Response to Reviewer #1

We thank the reviewer for a careful and thorough assessment of our manuscript. Below, we provide a point-by-point response to the comments.

Dear Editor,
I have now read the manuscript by Frieling et al. entitled "Tropical Atlantic Climate and Ecosystem Regime Shifts during the Paleocene-Eocene Thermal Maximum". The manuscript presents new dinocyst and sedimentological data from the Paleocene/Eocene thermal maximum as recorded at tropical Site 959. The present paper is companion of another paper on the same record submitted somewhere else, and in its main conclusion follows up what previously found by Frieling et al. (2017) at a tropical shallow marine section deposited in Nigeria. In this paper the authors suggest that extreme warming during the PETM within this shallow and possibly restricted basin knocked off planktonic eukaryotic productivity. In the manuscript under consideration they came to the same conclusion, arguing for a collapse of marine eukaryotic productivity due to extreme temperatures also at the fully open ocean setting of Site 959. This represents the main finding of the paper. This is an interesting finding as it highlights how in the open ocean temperature alone may threats pelagic food webs, especially at low latitudes. On the other hand, the paper in my opinion suffers from the fact that several important aspects concerning the record of the PETM at the studied site are shown but not treated in this manuscript. The reader throughout the text is referred to the companion paper that is still – at least to my knowledge – under revision/submitted somewhere else. Since the PETM from Site 959 has never been described before, this in my opinion hampers a full overview of the environmental/oceanic conditions at Site 959 and especially on how the PETM was expressed at this locality, and somehow weaken the author main findings. The sedimentary and isotopic records of the PETM at Site 959 are quite peculiar, and to my knowledge are not alike anyone else found. However what this might mean in term of depositional environment, water column conditions, and in relation with the biotic data presented is insufficiently discussed here, and the reader is referred to the companion paper. I think this paper could be published after some substantial revisions. However I also believe this paper would acquire more strength and relevance if published once the companion paper will be published, when the authors will be free to openly discuss their other findings.

Author response:
The reviewer points out that the manuscript currently suffers from a lack of background information, which is to be published elsewhere (referenced here as Frieling et al., submitted). This paper was first submitted early this year but it has suffered from an unexpectedly large delay. We see options to alleviate the reviewer's concerns without reiterating information in this manuscript. We would be more than happy to share the submitted paper with the editor and reviewers, or as the reviewer suggests, publication of the current manuscript may be halted until publication of the first submitted manuscript. We will await editorial advice on this issue.

Line by line remarks:
Abstract:
Lines 23-25: **"The early stages of the PETM are marked by a typical acme of the tropical genus Apectodinium, which reaches abundances of up to 95%. Subsequently, dinocyst abundances diminish greatly, as do carbonate and pyritized silicate microfossils"** Dinocyst absolute abundance drops already within the porcellanite layer coinciding with the onset of the CIE at Site 959. Why you do not consider it at all? This should be explained and discussed in the text.

Author response:
We agree with the reviewer this had to be clarified and in our revised text we argue this initial drop in dinocyst numbers is likely due to the very organic-lean (oxidized) nature of the porcellanite. It should be noted that we interpret the porcellanite layer to be of turbiditic origin (p. 4 line 3-5 in original manuscript) and it follows that its deposition was extremely rapid. In Frieling et al. (submitted) we argue that organic matter is mixed in from above due to bioturbation. We now include these findings into this manuscript to provide the necessary background information.

Introduction:
Lines 10-13 pag 2: **"The CIE has a distinct shape; a rapid "onset" (1-5 kyr; (Kirtland Turner and Ridgwell, 2016; Zeebe et al., 2016) followed by a prolonged (50-70 kyr) period, the "body", of stable low 13C values and a recovery that lasts 42 - 100 kyr (Röhl et al., 2007; Abdul Aziz et al.,**

2008; Murphy et al., 2010) to values that remain slightly 13C-depleted (0.5-1 ‰) relative to the latest Paleocene".
Not correct. The cited papers estimate a duration of the CIE "body" of 70-100 kyr and of 100-130 kyr for the CIE recovery interval, please amend.

Author response:
Fixed.

Line 19 pag 2: More reference needed here as the quoted paper is about a tropical locality.

Author response:
The referenced paper (Frieling et al., 2017) is not solely about a tropical locality, but also contains a state-of-the-art global data compilation of sea surface temperature (SST) data from PETM sites that is accompanied by a thorough point-by-point comparison to fully-coupled climate model simulations. To our knowledge, the referenced paper is the only work that clearly shows the persistence of extra-tropical amplification during the PETM.

Line 20 pag 2: A widespread expansion of suboxic to anoxic waters during the PETM has been suggested by several works. Some more should be mentioned here, e.g. particularly relevant are: Nicolo et al., 2010; Zhou et al., 2014; 2016; Stassen et al., 2015.

Author response:
The reviewer points out that other works have also suggested a global expansion of suboxic to anoxic waters, which is correct. However, the referenced paper by Dickson et al. (2012) is the only that may show truly global deoxygenation as it is a function of Mo-isotope burial averaged across the ocean. Even considering excellent spatial coverage, site-specific oxygenation records always contain a degree of additional local effects. One could therefore argue that several dozens of other papers support the Mo-isotope record, but in our opinion this does not warrant referencing those works here.

Line 21 pag 2: See also Giusberti et al. 2016.

Author response:
We added this reference.

Line 24 pag 2: Please add some more recent work (a number of paper is likely to have been published on this topic since 2014).

Author response:
This regards a very general statement that requires citation of a review paper. Although it was published already a few years ago, the cited paper includes a review that still represents the most complete overview of environmental changes during the PETM and thus covers the statement made here.

Material

Lines 23-25 pag 3: A more in depth description of how the PETM is identified at Site 959 and how it compares to other records should be part of this paper as well. It does not suffice to refer to the companion paper submitted.

Author response:
As noted in the original manuscript (page 3 lines 21-23) the interval containing the PETM was originally identified based on calcareous nanofossil evidence (Shafik et al., 1998). To accommodate this comment by the reviewer, we now also add the magnitude of the carbon isotope excursion and point out variable carbon sourcing plays a role within the turbiditic porcellanite.

Lines 3-5 pag 4: **"The organic-lean biogenic silica and siliciclastics (804.1 – 803.8 mbsf) are likely derived from an allochthonous, potentially turbiditic, deposit, as reflected by anomalous Ti/Al ratios (Frieling et al., submitted), and obscures the exact onset of the carbon isotope excursion (CIE)".**

Why the authors argue that this layer has an allochthonous origin? Ti/Al per se does not indicate reworked material. To the opposite a number of evidence would suggest an autochthonous origin (gradually decreasing **δ**13C values, the drop in dinocyst abundance, a peak in piritysed microfossils) possibly linked to the peculiar (extremely hot? euxinic?) seafloor and water column conditions at the onset of the PETM. This layer coincides with the onset of the CIE at Site 959, therefore its nature and possible origin and relation with the PETM should be better explained and discussed.

Author response, see below:

Lines 7-9 pag 4: **"The gradual decrease from ~ -27‰ at 804.09 mbsf to -30‰ at 803.8 mbsf is interpreted as a mixing line between organic matter produced during the Paleocene and the PETM (Frieling et al., submitted)".**
Mixing of pre-PETM and PETM organic matter would produce a flat line within the porcellanite layer, with isotopic values intermediate between those of the upper Paleocene and the early Eocene. In fact, the observed continuous decrease in carbon isotope values suggests the original onset of the CIE (or at least part of it) was being recorded. The CIE onset at Site 959 is somewhat similar to the onset of the CIE as recorded at Wilson Lake, New Jersey, see Stassen et al. 2015. This possibility should be explored, and the authors should better support their argument. If the porcellanite layer does record the onset of the CIE, this would imply an autochthonous origin for the layer. Why the authors so sharply disregard the possibility of an autochthonous origin?

Author response (for both comments above):
We include a substantial discussion on this matter in Frieling et al. (submitted), but we completely agree with the reviewer that this is difficult to interpret based solely on the evidence presented in this paper. In the revised version of this paper, we therefore explain that Frieling et al. (submitted) show that the gradual decrease can be replicated by a simple sediment mixing model that assumes bioturbation of PETM organic matter down into the organic-lean porcellanite and uses TOC(wt%) as a proxy for this process. This does not produce a flat line as the reviewer suggests, because mixing is never perfect and the presence of PETM organic matter in the porcellanite is accordingly variable. This scenario is consistent with bioturbation patterns in the core. Importantly, this simple model would not work if $\delta^{13}C$ continued to change during the deposition of the porcellanite.
More fundamentally, we see no reason why the onset of the CIE should be gradual at Site 959, since no truly unambiguous (single-specimen) intermediate $\delta^{13}C$ values have been recorded for the PETM. Indeed, most records with a gradual onset are evidently result of mixing.

Methods
The methods section must include a description of the age model mentioned in the discussion. Changes in accumulation rates affect microfossil absolute numbers, so an age model constraining accumulation rates must be described and be part of this work.

Author response:
We agree with the reviewer and will therefore include the required statements regarding sediment accumulation rates during the PETM. A more detailed discussion of the age model is presented in Frieling et al. (submitted).

Furthermore, this section must include how dinocyst absolute numbers were calculated.

Author response:
A description of how absolute dinocyst numbers are calculated was presented on p.4 line 27-28. We do not see how the use of this standard procedure (as described in Stockmarr, 1971) can be further clarified.

Also, the section lacks a description of the organic geochemistry methods used for lipid extraction and of the calibrations used to convert TEX86 into temperature. Since TEX86 data are discussed, the methods should provide this information as well.

Author response, see below:

Results

The results section should include a description of TEX86 data and the BIT index must be shown together with TEX86. The BIT index and its meaning in relation with TEX86 should then be discussed in the Discussion paragraph.

Author response (for both comments above):
The reviewer points out that a methods and results section is missing for the organic geochemical analyses in this manuscript. As stated in p. 3 line 6-7 these data are to be published in Frieling et al. (submitted) and we therefore do not include a methods or results section here.

Line 17 pag 6: **"The onset of the PETM is marked by an acme of Apectodinium…"**
In the result description the authors should refer to their (local) CIE signal rather than to the PETM, and they should always clearly keep this distinction. There should then be a section in the discussion in which the authors explain how they correlate the CIE at Site 959 to the PETM.

Author response:
The CIE has already been identified as representing the PETM based on calcareous nannofossil biostratigraphy, as stated in the materials section, so we do not quite understand this comment by the reviewer.

Line 18 pag 6: Not only the body of the CIE yields low abundances of dinocyst, the drop is firstly observed within the porcellanite layer. The author must explain why this is not taken into account.

Author response:
We now further clarify in the materials section that the porcellanite is regarded as a turbiditic deposit, with organic carbon mixed in from above. This causes low absolute numbers of dinoflagellate cysts deeper in the porcellanite, which was originally most likely deposited without any preserved dinocysts.

Line 22 pag 6: There should be "across" instead of "during".

Author response:
We will rephrase to 'within'.

Discussion:
Lines 28-29 pag 7: **"This observation is important since we can, although with caution, use these as indicators of environmental change that is not associated with background cyclic variability".**
The authors should further explore this.

Author response:
We now further clarify the implications of this statement.

Line 21-23 pag 9: **"The high percentages of Apectodinium in the porcellanite (804.1 - 803.9mbsf) are most likely mixed in from above (803.85 - 803.75 mbsf), similar to organic matter (Frieling et al., submitted), since assemblages and species are very similar"**
Again, why it could not be an original signal? Abundance peaks of **Apectodinium** are recorded almost everywhere at the onset of the PETM.

Author response:
We do not dispute the abundance of *Apectodinium* in the early stages of the PETM. However, we interpret the porcellanite to be of turbiditic origin with organic matter mixed in from above. Therefore, it is unlikely that these specimens are *in situ*.

Line 24 pag 9: paragraph title: **"5.2.3 Microfossil decline and deoxygenation during peak PETM"**
What's the peak of the PETM at Site 959? The author never states what they interpret to be the peak PETM at the studied site. This should be part of the discussion I mentioned above in this revision.

Author response:
We agree with the reviewer that this should be stated more clearly. We now include a statement that we interpret the "body" phase as peak-PETM.

Lines 26-27 pag 9: Accumulation rates are mentioned but an age model is not described in the methods. It looks like the period misses a verb.

Author response:
This will be clarified and included in the materials section.

Lines 2-4 pag 11: **"No such short-term extreme biotic variation is known for any PETM site across the globe, certainly not for the recovery interval, and indicates highly variable environmental conditions."**
This is very poorly phrased and can generate confusion. Short term extreme biotic variations are widespread observed, and they represent one of the peculiar features of the PETM as well as one of the reasons why it is so much studied. Think only to the abrupt benthic foraminiferal extinction event at the onset of the PETM. Besides, short term biotic variation across the recovery phase have been observed also at other localities, in particular at shallow/marginal settings, e.g. see Luciani et al., 2007; Stassen et al., 2015; Giusberti et al., 2016.

Author response:
The reviewer's comment shows our text may cause some confusion between the high-amplitude variations at Site 959 and low amplitude variations observed in other high-resolution studies. The amplitude and perhaps also the timescales of the observed variation are far more extreme than observed in any of the records referenced by the reviewer. Dinocyst assemblages at Site 959 are dominated by different species on cm/(sub-)millennial scales during the recovery. We will properly rephrase to optimally clarify this point in the revised text.

Lines 10-11 pag 12: **"The combined information suggests that eukaryote activity in the mixed layer was suppressed not only in Nigeria (Frieling et al., 2017) but also at the more offshore Site 959. Similar to Nigeria, there is hence no evidence that the low numbers of dinocysts resulted from**
severe stratification and anoxia…"
In a previous paragraph the authors state: **"At Site 959, we find strong indications of decreased oxygen concentrations in bottom waters in the form of increased organic matter burial fluxes, as assessed through reconstructed accumulation rates and TOCwt%. Moreover, increasing Corg/Ptot ratios (Fig. 3i) relate to preferential regeneration of phosphorus from sediments under anoxic conditions (Slomp et al., 2002; Algeo and Ingall, 2007)….. The combined information from Site 959 and Nigeria suggests that oxygen minimum zones during the PETM expanded upwards onto the shelf and downwards to the paleodepth of Site 959 (>1000 m) in the eastern tropical Atlantic, a phenomenon very similar to modern trends (e.g., Stramma et al., 2008). ".**

Please make your data interpretation consistent.

Author response:
The interpretations are fully consistent. The severity of anoxia at Site 959 was less compared to the shelf section. At Site 959 we find no evidence for photic zone or bottom water euxinia. More importantly, and similar to the data presented in Frieling et al. (2017), the deoxygenation was asynchronous with the demise of eukaryotes and must therefore be decoupled. We have clarified this in the revised manuscript.

Typing Errors:
Fig. 3: Core sections should be named 42R-2, 42R-1 etc instead of 42X-2 etc
Line 6 pag 7: "a" missing
Line 1 pag 9: "the" genus **Apectodinium.**

Author response:
Fixed.