Clim. Past Discuss., 9, C3257–C3266, 2014
www.clim-past-discuss.net/9/C3257/2014/

Climate of the Past Discussions
Open Access

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.

A. Riboulleau (Referee)

Armelle.Riboulleau@univ-lille1.fr

Received and published: 27 January 2014

Comments 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 Sluijs et al.

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

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, it 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.

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 insuffient 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). 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 paleoen-

CPD

9, C3257–C3266, 2014

vironmental conditions. 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 δ13CTOC. 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.

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.

p6461 l15 “The recognition ... stratigraphic interpretation”: nowhere in the main text is mentioned a sequence stratigraphic division, nor a maximum flooding surface.

p6462 l16: ref to Sluijs et al. 2008a. There are many other references to cite regarding the sea level rise. (also p6475 L16)

p6462 l22 and following “One of the proposed...”: Complex sentence. Rephrase.

p6462 l25: decrease in oxygen content in deep settings. 1) the references cited do not refer to deep settings (600m maximum), 2) p6478 l1 is said that “deep sea experienced only a limited reduction” in oxygen content.

p6464 l4 and follow “The Tuscahoma...”: it would be much simpler to illustrate with a stratigraphic column.

p6464 l19 “lacks calcareous fossils”: do you mean macrofossils? The sentence is not consistent with the occurrence of nannofossils.

p6464 l15 and following: a lithological description of the studied rocks is needed. The

CPD

9, C3257–C3266, 2014

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 l20)?

p6464 l27: 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.

p6466 l16 "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?

p6466 l19: 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),

CPD
9, C3257–C3266, 2014

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 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?

p6467 l4 "extreme warming": is this warming really extreme?

p6467 l15 and following "this can be explained...terrestrial palynomorphs": the increase of the BIT index and proportion of terrestrial palynomorphs indeed suggest that the 13C 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.

p6467 l19-23 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 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

CPD
9, C3257–C3266, 2014

Seas Conference and Exhibition, Stavanger 1984, Paper G6). This might explain an anomalously heavy isotopic signature.

p6467 l24 “A ∼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?

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.

p 6468 l22 “a sea level drop in MS”: do you mean “a drop in MS”?

p 6468 l 22 : “and to the Tuscahoma”

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

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.

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 synchroneously. In order to estimate the influence of terrestrial GDGTs ont 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 leeds to an overestimation of the temperatures.

p6470 l11: “by ∼7-8 to 35°C”: not clear.

CPD

9, C3257–C3266, 2014

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.

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

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

p6471 l6-8 “in their...below 30°C”. The model developped 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 δ18O 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, l9).

p6471 l8 “all available data”: add references.

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?

p6471 l14-20: 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.

CPD
9, C3257–C3266, 2014

p6472 l1 “... throughout many lower Paleogene pollen assemblages”: give references.

p6473 l9 and following “The ordination ... particular sample”: this is a methodological description that should better be put in section 3.

p6474 l10-11: Pediastrum is present, but is in low abundance.

p6474 l12-14 “near shore environment”: OK but the description in 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.

p6474 l17-19 “Apectodinium was ... during the PETM”: this is a reverse reasoning. The abundance of Apectodinium in this subtropical setting during the Paleocene indicates 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.

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

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?

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

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

CPD
9, C3257–C3266, 2014

p6476 l7 “it was suggested...”: give a reference.

p6476 l12 “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!

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

p6477 l4 “euxinic conditions developped 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 isorenieratene-producing bacteria to occur. Though currently, isorenieratene is apparently mostly produced in the water 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).

p6477 l13-15 “because isorenieratane ...short transport time”: indeed, isorenieratene 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.

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. 2) Only the presence of isorenieratane is reported and not its abundance, suggesting that it likely is present in relatively low abundance. 3) In a shallow and open marine setting where the sediment is sandy, the development of photic zone anoxia is

CPD
9, C3257–C3266, 2014

difficult to conceptualize as wave agitation prevents water stratification.

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).

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 Âż. ("The whole comprises the parts" and not "the whole is comprised of the parts").

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper