Climate of the Past Discussions

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Reply on RC1

Lydie M. Dupont et al.

Author comment on "Continuous vegetation record of the Greater Cape Floristic Region (South Africa) covering the past 300 000 years (IODP U1479)" by Lydie M. Dupont et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-93-AC3, 2021

RC1: Overall, I really enjoyed reading this paper and I am supportive of this high-quality manuscript, which fully deserved to be published in climate of the past once the following comments will be addressed.

Response: Thank you for your support. We answer you questions and address you comments point by point. Proposed text changes are given in italics.

RC1: L100-101: Chase and Quick (2018) deal with time periods much longer than this. A reference presenting this modern dynamic would be more appropriate. Or the sentence could be rephrased to highlight that this behaviour has been shown to exist on longer timescales and could have a role on shorter timescale.

Response: We will rephrase the sentence and add the reference of Jury et al. 2015 (Jury, M.R., Valentine, H.R., Lutjeharms, J.R.E. Influence of the Agulhas Current on Summer Rainfall along the Southeast Coast of South Africa. Journal of Applied Meteorology, 32: 1282-1287 (2015).

In addition, the Agulhas Current influences the climate of the coastal area (Jury et al., 1993), which on longer timescales could have propagated climate signals from the tropics to the southern Cape coast (Chase and Quick, 2018).

RC1: L157: The equation represented on Supp Fig. 1 is wrong. It should be H1 * 6.76 / 6 (and not dividing by H1)

Response: Of course; thank you for spotting this error. We'll correct the figure. The depths, however, have been correctly calculated.

RC1: I think the chronological uncertainties could be potentially quite large. If I read figure 4 correctly, only 4 dates were used (only 4 changes of sedimentation rate) for a sequence of 300,000 years? I cannot understand is why each sedimentation rate interval seems to be about 75 kyr long if the tuning was based on precession. This doesn't fit with my understanding of a chronology based on precessional cycles and deserves some clarification. And I would like the authors to also describe why/how 'sediment colour is demonstrably coherent with climatic precession'. This assumption seems key to the chronology, which looks very good in the end! So, this is enough evidence that the assumption was reasonable, but more details would be much appreciated.

Response: The reconstruction of the timescale of IODP U1479 using the orbital tunig approach covers the entire Plio-Pleistocene. It will be subject of a dedicated paper, that is still in preparation. The strong precessional cycles in colour were found throughout the sedimentary sequence, which extends into the late Miocene. Sediment colour depends largely on the carbon content, which variability could be the result of productivity in the surface ocean, dissolution in the deep ocean or dilution by terrestrial material. A full discussion about which effect is more important lies beyond the scope of the present paper. As the reviewer points out, the orbitally tuned colour chronology is close enough to the oxygen isotope chronology that our conclusions drawn from the pollen signal do not depend on choice of chronology. Only four control points were used as we refrained from fine-tuning.

We will insert more explanation in the section concerning the chronology as follows:

Lines 166-169 "Discrete shipboard measurements and XRF scanning indicate that the sediment colour essentially monitors variable carbonate content, but the orbital tuning approach requires no assumption about the actual mechanism through which orbital variability paces the carbonate variability." will be expanded into:

Throughout the ca 6 Ma long record of Site U1479, sediment colour displays strong cyclical variability at frequencies associated with orbital (climatic) precession, and the discrimination of these cycles with depth suggests a modulation of amplitude similar to that of precession. Discrete shipboard measurements and XRF scanning indicate that the sediment colour essentially monitors variable carbonate content, but the orbital tuning approach requires no assumption about the actual mechanism through which orbital variability paces the carbonate variability. The most detailed orbital tuning typically requires either the manual anchoring of the ordinal points of every cycle or else the use of deterministic or probabilistic mapping techniques (e.g. Lin et al. 2014). However, under the circumstances, the transfer of such higher order assumptions from the shipboard splice to the alternative splice used for pollen analysis would introduce additional errors and, therefore, be of debatable merit. Thus, for this work, we simply adopt the minimum number of chronological anchor points necessary to achieve significant correlation between sediment colour and precession.

Lin, L., D. Khider, L. E. Lisiecki, and C. E. Lawrence (2014), Probabilistic Sequence Alignment of Stratigraphic Records, Paleoceanography 29, 976-989, doi:10.1002/2014PA002713.

RC1: The methods section would gain clarity if some subsections were added.

Response: We'll divide the methods into three subsections: *Site location, composite depth, chronology; Sample preparation; Statistical methods*.

The latter will be updated (see below).

RC1: L219: The pollen concentration curve is only represented on Supp. Fig 3 and not on Figure 5. The same applies to charcoal particle concentration.

Response: We'll omit the reference to Figure 5 concerning the pollen concentration. Figure S3 actually gives accumulation rates (i.e. concentration multiplied by sedimentation rate). The charcoal particle concentrations are depicted in the upper part of Figure 5, just below the bar with MIS.

RC1: The sedimentation rate is fairly constant, while the rate of pollen and spore deposition varies hugely during the same period (factor 2 or 3 vs. a factor of 10+). How could more pollen grains be brought to the site without additional sediments? More pollen is produced during specific periods? Change of source? A quick word about this would tie everything nicely.

Response: The sediments at Site U1479 are mostly pelagic and the terrigenous component is low but variable (quartz: 6 ± 5%; clay: 8 ± 2%). Thus, terrestrial input is small compared to the marine material and fluctuations in the terrigenous component do not dominate the sedimentation, but variation in terrigenous input can lead to substantial variation in pollen deposition. In the discussion section of the manuscript, we extensively discuss the variability of the pollen record. We don't think a quick word in the results would be appropriate.

We'll start the paragraph at line 132 adding:

The sediments at Site U1479 are mostly pelagic and the terrigenous component is low but variable (quartz: 6 ± 5%; clay: 8 ± 2%).

RC1: L227-228: I am confused by how (and possibly why) this log transformation was applied. The authors argue here that it is to limit the effect that all percentages must sum to 1 (or at least this is my understanding). But the results are presented on Supp. Fig. 3 as counts / m2 / kyr, i.e. the different AR are independent. In this context, my question is: why the log?

Response: To avoid the interdependence introduced by the percentage calculation, we used the accumulation rates (with or without log transformation). However, the numbers in the accumulations rates are quite high. By applying a log transformation, the power results are expressed in much lower figures. We did run the analyses also on accumulation rates without log transformation, but the results were essentially the same.

We will change lines 227-228 to:

We performed spectral analysis on the accumulation rates (AR) to avoid interdependence between the data as is the case with percentages. A log transformation [log(AR)] allows for the comparison of variables that spread across several order of magnitude on a comprehensible scale.

RC1: I am also having a bit of a hard time following what data are used to create Table 2 and which ones are used in Figs. 5 to 7. Were the log(AR) or the AR used for the correlations? Or is it the percentages, since the authors suggest the data are plotted on Figs. 5 to 7. I think a clarification of all these elements are necessary to ensure that the reader can be certain o follow which data are used when and why.

Response: The headings of Table 2 are somewhat scambled up and we agree that the caption is not too clear. We used AR data only in the spectral analysis. We will also publish the p-values in a supplementary table and follow up the suggestion to apply a Bonferroni correction (see next comment).

The caption of Table 2 should be:

*Table 2. R-values for linear correlations between concentration per ml of micro-charcoal particles with percentages of selected pollen taxa (1. column), between pollen percentages and foraminiferal stable oxygen isotopes (5. column) as well as between pollen percentages and different possible forcing mechanisms such as the Southern Hemisphere summer latitudinal insolation gradient (LIG) (2. column), the Southern Hemisphere winter LIG (3. column) and sea level (4. column, Bintanja et al., 2005). Correlations were calculated using pairwise regression analysis on equidistantly interpolated values resampled every 3 ka between 5 and 305 ka. We applied a Bonferroni correction and r-values corresponding to a critical p-value < 0.0009 (0.05/55) are denoted in bold. Columns 6 and 7 indicate the periodicities in which power of the accumulation rates of selected pollen taxa exceeds the 90% X2 level (REDFIT spectral analysis). * Curves shown in Figures 5 -7.*

RC1: Table 2: Then I have a problem with Table 2. Serial correlation approaches (i.e. when one record is repeatedly compared with other records) require the p_values to be adapted to the risk of false positive (see for instance section 3 of www.doi.org/10.1016/j.epsl.2016.11.048 or any other references dealing with the topic). For instance, it is certain that the p_values that are presented as significant at the 0.05 but not 0.01 thresholds (underlined not bold) will not resist a basic Bonferroni correction. Depending on the p_values of the bolded and underlined values, some of them risk to also lose their significance once corrected. This must be accounted for, and the table corrected accordingly. The advantage of doing this is that it will more clearly differentiate the strongest relationships from the background noise.

Response: We'll follow your suggestion to apply a Bonferroni correction. The correlations in Table 2 are calculated using regression analysis of pairs and will be better described in the method section of the new version. A supplementary table with p-values will be added.

We'll include in the methods section:

We conducted pairwise regression analysis for a selection of pollen percentages against possible forcing mechanisms. Correlations are carried out using the Prais-Winsten regression method (Hammer et al. 2001). Significance of the correlations was determined after application of a Bonferroni correction; in this case we used a critical p-value of 0.05 divided by 55 (0.0009) indicating significance (p-values are given in Supplementary Table 1). The Breusch-Pagan test for heteroskedasticity, i.e. non-stationary variance of residuals, indicated that in most cases homoskedasticity could not be rejected. Exceptions in which homoskedasticity was rejected at the 5% level are Poaceae percentages vs summer LIG, Podocarpaceae and Stoebe-Elytropappus type percentages vs winter LIG, Anthospermum and Stoebe-Elytropappus type percentages vs sea-level. The residuals of all correlations failed the Durbin-Watson test for no positive auto-correlation. (see also reference manual for PAST vs 4: https://www.nhm.uio.no/english/research/infrastructure/past/downloads/past4manual.pdf)

We'll also amend the statement on line 384-385 to:

A weak correlation was found between concentrations of micro-charcoal and Pentzia-Cotula type (r=0.27) and between micro-charcoal and Podocarpaceae (r=0.21), while no correlation was found between the micro-charcoal record with Amaranthaceae nor with Aizoaceae pollen concentrations.

RC1: I found section 4.5 less structured and thus less convincing than other parts of the discussion. I think the section would benefit if the definition of SH winter LIG came earlier and if its expected role on regional climate was detailed. A summary figure of how all the different elements fit together would also help grasping the complex climate dynamics proposed here. This is the weakest point of the paper at the moment.

Response: We will restructure section 4.5 as follows:

4.5 Orbital forcing

Precessional forcing is thought to be an important driver of South African climates. Previously, Partridge et al. (1997) proposed precessional forcing of rainfall in eastern South Africa. Only more recently, studies of independently dated marine sediments from the western Indian Ocean confirmed the importance of precessional forcing on the discharge of southeast African rivers, such as Limpopo and Tugela Rivers (Simon et al., 2015; Caley et al., 2018). The influence of SST on the precipitation in the region does not seem to have an effect at precessional rhythms as the SST of the Agulhas waters in the western Indian Ocean do not show a precessional rhythm (Caley et al., 2011; 2018). Along the west coast of South Africa, Esper et al. (2004) found strong precession variability in the dinoflagellate cyst record of core GeoB3603-2 (located ca 15 km east of Site U1479) indicating that stratified oligotrophic waters prevailed when precession was weak.

Sensitivity modelling using maximum and minimum precession showed that austral summer precipitation in southeast Africa is higher when precession is at maximum. Maximum precession results in maximum austral summer insolation (Bosmans et al., 2015; Simon et al., 2015) increasing the extension of the summer rainfall zone (SRZ) and probably the year-round rainfall zone (YRZ). The model experiment indicates that maximum precession induces weakening of the Southern Hemisphere westerlies (Simon et al., 2015). The SH westerlies might be affected by the the latitudinal temperature gradient during SH winter, which is strongly related to the SH winter LIG (i.e., the difference in insolation between high- and mid-southern latitudes during austral winter). This gradient is minimal during maximum precession and would explain the weakening of the SH westerlies during maximum precession. Conversely, strong SH winter LIG during precession minima could induce strong westerlies resulting in more precipitation, intensified seasonality and an extension of the winter rainfall zone (WRZ) at the cost of the YRZ along the south coast. For that reason we compare our results with the SH winter LIG.

The spectral analysis results indeed indicate strong influence of precession (Tables 2 and 3; Figure 8). The precession component in the pollen accumulation rates is not an artifact of the age model based on tuning of the colour data as the age-model derived sedimentation rates are rather constant. Moreover, spectral analysis of pollen percentages which are independent of changes in sedimentation, also reveals precession variability (not shown, but see the correlation between Podocarpaceae pollen percentages and the SH winter LIG in Figure 6). Cross-spectral analysis indicates a negative correlation between the SH winter LIG and the accumulation rates of fern spores and pollen of Pentzia-

Cotula type, Podocarpaceae, Stoebe-Elytropappus type, Ericaceae, Cyperaceae, Poaceae pp, Asteraceae pp, and Restionaceae (Table 3). Increase in summer rain and decreased seasonality might have been favorable for the growth of Podocarpaceae, explaining the significant negative correlation (r=-0.39) of Podocarpaceae percentage values with the SH winter LIG (Figure 6, Table 2). Increased river discharge of rivers in the YRZ during maximum precession could explain the precession variability in the accumulation rates of other taxa (Table 2).

Accumulation rates of Podocarpaceae pollen show highest coherency with SH winter LIG and the maximum in accumulation rates occurs 1.5 ± 0.2 ka after the minimum of SH winter LIG (phase lag of 23° ± 3 between precessional forcing and vegetation response, Table 3). Our results are in line with a transient simulation of monsoon climate over the past 280 ka focusing on precession variability (Kutzbach et al., 2008). This simulation indicated that the South African monsoon (in the SRZ) responded with a phase lag of slightly less than one month (30°) to maximum December insolation, which is very close to the response of Podocarpaceae pollen accumulation rates.

Apart from precession variability our results also indicate some influence of obliquity. Obliquity forcing could be explained by the latitudinal temperature gradient during summer (Davis and Brewer, 2009) that can be estimated by the SH summer LIG. Cyperaceae and Poaceae accumulation rates show power at obliquity periods while Poaceae and Cyperaceae pollen percentages correlate negatively with SH summer LIG (Figure 6, Table 2). Also, pollen accumulation rates of Restionaceae and Stoebe-Elytropappus type values show significant power at the obliquity band. However, Ericaceae and other Fynbos related elements only show significant precession variability hinting at a heterogeneous response of different vegetation types in the GCFR.

RC1: Many taxa seem to also have an eccentricity component to their variability (Podocarpus, Asteraceae, Ericaceae and possibly more). I think these are important features that are a bit lost in comparison to the role of precession.

Response: The eccentricity component in the mentioned curves might be an effect of the precession related variability, as the amplitude of precession is modulated by eccentricity.

We thank the reviewer for these constructive comments, which will help to improve the manuscript.

Attached Supplementary Table 1

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-93/cp-2021-93-AC3-supplement.pdf