Climate of the Past Discussions

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# Comment on cp-2021-188

Anonymous Referee #2

Referee comment on "Greenhouse gases modulate the strength of millennial-scale subtropical rainfall, consistent with future predictions" by Fei Guo et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-188-RC2, 2022

The paper by Guo et al. submitted to CP is based on a new East Asian Summer Monsoon rainfall reconstruction from the northwest Chinese loess plateau over the last 650 ka.

In this study the authors address the following questions: i) is there a reliable proxy for East Asian summer monsoon (EASM) rainfall at the millennial timescale and ii) what are the factors modulating the millennial monsoon variability (MMV)? Overall the manuscript is clearly structured, well written and both topic and objectives are suitable for Climate of the Past.

Fist of all as I am not a specialist in "wavelet analysis" I will leave the evaluation of this approach to reviewers more familiar with statistical methods. On the other hand, as a geologist working on Loess-Palaeosol Sequences (LPS) for a long time, I am surprised (not to say displeased) by the complete absence of data presenting the loess and palaeosol record on which is based the present study.

This is a main concern and that alone would be for me a matter to reject this contribution. Indeed, this is the starting point of the study: before presenting and interpreting the variation in Ti/Ca ratio and grain size parameters along the 182 m of the Linxia record, the LPS itself should be exposed with a reasonable level of information. Even if the Linxia LPS was previously published in another paper by Guo et al in Catena (2021) this information should be provided in the present contribution because it is very important for the evaluation of the "age model" on which all the conclusions of the study are based.

The second main concern is indeed the "age model". As Reviewer 1 I will ask: « what is the implication of the age model errors for the wavelet coherence correlations that authors conducted (against GHG, ETP, Insolation and benthic $d^{18}O$ on Figure 4) and for the millennial-scale component extraction »?

Before to present and age model you should provide variations curves of climate proxies considered here regarding to depth (Ca/Ti and loess mean grain size). The age model proposed in the present contribution is directly extracted from the one published by Guo et al., 2021. Looking to Fig. 3 of this paper I can agree with the age-depth relation proposed for the last 60 ka where OSL dating are available but concerning the part of the record older than the Last Glacial no absolute data are provided and the age model is thus highly speculative.

The age-model is classically build using "tie points" that can be selected by "matching the loess (L)/paleosol (S) boundaries to the glacial/interglacial transitions ». This is the classical approach but they should know that it is only reliable if: 1) the sedimentation rates are more or less regular through time during each glacial period and 2) no erosion hiatus occur in the record … This is not the case for the Linxia LPS according to Fig. 3 published by Guo et al. 2021.

For example it is strange to note that the sharp boundary in MGS and MS data occurring at the base of L2SS2 soil (-60m) has no counterpart in MIS stratigraphy. Following the correlation methodology exposed above (tie points) this major limit would rather have been correlated to the base of MIS 7 interglacial and thus dated at about 220 ka and not at about 175 ka according to the present scheme. In addition why the Last interglacial (MIS 5e) is only marked by a short and relatively not intense peak in both MGS and Ca/Ti curves (SI SS3) whereas it is generally represented by a thick and well developed soil horizon in LPS?.

In addition the period corresponding to MIS 3 (± 60-30ka) exhibits only two weakly developed soil horizons (L1 SS1 and L1 SS2). These soils are likely corresponding to composite (upbuilding) soils developed over quite long periods (± 40-26 ka for L1SS1 and 60-45 ka for L1SS2) and thus including numerous DO events. So, the response to millennial timescale climate variations is clearly not recorded in the stratigraphic signal. In addition many of the peaks in proxies are so thin and of so small amplitude (MGS variation ≤ to 3 mm) that interpreting them as the result of a DO event is very difficult to support.

Finally the authors seem to ignore that millennial time-scale climate variations have been evidenced for more than 20 years in European loess series (e.g. Antoine et al., 2001 (QI), 2009 (QSR); Moine et al., 2008 (QI); Rousseau et al, 2002 (QSR), 2017 (QSR); 2020, (CP); 2021 (QSR) and that they have been definitely dated and correlated with NGIP record using $^{14}C$ dating (Moine et al., 2017(PNAS) and Ujvary et al., 2017 (PNAS).

Conclusion: The authors must answer to the major comments exposed above to demonstrate that the main conclusions of their paper can be fully supported by data before publication.