

Dear Editor,

We thank reviewer #2 for their comments and suggestions on our paper. Below, we discuss these point-by-point and indicate how we intend to adapt the manuscript in a revised version.

Sincerely, also on behalf of my co-authors,

Carolien van de Weijst

Summary:

The manuscript presents a new 15 Myr TEX86 record from ODP Site 959 in the Gulf of Guinea. Comparing the TEX86 record to other proxies of the same core and other sediment cores, it investigates the source of the recorded TEX86 variability and concludes that for this site, TEX86 is mainly a subsurface temperature proxy. Applying a subsurface calibration, it then discusses the climatic implications of the record and suggests that the M2 glacial was marked by AAIW cooling during an austral summer insolation minimum, and that decreasing CO₂ levels were a feedback, not the initiator, of glacial expansion.

The manuscript is well written and fits in the scope of Climate of the past. The paper is twofold; Based on the multiproxy comparison / interpretation, it provides an attribution of the source for the Tex86 signal for the specific core and supports a rather controversial hypothesis; that the Tex86 signal could originate from subsurface temperature variations and would thus also have a different temperature sensitivity than assumed in most studies. In addition to this proxy attribution part, it also provides a paleoclimatic interpretation of the record which is largely independent on the calibration but hinges on the timing of the records.

I have only three comments and would recommend the manuscript for publication in CP

REPLY: We thank the reviewer for their positive evaluation and thoughtful comments.

- *The manuscript concludes that the Tex86 record for this site should be interpreted as subsurface temperature (SubST) and provides a range of convincing arguments supporting this interpretation (magnitude of variations, similarity to benthic records, magnitude of trend, high GDGT 2/3 ratio). Despite this, in large parts, the manuscript clings on the SST interpretation and discusses Tex86 SST and shows Tex86 SST (Line 137, Line 164, Figure 2,3,4,5). At least, to me this is confusing and inconsistent as either the authors interpret the record as subsurface temperature and the SST interpretation is only used as a first guess before the depth attribution; or the authors interpret the record as SST... which would then change large parts of the manuscript and the conclusions. I would thus suggest clarifying that the first result is the Tex86H record (without calibration) and the calibration itself is an interpretation. As an example, in Line 135, instead of ‘Between 15 and 11 Ma, TEX86H-SST fluctuates...’, one could write “Interpreting TEX86H as SST proxy, the inferred SSTs (here called TEX86H-SST) fluctuate. For the figures, the authors should check if the SST interpretation is needed in all of the figures.*

REPLY: This is a good point and it was one of the points of internal discussion as we were drafting the manuscript. The intention here was to clarify that the assumption that TEX86 reflects SST leads to inconsistencies with all lines of evidence, to subsequently conclude that it cannot hold and subsurface calibrations are required. We will reconsider this presentation throughout the MS, but clearly need to plot TEX86-H to support paragraph 4.1.2.

- *In several occasions, (e.g. L25, L264, L315) the authors argue that the “depth-integrated TEX86 record can potentially be used to infer SST variability, because subsurface temperature variability is generally tightly linked to SST variability”. However, they also find that the time-series of the Tex86 record differs from the surface/thermocline temperature derived from other proxies and argue that this is due to the depth-integration of their record. Both statements seem contradictory. As support, the authors (L249) cite Ho and Laepple, 2016: “The ratio between temperature change in the surface and subsurface ocean is 1:1 when averaged across many sites and on longer timescales”. However, as the authors show themselves in their presented multiproxy records, this does not apply to any single site or on details as the phasing of temperature variations. It is also unclear why*

such a translation to SST would be needed as a subsurface proxy also provides important information on the climate dynamics; can be used to validate models etc. Thus, I would suggest removing the parts on the possible translation of SubST to SST or to provide arguments why the authors think such a conversion is possible and useful as the conversion is clearly not just an offset.

REPLY: Also this is a good point. Essentially, we aim to present this as an option for assessing SST based on TEX86, We consider it a valuable avenue to explore for the community and therefore prefer to retain it. We will, however, further clarify the set of assumptions that underly the approach, including the internal inconsistencies at Site 959.

- *One main argument for the choice of the calibration is the amplitude of the reconstructed variations. This is mainly demonstrated in Figure S1. As the choice of the calibration is a major result of the manuscript, I would suggest moving this Figure into the main manuscript.*

REPLY: This will be done