Climate of the Past Discussions

Clim. Past Discuss., referee comment RC1
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# Comment on cp-2022-14

Anonymous Referee #1

Referee comment on "Accurately calibrated X-ray fluorescence core scanning (XRF-CS) record of Ti□/□Al reveals Early Pleistocene aridity and humidity variability over North Africa and its close relationship to low-latitude insolation" by Rick Hennekam et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-14-RC1, 2022

Hennekam et al. provide a new calibration of XRF-CS derived Ti/Al measurements from Mediterranean core ODP 967 – a key site for the study of Plio-Pleistocene Saharan climatic variability. This is an important record which provides additional evidences for the timing and intensity of wetter/drier periods in the Sahara and the potential global/orbital controls of these fluctuations. The article is well written, with little grammatical revision required to the main body of the text. I believe this article asks two key questions: 1) how best can non-destructive and destructive geochemical methods be combined to provide an accurate record of past climatic variability? And 2) what can this new record inform about the long-term orbital influences on Saharan climatic variability throughout the Early Pleistocene to Mid Pleistocene?

The major strength of this manuscript is that it offers a valuable method to mitigate loss of material though WD-XRF analysis by instead selecting fewer (1060) samples to calibrate a non-destructive XRF-CS record (8497 samples). This permits a higher resolution Ti/Al record to be produced. However, I have a few concerns with this section.

- I believe the authors would benefit from emphasising the novelty of their study more clearly. Currently, on the basis of the text, it does not seem entirely clear how this calibration and XRF-CS record differs from that of Grant et al. (2022). Did the authors obtain new Ti/Al measurements? Or did they use those of Grant et al. (2022)? Similarly, did Grant et al. (2022) use the same WD-XRF dataset (Konijnendijk et al. 2014, 2015) to calibrate their record? Is this study using the same data and method as Grant et al. (2022), and simply testing how many samples are needed for accurate calibration? The authors must make the last two paragraphs of the introduction (and the materials and methods section) much clearer so that readers can establish the data output of this study.
- The results table (Table 1). Instead of a Y or N value to indicate whether the null-hypotheses have been rejected, the authors should provide the P-value and test

specific values. This could be included in supplementary material rather than the main text, but they must be accessible for researchers. Additionally, the authors need to account for the "multiple comparison problem" by adjusting the ð☐☐☐ value

- It is necessary for the authors to better explain why 53 samples are required for accurate calibration, and why, if this is sufficient, the 1060 sample calibration record is favoured for the subsequent discussion. I understand that it is necessary to reduce the number of samples to achieve the authors aims. However, I believe the justification for this amount is unclear as the test specific results have not been made available.

For the high-resolution XRF-CS Ti/Al analysis and correlation to orbital records, I would like to first say that I am generally supportive of this analysis. The authors provide a detailed insight into the varying controls of orbital parameters on African wetter/drier periods. Unlike hematite dust transport, Ti/Al ratios provide a method to study the intensity of wetter/drier periods. Their statistical analysis and interpretation, that high-latitude forcing played an increasingly dominant role after the Mid Pleistocene Transition, appears reasonable and well argued. However, I believe this section needs further work and clarification/justification.

Firstly, the application of a 401 kyr window running correlation (long eccentricity band), based on the text, does not seem justified to the reader. Why was this running correlation window selected? The authors must explain why such a large window is necessary and crucial to their analysis and interpretations.

Secondly, as can be seen from the very well-made figures, the 95% confidence intervals (while they do represent extremes) are large and, considering this, there is some uncertainty when distinguishing the shift from low to high running correlation between >1.2 and <1.2 Ma. This is more of an issue for the correlation with sea-levels. Additionally, the claim for constant high correlation with sea-level after the MPT is not so clear; it appears that higher correlations exist from about 1.7 Ma, with an abrupt dip at 1.1 Ma, after which the high correlation returns. Perhaps the authors could perform a t-test of running correlation values between these two periods to test for significant differences? Furthermore, both the correlation with insolation (is this SITIG, 65N, 35N or 15N? Please clarify on figures) and sea-levels timing may benefit from further investigation using ChangePoint analyses. If using the R statistical software package, this can be achieved with packages such as BCP or ChangePoint. This may result in slightly different ages identified for these changes, but combined with the current analysis, would add an additional line of support to the authors argument. In either case, I believe that, while there is a deal of statistical uncertainty, the authors analysis provides important information for understanding the orbital controls on Saharan wetter/drier periods throughout the Pleistocene.

While I am supportive of their analysis, the authors may benefit from additional reference to various studies which describe the suppressive effects of glacial termination melt-water discharge on low-latitude forcing during the Middle and Late Pleistocene, causing monsoon intensification to lag insolation (e.g., Marino et al., 2015; Menviel et al., 2021; Häuselmann et al., 2015; Böhme et al., 2015). While most of these studies are limited to the LIG or Holocene, this may provide an additional line of support for some of the authors arguments.

I recommend that this paper be published in Climate of the Past subject to the authors addressing the concerns and the few grammatical/technical notes below. I suggest minor revisions as 1) results of the statistical testing and consideration of the "multiple comparison problem" (this may have some impact on the results, but is difficult to estimate without seeing the test specific results); and 2) the interpretation/discussion needs further analysis and justification to support these arguments, and currently the novelty is not well emphasised. However, I believe that this work will make a valuable contribution once these concerns are addressed.

Technical/grammatical notes:

Line 37-39: References. The authors may benefit from adding a few references to palaeoanthropological/archaeological outputs and discussions, that are not necessarily climatic research initiatives, to highlight the broader relevance of their work. (E.g., Potts et al. 2020; Groucutt et al. 2015)

Line 58-86: The last two paragraphs of the introduction. I believe these paragraphs are, in short, saying "As WD-XRF is destructive, how many samples are required to accurately calibrate a non-destructive XRF-CS record?". The authors may benefit from revising these paragraphs to emphasise the aims of the manuscript more concisely (or perhaps directly). Maybe this is due to my unfamiliarity with the methods, but it took me a few attempts to work-out the novelty of this article, as Grant et al. (2022) is described as having conducted a very similar WD-XRF calibration of an XRF-CS record for the 5 Ma period of the core. The paragraphs must emphasise the novelty of this study more clearly.

Line 172: There have been various comments that WD-XRF analysis is more precise/better established than other methods. Can the authors provide further quantification of this?

Line 223-224. The authors may wish to add a comment on the work of Tzedakis et al. (2017). Nature, 542: 427-432.

Table 1. Please include the results of the statistical tests either here or in supplementary material.

Fig. 2 and caption. "XRF-bead". Perhaps change this to WD-XRF-bead for clarity?

Fig. 3 may benefit from the addition of correlation coefficients of the XRF-CS Ti/Al record and the respective humidity/aridity records from ODP 967.

Fig. 4g. Please clarify if insolation is the SITIG, 65N, 35N or 15N.

Fig. 5c. The figure may benefit from a dashed line running horizontally from 0. This would allow the reader to track changes more easily in the correlation.

Figures. (not necessary). The cyan text may benefit from being a few shades darker.