Our Responses are shown in **bold, red text**

Reviewer #1

Sharman et al. present a new and very important PETM record within deep-water strata of the Gulf of Mexico within this manuscript. Data include a new bulk organic d13C record and associated TOC record, palynology, nannofossils/forams, and a detailed sedimentologic history. Data spanning the PETM from the Gulf of Mexico has been of long-standing interest to the broader Paleogene community and this is a welcome contribution. Broadly, they document a 2 per mille decrease in d13C that corresponds with the biostratigraphic Paleocene-Eocene boundary. This negative carbon isotope excursion coincides with a decrease in TOC, increase in terrestrial palynomorphs, a shift towards finer grained deposition, and reduced bioturbation. This pattern bears a resemblance to several other PETM sections globally and suggests greater fine-grained sediment flux from continents and a shift to less oxic conditions. The manuscript is well-written, clear, and concise. Figures are quite good. My main comments surround some of the interpretations of the data, though even these are not major. Below I have separated them by topic.

Line 23: Would the increase in CaCO3 post-dating the PETM be more consistent with increased limestone deposition as a carbon sequestration mechanism? While there is evidence for ocean acidification and shoaling of the CCD illustrated by the Zachos et al. 2005 Science paper (and others) this was occurring in deeper water overall it seems. From the description of the GOM data it sounds as though carbonate was not particularly abundant in the late Paleocene/early Eocene and it is really just a spike post/late-PETM. If Wilcox Group strata examined here are shallower than 2000 m, the CCD might not have shoaled that high? And in such a case perhaps the increase is actually the pulse of carbonate deposition seen in other sections.

Our Response: This is a fair point. Given a lack of primary data on water acidity in our dataset, we have deleted the last sentence of the 1st paragraph in the abstract to avoid emphasis on the CCD position. We also now include a statement that allows for the possibility of enhanced carbonate deposition in Unit C reflecting coccolithophore blooms (citing Kelly et al., 2005: *Paleoceanography*), thus contributing to a global pattern of CO_2 drawdown via carbonate deposition. We have updated the Discussion section 5.1.1. to cite the interpretation for early dissolution within the lower part of the main CIE in the Logan-1 well (citing Vimpere et al., 2023: *Geology*), which is consistent with the lack of $CaCO_3$ in the early phases of the CIE in the Anchor 3 well. However, the Logan-1 well is some 150 km distant and more distal relative to our locality, thus presumably at a greater water depth.

Line 212-224: Removal of hydrocarbon material via the solvent extraction method is beyond my area of expertise. To an outsider, this seems like an appropriate approach, but again I am not an expert. However, this is a key sample treatment technique that needs to be 100% certain since the geologic and climatic interpretations hang on an accurate d13C curve.

Our Response: We now cite other studies that have used solvent extraction to remove petroleum contamination prior to d13C analysis. We also now include two additional

supplemental tables, one that illustrates the overall efficiency of oil contamination removal (Table S2 in the new submission) and one that provides comparisons of %C and d13C values for raw, solvent extracted, and solvent extracted + decarbonated samples as part of a pilot study conducted in the early phases of the research (Table S3 in the new submission). See also response to Reviewer #2.

Table 1: Lf-1 should be LF-1 to maintain consistency with text.

Our Response: We have fixed this.

The authors invoke a sea-level rise as a needed contributor to sequestering coarse-grained material in proximal marginal marine environments, while export of fine-grained component and associated terrestrial palynomorph were able to deposit in deep-water. Is there sedimentologic evidence for a short-lived transgression within the GOM at the P-E boundary? It seems that if nonmarine basins are preferentially storing coarser sediment loads in North America that this phenomenon in a of itself might be sufficient to cause a shift towards finer grained deposition in deep-water from a mass balance perspective. This assumes a similar grain size distribution of sediment within the routing system before, during, and after the PETM. Do the authors have thoughts/opinions on this hypothesis?

Our Response: We now cite Sluijs et al. (2008b) and Sluijs et al. (2014) in the second paragraph of Discussion section 5.4 as supporting our interpretation of sea-level rise. Sluijs et al., 2014 provides evidence for sea-level rise in the Gulf of Mexico specifically (Harrel core) and Sluijs et al. (2008b) reviews evidence for sea-level rise more globally.

It is an interesting question of whether coarser grained sediment was preferentially stored in onshore basins versus being exported to the coastline. Two additional PETM localities in the Gulf Coast (eastern Texas, Wilcox and Claiborne groups) have been proposed by Sharman et al. (http://dx.doi.org/10.2139/ssrn.4200185**). These authors interpret the PETM to coincide with the basal, sand-rich Carrizo Formation, suggesting that coarser-grained sediment did reach the deltaic centers of eastern Texas (versus being sequestered inland).**

Several studies invoke a shift in oxygen availability during the PETM that caused a reduction in bioturbation. However, it seems to me this also could result if sedimentation rates increased and biologic disturbance remained constant. In the case of greater evidence for terrestrial fine-grained sediment export this seems like a possibility to evaluate in the manuscript's discussion.

Our Response: This is a fair point. We now include a statement that allows for this possibility in section 5.3.

I think the authors may have over-interpreted the CIA shifts observed, particularly given uncertainties around the CaO. I think the index is too insensitive to evaluate whether a pulse of fresh source material has been provided.

Our Response: We have deleted the two paragraphs in Section 5.1.4. that interpreted the timing of the lag associated with changing CIA values (see also Reviewer #2 comment). We

have also deleted a sentence in the Conclusions pertaining to interpreting the CIA data as being in response to exhumation. We have also added text and references that state the influence of grain size, provenance, and carbonate/phosphate minerals on influenced CIA, in addition to silicate weathering (top of section 5.1.3).

Although we have deemphasized aspects of our interpretation of the CIA index, we do hold that the shift in CIA values from Paleocene to Eocene cannot be easily explained by increases in calcite or other carbonate minerals. This is because our XRD data do not show increased calcite content in Paleocene vs Eocene samples (excluding anomalously cemented or marly samples of Unit C), and biostratigraphic data do not indicate elevated counts of calcareous nannoplankton above the marly portion of the PETM. That said, we agree that additional research would be beneficial for exploring additional factors that may be influencing CIA values in early Eocene samples, outside of the effects of changes in silicate weathering.

Final thought, I think the authors could be slightly more conservative with the sequence of events and the overall structure of the PETM release, body, recovery. My concern is that for some data (Fig 4) there are not that many data points within the CIE (3-5) which makes interpreting the finer structure of response difficult and uncertain. I think it is entirely reasonable to treat the PETM CIE zone as a whole and discuss average responses until such a point when more samples and analyses are performed. This is also dependent on the fidelity and precision of the d13Corg data, which are notoriously noisy. I am not insisting more analyses are performed. The work is certainly comprehensive, and I think a more conservative interpretation may increase the impact of the work.

Our Response: We now add an additional statements in section 5.1.2 that highlight the uncertainty in the boundary between the main CIE and CIE recovery, given the relative noisiness of the d13C isotopic excursion. Vimpere et al. (2023) also noted uncertainty in the transition from main CIE to CIE recovery in the Logan-1 well.

Although it is true that the density of our biostratigraphic data, which were conducted for routine industry application, is limited over the relatively thin PETM interval, our lithologic, C-isotopic, and geochemical data were collected at much higher resolution (Figs. 4 and 7). For this reason, our interpretation of the biostratigraphic data is focused on the CIE zone as a whole, as suggested here (e.g., sections 4.4 and 4.5). However, we believe that a 4-fold division of the PETM interval is warranted given the density of lithologic and geochemical data (Fig. 7), particularly since a similar pattern of lithologic change was found by Vimpere et al. (2023) in the Logan-1 well. We now include additional statements in section 5.1.1 that compare the Anchor 3 core with the Logan-1 well as recently presented by Vimpere et al. (2023): *Geology*.