Clim. Past Discuss., 10, C2405–C2424, 2015
www.clim-past-discuss.net/10/C2405/2015/

Climate of the Past Discussions

Interactive comment on "Northern Hemisphere control of deglacial vegetation changes in the Rufiji uplands (Tanzania)" *by* I. Bouimetarhan et al.

I. Bouimetarhan et al.

bouimetarhan@uni-bremen.de

Received and published: 4 March 2015

Dear Prof. Martin Claussen,

Thank you for encouraging us to submit a revised version of our manuscript entitled "Northern hemisphere control of deglacial vegetation changes in the Rufiji uplands (Tanzania)". We are grateful to both reviewers for their constructive comments and insightful suggestions. Hereby we respond point by point to all the comments.

Response to Reviewer Sarah Ivory

General Comments: Although the main findings of this study of great interest, I find that there are a few problems that should be addressed throughout the paper. In particular, I thought the most novel findings in this paper are the implications for coastal processes

and ecosystems, rather than the broader regional paleoclimate synthesis. I don't think the paleoclimatic implications should be removed; however, I suggest a few changes to focus more strongly on these important and rare ecological insights.

-With the help of the specific suggestions of the reviewer, we have changed the text to lay more emphasis on the coastal vegetation development. Paragraph 5 is now entirely dedicated to the deglacial ecological implications.

Specific Comments: Abstract, Line 12, This sentence is a little confusing. I think the link the author is trying to make is a teleconnection between "arid" conditions in East Africa and cool northern hemisphere temperatures. This might be rephrased to show that. Also the term "dry spell" seems very colloquial, might change that to "arid period". This change should be made also for other instances of "dry spell" and "cold spell"in the paper.

-We have rephrased this paragraph in the revised manuscript as such it shows the link between arid East Africa and northern hemisphere cold Heinrich event 1. We have replaced "dry spell" by "arid period" here and throughout the entire manuscript.

Page 3933 Line 17, the author mentions that we don't really have a sense of what is influencing rainfall variability, then says that Indian Ocean SSTs are dominant on long time scales. I would back off a little on that, because it seems like the author is setting up a strawman or already making a conclusion on the most important mechanism in a very complex system. Another thing is that here the author compares the mechanisms controlling millennial scale variability in North Africa with those on all time scales in East Africa. Maybe just cite the mechanisms we think may influence rainfall in East Africa on millennial time scales here for consistency

-In this paragraph we are only citing the mechanisms that have been evoked to influence rainfall variability on both short and log-term scales. We did not attempt to make any conclusions or giving advantage to one mechanism on the other because we know that east Africa is definitely a very complex system and mechanisms are al-

ways a matter of debate. We agree with the reviewer that it is confusing to put short and long time scales mechanisms in this way and to compare millennial scale variability in North Africa with all time scales in east Africa. We have therefore, rephrased this paragraph in the revised manuscript as such we compare first the millennial timescale mechanisms, Indian Ocean SSTs and the latitudinal shift of the ITCZ and then, the interannual timescale mechanisms IOD and ENSO.

Page 3934 Line 9, The author says that there is no consensus about which definitive climatic pattern is related to vegetation change, but around Line 15, only one mechanism is mentioned (ie North Atlantic climatic perturbations).

-This sentence is a reminder of what we have mentioned earlier about the several mechanisms that have been proposed to explain climate and vegetation change. We agree with the reviewer that the paragraph is awkwardly written. We have rephrased it in the revised version.

I feel like the author is trying to find a reason to convince people that marine records have some advantages over terrestrial records, which I completely agree with, but I wonder if this is the best way to do it. I don't see how one extra record, just because its marine, has the power to resolve all of the complexity about East African climate.

-We are definitely not trying to underestimate terrestrial pollen records. We are stating in the previous paragraph the importance of terrestrial pollen records in reconstructing environmental changes in the area. To avoid any misunderstanding, we rephrased the text as such the marine pollen records, provided they have sufficient temporal resolution, can complement the existing records with giving a more regionally integrated signal.

The author talks in the abstract about being able to observe coastal processes and also mangrove changes. This to me seems like the real advantage of this record, that virtually no one has looked at coastal vegetation changes in the region. East African coastal vegetation is a major biodiversity hotspot (Myers, 2000), plus mangroves are

very important ecosystem that have not been intensively studied, so I think you could focus your justification for the project more in ecological terms than in climatic terms. This is just a suggestion, but I think focusing on the ecological implications rather than the climatic ones would highlight the real reasons this paper is cool and interesting!

-We agree with the reviewer about the importance of ecological implications. We have rephrased the paragraph as such it emphasizes more the advantage of looking at coastal vegetation changes in this region so far, overlooked. We also have dedicated paragraph 5 entirely to these new ecological implications for the coastal processes in tropical southeastern Africa. However, the climatic implications are also important for the understanding of this highly climatically complex area and the results obtained in this study have complemented the existing body of evidence that shows a strong link between Northern Hemisphere climatic fluctuations and tropical southeast African climate and further the north-south rainfall dipole between subtropical southern Africa and equatorial eastern Africa.

Also it may be of use to do a little comparison with other mangrove systems that have been looked at in paleo-studies. Anne-Marie Lezine has looked at Holocene age mangroves in Oman and there are a few other records from that region. They are more recent in age, but talk about some of the eustatic and local processes involved in expansion and collapse of these systems

-We have extensively compared our records with Punwong work on the Holocene mangrove in the Tanzanian coast (Rufiji Delta and Zanzibar coast). To meet the reviewer's suggestion we included additional comparisons with other records (Lézine's work in Oman) and extended the discussion part in paragraph 5 accordingly.

Page 3938 line 15, How were the pollen abundances calculated (ie. Including or excluding aquatics and Cyperaceae and mangrove taxa)? I just noticed that the author does state the mangrove is excluded later in the article. This might be relevant to mention in the methods.

-Pollen abundances are expressed as percentages of total pollen including herbs, shrubs, trees and aquatics.

-In Fig. 8, in order to get more insights into the upland environmental signal, salt marshes and mangrove that dominate the vegetation record, with pollen percentages accounting for up to 80% of the total assemblage and overprinting the signal of other taxa, have been excluded from the total pollen sum to get a clearer picture.

-We added a paragraph in material and methods in the revised version to make it clearer to the reader.

Page 3937 end of page, what is the interpretation of Al/Ca and why was this selected? Some interpretation of this proxy is needed.

-As mentioned in the text, we have measured the following elements Fe, Al, Ba, and Ca. Fe and Al are related to siliciclastic sediment components and vary directly with the terrigenous fraction of the sediment. Ca mainly reflects the biogenic carbonate content. Ba is mainly used as indicator of productivity.

-Elemental ratios such as Fe/Ca and Al/Ca are frequently used as proxy of the ratio between terrigenous and marine materials. Since Fe is a redox-sensitive element (unstable during the early diagenesis), we have chosen the Al and thus the Al/Ca ratio as a robust record of the terrigenous input, which in our study area is associated to river runoff as the wind system is dominated by northeasterly and southeasterly trade winds, which are not favorable for transporting terrigenous material from the continent to the Indian Ocean.

-We added a paragraph for a brief interpretation of the selected proxy as requested by the reviewer.

Page 3936 Line 17 –what is the temporal resolution?

-What we meant here by high resolution is that the core has high sedimentation rates. The average sedimentation rate is 52cm/kyr which results in an average temporal res-

olution of $\sim$19 years/cm. We have removed it from the revised version as it is quite confusing.

Page 3939 Line10–If most pollen is delivered via fluvial transport, how do variations in transport potentially influence your record? It seems like your high pollen concentrations occur mostly when you have higher sedimentation rates? Is that the case? A sentence about this might be good to include.

-Pollen grains are transported from the continent to the ocean, i.e. eastward via fluvial transport. Indeed, as the reviewer stated, when we have more fluvial activity, we receive more sediments and thus more pollen in our site. We have included a sentence stating the simultaneous increase of pollen concentrations, Al/Ca ratios and sedimentation rates at the end of the paragraph.

Page 3939 Line 15 –Most of this Results text should be in past tense when talking about events that happened in the past.

-We have changed the tense to the past in the revised manuscript.

Page 3941 line 14, Is there a sense of how much 80-120m sea level change would affect the proximity of the core site to the coast?

-Regarding the period considered in the study (HE1 / Termination 1), 80-120m lower sea-level than today (e.g. Siddal et al., 2003, 2010; Lambeck and Chappell, 2001) would get the core location much closer to the shoreline which would make it very sensitive to record the repercussions of sea level change on the stability of the Rufiji Delta deposits. We have included the bathymetric map of the area in figure 1 exactly for the purpose to visualize this. We have added an extra sentence for clarification in the revised version of the manuscript.

Page 3942 Line 24, reference for “Afromontane forest mainly developed in mountains favored by cold and humid conditions.” Is this based on knowledge of the environmental tolerance for these plants or correlation to a paleoclimatic record?

-It is based on knowledge of the environmental tolerance of this plant community (White, 1983, Kindt et al., 2011). We have added the references in the revised version.

Page 3947 line 13 The author mentioned earlier in the paper that some of the vegetation changes (lowering of afromontane vegetation) may also be linked to temperature, not just precipitation

-This comment is a little confusing, we are not sure if we understand what the reviewer means in this context. Afromontane vegetation that expands in mountains favored by cold and humid conditions was well developed before H1, indicating a lowering of this vegetation due to cooler conditions in lower altitudes. However, in this paragraph (L13), we are specifically talking about H1, the arid interval where afromontane forest declined steadily.

Table 1. The author lists Artemisia as a common Somali-Masai taxon in the description of the modern vegetation, but in this table you have it listed as Afromontane. Since your record integrates lowland and highland, it may be the case that it is difficult to say whether Artemisia here represents arid lowland vegetation or is part of the montane assemblages.

-We thank the reviewer for spotting this. Artemisia is actually assigned to the Somali-Masai grassland and shrubland. It has not been included in the Afromonatne group in anytime in the manuscript except as a mistake in Table 1. We would like to mention that Artemisia occur in very low relative abundances with an average percentages of 0.5% and thus not influencing considerably the interpretation of lowland and highland vegetation. We have rectified the assignment in Table 1 and we have corrected it in figure 4 (now figure 5).

Figure 6. I found this figure confusing, because you include forest and dry woodland percentages twice (calculated in two different ways). Perhaps, it might be better to simplify this by only including these groups once using the percentage calculation without

aquatics and mangrove.

-We agree with the reviewer that Figure 6 (now figure 7) might be confusing for the reader. We therefore, decided to separate it in two different figures in the revised version of the manuscript. Fig. 7 showing the pollen group abundances calculated as percentages of total pollen including saltmarshes. Fig. 8 showing relative abundances of the pollen group percentages excluding saltmarshes.

Technical Corrections: Abstract, Line 18 “consisting of well-developed salt...” -We have rectified it in the revised version. Page 3933 Line 2 “Climate and rainfall fluctuations” Do you mean temperature and rainfall fluctuations? -We mean here changes in hydroclimate and rainfall fluctuations. We have rectified it in the revised version. Page 3934 Line 6, remove “allow obtaining information about” -We have removed from the text Page 3941 line 18 “southwestern” -You mean L8: we have rectified it to southwestern Page 3942 line 2, “is likely the result of changes in local hydrologic conditions through...” -We have rectified it in the revised version. Page 3942 line 10 “Rhizophora pollen maximum” since its singular -We have rectified it in the revised version. Page 3942, line 27, “Therefore, the high abundances of the afromontane forest in the marine pollen record corroborates...” -We have rectified it in the revised version. Page 3943 line 18 “dry woodlands and shrublands”. Same change should be made for the rest of paper, figures and figure captions -We have rectified it throughout the whole revised version.

We thank the reviewer for her constructive remarks and helpful suggestions.

Response to Reviewer 2

General Comments The findings of this study are of broader interest since pollen records from East Africa are rare but extremely important to understand the response of the ecosystems to climate variability in this climatically highly complex region. The most advantage of the study in my eyes is the reconstruction of the response of the coastal vegetation to the sea level rise during the deglaciation period.

-We have put more emphasis on the development of the coastal vegetation in the Introduction as also suggested by reviewer 1. We have also dedicated paragraph 5 entirely to the new ecological implications for the coastal processes and ecosystems in tropical southeastern Africa. For more details see our answers to the specific comments below.

Despite this interesting topic, the manuscript hast some difficult parts that need some modifications. In particular, the paleoclimatic implications within the manuscript are yet not convincing. In particular, climatic systems today and for the studied time period are not well explained or incomplete and it feels that the authors discuss the different possibilities not objectively enough. I suggest for the manuscript to adjust the parts about the palaeoclimate implications.

-We have modified the manuscript to clarify the paleoclimatic implications especially the paragraph 6 that is now completely re-written to meet the reviewer suggestions. Please see our answers to the specific comments below.

Please find also specific and technical comments in the attached document.

Specific Comments 1. Page 3932 - Line 14 to 17: The shift of the ITCZ as the explanation of past vegetation changes in the study area is not convincing explained in the discussion. I suggest to adjust this sentence here as suggested further below. Also, the authors write that there was a return of humid conditions after the H1 implying that tropical East Africa was wet before the H1 as well, which was not.

-It is very obvious from reading the current literature dealing with paleoclimatic, modern (historic), and possible future changes that different mechanisms/processes have been proposed to have an effect on precipitation and vegetation changes in East Africa. The reviewer is probably aware of the different (and sometimes confusing) impacts of ITCZ, ENSO, Indian Ocean Dipole (IOD), sea surface temperatures (SST) and wind in the equatorial (tropical) Indian Ocean on East African rainfall. Moreover, the Walker circulation over the Indian Ocean also plays a key role in the interaction between the ocean and the atmosphere.

-We, therefore, incorporated in section 6, a paragraph explaining a physically plausible mechanism of an ITCZ shift and why it explains our observations. All the proposed mechanisms seem to be closely linked with each other and in all cases they are linked with precipitation changes due to the seasonal movement of the ITCZ over Tanzania driven by the SE- and NE-monsoon off East Africa resulting on ITCZ playing a key role in vegetation changes in our study area. The return to humid conditions here is relative to the droughts of H1. As it implied wetter conditions before H1, we replaced the term “return” to “shift” as suggested by the reviewer.

2. Page 3933 - Line 13: I would add here “eastern” or “south-eastern” instead of just saying “southern” since this study is about (South-) East Africa and the authors also refer later in the article just to eastern Africa or tropical Africa. I suggest to stick with one word explaining your study region - either tropical East Africa or tropical Southeast Africa

-We have rectified it in the revised version of the manuscript.

3. Page 3933 - Line 0 - 24: The introduction into the state-of-the-art about paleoclimatic knowledge of the region is very confusing. The authors jump from Northwest Africa to the southern tropics and then to East Africa and also between modern short-term and millennial scale influences. I suggest to structure the introduction better for consistency with explaining how the climate in tropical East Africa is believed to have been during the last 20,000 years, what are the existing views about forcing mechanisms for long-term humidity changes in East and Southeast Africa, and those responsible for millennial and centennial scale climate variability (and maybe inter-annual) in that region and what are the current debates. The study area is a very interesting and a highly debated region as it seems to be located in a climatic transition zone as proxy sites and modelling studies have shown over the past 15 years.

-We agree with the reviewer that this paragraph is a bit confusing. To put short and long time scales mechanisms in this way and to compare millennial scale variability

in North Africa with all time scales in east Africa is not the best way to introduce the paleoclimatic knowledge. As also requested by the first reviewer, we have rephrased the paragraph for more consistency. See our response to reviewer 1comments: Page 3933, L17.

4. Page 3933 - Line 18 - 20: The word 'reduction' should be better changed into 'variability' since ENSO (El Nino and La Nina) influences different regions of East Africa differently (e.g., Nicholson, 1996; Segele et al., 2009; Wolff et al., 2011).

-We have rectified it in the revised manuscript.

5. Page 3934 - Line 4 - 9: These 3 sentences are confusing. While the authors explain in the first sentences that existing pollen records from East Africa do correlate with climatic perturbations in the North-Atlantic, they mention in the third sentence, that abrupt changes are not clear to what they react as they vary geographically.

-We meant here that the response of southeast African ecosystems to climate fluctuations vary geographically and not the abrupt changes that vary. We have rectified the text to avoid this confusion.

Which time are the authors in the first sentences are talking about and also which locality are they referring to? And what do they mean with the sentence about abrupt changes? Do they mean millennial scale or centennial scale climate variability in tropical East Africa? Maybe just use instead of 'abrupt' here again the term of short-term climatic fluctuations (millennial or centennial scale).

-We are referring here to the last deglaciation and to tropical southeast Africa. The sentence has been re-written for more clarification. We have removed "abrupt" from the text and replaced "climate change" with "climate fluctuations" as also requested by the first reviewer.

The authors claim also that there is no clue about what climatic pattern influences millennial to centennial-scale climate variability in East Africa. There are various pub-

lications about the last 30,000 years in East Africa suggesting most likely scenarios (e.g., Gasse, 2000; Barker et al., 2004; Gasse et al., 2008; Foerster et al., 2012; Costa et al., 2014; Junginger et al., 2014). Or do the authors mean only the tropical southeast African region?

-Please see our response to your comment 1: Page 3932 - Line 14 to 17 and our response above to the same remark by reviewer 1: Page 3934 Line 9. Yes, we mean tropical southeast African region. We have added it in the revised version to avoid any misunderstanding.

6. Page 3934 - Line 16 - 25: I wonder whether these sentences are necessary to remain here as these occur in the abstract and also in the conclusion. In my opinion, the introduction should introduce the reader into the topic and a short information about how this new study will contribute to the current debates. Results and interpretation may not be placed here?

-We have re-written this part of the introduction as also suggested by reviewer 1.

7. Page 3935 - Line 4 - 5 / Figure 1: A notification that this chapter is explaining figure 1 is missing here. Also, the catchment of the Rufiji river is explained to lie entirely in Tanzania, and this is what I found in the literature, too, but in figure 1A, the outline of the catchment extends far beyond the Tanzanian boarders and makes no sense at all as the tributaries of the Rufiji river end also in Tanzania. I assume that this is just a drawing or export problem while producing the figure?

-We have actually mentioned Figure1 in this chapter (Page 3936 L2). In order to avoid confusion, we now mention it at the very beginning. The Rufiji catchment error has probably occurred during export of the figure.

8. Page 3935 - Line 24: What do the authors mean with "environmental gradients"?

-We mean precipitation gradients which are gradual changes of rainfall through time (or space) that affect plant distribution. We have replaced environmental by precipitation

to be more precise.

9. Page 3936 - Line 17: What temporal resolution is meant with high resolution?

-Same remark as the first reviewer. What we meant here by high resolution is that the core has high sedimentation rates. The average sedimentation rate is 52cm/kyr which results in an average temporal resolution of ∼19 years/cm. We have removed it from the revised version of the manuscript as it can be quite confusing.

10. Page 3936 - Line 20 - 24: That is convincing!

11. Page 3937 - Line 22 - 23: Would it be possible to add a short explanation why only Al and Ca were chosen for the study and what the Al/Ca ratio is standing for?

-We have added a short explanation as also requested by reviewer 1. Please see our response above to reviewer 1 and the added paragraph in the chapter of Material and Methods (paragraph 3.3. XRF scanning).

12. Page 3939 - Line 1 - 2: Is there an explanation why the authors think the pollen concentration is too low in the upper parts of the record, which have been excluded from the study?

-We assume that the lower pollen content of the upper samples covering the time from 10 to 2kyr BP is related to the very low sedimentation rates during this period. We can also speculate, based on the observation of different cores retrieved during our cruise, that geomorphologically speaking, the Rufiji delta may have moved its main discharge channel to a more northern location at the beginning of the Holocene . Therefore, the terrigenous input has decreased in our site but more sediments have been deposited during the Holocene in Northern locations (e.g., core GeoB16215 by Romahn et al., in revision for Marine Micropaleontology).

13. Page 3939 - Line 8 - 12: I see only comparatively high values in the pollen concentrations around 19.2, 14.8 and shortly after as well as around 12 ka BP. Couldn't it be that the sudden increase in pollen concentrations at 14.8 and 12 ka BP may be related

to the onset of the African Humid Period after the LGM drought period with higher rainfall causing enhanced erosion of sediment containing pollen from the catchment during the initial runoff?

-Erosion of sediment containing palynological material due to higher rainfall around 14.8 and 12 kyr BP could be a possibility but then the record would have to reflect an arid signal with a completely different pollen signature. However, based on the palynological reconstruction of this study, the establishment of complex and well developed plant communities in the uplands (humid woody plants) as well as the lowlands (mangrove) around 14.8kyr BP clearly indicates enhanced precipitation in the area allowing the environment to become more favorable for such a vegetation development.

The pollen concentration in the rest of the time fluctuates between 40 and 15 grains/cm3 over the entire studied period. Fluctuations seem to increase toward younger times but this might be due to the higher sampling resolution in the upper parts of the record?

-Indeed the younger part of the record is investigated in a higher resolution that is why we see more fluctuations.

I also do not see a very good correlation of high freshwater algae content and Al/Ca maxima

-The only time where freshwater algae concentrations do not follow the Al/Ca ratios and Sedimentation rates is between 19.2 and 16.8 kyr BP, an interval with a very low sampling resolution and which has not been the focus of our interpretation.

14. Page 3940 - 3941: The chapter about the dynamics of the lowland vegetation is convincing explained. I am wondering whether the authors have an idea why or if there is a slight decline in the mangrove communities shown in the record after 11.5 ka BP?

-There is a decrease of mangrove pollen percentages after 11.6 kyr BP as shown in Figure 4 (now Figure 5) and as mentioned in paragraph 4.3. Following our logic, it

is clear that the decline of the mangrove community is related to decreased terrigenous input indicated by low Al/Ca ratios and thus, low freshwater input which would be affected by the sediment routing to the north following the delta evolution at the beginning of the Holocene (please see our response to the first reviewer comment Page 3939 Line10 and to your comment 12). The mangrove development depends on the balance between the amount of sediment loads, perennial freshwater availability and sea-level rise. When sediments transported from the continent decrease along with river runoff, the intrusion of sea water occurs landward and this won't be favorable for complex plant communities to develop on the shelf and mangroves to survive. Other studies from the Rufiji Delta and Zanzibar would attribute the late Holocene decline of mangrove to anthropogenic activities as shown by increase in charcoal content (Punwong et al., 2013a, b, c see reference in the manuscript) but in our manuscript we are not able to speculate on human activities.

15. Page 3943: Line 3: I do not see a gradual decline in the afromontane taxa between 16.6 - 14.8 ka. I rather see a collapse of the taxa at 16.8 and 15.4 ka BP with a simultaneous increase in dry wood and shrubs and a kind of gradual decline after 14.8 ka.

-We do not agree with the reviewer, here. We rather see a general decline of the afromontane taxa, which still occur regularly after 15.4 ka. We removed a "gradual" so that the description is more neutral.

16. Page 3944: Line 8: The mentioned lowered lake levels in the cited literature were not also lowered during the H1, those have been low before as well, compared to the time after 14.8 ka. I think this is an important fact that has not been mentioned at all in this manuscript. It always feels like tropical East Africa has been wet before the H1 as well, which was not according to various publications.

-We agree with the reviewer that lake levels in the cited literature were also low before H1. But we are comparing here only the H1 time interval. We did not attempt to inter-

pret the time interval before H1 as the sampling resolution is quite low, Furthermore, throughout the entire manuscript, we have not mentioned wet tropical southeast Africa before H1 (See paragraphs 4.3 and 6).

17. Page 3944: Line 23: Instead of saying just 'changes' I suggest to clarify that an "increase in humidity" is meant here.

- We do not mean increase in humidity here. The meaning of this sentence is that the aridity observed during H1 and the increase in humidity after 14.8 kyrs BP correlate with climatic patterns inferred from continental records. We have rephrased this paragraph for clarification.

18. Page 3944: Line 24: All the cited publications present data sets from NW Africa. It would be better to indicate that more clearly than just writing northern Africa. -We have rectified it in the revised version.

19. Page 3945: Line 16 - 18: This sentence interrupts the discussion about the north-south anti-phase relation in African precipitation. Since you already started the discussion about ENSO on longer times scales before (see your discussion in line 7-10), you could add this sentence right after this statement and follow then with the discussion about the H1 experiments etc.

-We have rephrased this paragraph as suggested.

20. Page 3945: Line 20- Page 3946 Line 3: It was difficult to understand the mechanisms that the authors summarise here. I have the feeling some important informations are missing or are too little explained. For example: Line 28-3: I agree that shifts of atmospheric systems are physically possible and have been shown by various studies. My knowledge of atmospheric processes is restricted and I am happy to be corrected, but the shift of the ITCZ more to the south of East Africa does not explain to me, why it is dry in the Rufiji area during this time. The region of subsidence and ascendence and thus the location of the ITCZ over East Africa is dependent on the local insolation

maximum which in turn is dependent on the month of the year. The ITCZ migrates over the year between its northern and southern limits (∼10°N-10°S) and crosses in my opinion always the equator and thus producing the regular rainy seasons (e.g., Nicholson, 1996). A shift of the ITCZ further to the south might be of major interest for sites that usually are not reached by it? I am happy to be corrected when I am totally wrong, but dry periods in the study region should thus be caused by reduced rainfall amounts during the rainy seasons. Maybe the authors just forgot the word 'mean' annual position of the ITCZ, as it is used in the climate models such as by Mohtadi et al. (2014)? The mechanism behind, such as moisture export, SST changes, weakening of the monsoon strengths etc. as Mohtadi et al. (2014) also concluded should be noted here as well, as this is a whole coupled system and not just referring to a shift of the ITCZ further south.

- We agree with the reviewer that the paragraph was awkwardly written. In the new version, we rephrased the text to explain clearly the involvement of the ITCZ annual mean position shifts. In addition, we added a new figure 2 showing the modern rainfall seasonality, where the modern seasonality of East African rainfall indicates that a southward shift of the ITCZ-related rainbelt (by a few degrees) would lead to significantly drier conditions associated with stronger surface northeasterlies in the Rufiji catchment, only during the austral summer season (DJF). Furthermore, our hypothesis is in line with the north-south anti-phase relationship of rainfall between subtropical southern Africa and equatorial eastern Africa as suggested by model studies which can only be physically consistent with the ITCZ latitudinal shift. Please see the new version of paragraph 6 in the revised manuscript. Yes, we mean the "annual mean position" of the ITCZ. We have rectified it in the revised version.

21. Page 3946 Line 16 - 3947 Line 4 - 17 - YD Discussion: I am wondering whether the higher sampling resolution during the YD time interval might be responsible that larger fluctuations are observed compared to the H1 interval?

-As we mentioned in the manuscript, YD has been already defined as an ambiguous

time interval in the Indo-Pacific Warm pool (Denniston et al., 2013; Dubois et al., 2014) probably due to its short duration compared to H1. Therefore, we do not think that the sampling resolution would affect strongly the YD signal.

22. In general, I am wondering why there is a detailed discussion about Hadley Cell displacements for drought periods in the study region with focus on the NH influences, but there is no explanation, why East Africa became wet although the NH was still cold and dry. A few sentences about this important transition might provide the base to strengthen the discussion.

-We have completely rephrased this paragraph with further explanations to make it clear.

23. Conclusion chapter If the authors agree with the comments above, the conclusion should be changed accordingly. In particular between line 19-26, where they state that only due to a shift of the ITCZ southward, millennial scale droughts in the Rufiji catchment were caused. This alone is not plausible to me.

- We are positive that the changes and revisions made in section 6 supported by further explanations and extended discussion in the revised version of the manuscript have made the impact of the ITCZ on rainfall and Rufiji upland vegetation clearer.

24. Figure 1 The catchment of the Rufiji River seems to be wrong in this figure. It is explained as a basin that lies entirely in Tanzania. But the shape of the catchment extends far beyond the Tanzanian boundaries. It makes also no sense that it extends as far west and south beyond Lake Tanganyika and Malawi as it is shown in this figure. I assume that this is just a drawing or export problem while producing the figure?

-Yes, we will make sure that it appears properly in the final figure. Additionally, it would be nice for the reader to see at least the southernmost position of the present ITCZ (and maybe also for H1), Condo Air Boundary and wind directions for the rainy season in the study region.

-We have added the ITCZ southernmost position in Figure 1 but we do not understand why we should add the Congo Air Boundary as this is definitely out of the scoop of this paper. Wind directions are indeed important to illustrate the atmospheric circulation over southern Africa but putting them in Fig. 1 will only result in overly crowded figure. Instead, we have added now a new figure 2 showing the modern atmospheric circulation with wind directions and rainfall distribution.

25. Figure 5 What are the dashed lines are for? They do not mark the YD and H1, as they did in the other pictures. A sentence in the figure caption would be good.

-Dashed lines denote the four steps of the directional alternation of the 4 families (Poaceae, Cyperaceae, Amaranthaceae and mangrove). A sentence is added in the figure caption.

26. Figure 6 This figure is a bit confusing because only forest and humid woodland and dry woods and shrubs are chosen to be excluded from the dominant pollen taxa. While the authors discuss the pollen communities in figure 6a-6e in chapter 5, the discussion in chapter 6 is about figure 6f-6h. I do not see a big advantage in displaying just the selection of the green and orange curves.

-In order to get a better picture on how the upland vegetation changed during the last deglaciation, salt marshes taxa have been excluded in figure 6f, 6g, 6h (now 8a, 8b, 8c) because they overprint the pollen assemblage. For this reason, it is very important to display the figure with green (forest and humid woodland) and orange (dry woods and shrubs) curves and look carefully at both of them if we want to understand precipitation changes in the catchment area of the Rufiji river (unaffected by the local changes in the river delta and the downslope transport to the core site). Now the figure has been split into two figures to avoid confusion. See our response to the first reviewer.

Technical Comments 27. Page 3939 - Line 7: The notification about figure 3 is not necessary here, because it occurs already in the previous sentence.

-We have deleted the notification.

28. Additional figure suggestion: A figure showing a compilation of cited proxy data sites for the studied time period would be helpful to better follow the discussion about the paleoclimatic implications.

- We agree with the reviewer that such a figure will help follow the discussion but the aim of our paper is not to review climate and vegetation dynamics in different site in southeast Africa. It is more about examining the responses of lowland vegetation and highland vegetation during the last deglaciation and the influence of coastal and atmospheric processes on their composition and distribution. We would not attempt to do a synthesis of the tropical southeast African vegetation dynamics during the last deglaciation as this will be beyond the scope of this paper and would increase the number of figures to 9 which is not really necessary.

We thank the reviewer for his/her constructive remarks.

We are positive that the changes and revisions made in the revised version of our manuscript have improved it dramatically and hope that by addressing these issues you will find our paper now to be engaging and suitable for publication in Climate of the Past. For your guidance, we have submitted the new revised version as a supplement. All changes are marked in yellow.

Ilham Bouimetarhan

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/10/C2405/2015/cpd-10-C2405-2015-supplement.pdf

---

Interactive comment on Clim. Past Discuss., 10, 3931, 2014.