Climate of the Past Discussions

Clim. Past Discuss., referee comment RC2
https://doi.org/10.5194/cp-2022-40-RC2, 2022

Comment on cp-2022-40

Anonymous Referee #2

Referee comment on "Response of terrigenous weathering to the African monsoon during the penultimate deglaciation and the last interglacial period" by Christopher John Lepre et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-40-RC2, 2022

Publications in Climate of the Past are supposed to present substantial new knowledge or scientific understanding in paleoclimatology. This paper leaves the reader with the general impression that the authors are still trying to understand what their data mean, and what their significance might be. It does not help that some of the methods of data processing and presentation may be inappropriate. Overall I find the proposed paleoclimate inferences either speculative or unsubstantiated by the data. Below I highlight a selection of problematic issues.

Abstract: The XRF-based log(Rb/Sr) data is said to be a record of 'terrigenous delivery' to the tropical Atlantic Ocean representing 'continental weathering' in Africa, but nowhere is mentioned that the studied marine core sequence (VM30-40) is from a location far offshore and hence that the terrigenous fraction exclusively consists of dust rather than fluviatile input. Also the Rb/Sr proxy is inferred to reflect the intensity of chemical weathering on the African continent (supposedly promoted by wet climatic conditions) even though the highest Rb/Sr values of the entire 260-kyr record (at 135ka and 18ka) coincide with the glacial maxima MIS6 and MIS2. The main reason why the authors favour equating high Rb/Sr values with enhanced terrigenous weathering seems to be that Rb/Sr values also peak around 127ka, i.e. coincident with the NH summer insolation maximum defining MIS5e, when we know the African monsoon was strong and thus climate must have been wet. And the main reason why the authors claim that climate was also wet during Heinrich Stadial 11 (HS11) is that high Rb/Sr values are already achieved during ~133-129 ka, i.e. the HS11 chronozone.

Thus the authors basically negate the fact that the highest Rb/Sr values of the entire record are reached 135ka, i.e. coincident with peak penultimate glaciation during MIS6 as represented by peak foraminiferal d18O values (Fig. 3), when we know the African monsoon was weak. So either the presumed low-latitude continental dust source areas of VM30-40 must have enjoyed wet climate conditions during both peak glacial (MIS6) and peak interglacial (MIS5e) episodes (difficult to explain, climate-dynamically); or a switch must have occurred from high Rb/Sr values in VM30-40 mainly being due to Sr mineral phases being locked up in dry lake beds during MIS6 to mainly being due to enhanced continental weathering during the wet MIS5e (implying that the source of the dust proxy in VM30-40 is non-uniform through time, preventing robust climatic inferences); or the deep-sea marine Rb/Sr record of these two periods may have been affected differently by post-deposition Sr dissolution (but this possibility is downplayed by the authors).

In fact the apparent occurrence of high Rb/Sr values during HS11 may simply be due to mismatch between the chronology of the Rb/Sr data series (which is based on orbital tuning of the foraminiferal d18O record in VM30-40) and that of the reference climate-proxy records, shown in Fig. 5, with which it is being compared. To achieve a credible environmental and climatic interpretation of paleodata, authors must convince the reader that at least this kind of complication can be ruled out. Here this could have been done either by lining up the penultimate deglaciation trend in the d18O record of VM30-40 with that of GeoB7925-1 studied by Govin et al. (2014); and/or by cross-correlating these two marine records using XRF profiles of other elements (or ratios) known to reflect continental processes. Surely the authors must have obtained such data on VM30-40; I find it somewhat problematic that only the Rb/Sr data series is presented in this paper.

Introduction: The aims of this study are set up in a slightly grandiose manner, as if its results will represent a crucial advance in "understand[ing] the potential response of African landscapes to vegetation changes and erosion caused by future climates" (lines 42-43), "interpreting the paleoclimate context of prehistoric humans" (lines 52-53) and the debate about whether "high-latitude glacial cycles driven by obliquity" or "low-latitude changes in coupled ocean-atmosphere systems" may have been stronger drivers of environmental change within human habitats (lines 54-56). However, future changes in African landscapes will depend just as much (or more) on land-use change (cf. Mulitza et al. 2010 Nature), and the longstanding debate about the dominant climatic drivers of human/hominin habitat variation mostly plays at Plio-Pleistocene time scales (i.e. beyond the time scale of this study). Finally as regards the link between the presented data and the temporal distribution of ancient human populations in West Africa, in the end this theme is hardly developed: the authors only cite other work linking the timing of archaeological sites in northwest Africa (all in the Sahel region, 14-20° N) to presumed wet phases during NH Summer insolation maxima. At the very least the authors could have plotted the archaeological time line along their Rb/Sr record for direct comparison. But this exercise might have been futile, given that the continental landscape history reflected in Rb/Sr variation at VM30-40 is supposed to have seen wet phases (promoting chemical weathering and thus high Rb/Sr values) during NH Spring insolation maxima instead.

Methods: Table 1 can be omitted as it duplicates the chronological information shown in Fig. 2, except that the six principal time markers (at 12ka, 24ka, 59ka, 71ka, 128ka and 186ka) must be labelled such that it is clear these represent the end dates of the respective MIS (or MIS transitions). Further, the chronology of the Rb/Sr data series is anchored in 28 tie points between the record of foraminiferal d18O in VM30-40 and the SPECMAP oxygen-isotope age model of McIntyre et al. (1989). So it seems only natural to engage in spectral analysis after plotting the Rb/Sr data (being presented as a time series of proxy climate data!) versus time by linear interpolation between those 28 tie points. But as the authors transferred their Rb/Sr data to time by using the long-term average linear sedimentation rate (Fig. 2: 0.34kyr/cm), the result is no different than if the spectral analysis was done on a Rb/Sr data series expressed versus depth.

Results & Figures: The insolation curves plotted in Figs. 3 and 5 are stated to represent "June solstice" (i.e., 21 June) insolation at 23°N, whereas those in Fig. 6 are stated to represent "Mean spring (March 21 – May 21; MAM)" at the equator and "Mean summer (June 21 – August 21; JJA)" at 23° N. Clearly June solstice values are not equivalent to mean summer values, and if the latter represent only the 61 days from 21 June to 21 August they do not even equate to mean summer insolation. More fundamentally, it seems clear that the filtered log (Rb/Sr) 'time' series matches better with the 0° MAM insolation curve than with 23° JJA insolation precisely because the two most pronounced Rb/Sr maxima of the entire record (at 135ka and 18ka) occur nearer to 0° MAM insolation maxima (at ~130ka and ~16ka) than to 23° JJA insolation maxima (at ~125ka and ~10ka). But their inferred timing matches even better with the 23° JJA insolation minima at 135ka and 20ka, i.e. during the peak MIS6 and MIS2 glacial maxima when the African monsoon was weak and continental North Africa dry. This would have been clear if the insolation curves in Figs. 3 and 5 would be identical to those in Fig. 6 rather than showing June 21 insolation at 23° N.