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Climate of the Past Discussions

Interactive comment on "Precipitation variability in the winter rainfall zone of South Africa during the last 1400 yr linked to the austral westerlies" *by* J. C. Stager et al.

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GENERAL COMMENTS

This study presents a new late Holocene record of South African precipitation variability linked to changes in the austral westerlies. This work offers an important record of the westerly wind response to climate change from an underrepresented region. Assessed in a larger context, through the correlation with climate records from Siple Dome and South America, the study carries global significance as well. Overall, I consider this study to be a valuable scientific contribution that should be published in *Climate of the Past.*

Several details can and should be revised or improved before the final publication:

SPECIFIC COMMENTS

In several instances, you discuss a "poleward contraction" of the westerlies during intervals of global warming (i.e. page 4377, line 3; p. 4378, line 11; p. 4379, line 12). I believe that the context in which you use these words blurs the distinction between two prominent mechanisms proposed in the literature. In Toggweiler and Russel (2008), one of the original modeling papers you cited, the movements are described only as poleward "shifts" (not "contractions"). In contrast, a new mechanism proposed by Lamy et al. (2010) hypothesizes a westerly wind belt contraction during warm, summer-like conditions, such that "the intensity within the core is strengthened" (with expanded northward margin during cold, winter-like conditions), but does not seem to imply overall poleward movement of the westerlies. To me, "shift poleward" and "contract poleward" imply different mechanisms, so I would encourage you to carefully consider your word choices in each instance. In the context of describing what *climate models* predict (i.e. p. 4377 lines 3, 23) I believe that poleward "shifts" may be more accurate. However, you should also emphasize that there is a new, distinctly different mechanism proposed by Lamy et al., because you later invoke this mechanism to explain your own data (p. 4386 lines 5-8; p. 4387 lines 1-2). Additionally, if you use the idea of a contracting/expanding westerly wind zone to explain your data, I do not think that it is an appropriate conclusion to say that your results support modeling hypotheses (p. 4387 lines 17-19); rather, your prior discussions seem to support the mechanism from Lamy et al. Please clarify the mechanism you think may be supported by your work.

p. 4378 lines 14-24: In your consideration austral westerly wind records from S. America, I would encourage you to also cite Moy et al. 2008 (*Quat. Sci. Reviews*), which reconstructs the westerlies using lake records from Chile. This region also has winter rainfall linked to the westerlies and the study is on the same timescale as yours. Additionally, while you discuss that there are very few westerly wind records outside of S. America, there is no mention of several reconstructions from New Zealand (see

Knudson et al., 2011, *Quat. Sci. Reviews,* and references therein).

In the "Materials and methods" section, the subsection describing "Geochemical analyses" should be further developed (p. 4380, lines 21-27). This section would be improved with a brief explanation of why the LOI and CO3 were chosen for analysis (presumably to determine relative changes in runoff related to precipitation).

As your South African climate records are based on diatom analyses influenced by the precipitation-evaporation (P-E) net, I would like to see more discussion of why you know that the net P-E is only or mainly attributed to changes in the westerly winds, as opposed to other factors, such as temperature. While the "Site description" does discuss precipitation links to the westerlies, how much does evaporation (or other water loss) fluctuate and what controls that component of the net P-E balance?

p. 4386 lines 12-13 Fig. 8. (a) should be revised to accurately explain the interpretations of the Lamy et al. (2001) GeoB 3313-1 data. Here, Fe counts per second reflect the sediment *source region* (relative input of the iron-poor Coastal Range vs. the iron-rich Andes). Lamy et al. (2001) states that *higher* Fe intensity is linked to an Andes source, which is indicative of *less humid* conditions. So it is incorrect to say "higher intensity indicates greater terrestrial runoff." Please review the Lamy paper again for the specific proxy information.

TECHNICAL CORRECTIONS

Fig. 1. Most font in (a) and (b) are too small to be readable. Need to enlarge.

Fig. 8. (a): Something seems amiss in the units for the GeoB 3313-1 data that you graph. There looks like there is something wrong with the range of values for the counts per second (why are some negative?) and they are off by a factor of 10 from the data that was published in the original paper (Lamy et al. 2001).

p. 4385, line 16: need period at the end of the sentence.

Table 1: "calyr" change to "cal yr"

Interactive comment on Clim. Past Discuss., 7, 4375, 2011.