Dear Dr. McClymont,

Please find resubmitted the manuscript entitled “The C_{32} alkane-1,15-diol as a proxy of late Quaternary riverine input in coastal margins” (cp-2017-43). We thank you, and the reviewers, for your positive comments and we have used these to revise and improve our manuscript. The reviewers and you requested major changes and we took it into account. Below we have listed our responses (in bold) to the reviewers concerns and suggested changes. In addition, we added a co-author, E. Schefuß, to the manuscript as he has been instrumental in obtaining core GeoB 7702-3 and was accidentally omitted from the original manuscript. He has read and commented on the revised manuscript.

We hope that with this revision we have addressed all issues and that the revised manuscript is suitable for publication in CP.

On behalf of the co-authors,

Sincerely,

Julie Lattaud

Editor comments:

- The editor agrees with the reviewers that some more information is needed in explaining the sources of the diols, both the "riverine" and "marine". This relates to the point raised on what the "riverine input" really means (freshwater input? Organic matter production within the river?).

More details on the sources of the long chain diols have been added in the introduction (lines 69-77): "Long-chain diols (LCD), such as the C_{32} 1,15-diols, are molecules composed of a long alkyl chain ranging from 26 to 34 carbon atoms, an alcohol group at position C_1 and mid-chain position, mainly at positions 13, 14 and 15. They occur ubiquitously in marine environments (de Leeuw et al., 1979; Versteegh et al., 1997, 2000; Gogou and Stephanou, 2004; Rampen et al., 2012, 2014; Romero-Viana et al., 2012; Plancq et al., 2015; Zhang et al., 2011 and references therein), where the major diols are generally the C_{30} 1,15-, C_{28} and C_{30} 1,13-diols and the C_{28} and C_{30} 1,14-diols. In marine environments the 1,14-diols are produced mainly by Proboscia diatoms (Sinninghe Damsté et al., 2003, Rampen et al., 2007) and the 1,13 and 1,15-diol are thought to be produced by eustigmatophyte algae". Riverine input here relates mostly to the organic carbon carried by rivers and which comprises river-born and terrrestrial (soil+vegetation) carbon. The different proxies reflect different types of carbon which are carried by rivers. This has now been more precisely defined at lines 5, 7, 93.

-You note in your reply to reviewer 2 that in general there is good correlation between BIT and the diol index, but is that to be expected given different sources, or are you saying that high river flow for the most part brings more terrestrial organic matter? Addressing this point does not require extensive text, but just some clarity on what is being recorded by the diols.

Some details have been added to make the discussion clearer, especially with regards to the riverine input concept.

-The editor agrees with reviewer 2 also about some areas of speculation. It isn't always clear in the text when you are making inferences based on known soil properties (e.g. across regions) or if these are speculations. I recommend that in the revised version you try to use phrases such as "we hypothesize..." to flag up the areas which need more research for confirmation.

More clarity has been added when we are speculating with certain interpretations.

- Figure 3 is quite small and not easy to read in its current form. I recommend increasing its size so that the details are visible.

The size of Figure 3 has been increased and clarified.

Reviewer #1

We thank Dr. Bianchi for his helpful comments on our manuscript. Below follows our reply to the main comments and, where applicable, how we changed the manuscript.

-The reviewer states that we only compare the %C_{32} 1,15-diol to the BIT index and that there are better proxies for riverine input that we could use, especially because the BIT index can be influenced by productivity.

$F_{C32\ 1,15}$ was mainly compared to the BIT index and log(Ca/Ti), as these proxies represent riverine terrigenous input. Unfortunately, it could not be compared to lignin concentrations because this was not determined in our samples and we have little original sediment left but we agree it would be a good thing to test in a future study of the C_{32} 1,15-diol. We did not compare it to n-alkanes or long-chain fatty acids because both can also come from eolian input and not only from riverine material. This is especially true for the Nile site where it was shown that the n-alkanes mainly come from eolian input from the African peninsula (Blanchet et al., 2014). It is true that the BIT index is also influenced by archaeal productivity and that is why we also report the concentration of brGDGT to constrain the influence of the concentration of crenarchaeol (representing archaeal productivity) on the BIT index.

-The reviewer says that the discussion is focused on the comparison of proxies and not on the relationship between the riverine input and the climate, especially with the change in ITCZ location.

It is true that the discussion is more focused on comparing the new proxy to others, as this was the main goal of our paper. Both regions have been intensively studied for past climate change including the ITCZ (Blanchet et al., 2014, Castaneda et al., 2009, 2010, 2011, 2016, Schefuss et al, 2011, Just et al., 2014, Tierney et al., 2008, Wang et al., 2013, Thomas et al., 2009, Box et al., 2011), including studies which have used the same sediment cores as used here. We do not want to repeat their conclusions in our manuscript. Rather, we explicitly chose these cores and regions as much is already known about the paleoclimate, which makes it easier to understand the behavior of a new proxy. What is known about the past climate in these regions is now summarized in the method section: lines 126-132 for the Zambezi River: "To summarize, during H1 and the YD, the Zambezi catchment is characterized by higher precipitation and enhanced riverine runoff due to a southward shift of the Intertropical Convergence Zone (ITZC) resulting from Northern Hemisphere cold events, whereas during the Holocene drier conditions prevailed (Schefuß et al., 2011; Wang et al., 2013; van der Lubbe et al., 2014; Weldeab et al., 2014). The Last Glacial Maximum (LGM) in the Zambezi catchment is also recognized as an extremely wet period (Wang et al., 2013)." and lines 158-163 for the Nile River: "To summarize, the climate of the Nile catchment area was colder and drier (Castañeda et al., 2010, 2016) during the YD, H1 and the LGM. The LGM and H1 were extremely arid events with the likely desiccation of the Nile water sources, i.e. Lake Tana and Lake Victoria (Castañeda et al., 2016). To the contrary, the time period during S1 sapropel deposition was warmer and wetter resulting in an enhanced riverine runoff. The late Holocene is characterized by a decrease in precipitation (Blanchet et al., 2014)."

-The reviewer indicates that part of the difference in correlation between the C_{32} 1,15-diol and BIT between the sites can be due to the different hydrological setting of the rivers.

We find it difficult to explain the difference in correlation between $F_{C32\ 1,15}$ and the BIT index by different hydrological settings and/or the length of the river alone. Furthermore, as noted in the text, these two records generally agree well with each other.

-The reviewer mentions that the δD decreases between 35 and 38ky (line 319) and does not increase leading to drier conditions and not more humid as stated in our manuscript.
The reviewer is correct, and we will delete this part of the discussion as the change in sea level is enough to explain the increase in input of C_{32} 1,15-diol and brGDGTs in our core.

-The reviewer indicates that we use the same environmental factor, i.e. aridity, to explain two different observations, i.e. a decrease in riverine input and an increase in soil erosion (line 383).
Data supporting the change in vegetation and aridity during 0-5ky have been reported by Blanchet et al. (2014) and this is mentioned in the manuscript at line 429, while data supporting the extreme aridity during H1, more than during 0-5 ky, have been reported by Castaneda et al. (2016). It might be that the extreme aridity during H1 (when both Lake Tana and Lake Victoria, the sources of the Blue and White Nile, were desiccated) led to a lack of vegetation and increased soil erosion but it also substantially reduced river flow, such that net export of soil OM was reduced. In contrast for the period between 0-5 ky, aridity was not as severe and thus the increased soil erosion combined with a moderately reduced river flow still leads to export of terrigenous OM as also shown by the relatively higher Ca/Ti (Castaneda et al., 2016). We have expanded the explanation at lines 401-404: ". These low values can be attributed to extreme aridity in the Nile River catchment (Castañeda et al., 2016) which we hypothesized lead to a lack of vegetation and enhanced soil erosion but also leading to a severely reduced low river flow, thereby decreasing the net amount of river borne OM reaching our core site."

-The reviewer states that a decrease in log(Ca/Ti) during 0-5 ka indicates more riverine input.
It is true that log(Ca/Ti) is decreasing during 0-5ka and that it indicates more soil input, but it does not indicate per se more riverine input itself. We have added more detail concerning our idea at line 443-444: "This hypothesis is supported by the log (Ca/Ti) (Fig. 4c) which is decreasing at this time, suggesting that soil run off was increasing."

- The reviewer noticed that at line 421 the figure name should be S3b and not Fig. 3.
This has been corrected.

-The reviewer is asking why the axis for log(Ca/Ti) has been inverted.
In our view, by flipping the log(Ca/Ti) we highlight the similarity between this proxy and the BIT and C_{32} 1,15-diol, making the correspondence between the three proxies clearer.

- The reviewer says that in fig. 4 the BIT index and log(Ca/Ti) are more similar than the %C_{32} 1,15 and log (Ca/Ti).
The BIT index follows the log(Ca/Ti) better than the diol index as they both are influenced by riverine input of soil-derived carbon while the diol index is not tracing this pool but rather river-borne carbon.

-The reviewer is asking if the concentration of the 1,13 and 1,14 diols are available.

Unfortunately, it was not possible to quantify the concentrations of the 1,13 and 1,15 diols as they were measured from long term stored archived extracts.

Reviewer #2:

We thank the reviewer for his/her helpful comments on our manuscript. Below follows our reply to the main comments and, if applicable, how we changed the manuscript.

-The reviewer wishes to have more information on LCDs (potential sources, history).
We have extended the third paragraph of the introduction with more details about the discovery of LCDs and potential producers in marine and freshwater environments (lines 69-77): "Long-chain diols (LCD), such as the C_{32} 1,15-diols, are molecules composed of a long alkyl chain ranging from 26 to 34 carbon atoms, an alcohol group at position C_1 and mid-chain position, mainly at positions 13, 14 and 15. They occur ubiquitously in marine environments (de Leeuw et al., 1979; Versteegh et al., 1997, 2000; Gogou and Stephanou, 2004; Rampen et al., 2012, 2014; Romero-Viana et al., 2012; Plancq et al., 2015; Zhang et al., 2011 and references therein), where the major diols are generally the C_{30} 1,15-, C_{28} and C_{30} 1,13-ls and the C_{28} and C_{30} 1,14-diols. In marine environments the 1,14-diols are produced mainly by Proboscia diatoms (Sinninghe Damsté et al., 2003, Rampen et al., 2007) and the 1,13 and 1,15-diol are thought to be produced by eustigmatophyte algae"

-The reviewer would like to have the age control points indicated in figure 2 and 4 and a supplement with a summary of the age model.
The age control points have been added to the figures (black triangles) and a brief supplementary method section describing the previously published age-models of both cores is also added.

-The reviewer wishes to have a more synthetic discussion on the proxy, with more details about its advantages and disadvantages, as well as why, in the Mozambique core, the correlation between the C_{32} 1,15-diol and BIT index is better than in the Nile core but also why the C_{32} 1,15-diol works better than the BIT.
The C_{32} 1,15-diol is not working 'better' than the BIT index to trace riverine input, rather in our view it simply reflects a different pool of organic carbon being transported by rivers, i.e. river-born carbon versus soil and river-born carbon in case of branched GDGTs. This is now better detailed at lines 5, 7, 93. Like the BIT index, the $F_{1,15\text{-}C32}$ may also be affected by marine productivity as we discussed at lines 352-355, 414-417and 419-423. For the different observations between the cores, we added some speculation at lines 446-448: "Although some discrepancies are noted for both cores, the C_{32} 1,15-diol agrees well with other terrigenous proxies." A synthesis of the advantage/disadvantages of the proxy are discussed at lines 451-454: "Since the C_{32} 1,15-diol is produced in rivers itself, it is not impacted by vegetation abundance and soil composition, in contrast to other proxies like the BIT index and lignin concentrations. This may make it a more reliable proxy to trace past river input into marine environments."

-The reviewer asks for a clearer discussion on the source of the C_{32} 1,15-diol and what does the synchronicity/asynchronicity of the variation between BIT index and C_{32} 1,15-diol means in a broader sense.

In our view, this question has been discussed already at lines 345-364 and 405-417 and we want to point out that for most of the records, BIT index and C_{32} 1,15-diol actually agree quite well. Furthermore, in essence, the proxies record different things so they are not always behaving the same way. This is now better explained.

-The reviewer indicates that the hypothesis at lines 366-371 is more speculative than other part and that, if true, the low brGDGT concentration of the Northern Rivers would be reflected in surface sediment offshore these rivers.

We agree with the reviewer that this is speculative, and therefore we added at line 382: "We hypothesize that.." and at lines 387-390, we recommend for future studies to analyze surface sediments offshore the Northern rivers to confirm this hypothesis: " Further research examining the brGDGT contents of soils in the different river catchment areas as well as surface sediment from offshore these northern rivers is required to distinguish between the different hypotheses."

- The reviewer would like, if possible, to have more general discussion on the paleoclimate during H1 and YD.

We have added some details about the climate during the YD and H1 in the method section at lines 127-132 for the Zambezi River: "To summarize, during H1 and the YD, the Zambezi catchment is characterized by higher precipitation and enhanced riverine runoff due to a southward shift of the Intertropical Convergence Zone (ITZC) resulting from Northern Hemisphere cold events, whereas during the Holocene drier conditions prevailed (Schefuß et al., 2011; Wang et al., 2013; van der Lubbe et al., 2014; Weldeab et al., 2014). The Last Glacial Maximum (LGM) in the Zambezi catchment is also recognized as an extremely wet period (Wang et al., 2013)." and lines 158-163 for the Nile River: "To summarize, the climate of the Nile catchment area was colder and drier (Castañeda et al., 2010, 2016) during the YD, H1 and the LGM. The LGM and H1 were extremely arid events with the likely desiccation of the Nile water sources, i.e. Lake Tana and Lake Victoria (Castañeda et al., 2016). To the contrary, the time period during S1 sapropel deposition was warmer and wetter resulting in an enhanced riverine runoff. The late Holocene is characterized by a decrease in precipitation (Blanchet et al., 2014)."