

## ***Interactive comment on “Late Paleocene – early Eocene Arctic Ocean Sea Surface Temperatures; reassessing biomarker paleothermometry at Lomonosov Ridge” by Appy Sluijs et al.***

### **Anonymous Referee #1**

Received and published: 28 April 2020

In this work, Sluijs et al. revisit the ACEX early Paleogene sedimentary record to better examine the past temperature estimates from their earlier studies (Sluijs et al., 2006; 2009). I find this study important for paleoclimate, especially those who use GDGT-based proxy (e.g. TEX86), since there have been numerous improvements in constraining the proxy (‘screening methods’ such as Methane Index, Rind Index, GDGT-2/GDGT-3 etc) during the last decade. This implies that some of the earlier reported temperature reconstruction might contain data that are biased or unreliable. In addition, the Arctic region is an interesting study area which was very different from today. Many paleorecords and proxy studies indicate the Arctic was an ice-free warm environment, exceeding > 20 oC in SST. However, many climate models face challenge

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in explaining the remarkably high temperature in the high-latitude region, unless the atmospheric pCO<sub>2</sub> level was extremely high (e.g. 16 times higher than pre-industrial level; Pagani et al., 2014). This connects to the concept of meridional temperature gradient which has an important consequence in heat transport, ocean-atmospheric interaction, and global climate in the past.

Although there are several scientific interpretations to be addressed in the manuscript, I will save those for the next version, assuming some of them will be fixed in the revision. But I would like to comment on the data analysis here, which I think it should be addressed first.

Sluijs et al. used the previously analyzed samples which were stored for over a decade. As I am interested in GDGTs, I was curious how the old and new GDGT data would differ, although I assume the offset would be small if stored properly and measured in good condition of the HPLC/MS. Figure 3 shows the result and regression analysis between the old and measured GDGTs results. Both TEX86 and BIT look comparable. However, I found that there are few outliers in the TEX86 dataset from the supplementary data. I plotted all their new vs old TEX86, and the R<sup>2</sup> value is lower to 0.66. Still comparable statistically, however, the authors did not mention about the outliers.

I appreciate the authors for providing their valuable dataset and kindly included the spreadsheet calculation for the readers to follow. For RI (ring index), however, I found that the calculations were all missing while it can be calculated from the dataset. I calculated again from their data but the values were slightly different. The maximum difference between the reported value (column BX) and the calculation I did is up to 0.11 RI unit. Although the difference is small, this would impact on some of the samples that have  $\Delta$ RI near 0.3, screening whether the data is reliable or unreliable near its cut-off value.

Overall, I suggest a moderate revision of the manuscript, especially in the data analysis first, before it can be accepted by CP. Also, the manuscript contained plagiarism (line

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160-163) and many run-on sentences which made it difficult in absorbing the information when reading, therefore, I suggest a more improvement in the scientific writing for the next version.

Some specific comments are below:

Line 20-21: add “ACEX”

Line 20-52: the abstract seems to be too long and includes too much information of the study results in detail. Also, line 46-50 is just copied and pasted here from the main text (line 806-810).

Line 37: the background SSTs in early Eocene generally exceed

Line 71-77: run-on sentence: divided into two sentences

Line 77-84: I understand citing all the references to supplement, however, 17 citations are too overloaded in one sentence for the reader. I suggest organizing the citation to where they would belong. For example, link and cite Pagani et al. (2006) with “molecular fossils” which examined the  $\delta D$  of n-alkane addressing the hydrologic cycle. Or breakdown the sentence and cite only the important references.

Line 160-163: this sentence should be rephrased. It is exactly the same as written in Hollis et al. (2019) describing the BAYSPAR, but one word added here (plagiarism).

Line 169: add the TEX86 value range of which converted SSTs differs between linear & non-linear

Line 190-191: I suggest to remove “based on high BIT index value” and add the range of BIT results from the study after the equation.

Line 207: specify the GDGT. If just GDGT, does it mean both iso- and brGDGTs?

Line 219-224: add the depth range of the deep contribution (Talyor et al., 2013) and also the reconstructed water depth of Site M00004A, meaning shallower shelf environ-

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ment, to connect the interpretation of negligible deep source.

Line 233: use “[Crenarchaeol isomer]” for consistent compound name in all equations. this also implies for the names throughout the paper.

Line 234: “significant presence (or contribution) of anaerobic methanotrophy”

Line 242-243: provide references

Line 251: “Crenarchaeol isomer” for consistency of the compound name throughout the paper.

Line 299: I would rather suggest starting with ‘brGMGTs’ and supplement that this was previously reported as H-shaped brGDGTs, since the former is the major compound referred throughout the manuscript. Also, I suggest removing any description of ‘H-shape brGDGTs’ afterwards, as it makes it more confusing.

Line 344: the precision of TEX86 unit or converted SSTs unit?

Line 351-353: same comment with line 344. In addition, I am confused with what “both” labs means.

Line 409: interval should be between 371.0 to “369.0” mcd, based on Figure 4 and Sluijs et al. (2009),

Line 416-417: add the linear regression line in Figure 4 and supplement what “explaining 26 % of the variation” means

Line 428: I suggest to cite “Figure 6” in the first sentence, so the reader can easily compare the visualized data with the text, starting from the beginning of section or paragraph.

Line 442-452: Rather than directly moving on to the discussion of the method and result, I suggest to add a brief explanation of what lignite is and why lignite was used as the representative of terrestrial source for the readers to easily understand the concept.

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Line 445: supplement how the absolute concentration is calculated (e.g. what standard used).

Line 467: “GDGT-2 and -3”. Suggest describing the compounds be consistent throughout the paper.

Line 473-478: This is true based on the isoGDGT distributions of Paleogene lignite. The reported lignite samples’ paleolatitudes are located within 57 °S to 48 °N, outside the Arctic region. Is there any lignite record from the Arctic that could be a more direct source to constrain the isoGDGTs distribution? If not, then how can this anomalous abundance of terrestrial isoGDGTs be explained in the Arctic where terrestrial input (especially from peats) is highly suggested while it has not been recorded elsewhere?

Line 486-487: add the threshold value of GDGT-2/Cren (Weijers et al., 2011), as it is shown as MI’s cutoff in the following.

Line 492: I suggest the authors add a short interpretation of why these biomarker results are contrasted to the suitable depositional environment for abundant anaerobic archaea (methanotrophy and methanogen) which they indicated in the beginning of the section.

Line 508-510: interpreting BIT index with a distal position from the shoreline is problematic. Even in coastal marine or lacustrine settings, the BIT shows a large variation (Hopmans et al., 2004). Is the change of position interpreted from sea-level rise, similar to Sluijs et al. (2006)? Then what caused the sea-level rise (thermal expansion?) while the temperature proxy does not indicate significant warming?

Line 591: suggest the citation as “Figure 7b”. This applies to other figure citations in the text to be more specific, when available, rather than just citing the whole figure. Another example is - line 606 to change to “Figure 7d”

Line 633-635: suggest to divide the two methods with (1) and (2), which the dashed line makes it confusing, and remove the linear/non-linear calibration description since

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these are already explained previously.

Line 739: I find “lower temperature mean annual air temperature” very unclear.

Figure 1: (1) the word ‘using’ is used repeatedly – remove or organize with a different word (2) add gplate webpage link for the readers and reference (3) describe or indicate what the brownish lines in the map

Figure 2: (1) I suggest removing GDGT-4 since it is not discussed in the text nor measured in this study (see supplementary spreadsheet). Moreover, GDGT-4 is generally not included when calculating the relative fraction of isoGDGTs among the whole isoGDGTs pool. (2) add Crenarchaeol regioisomer’s structure or note together with the Crenarchaeol (3) suggest changing “chemical structure” to “molecular structure”

Figure 5: (1) describe the “modern peats” into two in the caption. (2) describe what the box and line, error bar, circles indicate (3) add the number of samples for statistical meaning

Figure 7: (1) I suggest 7d and 7e switch the order, since it is the

Supplementary material Data table: (1) a lot of blanks in the sample data, as well as an unknown words or sample core names below the data seat (see row 153-157). (2) in “iGDGTs in peats” sheet, cite the references (3) in “Lignite crenarchaeol”, Sluijs et al. reported the GDGTs (iso- and br-) data originally from Naaf et al. (2018) and their newly measured ‘Cren. Isomer’. Here, I suggest the authors to report the other iso- and br-GDGTs abundances (here which I assume is HPLC/MS integrated peak area) together since they clearly mentioned in ‘Material and Methods’ that they re-analyzed the polar fraction of the lignite samples. Although I expect that this will not significantly change the result, still comparing only the newly measured ‘Cren. Isomer’ with reported GDGT dataset is not acceptable. This is because even measuring the same sample in the same method, the peak area can be different among interlaboratory measurements, the analytical parameter of the analytical instrument etc. In addition,

I suggest to add the calculations and results of the 'fraction of isoGDGTs' in all lignite samples. Lastly, minor comment on style of the table (e.g. missing cell borders, missing compound names) to be consistent. Describe 'n.d.' and 'b.d' too.

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