Dear Prof. Guo,

We greatly appreciate the highly detailed and very positive evaluation of our work by Reviewer #1 and the nice words they convey regarding our intention and outcome of the study.

Below we reply to each point they raise and indicate how we intend to revise the manuscript.

Sincerely, also on behalf of all co-authors,

Chris Fokkema

Overarching comments and appreciation of the study (manuscript text)

In their study, Fokkema et al. study polar amplification (PA) of orbital-scale climate variability at a time of the early Cenozoic, when ice sheets were much less wide-spread than they are now, even absent. In comparison to studying Pleistocene climate variability and amplitude of PA, this setting allows separation of the impact of ice-related feedbacks on PA from non-ice-related mechanisms. The authors stress that a big step in their work is the construction of a multi-millenial data set of variability of tropical sea surface temperature. The authors describe the derivation of quantitative inference from cored sediment material at Site 959, discuss and define a calibration to the temperature derivation, and describe an age model that is refined based on previous work. Reliability of climate signals is discussed in detail, as a result of which delta18O is excluded from further analyses.

In their supplement, the authors present a concise overview on the calibration of sea water temperature to the geologic archive TEX^{86} that they use, arguing that the use of an exponential calibration is more suited to represent tropical temperatures of warm climates like the Paleogene. I find this argumentation quite important and suggest to move it to the manuscript text.

Author response: We thank the reviewer for this suggestion and will transfer the Supplementary Text to the main text.

The work by the authors, that led to this study, is greatly appreciated. For example, this work enables testing the ability of climate models to reproduce PA as recorded in the geologic archive - a valuable opportunity as PA is one of the relevant climate system metrics for understanding and projecting future climate at much warmer than modern high latitudes. This work may hence extend our model validation from the very short modern observational period towards past (future- analogue) climates. The utility of the presented data towards exactly this purpose is demonstrated by the authors at the example of PA in an ice-free EECO climate. Based on a comparison of their sea surface temperature reconstruction with DeepMIP model simulations presented by Lunt et al. (2021), the authors infer that models agree well with inferences from the geologic record, while noting that PA-causing processes and mechanisms, aside from those related to ice, may be underestimated in the models.

The authors present various insights that are very valuable. Their work supports linkages between climate variability of an early Cenozoic hothouse world with dynamics on glacial-interglacial cycles, posing the question to which extent carbon-cycle feedbacks played a role in Pleistocene Milancovitch cycles. Furthermore, the work illustrates the magnitude of current climate change in the context of Cenozoic climate history. I find the statement „modern GMSST warming of ~1 °C is already in the range of the early Eocene hyperthermal events“ particularly remarkable.

In my opinion the manuscript is very well, and carefully, written. I have located several minor issues and provide some comments at locations where I, as an interested reader, would like to have a bit more insight regarding specific aspects of the work. One addition to the discussion / conclusion outlook could be to propose testing whether the findings of this study will be reproduced in analyses from other cores and from other core locations at a similar latitude. I have no reason to doubt the assumptions made by the authors, regarding both spatial representativeness of the reconstructed signal of sea surface temperature variability and depth of the water column to which a reconstructed temperature signal is attributed; and, as the authors note, their assumptions are supported by auxiliary evidence, as for example by climate modelling. Nevertheless, further testing of these assumptions based on material from other cores is, at least in my opinion, worthwhile. Spending one or two related sentences in the discussion, and/or in an outlook section, would in my opinion provide a meaningful conclusion to the manuscript.

Author response: We thank the reviewer for this recommendation. We will include such a statement in the discussion, where we will highlight the need for additional high resolution, early Eocene SST reconstructions. Low latitude records for validating and extending our work, and high latitude records to provide an optimal comparison for assessing PA.

I note that I am not a climate scientist conducting analyses of the sedimentary records similar to those analyses described by the authors. Hence, my knowledge in details of sample preparation procedures and analysis methodology is not very deep. If the editors have any doubts regarding the validity of the analysis methodology, then I kindly ask to refer to an expert in that field for a second opinion.

In summary, I support publication of the study in Climate of the Past subsequent to addressing or rebuting comments.

Author response: We thank the reviewer for their appreciation of our work.

Specific comments (main text)

Terminology regarding geologic timescales: I noted that in their supplement the authors refer several times to Paleogene or early Paleogene, while the term barely appears in the main text. When the authors speak of the Early Paleogene, do they refer to the (early) Eocene that is often referred to in the main text? I do not think that the link is always clear - in particular, to my understanding, the early Paleogene would likely rather refer to the Paleocene than to the Eocene? I may be wrong here, but clarifying the text where necessary may be helpful for readers.

Author response: We realize this might be confusing for members of our community that are not familiar to the details of stratigraphy and stratigraphical nomenclature. For clarity, "Paleogene" will be changed to "Eocene" to remain consistent in the terminolgy.

Line 24: add comma after „orbital“

Line 44: add comma after „feedbacks to PA“

Author response: Commas will be added.

Lines 40-49: re origin of PA in climate models: I suggest to also refer to / comment on the latitude-dependency of the fraction of outgoing radiation (Pithan and Mauritsen, 2014) via temperature feedbacks, as these have been found to dominate the mechanism for PA in CMIP5 models.

Author response: This will be done.

Line 102: Meaning of the text „no ice and continental configuration“ remains unclear. Do the authors aim at the degree of detail of paleogeography considered in the Eocene simulations? If so, should this text rather read „adaptation of model geography to reconstructed continental configuration and absence of major ice sheets“ or similar?

Author response: This will be clarified to "(i.e., early Eocene paleogeography without ice sheets)"

Line 154: add „for“ after 0.07‰

Line 156: remove space between 13 and %

Line 158: were -> was

Line 164: remove spaces between values and ‰ and % signs, respectively

Line 219: I am not sure whether the term „neutralize“ is correct here. Tap water is not neutralizing afaik, it is rather diluting - does‘nt one need to add a base in order to neutralize an acid?

Author response: Above textual errors will be fixed, and "neutralized" (line 219) will be changed to "diluted"

Line 252: change to „a climate signal“ or „as a climate signal“

Author response: This will be done

Line 261: fix the format of the DOI? (remove the space and/or make the doi a hyperlink?)

Author response: this will be fixed.

Line 264: plots of CENOGRID are at least to me confusing due to the same / very similar color being chosen for both benthic d13C and benthic d18O. Based on the alignment of data and y-axis I can guess which branch represents which isotopic ratio, but the color coding is not helpful here. The „bracket“-like signal on the right, near 56 Myr, is unclear to me - please explain if relevant.

Author response: We will change the colors and flipping/ resizing axes to solve these problems.

Line 272: Maybe provide the modern temperatures in the region as a reference for the 35.2°C of Eocene SST towards providing a rough estimate of climatic difference wrt. to today?

Author response: We will add the modern regional SSTs (annual average of 27.7 °C Locarnini et al. 2018 (*WOAA*).

Page 10, Fig. 2: Clarify the meaning of „E-08“ of the susceptibility record - shall this be 10−8? Refer to my comment in Fig. 1 regarding colors of CENOGRID records. I assume the reference (c) in the figure caption should be moved from the end of the sentence to before „Calcareous nannofossil zones“?

Author response: "E-08" will be changed to "$\times 10^{-8}$". "(c)" will be moved to the front of the respective sentence.

Line 282: „The record shows“ or „The records show“

Author response: "The records shows" will be adjusted to "The TEX_{86} record shows"

Line 286: Refer to my suggestion to move much of the information from the supplement into the manuscript text. Yet, even if this is not done, my feeling is that a bit more information regarding the model simulations should be given here than just a reference to the supplement.

Author response: We agree, and will move the relevant parts of the supplement to the main text, include the information about the model simulations.

Line 288: „of both records“: Maybe once more explicitly state which records you refer to, for clarity. Same for „the dataset“.

Author response: This will be done.

Line 289: Should SD be in singular here? If plural, maybe there is a problem with the formulation.

Author response: "SDs" will be changed to "SD"

Line 292: Indeed, the calibration uncertainty / analytical errors are small regarding the absolute values, but they are large in comparison to the reconstructed SD?

Author response: We describe here that the analytical uncertainty on the recorded variability is smaller than the signal (SD), and the calibration error to absolute water temperature values is much greater. We will reconsider wording to optimize clarity in such a way that the shortcomings of simply comparing SDs, in particular related to the analytical errors, will be better emphasized. Note, however, that this method of comparing SDs is only included as a simple, first-order, approach of comparing the magnitude of variability of the two records,without any further stratigraphic correlations.

Line 325: Fig. 3 suggests that especially CIE J and K coincide with a state of relative warmth rather than a state or warming, that may even include a subsequent cooling, but not necessarily only a phase of transient warming - am I right with this observation?

Author response: For the hyperthermals we use the wording "transient sea surface warming" to describe the complete event, i.e. the period ‘above background temperatures’ that includes the initial and peak and generally also a return to background temperatures. We will clarify this by describing the temperature effects during the events here as "warmer intervals" .

Line 365: Do the authors refer to the whole tropical band or only to the Northern Hemisphere part?

Author response: We here refer to the complete low-latitude band, and this will be clarified by changing "<30º" to "30º S – 30º N".

Furthermore, regarding the statement „the dominant source of Eocene bottom waters in these simulations“ - is this an inference that authors make based on their own analyses, or do the authors refer to results by Lunt et al. (2021), or maybe even to results from authors who contributed model simulations towards the model intercomparison by Lunt et al. (2021)? If the result is not derived by the authors themselves, then I assume citing the relevant publication(s) in this specific context makes sense.

Author response: The statement is based on data and model-based inferences as described in the introduction: published by (amongst others) Cramwinckel et al. 2018 (*Nature*), Gaskell et al. 2022 (*PNAS*), Hollis et al. 2012 (*EPSL*), Zhang et al. 2022 (*P&P*). For clarity, we will add references to this statement.

Figure 5, caption: fix bracket of (Herbert et al., 2010)

Author response: The bracket will be fixed.

Lines 416-418: Simplify reading by adding some commas: „[...] carbon cycle feedbacks, that do not involve ice, snow and frost-related processes, were only inherent to past greenhouse climate, [...]“

Author response: Commas will be added.

Lines 424-426: please check the sentence „[...] we conclude that early Eocene PA is not impacted by non-ice feedback mechanisms that act on 10^4-year timescales or longer.“

Author response: Will be changed to: " Eocene PA is dominated by non-ice feedback mechanisms that act on 10^4-year timescales or shorter

Specific comments (Supplementary material):

In my opinion the supplementary material present information that is key to a full appreciation of the work presented by the authors. In my opinion the text on pages 2-6 is actually very relevant for a deeper understanding of the work. In particular section 1.1, but also the other sections, would in my humble opinion fit well into the manuscript text. If there is no good reason to put this text into a supplement, I would suggest to move it to the manuscript. I think the description of employed model output (lines 154-159) should really be presented in a data section to make the link between proxy data analysis and climate modeling more clear in the manuscript.

Author response: We agree with this point by the reviewer. We will move all the supplementary text to the main text, including the section on TEX_{86} and model outputs. Additionally, we will change "early Paleogene" to "(early) Eocene", to be consistent with the main text, as this encompasses all the intended stratigraphic range.

In this context please highlight that the simulations by Lunt et al. (2021) represent climate states of the early Eocene climate optimum (EECO, ~ 50 million years ago). Is there any need to „extrapolate“ results derived from these simulations to different periods described in the manuscript, in particularly an early Paleogene climate (I am not quite sure about the definition of early Paleogene)?

Author response: Our dataset covers the onset of the EECO (*ca.* 53 – 49 Ma, e.g. Westerhold et al. 2018, *P&P*), and match the target of DeepMIP simulations that we compare to (i.e., the boundary conditions regarding ice sheets and continental configuration).

Line 22-24: I think the text is potentially ambiguous, should it read as follows?: „Following the original linear TEX86-sea surface temperature (SST) calibration (Schouten et al., 2002), subsequently proposed calibrations include linear (O’Brien et al., 2017) models, including a spatially varying Bayesian approach ('BAYSPAR') (Tierney and Tingley, 2014), and as well reciprocal (Liu et al., 2009) and exponential (Kim et al., 2010) models.“

Author response: We will change it accordingly.

Line 77-82: Assuming for a moment that the thickness of the early Paleogene mixed layer might have been generally different from today, for example as a result of different intensity of stirring of the upper ocean layers due to invigorated atmosphere dynamics: how would a different water column structure of the early Paleogene tropical Pacific, in particular a different thickness of the mixed layer, impact on the calibration of the target depth, and potentially on results and inferences drawn by the authors in this work? There seems to be evidence that a different thickness of the mixed layer depth cannot be excluded (Quillevere and Norris, 2003; Barnet et al., 2020). Would the impact on peak integrated GDGT source depth be relevant, or are there indications that the effect would be negligible?

Author response: We thank the reviewer for pointing this out, the exact thickness of the mixed layer is an uncertainty. We implicitly included this uncertainty in our analyses by taking a conservative depth range of GDGT export and by using two TEX_{86} calibrations (TEX_{86}^{H} for SSTs, and $SubT_{100\text{-}250m}$ for SubTs) that together more than encompass the uncertainty of export depth.

We consider that most of the dominant GDGT export was likely from between 50 and 150 meters water depth, particularly because the mixed layer depth, especially in the tropics, is typically shallower than 50 m. GDGTs are typically not found in abundance at depths above the nitracline (e.g., Hurley et al. 2018 (*OG*) because the producing organisms are relatively sensitive to photoinhibition and generally outcompeted (Merbt et al. 2012 (*FEMS*). Two calibrations are used in our work, one at the surface and one between 100 and 250 meters depth, to account for production-depth related uncertainties and to obtain a range of TEX_{86}-temperature relationships. This will be further addressed and clarified in the discussion on the depth of the TEX_{86} signal.

Line 90: fix brackets of (Ho and Laepple, 2016)

Line 114: fix brackets of (Kim and O'Neil, 1997)

Line 155: fix brackets of (Lunt et al., 2021):

Line 175: fix „Dashed red dashed line"

Author response: All brackets and textual errors mentioned by the reviewer will be fixed.

Line 188: define CIE

Author response: Because the Supplementary Text, "CIE" will be introduced before this passage.

Line 196: Is there a specific reason for bold-typesetting the two publications? Fix brackets of (Miller et al., 2020).

Author response: This was unintentional and will be fixed.

Line 202: captialize after (c) and fix formatting of d13Corg

Line 211: Fix formatting of (Cramwinckel et al., 2018; Frieling et al., 2019; This Study) (this text is dashed underlined, it is not clear whether this is on purpose)

Author response: The mentioned textual and formatting errors will be fixed.

Line 208: Fig. S7 (a-d): Do I correctly interpret that all data points from this site (grey) and from this study (black triangles), are located exactly on the calibration line in subfigures c and d, without any kind of deviation that is apparent to the eye? My apology if I overlooked something obvious, but is there an explanation for this fact? I suggest to give the details of the calibration model, e.g. r-value and fit equation as done for Fig. S1.

Author response: For paleo data (grey and black points), SSTs can only be obtained by applying the calibration model to the TEX_{86} values, hence that they will always fall on the calibration line.

We agree that this may not be intuitive and will clarify this in the caption of Figure S7. Paleo-TEX_{86} data were plotted alongside the present day coretop data, to illustrate the respective TEX_{86} data ranges and the potential issues with extrapolation. Further details on the calibration models (the linear

calibration model from O'Brien et al., 2017 (*ESR*) and the exponential model from Kim et al. 2010 (*GCA*) will be given in the figure caption.

Line 225, Fig. S9: Black and grey dashed lines in a, b, and c look the same to me (insufficient color contrast). Are different colors needed here? If not, just use one color.

Author response: The figure and caption will be updated to show only one color.

References:

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