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Climate of the Past Discussions

Interactive comment on "Holocene climate variability in the winter rainfall zone of South Africa" *by* S. Weldeab et al.

Anonymous Referee #2

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The article of Weldeab et al. proposes what is considered to be a new suite of records reflecting conditions in southern Africa's winter rainfall zone. This is certainly a understudied region, but it is not clear that the records reflect, in fact, conditions in the winter rainfall zone, derived as they are from Orange River sediments, whose catchment is in the summer rainfall zone.

The record and questions are highly challenging, and will require a substantial amount of calibration work to determine sediment source. Until this work is done, and appropriate analyses are undertaken, the record presented will remain of ambiguous values.

With substantially more work, the data presented could be a valuable contribution, but as it stands I do not see the submitted paper to be a useful addition to the literature.

As the underlying data do not appear to be fundamentally flawed, major revisions, including significant calibration work on the Orange and coastal river sediment sources, could provide a context for a reliable treatment of the data. Until this work is done, I would not consider any interpretation reliable.

Detailed comments below.

p.2284, line 21-22; It is not a surface current per se that carries the coarse sediments northward, but rather littoral drift.

p.2285, line 5-6; Not clear what reference is cited for southern BUS upwelling be stronger in winter, but based on a range of works by Jury and other, this is not an

accurate statement. In fact, maximum upwelling in this region occurs during the summer. It is the northern BUS that is most intense in winter.

p.2285, line 13; small relative to what? Certainly not to any of the other rivers being discussed.

p.2285, line 26-29; What is meant by "a significant amount" of sediment? Work by Gavin Birch and others has been made to quantify Orange River vs. western coastal drainage sediment. As a critical element of this study, this needs to be more accurately understood and expressed.

Quantification of discharge of the 'rivers' being discussed is also required. One might get the impression that the perennial Orange (975,000 km2 catchment, 11.4 km3 annual runoff) and the ephemeral Holgat (1500 km2 catchment, fails to reach ocean for 50+ years) and Buffels (9000 km2 catchment, reaches ocean every 3-5 years) were in any way comparable.

Prospero et al show decidedly insignificant amounts of dust originating from the region and being deposited at/near the core site. These vagueries are cause for concern, considering the extreme complexity of the region and the proxies being employed.

CPD

8, C1282–C1290, 2012

p.2286; Prior to methods, it is necessary to better describe in the introduction the complexities posed by the setting (reconstructing winter rainfall with what are in

very large part sediments from a summer rainfall river) and how each of the proxies will be applied.

I commend the authors on undertaking such a complex record, but much more care and detail needs to be taken to explain the situation and how they believe they have

adequately resolved the inherent questions.

p.2287, line 1; Considering this very unfortunate hiatus, why did the authors not analyse sediment from nearby GeoB8331, which has very high sedimentation rates across

the middle Holocene? Would certainly make for a more complete study of the question.

Indeed, looking at Figure 3, it seems an incredible coincidence that pre- and post-hiatus samples should have such similar values, but the ages as presented do indicate such a break in the record.

p.2290, line 8; While this may be a reasonable assumption for the Stuut et al., 2002 record, it seems rather odd in the context of this core; and the subsequent sentence, which states that EM1 (modal grain size 20?) and EM2 (modal grain size 10?) comprise the bulk of the Orange River suspended fluvial sediments (70

Considering that the core site is located on a subaqueous delta, and that all end-members are well within the range of the primary river's suspended load, it is not at all clear how the indicies of Stuut et al., 2002, which were applied to a site 350 km off a coast with extremely limited fluvial activity, are at all appropriate.

Can the authors explain how EM1 and 2 can, in this context, be reliably identified as being non-fluvial?

Section 4.2; This set of proxies needs to be more clearly discussed/explained/calibrated. How does increased K equate to more humid conditions.

CPD

8, C1282–C1290, 2012

How can this be distinguished from a shift in source area? Compton and Maake do not discuss Ti, as suggested by the authors. Why is this a proxy for aridity and/or aeolian transport?

Many questions regarding this section, and much to be more clearly discussed explained.

Section 4.3; This is a critical section. The paper, based on the title, proposes a reconstruction of the winter rainfall regime, but examines sediment that is primarily derived from a summer rainfall zone river. It is very important that it be able to distinguish between winter and summer rainfall sediments/records.

The authors recognize that a more comprehensive calibration is required, but it appears quite clear that none of the samples (based on these data) come from winter rainfall zone rivers. The question then is, how are these records thought to reflect changes in the winter rainfall zone? If they are Orange River sediments, as suggested here, they reflect conditions in the summer rainfall zone, and the title of this article is inaccurate/misleading.

p.2293, line 14-17; Considering the nature of the site and the fluvial sediments of the Orange River, why is this not an increase in river discharge? Considering atmospheric circulation patterns in the region, it would be expected that increased aeolian activity during mid-Holocene (which has been indicated by Chase and Thomas) would bring more material from the winter rainfall zone. However, this is not reflected at all in changes in the radiogenic isotopes.

p.2293, line 16-22; As mentioned above, the grain size end-members are all included in the dominant fractions of the Orange River fluvial sediments, and the argument that they reflect aeolian vs. fluvial inputs is unconvincing.

The change in Nd and 87Sr/86Sr values is very slight indeed, remaining soundly within the range of an Orange River 'signature', and clearly distinct from the values obtained

CPD

8, C1282–C1290, 2012

from winter rainfall zone rivers. There may be an increase in sediment input from local rivers, but based on these data it is inconsequential.

Section 5.1 p.2295, line 18; Low dust accumulation? Based on what evidence precisely? I assume we are talking about the grain-size analysis, and the attribution of different size classes to different depositional mechanisms, but again the argument for this interpretation needs to be made much more clearly and strongly considering the nature of the Orange River sediments.

p.2295, line 19-20; None of the Nd values are positive. Do you mean more positive /less negative?

Also, the wording of this is slightly misleading. The Nd values are soundly within the range of the Orange River sediments, and nowhere near the values of the coastal rivers. There is the (again, slight) implication that coastal rivers played some role, and this is not indicated.

More importantly, these data do not clearly indicate more humid conditions in SW Africa. Even if the attribution of the end-members is correct (which I do not accept based on the data presented and that from other sources), low dust does not necessarily indicate humidity and the increased fluvial input is identified as coming from the Orange River, whose catchment is not primarily in SW Africa, but which is primarily fed by summer precipitation in the east of the subcontinent. Comparison with the Kristen et al records is complex, as those records are themselves quite intricate. This comparison should be shown more clearly in a figure, explaining precisely which proxies are seen to shown similarities.

Considering that the 'dust' and the 'fluvial' sediments have different sources (dust=coastal/winter rainfall zone, and fluvial=Orange River-summer rainfall zone), how is the general coeval inverse relationship described by Cockcroft et al., 1987 expressed? Drier winter rainfall zone would be predicted to result in increased dust at the same time that there is increased fluvial input from increased precipitation n the

CPD

8, C1282–C1290, 2012

summer rainfall zone. Similar changes in climate in both regions may produce the signal that the authors focus on, with more dust at times of reduced fluvial input, and vice-versa, but this is not what is generally understood to have occurred during the late Quaternary.

It is not clear how the inconsequential (statistically indistinguishable) change in Nd indicate what the authors refer to as a "significant contribution of eolian particles from Namaqualand". The Nd values much higher than the "coastal area values", virtually identical to early Holocene values, and well within the range of the Orange River sediments.

p.2296, line 1-2; In this region, decreases in precipitation are more likely to be associated with increases in southeasterly winds, as the South Atlantic Anticyclone gains dominance.

p.2296, line 2-3; Two issues here: 1) the authors report an increase in wind strength at the same time that there is a decrease in upwelling, which is not consistent with regional circulation systems and the factors that increase upwelling (increases in the same winds that dominate in the region, and would be capable of bring dust from aeolian sediment from Namaqualand); and 2) the record of upwelling presented here is inconsistent with other from the region (e.g. Farmer et al., 2005), which have been validated by comparison with records such as the hyrax midden record the authors cite from Spitzkoppe (Chase et al., 2009).

p.2296, line 4-6; Is this basis for finding that the data presented here reflect changes in the winter rainfall zone? That it shows the opposite of what is observed in the summer rainfall zone? It should be considered that the data from Tswaing are complex/contentious (see both Kristen and Partridge articles), and that the Cold Air Cave data have been interpreted in different ways (Holmgren articles and Lee-Thorp et al., 2001). This has been synthesized to some extent by Chase et al., 2010, but a fuller treatment of the current debate on Holocene climate change in southern Africa is criti-

CPD
8, C1282–C1290, 2012

cal for this paper.

To summarise, it is the opinion of a growing number of researchers, based on more, improved data, that the early Holocene in the southern African summer rainfall zone was more humid, and that the mid- to late Holocene was characterized by increasingly arid conditions. Evidence from the Cederberg Mountains (Meadows, Scott Woodborne, Chase) suggests that a similar pattern existed in at least this portion of the "WRZ".

p.2296, line 12; Meadows et al. find very little change across the early to mid-Holocene. Broadly, to this point, considering that the sediments being analysed are shown to be of Orange River, summer rainfall zone origin, it is not clear why comparisons are being drawn with records of the WRZ. How do Orange river sediments record changes in winter rainfall zone climate, particularly if EM1 and 2 cannot be considered as exclusively/predominantly aeolian?

Section 5.2 Issues raised previously also exist here: interpretation of grain size, insignificant changes in radioisotopes being interpreted as indicating "significant" contributions of Namaqualand rivers, and a reasonable, but superficial understanding of the regions palaeoenvironmental records.

p.2296, line 6-8; it is again unclear how this minor change in radioisotopes reflects a significant contribution of WRZ rivers. (y-axis for Sr should not be clipped to allow for coastal sediment values to be shown. Figure 5 clearer, and this figure should have labels for the samples.

p.2296, line 9-10; Which cave deposits? Unnecessarily vague. The authors should also consider what proxies are used to infer "cold, dry conditions". Tyson et al. are quite liberal with their treatment of the data, and with a wider range of data sets, difficulties and inconsistencies in the d18O records have been revealed. The relationship between the WRZ proxies and those reported here is unconvincing. The Verlorenvlei record shows no clear similarity with the K/Al record. The variability in the former is also not seen in the grain size record (to be explained by authors). The plot of Buffels River

CPD
8, C1282–C1290, 2012

flood events may be confusing the authors, as at first glance it appears to indicate a trend similar to the grain size analysis. It is, however, a cumulative plot and it does not show that it is drier at the beginning of the record, just that there is only one record considered. The authors should familarise themselves with this kind of plot so as to be able to interpret it properly. Key period are when the line is steeper, indicating clusters of flood events. These show broad correspondence with the Verlorenvlei record, but not the GeoB8332 records.

Perhaps because the sediment is derived from the Orange River, and does not therefore (primarily, or perhaps at all) reflect the WRZ.

In fact, the greatest similarities with the K/Al record are from the summer rainfall zone and tropical Africa, which is not surprising considering position of the Orange River catchment.

p.2299, line 27; “amelioration” is a subjective thing. More humid.

p.2300; Considering my reservations with the authors interpretations, I am not convinced that an adequate basis has been established for the comparisons and arguments that the authors develop here.

p.2300, line 20-21; Again, amelioration is an inappropriate term. Further, what is the expectation that the WRZ and SRZ will be in/out of phase or leading/lagging? The authors appear to be suggesting that frontal, winter rainfall systems created the more humid conditions at Spitzkoppe in the last 1000 years. This is inconsistent with both conceptual models from the region and the existing data. A more thorough reading and analysis of the existing data from the region should be undertaken, if this is the case.

p.2300 It would great to see other upwelling and SSTs proxies from these sediments that were comparable with other records from the region. A high resolution record would be extremely useful. As it stands, strong upwelling at this site is at odds with the records of Farmer et al., 2005 to the north, and the records from Spitzkoppe, which

CPD

8, C1282–C1290, 2012

show similarities with the K/Al record,but reflect summer rainfall conditions

Summary and Conclusions Unconvinced by the interpretations of the authors, I do not find the narrative here to have an adequate foundation.

p.2283, line 14; “has so far relied on” p.2285, line 1-3; sentence very awkward. Clarify. Locarnini et al., 2010, not in the reference list. p.2292, line 6-8; not a complete sentence

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