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Climate of the Past
Discussions

Interactive comment on "Life and death in the Chicxulub impact crater: A record of the Paleocene-Eocene Thermal Maximum" *by* Vann Smith et al.

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General comments:

The authors present a new partial PETM section from the Gulf of Mexico, recovered from Site M0077, IODP Expedition 364. The ~body of the PETM is identified in this core on the basis of multiple lines of biotic (e.g. calcareous nannofossil, planktic foraminiferal) and geochemical evidence (e.g. a 4 per mille shift in d13CTOC), and appears to be robust, albeit incomplete and bracketed with significant hiatuses of >1 myr. The authors have applied a multi-proxy approach including palynology and organic geochemistry to shed light on both the marine and terrestrial expression of this

global event in this region.

This study presents new multi-proxy data on the ~body of the PETM from an under-studied region, and so is a valuable contribution to our understanding of this enigmatic event. It appears the palynological data have already been published in terms of the species/genera present (Smith et al., 2019; 2020), but the relative abundance data and the geochemical data are new. We have a particular dearth of information on the terrestrial impact of the PETM in this region, so this contribution is welcome. The lack of late Paleocene data predating the event is a shame, and so the study does lack some temporal context, but sometimes the lithologies in question just don't play ball with our proxies. The TEX86 data needs more careful treatment and exploration of the caveats. Overall, I think the paper will be of interest to the readership of COTP and the Palaeogene community more specifically.

The manuscript is concise and clear, with a high standard of writing throughout. There are minimal typos, and instances of poor syntax and grammar are rare. The two figures are clearly drafted and well captioned. The paper is well referenced and the bibliography is largely complete.

Specific comments:

1. The Methods section is a bit skimpy and should be expanded, especially considering there is no extra methodological detail in the Supplementary Information (just the data tables xlsx file – unless I am missing something?).

- More needs to be said regarding the overall sampling strategy of the samples taken for geochemistry and palynology, and the resultant resolution of your samples (both in terms of depth and time). "N= 51 samples" is not sufficient.

- Line 71: Further elaboration on the Bioturbation Index would be welcome, e.g., what defines more or less bioturbation in a section? What does a score of 1 entail vs. a score of 6? When looking at Figure 2 I assume a higher number means more bioturbation, but we shouldn't have to go to Taylor and Goldring, (1993) to check.

-Line 75: Please state at which institution the ECS and Delta instruments are located. It is also typical to state which external (and internal) standards are used to calibrate the isotopic measurements. Is the instrument precision you quote to one or two sigma? Please note that δ13CTOC data is reported relative to VPDB and δ15N is reported relative to atmospheric N2, (as is stated in the caption of Fig. 2 but not here in the Methods).

-What instruments did you use to measure the biomarker data with (GC-MS... HPLC-MS...) and where were these located? How did you extract the samples? What is the $\pm$ error on the TEX86 measurements (incorporating both the analytical and calibration error)? You should state that the GDGT distributions were also used to construct the BIT Index data.

2. Broader temporal context: 2a. The lack of Paleocene palynological and geochemical data is challenging (but not the authors' fault) as it removes some of the context for the PETM. For example, one is left wondering if the elevated temperatures or the distributions of palynological components is unique to the "event" or is merely similar to the background Palaeocene-Eocene signature of the region? I note that the palynology data presented in the Supplementary Info file stretches way up into the early Eocene (up to 505.88 mbsf, ~48 Ma). I realise the focus of this MS is on the PETM but have you considered presenting and discussing this Eocene data in this MS to give better context to your PETM data? I know much of this data is already published as part of Smith et al., 2019 and 2020, but the data in these publications is presented as taxonomic reports (illustrated in the form of plates) and is not graphed up in the same manner as you have done for the PETM data here. This context would be welcome and would help to support statements in your MS such as "The PETM pollen and plant spore assemblage is broadly similar to later Ypresian assemblages observed higher in the core, with angiosperm pollen dominant and rare lower plant spores and gymnosperm pollen." (line 235), for which you provide no supporting reference or data at

present.

2b. I'd like to see more discussion of whether the "PETM black shale" is unusual in the overall context of the Paleocene-Eocene strata at this site, or are there many higher TOC intervals? I see from Fig 2 that there is another little black shale at 606.6 mbsf and another at 606.15 mbsf. These apparently sit within E5 and CP10/11 so are likely to be within ~EECO (rather than being ETM-2 or -3)- are there any others, and if so what does this mean for the oceanographic conditions of the region with time? Are the low oxygen, low bioturbation conditions during the PETM here unique or not?

3. Sedimentology and stratigraphy: 3a. There is good discussion of the sedimentology of this part of the core in section 4.1., including the unconformities, but this is not then clearly annotated onto Figure 2. It takes quite a bit of reading and flicking back and forth between the figure and text to work out which packstone or hardground you're talking about, so these could be linked more clearly using specific mbsf and arrows/ annotations on the figure. Perhaps a graphic log would help too? In particular I don't think the major hiatuses surrounding the PETM are signposted clearly enough in Figure 2. In <10 cm around 607.3 mbsf you jump from CP5 to CP8, which is about 2 million years of time at the minimum. The way the data is presented in the graphs this is not immediately clear.

3b. On the basis of both the sedimentological, geochemical and palaeontological data, (and by your own omission in lines 159-167) there is only a partial record of the PETM preserved in this core. It likely represents either the onset and body or just the body of the event. I would recommend you therefore use "partial PETM" and/or "body of the PETM" throughout the MS to make this clear to readers.

4. The limitations of the TEX86 data need a bit more discussion and exploration. -In addition to the TEX86H calibration (Kim et al., 2010) it would be a good idea to also calibrate the TEX86 data using the BAYSPAR calibration (Tierney and Tingley, 2015. Scientific Data), which would yield broadly the same trends in SST but with different

absolute values. This may change some of your interpretations with relation to heat stress and the tolerance of marine organisms during the PETM. -In the Supplementary Information I would recommend that you present the raw GDGT abundance data (not just the TEX86 ratio), because others can then recalibrate your data when, inevitably, a new calibration comes along. - In order to have a bit more confidence in your TEX86 data (and SSTs) it's a good idea to apply the series of "tests" to the data. These include the Methane Index, %GDGT-0 index etc... (see Hollis et al., 2019. Geosci. Model Dev. Section 4.4, for the links to the all the relevant papers and methodologies). You presumably already have all the data you need (the GDGT distributions) so it's not more analytical work. - Have these samples been checked for maturity (e.g., using the hopane distribution) which can skew TEX86 values (e.g., Schouten et al., 2004. Organic Geochemistry)? Indeed, considering the samples lie at >600 mbsf in the section is there any evidence for diagenetic alteration?

5. I find it interesting that the purple sulphur bacteria markers (e.g. isorenieratane) are apparently detectable throughout the sequence including in the late Paleocene carbonates, not just in the black shale interval. Can you comment on this in terms of the oceanographic conditions at this time? Was some degree of euxenia common within this crater basin over the long-term?

6. In the Supplementary Info data file ("Pollen and Spore Counts" tab) the data is also plotted against absolute age. Please either detail the age model used to calculate these ages (in the MS) or remove the column. It may seem petty but when other people come to re-plot your data in their own publications it may get copied over without further scrutiny.

I look forward to seeing the final corrected paper published. Best wishes, Kate Littler.

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