Author's response to Anonymous Referee #2

We would like to thank this reviewer for their thorough review and useful comments. We reply to each comment individually below. The reviewer comments are in black and our responses are in blue.

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.

- We appreciate the acknowledgement from the reviewer about the relevance and importance of our work, and thank him/her for their comments, which we address below. We will address all comments and will make substantial amendments to the manuscript as such.

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

- We will include pollen % data of key taxa from the original palynological dataset (Finch and Hill, 2008) to our paper (in Fig. 6). In order to compare the datasets we have produced a new age model for the old palynological data (from core; MF1). We will add this new age model to the supplementary material of our paper (S2). Throughout the paper, we will discuss our results in light of the palynological results.

The authors also appear to base their palaeoclimatic interpretations on the supposition that the amplitude of dD change (~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 dDwax and mean annual precipitation are strongly correlated in the tropics (e.g. Sachse et al., 2012 and references therein)).

- The peatland today is extremely sensitive to changes to changes in both precipitation and evaporation, with the modern water balance dominated by the interplay between evapotranspiration (ET; 1035 mm) and precipitation (1053 mm). Consequently, at Mfabeni, any changes in both ET and precipitation have great potential to influence the δD_{wax} values. However, it is difficult to disentangle whether changes in δD_{wax} are due to changes in ET or precipitation amount, and indeed the isotopic variability within the Mfabeni record could be a result of either. Nevertheless, both high (low) ET and low (high) precipitation amount lead to δD_{wax} enrichment (depletion) and thus imply generally drier (wetter) climatic conditions. We will amend the label of Fig. 4c to 'drier and wetter' and will clarify the δD_{wax} interpretation throughout the manuscript.

In the African tropics, dDwax records from lake and marine sediments exhibit ranges of ~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.

- We agree with the reviewer that lower temperatures during the last glacial period would result in lower ET. For example using the equation of Kosa (2009), a 2 degree annual temperature increase from the last glacial period (*c*. 24°C) to the present day (average temperatures *c*. 26°C) would equate to a change in mean actual evapotranspiration of 0.72 mm/day. Nevertheless, numerous additional factors control evapotranspiration, not only temperature, such as the amount of incoming solar radiation, humidity and wind speed. We will amend the 'Interpretation of the proxy signals' section to make clearer the drivers of Mfabeni δD_{wax}.

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 dDwax record as a wind strength proxy.

- We agree with the reviewer and will change this interpretation throughout the manuscript. Previously, in places, too much focus was on ET and how this could be influenced by wind. δD_{wax} values at Mfabeni are driven by changes in **both** ET and precipitation amount. We will now discuss more thoroughly the climatic mechanisms driving ET and precipitation amount, especially focussing on the latitudinal position of the westerlies, the South African high-pressure cell and the South Indian Ocean Convergence Zone (SICZ).

Considering the remarkable similarities between their dDwax 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 dDwax 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 dDwax records from tropical Africa be revisited, and mechanisms for increased glacial-age ET at each site be found?

- With the δD_{wax} record similar to both the pollen-based precipitation and aridity stacks of Chevalier and Chase, 2015 & 2016, and as it is impossible to disentangle the effects of precipitation and ET amount on Mfabeni δD_{wax} values, we suggest that the pollen-based precipitation stacks may also include an element of ET variability. We do not suggest that other δD_{wax} records from tropical Africa be revisited, but as the system at Mfabeni is extremely sensitive to changes in ET we should not exclude the role that ET plays in the water balance at this site.

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 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.

- We aim to amend our discussion and interpretation as follows. We will include more details on how the position of the southern westerlies (and the South African high-pressure cell and the SICZ) may control precipitation amount/ET at Mfabeni. For example, rainfall along the eastern coast of South Africa is today strongly correlated to the latitudinal position of the subtropical high-pressure system (e.g. Dyson and van Heerden, 2002). Following the modelling results of Vigaud et al. (2009), we now attribute a northward shift of the South African high-pressure cell to reduced moisture in eastern South Africa. When the cell is shifted southward, during the summer, the tropical easterlies are able to penetrate further inland, resulting in more continental moisture availability. Conversely, when the cell is shifted northward, during the winter, monsoonal circulation south of 25°S is weakened, creating a deficit in moisture advection from the ocean to the continent. The more northerly location of the high pressure cell (which we propose occurs during the LGM) reduces the ability for the tropical easterlies to penetrate inland, limiting the advection of moisture over the continent, resulting in arid conditions at Mfabeni. We will also discuss the role of the SICZ and the rain-bearing cloud band associated with tropical temperate trough (TTT) development. A northward migration of the westerlies is associated with a weaker South Indian Anticyclone, which results in a north-eastward shift of the SICZ, and thus lower precipitation at Mfabeni. We will also amend the regional settings section, including more information about how the westerlies, the subtropical high-pressure cell and the SICZ control climate across South Africa.

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

- We agree and will change the title to 'Late Quaternary climate variability at Mfabeni Peatland, eastern South Africa'.

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.

- We agree and will amend this sentence.

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.

- It is difficult to go into much detail in the abstract but we will add a little more detail to include the climatic process behind how changes in the latitudinal position of the westerlies (and the high-pressure cell and SICZ) may result in dry conditions at Mfabeni.

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.

- We agree and will change the sentence to 'Changes in vegetation, precipitation and temperature from the beginning of the Last Glacial Maximum (c. 26.5 ka; Clark et al., 2009) to present-day, in eastern South Africa are poorly constrained.'

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 use in Line 268).

- We will first define the LGM here. The duration of the LGM will be defined slightly later, following the definition of Clark et al. (2009).

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

- We will add the reference Chevalier et al., 2017.

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.

- We will add reference to the greenhouse gas climate driving mechanism from Otto-Bliesner et al., 2014. We will also add the Chevalier and Chase, 2015 reference to the parts of the paragraph here describing insolation and SST forcing.

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.

- We agree with the reviewer and will change the term ITCZ to 'tropical rainbelt'.

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.

- We agree with the reviewer and will remove reference to the winter rainfall zone here. We will change the sentence to 'This possibly resulted in an eastward displacement of the coastal cloud band and thus a drier summer rainfall zone (SRZ; Tyson, 1999).' We will also changed the reference to Tyson (1999).

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.

- We will amend the regional setting section thoroughly giving a better overview of climate systems. We will start off broad-scale, focussing on the whole of SA, and then in the following paragraphs focus on Mfabeni and eastern South Africa. This will create a better flow and assist later in the interpretation. Furthermore, we will change figure 1 (as requested by reviewer #1) to include regional climate dynamics and differences between the summer and winter seasons.

Line 65: "OVER the Indian Ocean"

- We will correct to 'over' the Indian Ocean.

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

- We will remove the two previous references and replace them with van Zinderen Bakker 1976 and Stuut et al., 2004.

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.

- We agree, and will remove the reference Chase et al., 2017 and replace it with the reference Chevalier and Chase., 2015.

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

- We will remove the capital and add 'period'.

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

- Again we will remove the capital.

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).

- We agree and will replace ITCZ in all cases throughout the paper with 'tropical rainbelt'.

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.

- We will remove the Chevalier and Chase and Holmgren et al., 1996 and add instead Holmgren et al., 2003 as a reference here.

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

- We agree and will change this to 'can be divided'.

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.

- Firstly, we will discuss tropical temperate troughs (TTT) more thoroughly throughout the paper, beginning in the regional settings section and then later in light of how changes in the SICZ result in changes in the location of TTT formation. We appreciate that TTTs are lumped together as TTIs in Chase et al. (2017) but as the majority of literature (both old and new) refer to TTTs we would like to keep our terminology consistent with those papers. We will also add a paragraph later in section 6.3 where we will discuss TTTs and millennial-scale variability over the G-IG transition.

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.

- We agree and will include this within the 'regional setting' section.

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

- We will change temporal to temperate, and reference Tyson, 1986 and Tyson and Preston-Whyte, 2000.

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?

- We agree and will re-phrase this sentence.

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

- We agree, and will remove this reference.

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.

- We will indicate where ages are calibrated with 'cal'.
- We will use ka for ages, and kyr for durations.

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?

- We will change this sentence and remove the wind focus and the Kruger reference. The sentence will now read 'Occasional rainfall during the winter months at Mfabeni is associated with the passage of cold fronts, which develop in the western Atlantic and move across southern Africa (Grab and Simpson, 2000).'

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

- We agree and will disentangle and re-structure this section to improve the flow. The section will now follow on from the regional climate settings to Mfabeni local climate. The next paragraph will focus on the geological and morphological settings and how this effects groundwater. Finally the section will end with a paragraph on the sensitivity of the modern water balance at Mfabeni and the suitability of Mfabeni for palaeoenvironmental reconstruction.

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

- This will now be explained in detail in the section '6.1 interpretation of the proxy signals'.

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.

- We will add an extra paragraph here explaining the environmental conditions favouring the expansion of C_3/C_4 distribution.

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

- We will change this to 'higher'.

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.

- We agree and will remove colder conditions from the list of conditions enabling C_4 vegetation expansion. In fact, higher temperatures result in C_4 vegetation expansion. This paragraph will now be more comprehensive, and will include additional factors.

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).

- We agree and will remove reference to tropical rain provided by the easterlies here. It will now state 'less precipitation'.

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.

- We agree and will change the LGM–Holocene temperature change estimate to 6°C (from Gasse et al., 2008). This will give a potential LGM to Holocene δD_{precip} *enrichment* of 24‰. Like the reviewer states, the point here in the paper remains the same, our δD_{wax} data displays a LGM–Holocene *depletion*, and thus temperature did not exert a dominant control on Mfabeni δD_{wax}.

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

- We will rewrite this section to explain how different vegetation types have different δD_{wax} signals.

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.

- Agreed. We will remove this statement, as it was unclear and unnecessary.

Lines 336-241: Again here, how do the authors determine that ET is significantly more important that 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.

- We will re-write this section to include more information about the amount effect.

Lines 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?

- We will re-write and restructure the whole δD_{wax} interpretation section. We will first discount temperature and vegetation effects, and then go on to discuss the importance of the amount effect and evaporation in controlling δD_{wax} at Mfabeni.

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 dDprecip (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?

- The δD_{wax} interpretation section will be re-written and re-structured. Our results do not invoke that ET is necessary to explain the δD_{wax}, rather, that we cannot disentangle whether ET or precipitation amount is responsible for the changes. Ultimately, both ET and precipitation amount are highly related and the effect is the same: high δD_{wax} values = drier, low values = wetter.
- In section 6.3, 'climate driving mechanisms', we will explain the mechanisms further. We will attribute the arid climate and the associated expansion of drought tolerant C_4 plants and a low water table at Mfabeni during the LGM, to both lower SSTs and to a northward displacement of the southern hemisphere westerlies, the South African high-pressure cell and the SICZ, shifting the hydroclimate to a more evaporative regime, where ET exceeds precipitation. We understand that cooler temperatures during the LGM would limit ET, but with the latitudinal position of these systems directly controlling the amount of moisture advection towards the southern African subtropics (e.g. Vigaud et al., 2009), we believe that this would easily shift the sensitive balance between ET and precipitation at Mfabeni to a more ET regime.
- The source of precipitation is always from the Indian Ocean, whether the precipitation comes from frontal systems during the winter months or from monsoonal precipitation during the summer (see Gimeno et al., 2010). Thus, as the dD is mainly controlled by ET and precipitation amount, it is not possible to differentiate between winter or summer rain-bearing systems, only the balance between precipitation and ET.

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

- We will change this sentence to 'Drier conditions during the LGM correspond with low Paq values that indicate a higher relative contribution of terrestrial-over-aquatic n-alkanes, likely a consequence of a lower water table (Fig. 4f).'

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

- With the majority of precipitation at Mfabeni (76%) falling within the summer months, sourced from the tropical easterlies (and no evidence for any change in water source during the last *c.* 30 ka, we infer changes in precipitation to represent changes in summer precipitation.

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

- A lower water table and drier soil conditions would likely serve to increase the D enrichment, having a similar effect as increased ET/reduced precipitation. We will now include this in the manuscript.

Line 379-381: How do the authors explain an increase in temperature resulting in more C3 grasses? This really must be explained.

- We will re-write this section to explain how the climatic changes from the LGM to the Holocene could have resulted in an expansion of more C_3 grasses. We do not think that temperature drove the changes in vegetation at Mfabeni.

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).

- We will add the pollen % data of key taxa to Fig. 6 and replace the descriptive pollen summary. We will discuss this pollen data in light of our new isotope data.
-

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

- We will 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.

- We agree with the reviewer here and will add a little more information to the climate driving mechanisms section, that the results here provide support to previous studies e.g. Schefuß et al., 2011; Chevalier and Chase, 2015) that insolation controls on precipitation variability was only significant since the Holocene.

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.

- The δD_{wax} record at Mfabeni is predominantly a proxy for moisture availability, whether it be changes in precipitation or ET amount. We will make our interpretation of the proxy signal section clearer (section 6.1).
- The key question is what is driving the changes in precipitation and ET amount? We agree that the grain size records from the site are not overly convincing. We will amend the climate driving mechanisms section and remove focus from wind strength as a driver for changes in ET amount. Although the focus of our discussion will still involve the latitudinal position of the westerlies in driving environmental change at Mfabeni, we will provide more discussion regarding the processes behind this mechanism and how this influences Mfabeni climate.

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

- Firstly, we will completely remove the final sentence 'The long-term drying trend is unlikely to be caused by decreased summer precipitation because local summer insolation and Mozambique Channel SSTs are high (Fig. 5a & Fig. 4j). Instead, the general drying trend is more likely a result of heightened ET during the late Holocene.' This will keep the section (climate and environmental conditions) as solely for describing the climate and environmental conditions, with no suggestion of driving mechanisms. That said, our evidence points to the period between *c*. 5–0 cal kyr BP as arid.
- We agree with the reviewer and will replace the SST record of Wang et al. (2013), with the older record of Sonzogni et al. (1998) in our Fig. 4j. We will also remove the record of Wang (GIK16160-3) from Fig. 1 and replace it with MD79257 (Sonzogni et al., 1998).
- Nevertheless, even with our data plotted up against the Sonzogni et al. (1998) record, as suggested by the reviewer, we still do not see a clear relationship between SSTs and climatic change at Mfabeni. For example SSTs during the last glacial period remain low until c. 16 ka, whereas a switch from arid to wet conditions at Mfabeni (and within the precipitation stack) appears to occur well before this, at *c*. 19 ka. Thus we stick by our original statement that 'Mfabeni vegetation and hydrology reconstructions over the last 32 cal kyr BP do not show a clear relationship with changes in southwest Indian Ocean SSTs'. Thus we suggest an additional driver to SSTs (i.e. the migration of the westerlies, high-pressure cell and the SICZ).
- We agree with the reviewer that previously too much focus was on ET, when the δD_{wax} record here is predominantly a proxy for moisture availability. We will remove this from the discussion here and now will give multiple possible reasons that the climate at Mfabeni was arid during the last 5 ka. These are: i) equatorward migration of the westerlies, the high-pressure cell and the SICZ (and the mechanisms of these will be explained in the 'climate driving mechanisms' section), ii) a decrease in Mozambique Channel SSTs, iii) anthropogenic influences and/or, iv) ENSO activity.
- Finally, we much appreciate the reviewer highlighting the Sonzogni et al. (1998) SST record. The slight decrease in SST over the last 5 ka is particularly interesting and potentially a cause for the aridity evidenced at Mfabeni. We will now add this observation to the manuscript.

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.

- In this paragraph we point out that SST is not the **sole** mechanism driving vegetation and hydrological changes at Mfabeni. We stand by this statement. We acknowledge that SSTs may be important during the LGM and the last *c*. 5 ka. Our data supports previous hypothesis that various mechanisms are important, with insolation only becoming important during the Holocene when the westerlies are located poleward.

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.

- As discussed above we will now compare the Mfabeni record with the SST record of Sonzogni et al. (1998). Indeed there certainly is similarity, but the SST record does not explain the entirety of the Mfabeni record.

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.

- We will amend the discussion to incorporate the previous hypothesis of Chevalier and Chase (2015) and Chase et al. (2017). Our data support the previous hypothesis that the differences observed between SSTs and the records comprising the 'central and eastern precipitation stack' was due to the modulation of precipitation by the position of the westerlies.
- We will also add a paragraph of discussion about the interaction between tropical and temperate systems resulting in the formation of 'tropical temperate interactions'. In the Chase et al. (2017) paper these interactions are used to explain millennial-scale climatic variability especially during the glacial-interglacial transition.

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"?

- We will re-write the climate driving mechanisms and discussion section. Here we will explain how the westerlies drive climate in this region. Our hypothesis differs from that of Chase et al. (2017). We invoke that the position of the westerlies is important (and possibly SSTs), no matter the state of local summer insolation. Although millennial scale variability may be explained by TTTs, they are not necessary to explain the climatic variability within the Mfabeni record. Our hypothesis is quite simple and we will better explain. When Antarctic sea ice is expanded this causes an equatorward shift of the westerlies (i.e. during the LGM and last *c*. 5 ka). The expanded sea ice also causes a northward displacement of the high-pressure system (e.g. Vigaud et al., 2009), which is responsible for limiting rainfall across much of the interior of south Africa during the austral winter months. When the high-pressure system is shifted northwards, then monsoonal circulation south of 25°S is weakened, because the monsoon cannot penetrate the continent. This results in aridity at Mfabeni (lower precipitation and/or higher ET). Furthermore, a more north-easterly displaced SICZ, as a result of the equatorward shifted westerlies (e.g. Cook, 2000), may also play a role in driving aridity at Mfabeni.

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

- Noted, we will change this sentence.

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.

- We agree with the reviewer. This section will now be re-written and reference to wind as a mechanism for explaining the aridity removed.

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

- We agree and will re-write this section. The source of precipitation in eastern South Africa is always from the Indian Ocean/Agulhas region, whether it be from frontal systems during the winter months or from direct monsoonal precipitation during the summer (see Gimeno et al., 2010).
- With more northerly-displaced westerlies (i.e. during the LGM), more frontal systems would likely reach the region. Although today frontal systems do bring moisture during the winter months to the region, an increase in the occurrence of frontal systems (with more northerly displaced westerlies during the LGM) did not result in an increase in moisture availability at Mfabeni. We propose that the more northerly location of the high-pressure cell over the South African interior during the LGM may have limited the amount of moisture advection towards Mfabeni, thus even with increased cyclone occurrence, arid conditions persisted.

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 influences at that time.

- Agreed, we will remove this reference here.

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.

- We completely agree, the switch in vegetation is surely related to more rainfall. However, please note, the G-IG shift in vegetation we observe is likely not a shift from grasses to trees (with the palynological data suggesting a continuation of grassland). Furthermore, with the vegetation shift abrupt and the dD record displaying a more gradual shift to wetter conditions it is worth noting that a critical threshold in moisture availability must have been crossed to allow the establishment of C_3 type vegetation.

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?

- This sentence will now be removed and the influence of the westerlies explained in more detail within the climate driving mechanisms section.

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 a show a shift to more arid conditions.

- We will remove this reference and this sentence.

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).

- We agree and will amend this sentence accordingly.

Again here, the westerlies shift south and then what occurs to increase precipitation/moisture? A fuller perspective including all the related systems is required.

- The westerlies and the mechanisms for controlling climate will be better explained within the Climate driving mechanisms section.

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

- Very good point. This sentence will be amended.

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.

- Agree, we will remove this section regarding the enhanced climatic variability during the last *c*. 5 kyr BP.

References cited in this rebuttal:

Chase, B.M., Chevalier, M., Boom, A., Carr, A.S.: The dynamic relationship between temperate and tropical circulation systems across South Africa since the last glacial maximum, Quaternary Sci.Rev., 174, 54-62, 2017.

Chevalier, M., Chase, B.M.: 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 Sci.Rev., 125, 117-130, 2015.

Chevalier, M., Chase, B.M.: Determining the drivers of long-term aridity variability: a southern African case study. J. Quaternary Sci., 31, 143-151, 2016.

Chevalier, M., Brewer, S., and Chase, B. M.: 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, 2017.

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.: The Last Glacial Maximum, Sci. J., 325, 710-714, 2009.

Cook, K. H.: The South Indian Convergence Zone and Interannual Rainfall Variability over Southern Africa, Journal of Climate, 13, 3789-3804, 2000.

Dyson, L. L. and Van Heerden, J.: A model for the identification of tropical weather systems over South Africa, Water SA, 28, 249-258, 2002.

Finch, J.M., Hill, T.R.: A late Quaternary pollen sequence from Mfabeni Peatland, South Africa: Reconstructing forest history in Maputaland. Quaternary Res., 70, 442-450, 2008.

Gimeno, L., Drumond, A., Nieto, R., Trigo, R. M., and Stohl, A.: On the origin of continental precipitation, Geophysical Research Letters, 37, 2010.

Grab, S. W., and Simpson, A. J.: Climatic and environmental impacts of cold fronts over KwaZulu-Natal and the adjacent interior of southern Africa South African Journal of Science, 96, 602-608, 2000.

Gasse, F., Chalié, F., Vincens, A., Williams, M. A. J., and Williamson, D.: Climatic patterns in equatorial and southern Africa from 30,000 to 10,000 years ago reconstructed from terrestrial and near-shore proxy data, Quaternary Science Reviews, 27, 2316-2340, 2008.

Kosa, P.: Air Temperature and Actual Evapotranspiration Correlation Using Landsat 5 TM Satellite Imagery, Kasetsart J. Nat. Sci., 43, 605-611, 2009.

Holmgren, K., Lee-Thorp, J. A., Cooper, G. R. J., Lundblad, K., Partridge, T. C., Scott, L., Sithaldeen, R., Siep Talma, A., and Tyson, P. D.: Persistent millennial-scale climatic variability over the past 25,000 years in Southern Africa, Quaternary Science Reviews, 22, 2311-2326, 2003.

Otto-Bliesner, B. L., Russell, J. M., Clark, P. U., Liu, Z., Overpeck, J. T., Konecky, B., deMenocal, P., Nicholson, S. E., He, F., and Lu, Z.: Coherent changes of southeastern equatorial and northern African rainfall during the last deglaciation, Science, 346, 1223-1227, 2014.

Schefuß, E., Kuhlmann, H., Mollenhauer, G., Prange, M., Pätzold, J., Forcing of wet phases in southeast Africa over the past 17,000 years, Nature, 480, 509, 2011.

Sonzogni, C., Bard, E., and Rostek, F.: Tropical sea-surface temperatures during the Last Glacial Period: A view based on alkenones in Indian Ocean sediments, Quaternary Science Reviews, 17, 1185-1201, 1998.

Stuut, J.-B.W., Lamy, F.: Climate variability at the southern boundaries of the Namib (southwestern Africa) and Atacama (northern Chile) coastal deserts during the last 120,000 yr, Quaternary Res., 62, 301-309, 2004.

Tyson, P.D.: Climatic Change and Variability in Southern Africa, Oxford University Press, Cape Town, 1986.

Tyson, P.D.: Atmospheric circulation changes and palaeoclimates of southern Africa, S. Afr. J. Sci., 95, 194-201, 1999.

Tyson, P. D. and Preston-Whyte, R. A.: The Weather and Climate of Southern Africa, Oxford University Press, Southern Africa, Cape Town, 2000.

van Zinderen Bakker, E. M.: The evolution of Late Quaternary palaeoclimates of southern Africa, Palaeoecology of Africa, 9, 160-202, 1976.

Vigaud, N., Richard, Y., Rouault, M., and Fauchereau, N.: Moisture transport between the South Atlantic Ocean and southern Africa: relationships with summer rainfall and associated dynamics, Climate Dynamics, 32, 113-123, 2009.

Wang, Y.V., Leduc, G., Regenberg, M., Andersen, N., Larsen, T., Blanz, T., Schneider, R.R.: Northern and southern hemisphere controls on seasonal sea surface temperatures in the Indian Ocean during the last deglaciation, Paleoceanography, 28, 619-632, 2013.