Dear authors,

I would like to thank you for substantially revising your initial manuscript, taking into account most of the reviewers' comments and suggestions. I agree with them that you have done an excellent job with your revision. However, there are still some minor points that the reviewers, at least two of them, would like you to improve, asking to check this final revision. This is why I have selected "major revision", as this is the only option that the Copernicus system allows me to involve these two reviewers without going back on the scientific merit of your revision. Please take these final adjustments into account in this latest revision.

I look forward to reading from you.

All the very best

denis-didier Rousseau

CP co-editor in chief

Dear editor, dear Prof. Rousseau,

Thank you for your positive evaluation of our revision. Please find enclosed our revised manuscript titled "*Towards quantitative reconstruction of past monsoon precipitation based on tetraether membrane lipids in Chinese loess*". We thank Dr. David Naafs and two other anonymous reviewers for their second-round comments and your invitation to revise our work based on their feedback. We have followed most of their suggestions and have included the concentration of iso- and brGDGTs to Fig. 2 in the manuscript as suggested by Reviewer #1, and the CBT′ and iso-GDGT related proxies (i.e., $R_{i/b}$ and BIT) to the supplementary figure as suggested by Reviewer #1 and #2. All changes in the manuscript are made with track changes on.

We hope that you find this revised version suitable for publication in *Climate of the Past*.

On behalf of all co-authors,

Jingjing Guo

Referee comments #1 (Dr. David Naafs):

Main assessment

-I like the new title

Reply: We thank Dr. Naafs for his positive evaluation of the new title.

-The isoGDGT and CBT' and other indices as shown in figure 1 of the reply letter should be included in the main manuscript and not hidden in the supplementary information. Exploring this data is informative and provides further insights into the use of biomarkers in loess.

Reply: We thank Dr. Naafs for his comment. However, as we explained in our previous reply, CBT' is a proxy that includes changes in both the degree of cyclization and isomerization (see Eq. 1), and therefore does not allow us to identify which adaptations in the molecular structure of the brGDGTs is driving changes in the CBT'. From our Fig. 2 in the manuscript it is evident that DC and IR show different trends in the Yuanbao section, and thus likely respond to different drivers. Therefore, we prefer to keep our focus in the manuscript on DC and IR. Notably, as suggested by Reviewer #2, we have added a figure showing the records of CBT' and isoGDGT-related proxies (i.e., BIT and $R_{i/b}$) to the Supplementary materials.

$$CBT' = {}^{10}log[(Ic + IIa' + IIb' + IIc' + IIIa' + IIIb' + IIIc')/(Ia + IIa + IIIa)] \quad (1)$$

-The reply letter states that GDGTs were not detected in wind-transported dust. Although this was shown in Hopman's 2004, a later publication (Fietz et al., 2013, doi: 10.1016/j.orggeochem.2013.09.009) shows that GDGTs can be transported by dust. So this part of the manuscript needs to be revised with taking dust-transported GDGTs into account.

Reply: We thank the reviewer for their comments and providing relevant references. We have added this reference to the manuscript (Line 105-106). Although this reference shows that aeolian transport of brGDGTs is indeed possible, it should be noted that the concentration of brGDGTs in the source region of the dust that accumulates on the Chinese Loess Plateau is below the detection limit, as reported by Gao et al. (2012). Therefore, brGDGT signals in loess-paleosol sequence can be interpreted to reflect the local climate conditions.

-Add GDGT concentrations to the main manuscript as shown in reply letter fig. 2

Reply: We thank Dr. Naafs for his comment. We have added the concentration of iso- and brGDGTs to Fig. 2 of the manuscript and included the description in the Results. Line 186-187.

-As the LR04 is used for tuning, there is some circlular reasoning here. The LR04 stack is tuned to astronomical cycles, so when you tune your record to the LR04, you will be default get astronomical cycles. This caveat needs to be acknowledged. I dont say that this will explain all variance, but it will contribute.

Reply: We thank Dr. Naafs for his comment. Tuning to LR04 can indeed lead to circular reasoning. However, it is important to note that for our age model, only the loess/paleosol boundaries are tied to glacial/interglacial transitions in the LR04 record. The rest of our age model primarily relies on millennial-scale events that are aligned with speleothem records, which benefit from absolute dating methods (Cheng et al., 2016), and is further independently supported by OSL dates from a nearby loess/paleosol section (see Fuchs et al., 2023 for details).

Regarding the spectral analysis results and our discussion on different drivers of loess proxies (100-kyr cycle) and the degree of cyclization (23-kyr cycle): these different proxy records are derived from the same core and analyzed on the same material. In the theoretical case that the 100 kyr cycle is an artifact of the link to the LR04 record, the shorter cycles are truly present. Besides, the presence of astronomical cycles in loess proxy records is not the novelty of our study, but has long been recognized in loess/paleosol sections that were independently dated using paleomagnetism, magnetic susceptibility, grain size, and OSL (e.g., Heller and Tung-Sheng, 1982; Kukla et al., 1988; Stevens et al., 2018). Nevertheless, we have added a line on the possible introduction of a 100 kyr cycle in our records by using this approach. See Line 120-123 in the revised manuscript.

-I like the addition of fig. 5 from the reply, but what is not clear to me is what "small spectral density" means in the reply. It looks like the density of the IR record is significant for 23-kyr and around 30% of the density of the MS record. This needs to be discussed in the revised manuscript.

Reply: *We agree that IR shows 23-kyr cycle in the spectral analysis. However, we have decided not to elaborate on this information in the revised manuscript, as this record is also part of the discussion and interpretation of the* MBT'_{5Me} *record in a temperature-focused manuscript that is currently under review with Organic Geochemistry. More importantly, the climatic meaning of IR in this downcore remains unclear, and thus discussing astronomical cycles related to this proxy would not be pertinent at this stage.*

Referee comment #2:

The authors responded in an adequate way to the comments of three reviewers. I would only have a last suggestion left: while the decision is explained not to include qualitative proxies for precipitation (Ri/b and BIT index), I think that mentioning in the manuscript text that there is no response of these proxies on the timescale and environmental change-scale adds to our understanding of precipitation proxies in loess in general. The figure presented in the rebuttal could for instance be included as a supplementary figure.

Reply: We thank the reviewer for their positive evaluation of our revision. We agree that the addition of these proxies ($R_{i/b}$ and BIT) can be informative, and have added these panels to Fig. S3 in the supplementary information.

Referee comment #3:

Guo et al. have substantially improved their manuscript using the comments by David Naafs, Anonymous Referee #2, and myself. The authors' descriptions of applied changes accurately reflect those applied to the revised manuscript. To keep their manuscript to-the-point and focused on quantitative reconstruction of monsoon precipitation with brGDGTs, the authors maintained the focus on the degree of cyclization (DC) and isomer ratio (IR).

Overall, I am happy with the authors' responses and revisions. In my opinion, the authors provided valid reasons for keeping the focus on DC and IR. I thus see no reason to insist on the addition of isoGDGT-based proxies (e.g., BIT index and Ri/b) and other brGDGT-based proxies (e.g., CBT(′) and the precipitation index, PI). Instead, I only have a few, mostly editorial suggestions for further revision, and I would not need to read the manuscript again before publication in Climate of the Past: all line numbers refer to the clean version of the revised manuscript.

Reply: We thank the reviewer for their positive evaluation of our work and their careful check in the details. Please find our point-by-point response below in italic.

Detailed comments:

Main text

Line 51: Here, Fig. 3 is now first cited before Fig. 1.

Reply: We have changed this in the revised manuscript.

Line 259 [Eq. (3)]: As I stated in my previous report, the r value—the coefficient of correlation—is to be reported when describing correlations and I thank the authors for having followed my suggestion. However, when describing regression model results as done here, it is the R2 value—the coefficient of determination which represents the percentage of explained variance—which is to be reported rather than the r value. The authors may also consider reporting the R2 rather than r value in Fig. 4F—but not the other Fig. 4 panels.

Reply: We thank the reviewer for their detailed explanation of the difference between r value (the coefficient of correlation) and r^2 value (the coefficient of determination). We have changed and reported the r^2 value for the coefficient of determination in Fig. 4F, Eq. 3 and main text in the revised manuscript. Line 267 and 269.

Lines 340–341: Should be "(e.g., Clemens et al., 2010)", not "(e.g., (Clemens et al., 2010)Clemens et al., 2010)".

Reply: We thank the reviewer for their detailed check. We have corrected this in the revised manuscript. Line 346.

Line 360–361 (Data availability): I thank the authors for providing the PANGAEA and Open Science Frame links to their individual GDGT data—individual brGDGT and isoGDGT relative abundances—in response to David Naafs' comment. However, it would have been even better if the authors also provided individual GDGT peak areas and/or concentrations in μg g soil–1, unless the authors prefer to do so when their other manuscript under review with Organic Geochemistry is accepted or published.

Reply: We thank the reviewer for their suggestions. We have added the concentration of GDGTs into the supplementary file archived in the Open Science Frame. But note that the latest round-robin test has claimed that the comparison of concentrations of brGDGTs between labs still remains challenging, although quantification within laboratories was generally consistent (De Jonge et al., 2024).

Line 525: Should be "Sinninghe Damsté, J. S.", not "Damsté, J. S. S.".

Reply: We have corrected this in the revised manuscript. Line 529.

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