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Climate of the Past Discussions

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Interactive comment on "Microbial Membrane Tetraether lipid-inferred paleohydrology and paleotemperature of Lake Chenghai during the Pleistocene-Holocene transition" *by* Weiwei Sun et al.

Anonymous Referee #2

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In their paper, Sun and coworkers have aimed to quantitatively reconstruct lake level and paleotemperature for a short interval during the Pleistocene-Holocene transition based on tetraether membrane lipids in the sedimentary record of Lake Chenghai. With their record, the authors want to contribute to our understanding of the Indian Summer Monsoon, for which only few records exist.

Although I appreciate the intention and effort of the authors – we are indeed in need of quantitative records of (past) ISM climate dynamics- the paper will need more work before I can recommend it for publication in CoP.

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Main comments: - Proxies. The authors use a suite of proxies based on GDGTs, such as TEX86 for temperature, the BIT index, %cren, cren'/cren, GDGT-0/cren. However, the proxies and the mechanisms underlying the proxies are only poorly introduced and explained, if at all (e.g. cren'/cren results are presented (L199) but the ratio is not mentioned in the introduction). Also the interpretation of the proxy data and the assessment of the applicability of the TEX86 proxy, and thus the reliability of the produced temperature record, is very marginal and should be improved.

- Structure: This comment may already resolve part of my comment on the proxies, as some of the explanation is presented in the discussion rather than in the introduction. Actually, most of section 4.1 consists of a literature overview of the proxies. This should be moved to the introduction. Instead, use the discussion to actually interpret and discuss your own data. This is also true for the other sections of the discussion.

- Lake Chenghai: In order to interpret the GDGT data it is important to provide some more details on the modern lake. Please add basic information on the lake type (i.e. mixing regime), nutrient status (ammonia!), oxygen content, etc, and possible links to climate (e.g. is mixing related to windiness or precipitation, or. . .?).

- Lake level reconstruction: The authors use %cren and cren'/cren to reconstruct the lake level over time, for which they assume that crenarchaeol will be produced more during lake highstands, and less during lowstands. This is in turn linked to mixing of the lake, where more mixing is related to oxic conditions, supposedly occurring during low lake levels. In order to go with this interpretation it is crucial to understand the production of crenarchaeol in lakes, for which you need to discuss the exact niche of crenarchaeol-producing Thaumarchaeota in the lake water column. Several studies have shown that they primarily occur just above the oxycline (as correctly reported in L238). This means that the position and the stability of this oxycline is very important for the amount of crenarchaeol that is produced in a lake. Hence my request for more information on the mixing regime of Lake Chenghai. For example, Buckles et al. (2013, Environmental Microbiology) hypothesize that crenarchaeol is mainly produced during

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stratified water column conditions in Lake Chala (Africa), as mixing results in an oxygenation of the water column, disturbing the niche of the Thaumarchaeota. However, increased mixing (occurring with lower lake level) has also been coupled to enhanced nitrogen recycling, enabling higher production of crenarchaeol (Sinninghe Damsté et al., 2012, QSR). The latter hypothesis is opposite to the assumptions made by Sun et al, who couple low %cren for a shallow lake status with more mixing. Hence, the rationale behind this proxy (and the final interpretation) clearly needs more discussion, for which several other studies on the production of crenarchaeol in lakes could also be taken into account (e.g. Woltering et al., 2012 GCA, Blaga et al., 2012 GCA, Kumar et al., 2019 Org Geochem). And, importantly, how does this relate to the mixing regime in Lake Chenghai? Furthermore, the high GDGT-0/cren values between 15.4-14.4 BP are interpreted as methanogenic activity (L284-286), which would imply anoxic bottom water conditions and thus stratification/reduced mixing. According to your own interpretation of %cren (L235-248) this would imply high crenarchaeol (no mixing is linked to lake highstands), but Fig 3c shows that %cren is low in this interval. I think that this needs a re-interpretation.

- Applicability of the TEX86 and sources of isoGDGTs: Most TEX86 records so far are based on isoGDGTs in large, oligotrophic lakes. How does this relate to the nutrient status in Lake Chenghai? Secondly, the assessment of sources of isoGDGTs may need some more attention. The authors point to soils as additional source of isoGDGTs when the cren'/cren ratio increases, as well as when the BIT index increases. Note that an relative increase in cren' can indeed be attributed to group I.1b Thaumarchaota, but to those occur in deeper water layers, not necessarily in soils (e.g. Kumar et al., 2019 Org Geochem – they find that cren'/cren ratios vary between 0 and 0.12 in the modern water column of Lake Malawi, which is the exact same range as found in the sedimentary record of Lake Chenghai). In addition, it should be considered that the BIT index in lakes can no longer be interpreted as a proxy for soil input. Over the past years it has clearly been shown that the vast majority (if not all) brGDGTs in lakes are produced in situ, and that the amount of production varies per season and between years. Check Loomis et al., 2014 GCA, Weber et al., 2015 GCA, Weber et al., 2018 PNAS, Colcord et al 2015 Org geochem, Colcord et al., 2017 Org geochem , Buckles et al., 2014 GCA. The BIT index is basically an indication of crenarchaeol and/or brGDGT production in the lake. As BIT is a ratio, both cren and brGDGTs can drive changes in BIT, which can only be assessed with absolute abundances of the GDGTs. Without these data any changes in BIT should be interpreted with care. Since BIT and GDGT-0/cren practically show the same trends in Lake Chenghai (Fig. 3) it can be assumed that these changes are caused by changes in cren rather than brGDGTs and GDGT-0. So instead of enhanced soil input, the absence of crenarchaeol production then explains a high BIT (and high GDGT-0/cren) in the interval from 6-14ka. It is up to the authors to find an explanation for the limited/disturbed niche of the Thaumarchaeota in the water column (outcompeted? Ammonia depletion?).

Also, if BIT is so high (>0.5) that application of the TEX86 is limited, then why did the authors not attempt to use brGDGT-based paleothermometry?

Minor comments: L52: Introduction ;) L82 ff: take more time to introduce the proxies here and to explain their underlying mechanism(s). L89: index is not reliable in small lakes – mention why not? L91 ff: explain this better. Also elaborate on the link between lake level and depth of the oxycline. L141: was any standard added for GDGT quantification? L180: Castaneda and Schouten 2015 is not correctly listed in the reference list. Please check. L189: it would make sense to start with presenting the age model. If these are not your results (it seems like they are already published?), then add a brief description to the methods. This also allows you to already indicate the position of H1 and the YD in your record. L199: This is the first mention of the crenarchaeol'/crenarchaeol ratio. Include this in the introduction (if you want to use it)! L204: GDGT-0/cren ratios >2 generally indicate anoxic bottom water conditions. Is this also visible in the lake core? Is the interval with values >2 also laminated? Such an easily obtained visual aspect of the core can be used to confirm/strengthen your interpretation of the GDGT record. L219: the title of this section suggests a discussion of all proxy records, however, it mainly comprises a literature review that focuses on aspects that may or may not affect the applicability of the TEX86 proxy. Hence, content does not fit the title. All proxy description should go in the introduction, and this section should focus on the data presented here. Interpretation of the data may be more thorough and critical. As illustrated in my main comments there may be multiple explanations for certain trends in the proxy records (e.g. BIT, influence of mixing regime on cren production) that need to be evaluated here. L221: instead of citing the GDGT review by Schouten et al., 2013, refer to the original paper instead, giving credit to the right people. L241-248: pay special attention to linking %cren to high- and low lake levels, as there are multiple ways to explain cren production in lakes. Think about the niche of the Thaumarchaeota and the mixing regime of the lake, and how this is related to climate. L253: as outlined above, the BIT index can no longer be linked to soil input. There is too much evidence for a primarily aquatic source of brGDGTs in lakes. See suggested references in main comments. Also note that brGDGT production in lakes takes place in the anoxic part of the lake. Hence, high BIT could be coupled to stratified water column conditions and reduced mixing. Check if this coincides with the concentrations of GDGT-0 and potential lamination of the core. L267: check the cren'/cren ratios and associated DNA analysis in the water column of Lake Malawi (Kumar et al., 2019, Org Gechem). They reach values up to 0.12 without soil input. L283-286: see earlier comment on the contradiction between high GDGT-0/cren ratios implying anoxic bottom waters and reduced mixing and low %cren supposedly indicating more mixing due to a lowstand. L327: different responses in GDGTs between H1 and YD, where similar climatic conditions are expected, should be better explained. Also take into account that not only temperature changed during the YD, but that also windiness and precipitation varied. All these parameters have different effects on the GDGT signals in the lake. L336: How realistic is the reduced sensitivity of %cren at high lake level? How much variation in lake level do you expect? The %cren in your record varies between 0 and 60%, which would correspond with a lake level change of ∼1000m based on the relation of Wang et al 2019. Is this feasible? L344: I will refrain from providing

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detailed comments on the spatial context of the record, as there are currently too many aspects about it that are not well known or explained. In a next version however, do pay (more) attention to differences between the records and what they mean (are they really caused by climate or are they caused by comparing different proxies that record not exactly the same).

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