

Clim. Past Discuss., referee comment RC1
<https://doi.org/10.5194/cp-2021-93-RC1>, 2021
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Comment on cp-2021-93

Anonymous Referee #1

Referee 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-RC1>, 2021

In this paper, Dupont and colleagues present a new pollen record from marine core U1479 off the southwestern coast of Africa. The paper is nicely written and illustrated (even if some bright colours could be toned down), and the results are overall well presented and discussed. The palynological results are interesting and the conundrum created by the apparent contradictions between the terrestrial observation of faunal archaeological remains will undoubtedly generate interesting research in the future. In comparison, I found the climate part of the paper less clear. The authors are trying to present a complex mechanism and at this stage I am not fully convinced that it all works together. If the authors could work on this section and expand their presentation and discussion of the different elements (and include additional figures?) it could all come together nicely.

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.

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.

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

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 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.

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

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

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.

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?

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 to follow which data are used when and why.

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 (not bold) will not resist a basic Bonferroni correction. Depending on the p_values of the **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.

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.

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.