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Reply on RC2

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-AC4>, 2021

Urrego: Dupont et al present a novel record of vegetation change and fire activity from the Greater Cape Floristic Region that spans over 300 ka. The paper provides insight into the development of vegetation in the southern tip of the African continent, from a site under the influence of major oceanic and atmospheric systems relevant not only for the understanding of environmental change in Southern Africa, but also for understanding of the global climate system. The authors develop a chronology that is not only dependent on the global isotope stack, giving the record some independence. The paper also includes a strong statistical treatment of pollen data and other independent variables that should be praised. I recommend the paper for publication in Climate of the Past as my suggestions are largely of format.

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

Urrego: The authors mention that 'existing paleo-environmental records (from the GCFR) do not encompass a full glacial interglacial cycle'. This is inaccurate and should be modified in the abstract and introduction. Instead, the pollen and charcoal record of site MD96-2098 should be incorporated as one existing record of vegetation and fire change in the GCFR also covering two full glacial cycles (Daniau et al 2013 PNAS, Urrego et al 2015 Climate of the Past). The charcoal record from MD96-2098 (Daniau et al 2013) seems particularly relevant as this is the first paper to test and discuss the major influence of precession on fire activity in the GCFR. The author's results from IODP site U1479 should therefore be put in the context of these earlier findings. Additionally, the authors will find that the pollen calibration presented in Urrego et al 2015 Climate of the Past is highly relevant to this research and could be incorporated in the interpretation of pollen signals from the GCFR and their ecological grouping.

Response: In this manuscript we focus on the vegetation of the western and southern Cape of South Africa. We discuss the record of Site U1479, which covers a region situated much further south than the one covered by the record of Site MD96-2098. The latter is located at 25°36'S, i.e. almost 10 degrees of latitude north of Site U1479. In addition to the pollen blown from Namibia, sediments at Site MD96-2098 probably record the vegetation in the region of western South Africa and southern Namibia drained by the Orange River. Although pollen from Fynbos elements have been found in the Marion

Dufresne core, we don't think it can be considered a record of the GCFR vegetation. A discussion about the variability of the Cape Flora vegetation in relation to the variability in the desert and savanna regions of southern Africa is beyond the scope of the present paper.

Urrego: In section 1.1 Modern climate and vegetation, the authors present a large amount of information that should be supported by primary literature. Between lines 104 and 109 the authors use barely any citation, which bears the question about the source of this information. Precipitation ranges and species composition of vegetation types are described but they lack scientific sources.

Response: In the description of the modern vegetation we refer to several chapters of Mucina and Rutherford (2006), the standard work about South African vegetation. To be more explicit, we'll add the reference of Rebelo et al. (2006) on line 106.

Urrego: The methods section and statistical analysis description should include a justification for choosing the taxa used for the correlation analysis and presented in Table 2. There are taxa that show significant correlations in Table 2 but are not plotted in the figures. It is not clear how the decision of what pollen taxa would be used in these analysis has been made. The same applies to the taxa shown and presumably chosen for the Spectral analyses and Black-Tukey cross-spectrum analysis. Again, there should be a justification in the methods for choosing these taxa.

Response: We selected the taxa based on the possible ecological meaning paired to sufficient occurrence of specific pollen grains to run meaningful statistics on the results.

Urrego: The section 4.1 Source area and pollen transport should be moved to the methods and environmental setting part of the paper. Here it is presented as part of the discussion, where the narrative is in danger of falling in a circular argument. First it is established that the pollen assemblage represents the nearby continental vegetation and later the pollen assemblage is used to reconstruct the composition of the continental vegetation. When this section is incorporated into the methods, it should be phrased in a way that establishes the pollen sources more independently. Another point that should be incorporated in this section of pollen sources is the potential effect of the Agulhas leakage on the pollen record and the recorded vegetation. Is it possible that at some points during the Late Pleistocene, the pollen record may have originated from vegetation growing further East and carried over to the core site by oceanic currents? This is probably not the case, but it should be explicitly discussed to pre-empt reservations from the reader.

Response: Here, we do not agree with the reviewer. The section about pollen transport definitively belongs in the discussion and not in the methods section. We argue that the pollen flora represents the vegetation of the nearby continent, Africa, in contrast to hypothetical wind transport of pollen from South America. It is very well possible that some pollen grains derived from East Africa. However, we presume that the influence of the nearby vegetation of the Cape dominates the pollen record of sediments of Site U1479.

We'll change the first sentence of section 4.1 into:

The floral composition of the palynological assemblage of Site U1479 indicates that it

records the biomes of South Africa, in particular that of the GCFR, although limited pollen transport from the east coast cannot be excluded.

It turned out to be difficult to estimate the quantitative role of the Agulhas Current in pollen transport to Site U1479, although we assume that its influence is considerable (lines 260-261). Emergence of the PAP might have reduced the influence of the Agulhas Current transport to Site U1479, as in that case pollen grains transported by the Breede River are likely more abundant. Although there is a minimum in the pollen concentration around 125 ka (max sea-level), we do not observe strongly increased pollen accumulation rates during periods of low sea-level.

Urrego: The pollen and fire record seem to be given less weight than results from previous work on vegetation modelling the Palaeo-Agulhas plain (PAP) and modelled sea level change. While the modelling results are very valuable, the direct nature of empirical data such as this pollen record should be recognised. For instance, the pollen concentration changes are subtle during the glacial periods, potentially suggesting that the modelled sea level change may be less drastic and comparable in magnitude between glacial cycles. A 100-m sea level decrease would have probably been recorded as a prominent increase in pollen concentration because vegetation would have been closer to the site. Does the pollen record suggest the global modelled estimates need to be tweaked for the south African region? Likewise, the pollen results suggest that grasslands were not as extensive in PAP as previously thought, but it seems like the authors are attempting to find an interpretation that still fits the modelled vegetation of PAP. A balance between the value given to information provided by this new record and previous modelling efforts should be attempted here.

Response: We do not exclude the possibility of glacial/interglacial climate variation influencing the vegetation of the Cape; see lines 294-296, 496. However, the exposure of such a large shelf area as the PAP in the vicinity of the site probably had the greater impact. We unexpectedly do not find a prominent increase in pollen concentration during periods of low sea-level, but we do record a different pollen assemblage during those times. We compare our results with the vegetation modeling of the PAP, but we also mention differences between our results and those of the modelling effort; see line 320 ff.

Urrego: The information presented from isotopes of mammal teeth suggests changes in the abundance of C3 and C4 plants but this is not discussed in light of this new pollen record. How do these compare? Is the pollen record adding some insight into the composition of the vegetation that has only been inferred from a spotty set of mammal fossil records?

Response: We do not understand this comment. A full section (4.3) is dedicated to the discrepancy between the mammal record (including isotope information) and the marine pollen record. Our discussion of isotope data is primarily concerned with C3 versus C4 grasses, which cannot be distinguished on the basis of their pollen (and is noted in the text)

Urrego: Figures 1 and 2, and all their panels, should be consistent in their geographical extend. They should all show the same latitude and longitude ranges for consistency and to allow more effective reading.

Response: The geographical extent of the panels should be obvious as all maps have coordinates. We choose the size of the maps according to the structure we want to depict. Some features, such as the direction of ocean currents, extend over larger areas than others, such as the bathymetry of the Agulhas Bank.

Urrego: Figure 3. Indicate in the legend what the blue circles are.

Response: We'll add '*position of the samples are denoted with blue circles*' in the caption of Figure 3.

Urrego: Figure 4. The differences between the oxygen isotopic record and the orbitally tuned chronology are said to be within error (lines 174-175). These errors should be shown in the Figure to support the statement.

Response: We refer to the estimated error for the LR04 timescale of ± 4 ka for the past million years. We'll adapt the sentence on lines 174-175 as follows:

the differences are generally within the error of ± 4 ka for the past million years stated for the LR04 stack (PAGES, 2016), though there is isolated divergence in discrete intervals such as the penultimate deglaciation (Figure 4).

Urrego: The results of the cluster analysis used for the zonation should be presented in the supplementary figure. At the moment they are quickly described in the text but not presented in a figure. It is hard to evaluate the validity of this zonation exercise without the complete results from the cluster analysis.

Response: Due to restricted space in the supplementary figures, we'll provide a separate supplementary figure with the cluster analysis results. We spotted a mistake in Table 1; Zone I includes 5 samples from 4-22ka and Zone II includes 11 samples from 25-61ka. We'll correct Table 1 in the final version.

Urrego: Figure 6. The sea level curve should be labelled "modelled (global?) sea level".

Response: We'll amend the sentence on line 301: *correlating significantly with modelled global sea level curve (Bintanja et al., 2005)*

Urrego: Table 2. Include citation for sea-level reconstruction in the legend. This is included in Figure 6 but not here.

Response: We'll adapt the caption of Table 2 (see also the response to RC1)

Urrego: Table 4. This table should include references on which the ecological grouping is based. It is also hard to read what taxa correspond to each ecological group. For example, is Cyathea type the only taxa included in the Thicket/forest? Or is this Pteris-Cyathea type

and is grouped in the succulent and drought adapted? Horizontal lines separating each group could help avoiding confusion with this.

Response: We'll follow this suggestion and adapt the lay-out of Table 4. We'll add to the caption: *Grouping follows Quick et al., 2015, 2016.*

We thank the reviewer for her constructive comments.

Attached Supplementary Figure 5

Please also note the supplement to this comment:

<https://cp.copernicus.org/preprints/cp-2021-93/cp-2021-93-AC4-supplement.pdf>