

Interactive comment on “The drivers of late Quaternary climate variability in eastern South Africa” by Charlotte Miller et al.

Anonymous Referee #2

Received and published: 18 February 2019

This paper presents interesting new data from the Mfabeni wetland in SE Africa, a site that has received considerable attention in recent years. The data presented here is, in my mind, critical for the fuller understanding of how the other proxy records relate to past climate change.

While I find the data to be extremely interesting, the text itself becomes slightly contorted at times and I think there are several aspects that could be reconsidered, or at least clarified.

The first aspect is a more thorough integration of the pollen data from the site. Much of the text relates to vegetation change, but the pollen data is only included in an extremely rough summary form in the final figure. Significantly more effort should go into including the pollen data in comparative diagrams with the original data presented

C1

here. This may require some work (creating comparable chronologies, etc.) but it is necessary.

The authors also appear to base their palaeoclimatic interpretations on the supposition that the amplitude of dD change ($\sim 53\%$) cannot be explained by changes in source water dD , and must be (even primarily be) the result of changes in evapotranspiration (only explanation for observed changes in Fig 4c). This contrasts with most authors' interpretations, which acknowledge the potential role of ET on leaf wax dD , but focus more on precipitation amount/intensity (and the general observation that dD_{wax} and mean annual precipitation are strongly correlated in the tropics (e.g. Sachse et al., 2012 and references therein)). In the African tropics, dD_{wax} records from lake and marine sediments exhibit ranges of $\sim 35\%$ – 55% usually with the lowest values occurring during the last glacial period/Last Glacial Maximum. Considering changes in temperature alone, cooler conditions at this time would have lowered ET, rather than raising it. At Mfabeni, the authors seem to conclude that it is wind strength that drives what is inferred to be increased glacial-age ET, thus essentially interpreting their dD_{wax} record as a wind strength proxy. Considering the remarkable similarities between their dD_{wax} record and pollen-based precipitation reconstructions from the region this seems to become unnecessarily contorted through the discussion and conclusions. Acknowledging that ET can certainly have an impact on dD_{wax} values, is it not still more parsimonious to interpret the Mfabeni record as primarily reflecting rainfall amount/intensity? Or is the suggestion that the other dD_{wax} records from tropical Africa be revisited, and mechanisms for increased glacial-age ET at each site be found?

The authors also focus on the southern westerlies as being the/one of the primary drivers of the changes observed in their record. This strikes me as an odd perspective, as the westerlies are not implicated as being a significant moisture-bearing system in the region. Rather, the authors focus on shifts in the westerlies as somehow (not well-described) inhibiting precipitation and probably increasing wind strength (no reliable data provided (grain size data from the sediment core don't satisfy this requirement) at

C2

the site. To follow their interpretive logic further, they strongly associate the position of the westerlies with Antarctic sea ice extent, and thus that changes in Mfabeni hydroclimate are primarily driven by changes in sea ice. Of course the myriad elements of the Earth's system are inter-related, but this focus on the mid- to high latitudes without more detailed description of tropical dynamics and those systems that are responsible for precipitation at the site seems not to be the clearest, most straight-forward way of describing the changes observed.

More and more detailed comments and questions are here below:

TITLE: This seems a grand title for a paper describing a single site. It is not sufficiently synthetic to make this claim.

ABSTRACT: Lines 23-24: Saying that the conditions ARE a consequence of low SSTs and westerlies/sea-ice is a pretty strong statement. It suggests that the authors are SURE this is the case, but considering the quality of the sea-ice and westerlies proxies this does not seem like a realistic claim. I suggest dialling back and being more circumspect.

Lines 32-34: I imagine this will be explained more fully in the text, but this statement here is unsubstantiated in the sense that a mechanism for the influence of the westerlies at a site so far north. There should be at least some further clue given here as to what the thought process is.

INTRODUCTION: Line 40: "Last glacial" what? Period? Maximum? In any case, 21 ka is neither. MIS 2 ended at 11.7 ka, and the Last Glacial Maximum ended at ~19 ka (Clark et al., 2009). This should probably be stated differently. Also, (pedantically) "BP" should be removed here as it refers specifically to radiocarbon chronologies and indicates before AD 1950. "ka" alone is more appropriate.

Line 45: Define LGM here, as first instance in body of text. The authors should also define what it is. I suggest the 19-26.5 ka Clark et al., 2009 definition (as the authors

C3

use in Line 268).

Line 48: The authors may want to consider/add Chevalier et al., 2017, as this paper deals directly with this question.

Line 61: Here the authors may want to consider Otto-Bliesner et al., 2014 and Chevalier and Chase, 2015, as they discuss the relative importance of these and other drivers specifically, providing some detail on precisely how they operate in SE Africa.

Line 62: The ITCZ is really only clearly defined over the oceans. Have a look at the works of Nicholson et al., but in any case Mfabeni is well to the south, and easterly flow better describes the moisture-bearing vector.

Lines 63-67: Certainly the component parts of the Earth's climate system are linked, but I wonder from these statements and the abstract how the relationships are described in this paper. Here, for instance, it is said that that the winter rainfall zone gets wetter because the ascending limb of the Walker Circulation shifts eastward. It may well have shifted eastward, and it doing so may have either allowed or been the result of changes in circulation systems that brought more moisture to the winter rainfall zone, but the position of the Walker Circulation over the Indian Ocean does not directly impact the winter rainfall zone. Said differently, these dynamics are most clearly expressed when the primary moisture-bearing system is included in the discussion. Here, the authors account in part for the SRZ, but not at all for the WRZ as the authors do not include the westerlies and associated storm track. I suggest the authors take a few lines to describe the regional climate systems, and define things like the WRZ and SRZ more clearly so that the reader can have a clearer spatial understanding of how it all fits together. I see the authors do this later, but by then it is a bit late. Rework these sections to create a logical development of information.

Line 65: "...OVER the Indian Ocean..."

Line 71: These references are only Holocene. Considering that this paper has greater

C4

scope surely some of the seminal works like van Zinderen Bakker 1976, Cowling et al., 1999, etc. should be mentioned? The papers mentioned hardly define the concept.

Line 73-74: Chase et al., 2017 isn't a very apt reference for this statement. Chevalier and Chase., 2015, perhaps? This paper addresses this region/topic more specifically, and comes to the stated conclusions, at least to some extent.

Line 75: "last Glacial" what? Period? Maximum? Glacial shouldn't be capitalised here, unless it is for the LGM.

Lines 75-78: Glacial shouldn't be capitalise here.

Line 80: Again, and here and elsewhere, the ITCZ isn't the correct term here. African or tropical rainbelt is preferable (again, see the papers of Nicholson et al., esp. 2009).

Line 84: Chevalier and Chase should be removed as that is neither a marine nor speleothem study. Holmgren et al., 2003 is a better speleothem reference, as it extends to the 25 ka.

REGIONAL SETTING: Line 93: CAN be divided. That is just one way of looking at it, and it is not without its shortcomings.

Line 97: tropical-temperate troughs are only one of the composite synoptic systems to consider. Chase et al., 2017 lumped the ensemble as "TTIs", tropical-temperate interactions, but look at Tyson, 1986 and Tyson and Preston-Whyte, 2000 for more specific details.

It might also be worth mentioning that suggestions have been made that at least two subdivisions of the SRZ have been made, with, according to Chevalier and Chase, 2015, the northern and central/eastern summer rainfall zones operating in substantially different fashions.

Line 100: "temporal frontal systems"? Temperate rather? And Tyson, 1986 should perhaps be referenced here?

C5

Line 101: "Sandwiched" doesn't suggest the transitional nature of the YRZ very well. For me it suggests solidity for an extremely ephemeral region. Maybe rephrase?

Line 104: Probably don't need a reference for the Namib Desert.

Line 110: Here and elsewhere in the text, "ka BP" does not indicate if the ages were calibrated. If this is based on a radiocarbon chronology ("BP"), it should be "cal kyr BP" (ka is for an age, kyr is for a span of time). If the chronology includes OSL ages (that do not require calibration) "ka" is appropriate for mixed or non-radiocarbon chronologies.

Line 121: The paper referenced focusses on wind, but strong winds do not bring precipitation, as could be inferred from this sentence. Reword or reconsider in terms of the circulation dynamics that the passage of cold fronts induce?

Lines 105-144: These paragraphs mix around elements of topography/geology and climate. Perhaps disentangle? With topography and geology coming first (as the backdrop) and then the second paragraph looking at climate?

Lines 177-179: Maybe specify where C3 vs C4 grasses generally grow?

DISCUSSION: Line 307: C3 grasses are found in the WRZ (and YRZ. . .) AND at higher elevations. As mentioned previously, some clue as to the climatic mechanisms that drive C3 vs C4 grass distributions would be very helpful.

Line 314: "higher" d13C values would be clearer.

Line 315: How would colder conditions lead to an expansion of C4 grassland? The authors are walking a fine line here, presumably citing frost-intolerance of arboreal and shrub taxa, but not to the point of enabling significant C3 grass expansion? Please clarify.

Line 315-316: Why just less tropical/summer rain? If compensated for by an increase in winter rain (not likely, I'd suggest) conditions would actually favour arboreal taxa like Podocarpus. Maybe just say less precipitation (as in the figure).

C6

Line 321: I'd be careful about that temperature reconstruction for the site. It appears rather insensitive to my eye, especially as other reconstruction of both continental and sea-surface temperatures are more like 4-6 degC. Perhaps include a more conservative estimate? The point remains the same, but the basis is sounder, perhaps.

Lines 331-332: Please describe what those physiological differences have on the dD signal.

Lines 334-335: It isn't entirely clear how the paragraph leads to the statement that the influence of ET is dominant over precipitation amount. Please clarify.

Lines 336-241: Again here, how do the authors determine that ET is significantly more important than amount effect? I agree it can have significant influence, but the authors have not described amount effect at all, but the authors do say 'amount/heightened ET' in several places.

Line 342-353: OK. The authors get there now. Can the authors please reorganise these paragraphs to first outline the mechanism and role of each factor considered, and then bring them together?

There is an issue here though. The inference (and statement in Figure 4) is that there is stronger ET during the last glacial period and it is lower during the Holocene. Considering that the former was significantly cooler, what is driving higher ET? It seems to be the suggestion that 53% of dD variability can only be explained if ET is included (and perhaps even dominant). The cited Gat et al. study may suggest this, but other work like Wu et al. (2015), the examples in the cited Gat et al. paper and the Harris et al. study from Cape Town (2010) show very large changes in inter- and intra-annual dD-precip (~60%). Can the authors expand their discussion here to include consideration of this more clearly? An aspect that hasn't been considered is moisture source. Considering the types of synoptic systems that have been suggested to dominate at the timescales considered here, how might these changes have influenced the dD record?

C7

Line 360: Not then "drier conditions" as such necessarily, but potentially just a lower water table (to explain specifically Paq)?

Line 362: How do the authors infer "summer" precipitation specifically from the dD record?

Line 363-366: How would changes in the water table change the dD record?

Line 379-381: How do the authors explain an increase in temperature resulting in more C3 grasses? This really must be explained. The pollen data from the site is included in descriptive form in Figure 6, but please add real percentage data to a figure for comparison with the d13C and dD data. A summary of C3 arboreal and shrub taxa might be one idea. (keep in mind the differences in the chronologies, and make sure to plot using comparable chronologies (perhaps using lithology as a basis for correction).

Lines 382-383: '...plateau... indicating continued expansion...' is awkward wording. Please rephrase.

Line 389-392: Studies such as Chevalier and Chase., 2015, Schefuß et al., 2011 and other have indicated that direct insolation was unlikely a dominant control on precipitation until the Holocene, with Northern Hemisphere influence dominating. Thus, this is something of a logical fallacy, considering the data available. Insolation did not apparently drive precipitation variability at this time in this region.

Regarding ET, the authors are saying that wind strength drove the inferred variability? Are the authors thus saying that the dD record is predominantly a proxy for wind strength (if ET is dominant, and ET is driven by wind)? If that is the case it should 1) be stated more clearly, and 2) be substantiated with some independent records of wind strength variability. The grain size records from the site is not convincing here, as it show a general increase in grain size from 23-16 cal ka BP, when the authors interpret a reduction in ET (lower dD, Figure 4), and otherwise shows little similarity.

Line 421: The SST record the authors have chosen may indicate little change, but

C8

the Sonzogni et al. (1998) record from the Mozambique channel seems to be compared quite convincingly with continental temperature reconstructions in Chevalier and Chase, 2015, both showing temperature declines in the mid-Holocene. I understand what the authors are saying, in that there is not a consistent, linear relationship between insolation, SSTs and Mfabeni/regional hydroclimates

What would have been the cause of the late Holocene increase in ET that the authors infer? Their Figure 5e is interpreted as indicating LOWER ET during the late Holocene, after the pulse in higher ET from 2-3 ka. Also, this pulse, seems much more consistent in a multi-millennial context as occurring in time with a period of particularly low dD values, which the authors have said indicate lower ET. It may be that the authors' ET focus is becoming problematic for their interpretations.

Lines 425-436: I think the authors may have missed some points in the papers they have cited. And the authors' expectation may be to find a single mechanism that explains the whole of the record the authors present. If the authors compare SST records such as Sonzogni et al., 1998, the authors will see period of similarity, such as during the LGM and MIS 3, from HS1 to the early Holocene and to a lesser/less visible extent the late Holocene. The significant differences that are evident occur during HS1 and the mid-Holocene. These were highlighted by Chevalier and Chase (2015), and cited as an important distinction between the northern SRZ, where a simpler relationship appears to exist between SSTs (glacial period), orbital forcing (Holocene) and precipitation. Chevalier and Chase concluded that these mechanisms did not so simply drive central/eastern SRZ region. Instead, they find that in this portion of the SRZ, which includes Mfabeni, climates "may have been significantly modulated by the position and influence of the westerly storm track". This idea has subsequently been developed significantly in Chase et al., 2017, where the combined influences of tropical and temperate systems, and the significance of the development of composite synoptic systems has been described in detail. Thus, it comes as some surprise the potential role of the westerlies is raised as a novel suggestion in lines 435-436.

C9

Lines 437-443: And then the descriptive logic becomes rather twisted about, from my point of view. The southern westerlies are a 'driver of changes in hydroclimate', but this is done by shifting northward and NOT bringing moisture to Mfabeni. How are the westerlies a driver in this case? What is the mechanism? What is the link between the westerlies and the systems that are perceived as bringing moisture to the region at these times? How do the westerlies induce "a more evaporative regime"?

Line 445: Cockcroft et al is not a climate model simulation. It is a theoretical model.

Lines 447-455: Most people don't think, based on the evidence available, that the WRZ (>66% winter rain) expand so far. Strong frontal systems impact the region today, so what are we really talking about? They moved north, but not far enough to bring increased rain (but enough to bring wind), and the easterlies affected the region for a shorter period each year, extending the dry season. Based on the available evidence this seems like quite a story, and one not firmly based in evidence. The authors should be aware too that more sand in the Mfabeni sequence is not necessarily just a function of wind strength. Shifting sea levels and changes in sediment supply, precipitation and vegetation could also have changed the nature of aeolian sediment fluxes. For the wind story to be solid the authors should seek another record for support.

Lines 456-465: Where is the imagined moisture source for the frontal systems that influence this region? It isn't the SE Atlantic. And really, a northerly shift of the westerlies would (based on observance of the modern annual cycle) probably be manifested through the development of composite systems that primarily draw moisture from off of SE Africa, albeit with a slightly more southerly component. I am having a hard time understanding this logic, so please include a clearer map of moisture sources and transport vectors.

Lines 469-470: Are the authors saying that Chevalier and Chase, 2015 suggested that increased precipitation after 19 ka was related to insolation? This is not my reading, and they rather suggest that the region is still dominated by Northern Hemisphere

C10

influences at that time.

Lines 471-474: The link between vegetation and hydroclimate is rarely strictly linear, but looking at the dD and central and eastern SRZ precipitation reconstruction, it seems pretty straightforward. More rain/moisture, more trees.

Lines 474-475: Need again to clarify what the authors mean by the westerlies having an influence. This regional dynamic needs to be described in much more detail and clarity. Where does this moisture come from?

Lines 475-477: The site described in Chase et al., 2017 is in the YRZ, not the WRZ. From the WRZ there is the site at De Rif (Chase et al., 2015). It does not show a shift to more arid conditions.

Lines 486-487: In fact, Schefuß et al., 2011 and Chevalier and Chase, 2015 have said that insolation is only significant during the Holocene, not the late Quaternary as a whole (the data of Partridge et al., 1997 also show this, albeit more coarsely). Again here, the westerlies shift south and then... what occurs to increase precipitation/moisture? A fuller perspective including all the related systems is required.

Lines 488-492: Neither Seweweekspoort or Eilandvlei are in the WRZ. Both are in the YRZ.

Lines 534-541: As the authors state, the difference in sampling resolution means that little can be conclusively determined from the apparent increase in variability. It would be wiser to step back from commenting on this, I feel.

REFERENCES INCLUDED IN THIS REVIEW: Chase, B.M., Boom, A., Carr, A.S., Carré, M., Chevalier, M., Meadows, M.E., Pedro, J.B., Stager, J.C., Reimer, P.J., 2015. Evolving southwest African response to abrupt deglacial North Atlantic climate change events. *Quaternary Science Reviews* 121, 132-136. Chase, B.M., Chevalier, M., Boom, A., Carr, A.S., 2017. The dynamic relationship between temperate and tropical circulation systems across South Africa since the last glacial maximum. *Quaternary Science*

C11

Reviews 174, 54–62. Chevalier, M., Brewer, S., Chase, B.M., 2017. Qualitative assessment of PMIP3 rainfall simulations across the eastern African monsoon domains during the mid-Holocene and the Last Glacial Maximum. *Quaternary Science Reviews* 156, 107–120. Chevalier, M., Chase, B.M., 2015. Southeast African records reveal a coherent shift from high- to low-latitude forcing mechanisms along the east African margin across last glacial–interglacial transition. *Quaternary Science Reviews* 125, 117-130. Clark, P.U., Dyke, A.S., Shakun, J.D., Carlson, A.E., Clark, J., Wohlfarth, B., Mitrovica, J.X., Hostetler, S.W., McCabe, A.M., 2009. The Last Glacial Maximum. *Science* 325, 710-714. Cowling, R.M., Cartwright, C.R., Parkington, J.E., Allsopp, J.C., 1999. Fossil wood charcoal assemblages from Elands Bay Cave, South Africa: implications for Late Quaternary vegetation and climates in the winter-rainfall fynbos biome. *Journal of Biogeography* 26, 367-378. Harris, C., Burgers, C., Miller, J., Rawoos, F., 2010. O- and H-isotope record of Cape Town rainfall from 1996 to 2008, and its application to recharge studies of Table Mountain groundwater, South Africa. *South African Journal of Geology* 113, 33-56. Holmgren, K., Lee-Thorp, J.A., Cooper, G.R.J., Lundblad, K., Partridge, T.C., Scott, L., Sitaldeen, R., Talma, A.S., Tyson, P.D., 2003. Persistent millennial-scale climatic variability over the past 25,000 years in Southern Africa. *Quaternary Science Reviews* 22, 2311-2326. Nicholson, S.E., 2009. A revised picture of the structure of the “monsoon” and land ITCZ over West Africa. *Climate Dynamics* 32, 1155-1171. Otto-Bliesner, B.L., Russell, J.M., Clark, P.U., Liu, Z., Overpeck, J.T., Konecky, B., deMenocal, P., Nicholson, S.E., He, F., Lu, Z., 2014. Coherent changes of southeastern equatorial and northern African rainfall during the last deglaciation. *Science* 346, 1223-1227. Sachse, D., Billault, I., Bowen, G.J., Chikaraishi, Y., Dawson, T.E., Feakins, S.J., Freeman, K.H., Magill, C.R., McInerney, F.A., van der Meer, M.T.J., Polissar, P., Robins, R., Sachs, J.P., Schmidt, H.-L., Sessions, A.L., White, J.W.C., West, J.B., Kahmen, A., 2012. Molecular paleohydrology: interpreting the hydrogen-isotopic composition of lipid biomarkers from photosynthetic organisms. *Annual Review of Earth and Planetary Sciences* 40, null. Sonzogni, C., Bard, E., Rostek, F., 1998. Tropical sea-surface temperatures during the last glacial period: a view based

C12

on alkenones in Indian Ocean sediments. *Quaternary Science Reviews* 17, 1185-1201. Tyson, P.D., 1986. *Climatic Change and Variability in Southern Africa*. Oxford University Press, Cape Town. Tyson, P.D., Preston-Whyte, R.A., 2000. *The Weather and Climate of Southern Africa*. Oxford University Press, Cape Town. van Zinderen Bakker, E.M., 1976. The evolution of late Quaternary paleoclimates of Southern Africa. *Palaeoecology of Africa* 9, 160-202. Wu, H., Zhang, X., Xiaoyan, L., Li, G., Huang, Y., 2015. Seasonal variations of deuterium and oxygen-18 isotopes and their response to moisture source for precipitation events in the subtropical monsoon region. *Hydrological Processes* 29, 90-102.

Interactive comment on *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2019-4>, 2019.