Referee 1:

The authors thank both referees for their constructive comments. We attempt to address each of their comments point by point below. For referee 1:

> I suggest the authors not to use informal terms in their biostratigraphic schemes and in any case to present them in detail in the methods. Specifically, the use of lower/upper before the biozone cannot be added in the biostratigraphy column of the figures. The authors can instead add events in the figure in order to highlight the possible informal subdivision of the biozone. However, these events should be detailed in the text (more details in the text).

We have updated the biostratigraphic schemes to refer only to formally defined zones and removed the references to the upper and lower zones. More background information has been added about the biozones.

> In the pdf file, I have included many comments and suggestions that the authors can use to implement their manuscript. In particular the authors must make sure not to mix results and discussion of them throughout the ms (details are provided in the edited file).

Comments from the pdf have been acknowledged and incorporated, particularly as it pertains to separation of results and discussion.

> As for the discussion, there are some points, especially in the part that offers scenarios useful to explain the peculiar results of this study, which are either superficially presented or are inconsistent with the available literature (e.g. , the AMOC hypothesis, more details on specific points are provided in the edited file).

Reference to AMOC has been removed, alternate hypotheses have been put forward (e.g. sampling bias).

> Another point that could potentially be improved is the comparison with all the available data and in any case with those of the Spanish sections which, in addition to having played a important role in the development of fundamental hypotheses related to mechanisms active during the hyperthermals (e.g., weathering and clay mineralogy)also represent a suitable continuous hemipelagic-continental transect ideal for a comparison with the record of the Mid Atlantic coastal plain.

We have attempted to edit this section for clarity.

> Finally, I provide there are some suggestions to improve the figures, These are related to both the lack of units and the use of informal nomenclature (more details in the text)

Notes on the figures have been taken into account and the figures have been updated accordingly.

Referee 2:

The authors thank both referees for their constructive comments. We attempt to address each of their comments point by point below. For referee 2:

> In my opinion, the primary finding of the study is the counterintuitive response of temperature in the region to CO_2 release for ETM2. ETM2 cooling like this has not been observed elsewhere, thus a detailed discussion of changes in the depositional environment, hydroclimate and ocean circulation is warranted. Therefore, I think the manuscript would benefit from a more detailed sedimentological interpretation of the changes in the depositional setting (i.e., physical sedimentology), in addition to the clay mineralogy work presented here.

Regarding the physical sedimentology, we believe we have provided a detailed characterization of the event in terms of weathering, sedimentation, and environmental changes, and have provided context to both the PETM and ETM2 at other sites. Questions of large-scale ocean circulation would likely require additional sites and go beyond the scope of this study.

> Unfortunately, there is little to no consideration of proxy uncertainty in the manuscript. Temperature reconstruction figures provide no graphical estimation of uncertainty. Given the current state of the science, this is somewhat misleading and should be addressed prior to publication. Specifically, the authors should note 2sd or 0.95 quantile uncertainty. The paired mean δ18O and mean TEX86 warming responses add to the validity of the interpreted cooling during ETM2, but a more detailed discussion of potential mechanisms influencing these proxy systems besides surface cooling would improve the manuscript.

Figures have been updated to reflect uncertainty. However, in interpretation of $\delta^{18}O$, the largest uncertainty is not due to instrument precision or proxy calibration, but due to uncertainty in bottom-water salinity, as there are no estimates of $\delta^{18}O_{sw}$ for this time in this area. Therefore, this figure has been updated to reflect the uncertainty related to a +/- 1 psu change relative to our estimates. Error associated with TEX_{86} measurements is on the order of 0.2 °C and has been incorporated into the manuscript and figures.

> In my opinion the manuscript would benefit from a more organized approach to interpreting changes in sedimentation, salinity, bathymetry, ocean and atmospheric circulation, surface productivity, and carbonate chemistry. The narrative of the discussion could be focused somewhat or organized in a way that considers how all the above-mentioned influence sedimentation, perhaps with a schematic figure. As written, the discussion tends to be harder to follow than necessary.

The section on the CIE magnitude and low carbonate intervals has been reworked and expanded.

> Additionally, some specific aspects of the discussion could be improved. I provide more detailed comments below where I think the authors could bolster their arguments by providing a more thorough discussion. The authors may find that quantitative salinity, seawater $\delta^{18}O$, reconstructions using $\delta^{18}O$ and TEX_{86}-based temperatures would aid certain arguments in the discussion section.

While we agree that quantitative salinity estimates would enhance the manuscript, we were unable to extract planktonic foraminifera in large enough quantity in order to obtain surface $\delta^{18}O$ values. While the

TEX_{86} values record surface temperatures and could be used for salinity reconstructions when paired to surface $\delta^{18}O$ values, at this time only bottom water $\delta^{18}O$ values are available.

Line 27: Statement of novelty removed.

Line 30: We have clarified the terminology. “This study identifies two events, Eocene Thermal Maximum 2 (ETM2 and H2) in shallow marine sediments of the Eocene-aged Salisbury Embayment of Maryland, based on magnetostratigraphy, calcareous nannofossil and dinocyst biostratigraphy, and recognition of negative stable carbon isotope excursions (CIEs) in biogenic calcite.”

Line 33: Done

Line 37: Changed to “related to CIE warming”

Line 48: Removed “well” and “relatively”

Line 107: Clarified

Line 139: Rephrased to clarify that we were measuring a mass between these values

Line 214: Sure

Line 263: As noted above, this is not possible with $\delta^{18}O_{Benthic}$

Line 282: They are rare. This is the first time it has been identified in this region. Our earlier statement of novelty has been removed for clarification.

Line 302: This argument has been thoroughly explored in Bralower et al., 2018 for this region during the PETM. Further exploration of the changing lysocline during ETM2 could be a manuscript on its own and goes beyond the scope of this study. With respect to productivity, our results show little correlation between sedimentation rates/palynological markers for productivity and the timing of either event. We have expanded upon this in the discussion.

Line 309: We have clarified and expanded upon the sedimentation rate changes.

Line 360: Again, our dataset in unable to quantifiably reconstruct salinity

Line 391: Removed reference to linearity

Line 410: The entirety of the CIE is not preserved in foraminifera samples and is partially reconstructed from bulk carbonate. This has been clarified in the manuscript.