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Comment on cp-2021-93

Dunia H. Urrego (Referee)

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-RC2>, 2021

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.

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.

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.

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.

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.

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.

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?

Suggestions on figures and tables:

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

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

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.

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.

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

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

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.