text.

p6476 I12 “which we consider ... lithological constraint”: This indeed seems reasonable, however, in proximal environments, sediments can rapidly change from one place to another. The 10 km distance between the Harrell core and the Red Hot Truck Stop may be sufficient to have horizontal lithological variations. One way to be sure that the glauconitic sands of the RHTS are contemporaneous to those of the Harrell core is to analyze carbon isotopes!

Reply: We completely agree and will include this discussion in the revised text.

p6476 Deoxygenation Where isorenieratane was observed in the desulfurized extract, was it abundant? Were other carotenoids identified i.e. chlorobactane?

Reply: We did not quantify concentrations of S-bound biomarkers but isorenieratane was present in similar concentrations as of other planktonic biomarkers such as the sterane. The signal is, therefore, very clear. We did not encounter chlorobactane. Note that aromatic carotenoids can also be sulfur-bound to the kerogen (Hartgers et al., 1994), in varying amounts, and thus the extractable amounts of S-bound isorenieratane do not need to be representative of the total amount of isorenieratane deposited. Its presence indicates that there have been episodes of time with photic zone euxinia. We will include a brief section on this.

p6477 I4 “euxinic conditions developed in the photic zone”: From the geological description, it appears likely that the deposition depth was less than 50m, and therefore that the sea bottom was within the photic zone. This implies that possibly, anoxia did not have to develop in the water column for isorenieratane-producing bacteria to occur. Though currently, isorenieratane is apparently mostly produced in the water

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column, sulfurized isorenieratane was described from the carbonated sediments of a very shallow Jurassic lagoon (van Kaam-Peters et al. 1998 Organic Geochemistry 28, p151-177).

Reply: This is correct. We cannot exclude the possibility that the sea floor was within in the photic zone in this shallow setting. We will make this more explicit in the revised text.

p6477 l13-15 “because isorenieratane ...short transport time”: indeed, isorenieratane is highly reactive. Its sulfurized counterparts, however, are much more resistant. One possibility therefore is that this sulfurized isorenieratane was transported from further offshore; in particular if this compound is not very abundant in the extracts.

Reply: We are not aware of any study that has shown that sulfur-bound organic matter can be transported from offshore euxinic locations to coastal oxic sections. Although this could theoretically occur, we find it extremely coincidental that this relatively rare component is only documented during the PETM. The appearance of S-bound isorenieratane during the PETM is not unique to this section and has been documented elsewhere including other coastal sections (Figure 4), so its appearance is not necessarily surprising.

p6477 l25 “Collectively ...”: overall, I agree that if anoxia occurred, it likely was in an intermittent way similar to present day “dead zones”. However, the arguments presented here for water column anoxia are not fully convincing. 1) Only 3 samples were analyzed.

Reply: we analyzed 5 samples. Isorenieratane was not detected in two Paleocene samples, while it was detected in three samples from the PETM.

2) Only the presence of isorenieratane is reported and not its abundance, suggesting that it likely is present in relatively low abundance.

Reply: Concentrations of S-bound isorenieratane are similar to those of the other

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recorded planktonic biomarkers such as the sterane. The signal is, therefore, quite clear. We will include this in the revised paper.

3) In a shallow and open marine setting where the sediment is sandy, the development of photic zone anoxia is difficult to conceptualize as wave agitation prevents water stratification.

Reply: We incorporate the suggestion by Thomas that this paradox might represent seasonal variations in oxygenation.

p6478 l9: The Frasnian-Fammenian boundary (Devonian) also is characterized by water column euxinia and the presence of isorenieratane in several marine settings (e.g. Joachimski et al, 2001, *Chemical Geology*, 175, p109-131; Brown and Kenig, 2004, *Palaeo3*, 215, p59-85; Bond D. P. G. & Wignall P. B. 2008. 3, 263, p107–118).

Reply: We will include this in the revised text

Technical corrections p6463 l2 : missing “.” after Beard, 2008) p6463 l22 : no figure S1 in the supplement. p 6468 l3 “presence” p6467 l8: supplement table 2 p6473 l26-27 “cpx. in the Harrell core almost exclusively comprises Senegalinium $\hat{A}\hat{E}\hat{Z}$. (“The whole comprises the parts” and not “the whole is comprised of the parts”).

Reply: all the above inaccuracies will be corrected.

References cited in our replies Beard, K. C.: The oldest North American primate and mammalian biogeography during the Paleocene–Eocene Thermal Maximum, *Proceedings of the National Academy of Sciences of the United States of America*, 105, 3815-3818, 2008. Chun, C. O. J., Delaney, M. L., and Zachos, J. C.: Paleoredox changes across the Paleocene-Eocene thermal maximum, Walvis Ridge (ODP Sites 1262, 1263, and 1266): Evidence from Mn and U enrichment factors, *Paleoceanography*, 25, PA4202, 2010. Harding, I. C., Charles, A. J., Marshall, J. E. A., Pälike, H., Roberts, A. P., Wilson, P. A., Jarvis, E., Thorne, R., Morris, E., Moremon, R., Pearce, R. B., and Akbari, S.: Sea-level and salinity fluctuations during the Paleocene-Eocene

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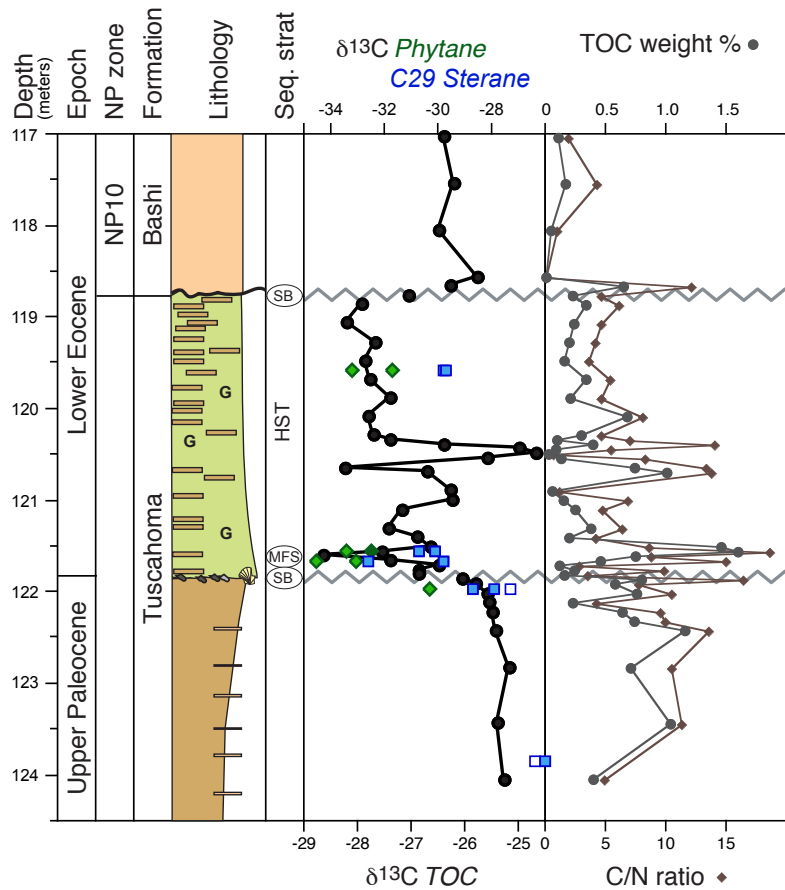


Fig. 1. TOC and C/N records

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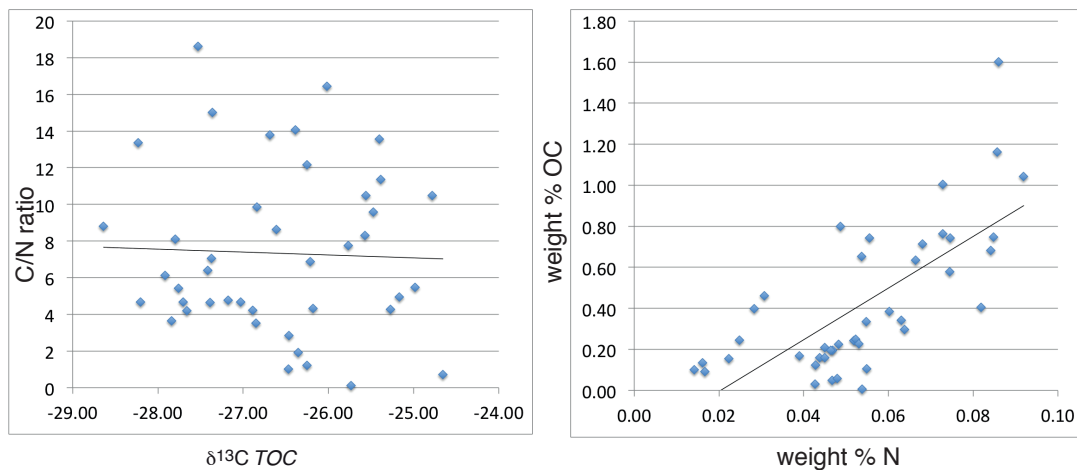


Fig. 2. C, N and d13C scatter plots

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