Reply to the Editor

Dear Christo,

Thank you very much for your comments on our manuscript and for inviting us to submit a revised version. We replied to all reviewer comments and your comment. In the following we describe how we addressed your concerns. The lines refer to the track-changes version of the revised document. Our answers are given in italics.

Kind regards

Vera Meyer and co-authors

Dear Authors,

Your manuscript has now been seen by two reviewers. Both evaluations were overall quite positive, but identify some issues that need to be addressed. Please respond to all of the reviewer comments, as well as my additional minor comments below this note. I will likely be encouraging you to submit a revised version of your manuscript, so feel free to respond to the reviewer comments in the form of proposed changes to the manuscript.

I look forward to reading your response.

All the best, Christo Buizert (CP editor)

Additional comments from the editor:

While the d2H is a good proxy for low-latitude monsoon precipitation via the amount effect, in the mid-latitudes water isotope fractionation is dominated by the temperature effect. This may also be true for the westerlies that you invoke for the winter precipitation. Can you please elaborate on what you think is driving the isotope signature of the winter precipitation?

*As described in section S2 (supplementary material) we retained from performing a temperature correction. It is correct that temperature exerts control on the dD of the lipids and that this control increases with increasing latitude. Considering that Tierney et al. (2017) show that the amount effect dominates over temperature effects in the arid Sahara the temperature effect accordingly should have minimal influence on the dD in our record. Moreover, finding a suitable temperature record to perform corrections in the Nile region is difficult because temperature estimates provide an inconsistent picture regarding the amplitude of glacial-Holocene warming and some of the records do not cover the last 18 ka (Lakes Tana and Victoria). There are $TEX_{86}$-based and alkenone-based sea surface temperature (SST) reconstructions from the eastern Mediterranean Sea and the Red Sea but their amplitude of change varies between 6-10°C (Castaneda et al., 2010; Arz et al., 2003; Mathews et al., 2021) which is up to 2 times as much as climate models suggest for the change in mean annual surface temperature (5-6°C; Kageyama et al., 2021). On top of that, the SST-proxies are probably also affected by seasonal and other biases that may lead to an overestimation of the true amplitude of deglacial warming (Castañeda et al., 2010).*

*Therefore, we might overcorrect by using these SST reconstructions from the Mediterranean to correct the $\delta D_{wax}$ for the temperature effect. A brGDGT-based record on mean annual air temperature was established on core GeoB7702-3 (Castañeda et al., 2016) but brGDGTs were analyzed with an outdated HPLC-method that does not allow for adequate separation of the brGDGT compounds. The record may thus be significantly influenced by in situ production and may not reflect MAT-changes in the Nile-River watershed. Given these circumstances we decided not to correct for temperature. Since lower temperature conditions during the glacial lead to isotopically depleted rainfall the reconstructed dDp of the last glacial can be regarded as minimum values, respectively maximal rainfall estimates.*

*We added a few lines to section S2 (supplementary material).*

Figure 1A: Could it be that the NAO+ and NAO- labels have been switched? I believe the description in the text (section 5.5.1) is correct. Please double check.

*Yes, you are right. Thank you for pointing out. In the figure + and – were accidentally mixed up while the NAO states are correctly described in the text. We changed Figure 1.*

Line 13: I prefer to use d2H rather than dD, as it reduces unnecessary jargon and is more consistent with notation of all other isotope ratios. Please give both notations at this first use of dD. It would be too much to ask you to change this throughout the manuscript, but please keep this in mind for future publications.

*As you suggested, we stick to dD in the new version but we add d2H when dD is introduced in the introduction (line 113).*

Line 185: "set to 50": what are the units on this number?

*In BACON the "acc.mean" prior is given in yrs/cm. We added this to the value (line 185).*

Line 218: I assume you apply this to correct for thee mean d2H of ocean water? The LR04 stack gives the d18O of benthic foraminifera, which is impacted by both the d18O of the ocean, as well as the temperature effect. The latter effect needs to be removed – how did you do this? I assume you multiply by 8 to get the d2H magnitude?

*We performed the ice volume correction according to Ruan et al., 2019. We assume the larger ice volume introduced an enrichment of 1‰ (Schrag et al., 1996) to the glacial ocean. The glacial-Holocene amplitude of the benthic stack is 1.8‰. This is normalized to 1‰ and accordingly this step corrects for any other effect on the benthic d18O, including bottom-water temperature. Multiplying this by 8 (global meteoric water line) yields a change of 8‰ in dD due to ice-volume changes. In order to avoid any questions regarding the temperature correction, we added a citation to line 219.*

Line 346: Source areas (remove plural "s")

*Done.*

Line 577: Note that a similar division of HS1 is seen in North America; Broecker referred to these as the "big wet" and "big dry" (Broecker et al., 2009)

*Thank you for mentioning this study. That is very interesting and we were not aware of these findings before. We included a reference to Broecker et al. (2009) in the discussion about HS1 (lines 579).*

References:

Arz, H., Lamy, F., Pätzold, J., Müller, P. and Prins, M. A.: Mediterranean Moisture Source for an Early-Holocene Humid Period in the Northern Res Sea, Science, 300, 118–121, 2003.

Broecker, W. S., McGee, D., Adams, K. D., Cheng, H., Edwards, R. L., Oviatt, C. G., & Quade, J. (2009). A Great Basin-wide dry episode during the first half of the Mystery Interval? *Quaternary Science Reviews, 28*(25-26),

Castañeda, I. S., Schefuß, E., Pätzold, J., Sinninghe Damsté, J. S., Weldeab, S. and Schouten, S.: Millennial-scale sea surface temperature changes in the eastern Mediterranean (Nile River Delta region) over the last 27,000 years, Paleoceanography, 25(1), 1–13, doi:10.1029/2009PA001740, 2010.

Castañeda, I. S., Schouten, S., Pätzold, J., Lucassen, F., Kasemann, S., Kuhlmann, H. and Schefuß, E.: Hydroclimate variability in the Nile River Basin during the past 28,000 years, Earth Planet. Sci. Lett., 438, 47–56, doi:10.1016/j.epsl.2015.12.014, 2016.

Kageyama, M., Harrison, S. P., Kapsch, M. L., Lofverstrom, M., Lora, J. M., Mikolajewicz, U., Sherriff-Tadano, S., Vadsaria, T., Abe-Ouchi, A., Bouttes, N., Chandan, D., Gregoire, L. J., Ivanovic, R. F., Izumi, K., Legrande, A. N., Lhardy, F., Lohmann, G., Morozova, P. A., Ohgaito, R., Paul, A., Richard Peltier, W., Poulsen, C. J., Quiquet, A., Roche, D. M., Shi, X., Tierney, J. E., Valdes, P. J., Volodin, E. and Zhu, J.: The PMIP4 Last Glacial Maximum experiments: Preliminary results and comparison with the PMIP3 simulations, Clim. Past, 17(3), 1065–1089, doi:10.5194/cp-17-1065-2021, 2021.

Matthews, A., Affek, H., Ayalon, A., Vonhof, H., Bar-Matthews, M., 2021. Eastern Mediterranean climate change deduced from the Soreq Cave fluid inclusion stable isotopes and carbonate clumped isotopes record of the last 160 ka. Quat. Sci. Rev. 272, 107223

Ruan, Y., Mohtadi, M., van der Kaas, S., Dupont, L. M., Hebbeln, D. and Schefuß, E.: Differential hydro-climatic evolution of East Javanese ecosystems over the past 22,000 years, Quat. Sci. Rev., 218, 49-60, doi: 10.1016/j.quascirev.2019.06.015, 2019.

Schrag, D. P., Hampt, G. and Murray, D. W.: Pore fluid constraints on the temperature and oxygen isotopic composition of the glacial ocean, Science, 272, 1930–1932, 1996.

Tierney, J. E., Pausata, F. S. R. and De Menocal, P. B.: Rainfall regimes of the Green Sahara, Sci. Adv., 3(1), 1–10, doi:10.1126/sciadv.1601503, 2017. (supplementary material)

Reviewer 1

Dear reviewer,
thank you very much for your constructive suggestions how to improve our manuscript. In the following we explain how we address your concerns in the revised version of the article. Our reply is written in italics. Line numbers refer to the track-changes version of the manuscript.

Kind regards,

Vera Meyer and Co-authors

This paper presents new analyses of the C26 and C28 alkanoic acids from a core the Levantine basin (GeoB7702-3). There was a previously published C-31 alkane record from this site, so analysis of these new chain lengths offers an interesting complement. More could be done with analysis of the offset between different chair lengths, though they largely reflect a very similar signal. There is a need to hone explanation of the mechanisms underlying changes in the record, but otherwise it is a very nice study.

*In the new version of our manuscript we expanded our discussion about the source apportionment (section 5.1) and on the mechanisms controlling the evolution of winter precipitation in the Nile River delta (section 5.4-5.5.2).*

The introduction is very clear and very well written. However, I would try to discuss more uncertainties about winter rainfall in the last glacial and mid-Holocene. See the dynamical papers linked below.

*Thank you for pointing to these studies. As requested, we added a short paragraph about the uncertainties regarding the balance between evaporation and precipitation during the LGM to the introduction (lines 80-92). We also extend the discussion as the reviewer asked for honing the discussion about the mechanisms controlling the hydroclimate in Nile delta region. We shed more light on the P-E balance by adding few lines focusing on the effects of evaporation on dD, an aspect which did not receive much attention in the previous version. We also cite the studies in this context (lines 528-541; 558-564; 648-655).*

Methods:

Table 1: I would actually rather see the age model figure (e.g. the pdf output from BACON) and a table like this in the Supplementary information, especially since many of the dates are already published in other sources).

*As requested, we included a figure depicting the BACON output (line 241). However, we keep Table 1 in the main article to present all information about the age model at the same spot.*

3.2 Lipid extraction: do you worry about an aquatic signature with C26 and C28 at all? There is some literature suggesting that mid-chains alkanes could be produced by submerged macrophytes (see Ficken et al 2000)?

*You are correct that mid-chain homologues of n-alkanes have been found to sometimes stem from aquatic origin. Biases from algal and bacterial origin are also possible for mid-chain HMW n-alkanoic acids but predominantly affect homologues with a chain length <28 (e.g. Kusch et al., 2010). Biases on homologues with a greater chain length are quite unlikely (Kusch et al., 2010) which is why the n-$C_{28:0}$ is very often used for the reconstruction of terrigenous environments (e.g. Costa et al., 2014, Berke et al., 2012; Tierney et al., 2008). In our core the n-$C_{26:0}$ and n-$C_{28:0}$ have very similar $\delta D$ signals suggesting that they have the same source. The $\delta^{13}C$ values of the n-$C_{26:0}$ and n-$C_{28:0}$ vary between -22.6 and -26.9‰ values of submerged plants often are > -20‰ (e.g. Liu et al., 2022). Therefore, we consider both homologues reliable proxies for hydroclimate in the Nile-River watershed.*

Discussion:

I disagree with the logic that if d13C and dD are not significantly correlated then it is not necessary to consider the impact of vegetation change. A step change in d13C can possibly mask a change in dD of precipitation in a leaf wax record if there is a shift in the dominant physiological pathway during an interval when dD of leaf wax appears relatively complacent. I think a more defensible argument to make is that the amplitude of the carbon isotope change in your record is very small, and therefore a constant epsilon/apparent fractionation would be more appropriate at this site.

*You are right stating that the amplitude of change in the d13C records are very small and therefore do not introduce large uncertainties into the dDwax. This is best documented by the shape of the dDwax record when correcting it for vegetation and ice-volume changes. The trends and amplitudes are similar to the original dDwax record which shows that the effect of changing vegetation only marginally affected the dDwax signal. We added a short paragraph to lines 327-331 extending the discussion about the impact of vegetation changes.*

Line 422: I am not sure I agree that the alkanoic acids and alkanes really show completely dissimilar patterns - if you think they do, it would be good to describe where the dissimilarities arise. To my eye, it appears that the n-acids and n-alkanes show similar trends but different amplitudes of variability.

*"Dissimilar patterns" refers to the different developments during the deglaciation and the Holocene as described in section 4 (results). The amplitude of the long-term development is larger in the n-alkanes than in the fatty acids. In the revised version we re-wrote the discussion about the source apportionment (section 5.1) because we implemented the Dead Sea record from Tierney et al. (2022). In the course of re-writing lines 415-423 were deleted.*

While it is lower resolution, there is actually an n-acid leaf wax record of hydrogen isotopes from the Dead Sea that was recently published (Tierney et al 2022, Quaternary Science Reviews). It might be useful to compare your findings to that record, as it will show whether over time the isotopic signature at GeoB7702-3 more strongly reflects African records influenced by the AHP or Levantine records.

*Thank you very much for pointing to this study. The record is a very valuable support for our interpretations regarding Mediterranean winter precipitation in our records. It is very useful to have inferences on hydroclimate from the Eastern Mediterranean realm that are based on the same proxy. We included the dD record from the Dead Sea into Figures 4 and 5. As elaborated in the discussion (lines 433-454) it broadly tracks changes in our dDwax n-alkanoic acids records. The range of absolute values covered in the two records are in agreement. By contrast, the HMW n-alkanes exceed the range matching values found in the records of Lake Tana. This underscores our inference that HMW n-alkanoic acids reflect Mediterranean winter rainfall while HMW n-alkanes are substantially influenced by summer monsoon rain.*

Section 5.1 and 5.2:

The idea of using modern precipitation isotopes to essentially 'fingerprint' the source area from different leaf wax isotopes is an interesting one. I would like to see this information incorporated into the figures a little more - you could potentially plot dD of precipitation (inferred by applying a constant epsilon to your wax records) and plot modern values of dDp from different parts of the Nile catchment on this plot.

*When we developed the manuscript we also considered plotting the modern values on the axis of the dDp record. However, we discarded this idea since the source apportionment based on the end-members from the different parts of the watershed actually only works for the present-day situation. Therefore, we decided to present the data in a table. While at present the distribution of dDp along the watershed is known in detail, it is unknown for the deglacation and the LGM. However, we know that rainfall amount changed drastically as the monsoon intensified and changes in the end-members must be expected. Given these uncertainties on paleo end-members of dDp we would like to avoid to present the modern values along with the paleo dDp estimates from our core in order to avoid confusion. By including the Dead Sea dD record to our Figures 4 and 5 the source apportionment is strengthened (see our response to the first comment on section 5.1) and lines 433-454 in the manuscript. In the revised version we explain why we do not use the modern dDp values for the source apportionment through time (lines 423-433).*

Given the strong focus on organic matter source and provenance, it would also be nice to see changes in concentration of different leaf waxes over time plotted in one of the main text figures (at least for C26 and C28, the chain lengths measured in this study)

*For the revised version we created a new figure showing the concentrations of HMW n-alkanes and HMW n-alkanoic acids (Figure 3). We plotted the data together with records of Saharan dust input to the eastern Mediterranean and with d18O-records on planktic foraminifera representing Nile River runoff. Sections 4, 5.1 and 5.2 were extended by a few lines discussing concentrations of n-alkanes and n-alkanoic acids in context of fluvial and aeolian transport (lines 254-263, 352-368, 379-383, 492-469, 503-508). We also added information about the analytical methods used for the quantification of the lipids (lines 202-207, 222-235). While the concentrations of n-alkanoic acids were analyzed in our study, the concentrations of n-alkanes were measured by Isla Castañeda and Stefan Schouten several years ago but were unpublished. For providing their data, Isla and Stefan became co-authors of our manuscript.*

5.3:

Again, I think more analysis is needed to clearly show that alkanoic acids vs. alkanes are picking up different signatures of winter vs. summer precipitation. Perhaps I am missing something, but Table 2 seems to suggest that the modern near-foretop values of both the C-31 alkane and the C-26 and C-28 alkanoic acids appear similar to precipitation isotopes in the Delta compared to the isotopic signature farther south?

*Yes, that is correct and a key finding for the discussion about provenance. In section 5.1 we described that both types of compounds derive predominantly from the delta at present and also during the LGM when the dDwax and dDp match. During the deglaciation the offset of the dDwax and dDp between n-alkanes and n-alkanoic acids suggests that the source areas of the n-alkanes and n-alkanoic acids differed substantially. The comparison with the dD record from the Dead Sea provides further support for the inference that the HMW n-alkanoic acids record changes in the Mediterranean hydroclimate. The records show similar trends with parallel occurrence of the wet -phase at the time of the AHP. Also, the range of dD values covered by the two records is almost the same while the HMW n-alkanes and wax records from headwaters (in particular Lake Tana) have a larger range. By including references to the Dead Sea record we strengthen the discussion about the signals of winter versus summer precipitation in our dDwax. Lines 433-454. We also added the Dead Sea record to Figures 4 and 5.*

A broader point to consider: it appears you are primarily interpreting modern precipitation isotope seasonality in terms of seasonality (e.g. summer vs. winter end members) - how does this contrast with the ‘amount effect’ often used in paleoclimate studies?

*Based on the offsets of the dDwax of n-alkanes and n-alkanoic acids during the deglaciation we conclude that the compound types reflect climate change in two precipitation regimes, i.e. in the winter rainfall zone (Nile delta region; n-alkanoic acids) and the summer monsoon zone in the headwaters (n-alkanes) as described in the source apportionment in section 5.1.. The deglacial variability in the two records is a result of the amount effect in the two catchments, i.e. winter rainfall amount in the delta region (alkanoic acids) and monsoonal rainfall amount in the headwaters (n-alkanes). In short, the offset between the homologues stems from different source areas, the variability in the records reflects the amount effect.*

A few dynamical studies that may be useful to consider

Ludwig, P. and Hochman, A., 2022. Last glacial maximum hydro-climate and cyclone characteristics in the Levant: a regional modelling perspective. Environmental Research Letters, 17(1), p.014053.

Goldsmith, Y., Polissar, P.J., Ayalon, A., Bar-Matthews, M., DeMenocal, P.B. and Broecker, W.S., 2017. The modern and Last Glacial Maximum hydrological cycles of the Eastern Mediterranean and the Levant from a water isotope perspective. Earth and Planetary Science Letters, 457, pp.302-312.

Kusch, S., Rethemeyer, J., Schefuß, E. and Mollenhauer, G.: Controls on the age of vascular plant biomarkers in Black Sea sediments, Geochim. Cosmochim. Acta, 74(24), 7031–7047, doi:10.1016/j.gca.2010.09.005, 2010.

Liu, H., Liu, J., Hu, J., Cao, Y., Xiao, S. and Liu, W.: Systematical δ13C investigations of TOC in aquatic plants, DIC and dissolved CO2 in lake water from three Tibetian Plateau lakes, Egol. Indic., 140, 109060, 2022.

Response letter to Reviewer 2

Dear Reviewer,

Thank you very much for your constructive suggestions how to improve the quality of the manuscript. You identified some paragraphs which were a bit to vaguely written. By adding a few more details to the discussion in order to improve the clarity. Below we describe how we address your concerns in the revised version. Our responses are given in italics. Line numbers refer to the track-changes version of the article.

Kind regards

Vera Meyer and co-authors

The authors use stable hydrogen isotopic compositions of high molecular weight n-alkanoic acids in a marine sediment core from the Eastern Mediterranean, to provide a continuous record for winter precipitation of the Nile watershed since the end of the last glacial period.

Overall the paper is well written and easy to follow; however, I second Reviewer #1 on explanations regarding the mechanisms: I feel the authors easily draw conclusions on large scale dynamical changes with little support, except for some sparse paleoclimate archives (e.g. storm track boundary located at 31-33°N based on just one proxy from Jeita cave – LL622-623). Given the authors confusion of ITCZ and WAM (see below), they should be very careful before making atmospheric or ocean dynamics claims. Regarding the analysis of the paleoclimate archives per se, it is outside my field of expertise so I leave it to the other reviewer(s).

*You are right that some single inferences in section 5 were based on few data and might have been a bit too vague. Therefore, we removed the paragraph encompassing lines 609-623. All remaining inferences made on large-scale atmospheric circulation patterns are based on several studies from the western, central and eastern Mediterranean realm as well as on some results from general circulation models (sections 5.4 and 5.5.1). As we discuss our data in context to those records and findings we feel that the conclusions made are more solidly justified than the conclusions the reviewer was criticizing in the previous version.*

*Reviewer #1 also asked for a more detailed discussion about the mechanisms controlling the hydroclimate in the Nile-Delta region. In order to acknowledge his concerns we included some paragraphs about the development of the P-E balance in the region (lines 528-541; 558-564; 648-655).*

Furthermore, I feel the way the authors cited paper is a bit random. I could not verify all of them but in the few cases I did, the references were not appropriate:

*Regarding your concerns on our way of citing, we went into more detail describing the findings of the studies cited. We feel that some passages were kept too short and produced confusion. By extending the discussion we hope to avoid misunderstandings. We rephrased* *passages in the introduction (section 1) and section 5. Below, we elaborate how we addressed the instances specifically.*

- In L29 the authors cited de Menocal et al 2000 when referring to “dramatic oscillations between dry and wet climate states in the course of glacial-interglacial cycles”. De Menocal et al. 2000 just showed one oscillation during the last deglatiation! The authors should rather cite Larassoana et al. 2013 or other more relevant paper.

*We additionally cite Menviel et al. (2021); Ziegler et al. (2010) and Larrasoaña et al. (2013), which all mention the last interglacial AHP.*

- In LL71-74 the authors wrote “However, the comparison of simulations and vegetation reconstructions shows that climate models probably underestimate precipitation in the northern Sahara and that the summer monsoon alone may have been insufficient to sustain a vegetated Sahara (…)” and refer among others to Hely et al. 2014 who did not claim something like that.

*In order to avoid any confusion about the references, we extended the paragraph and give more details about precipitation estimates from models and proxies (lines 50-70).*

- In LL653-654 the authors claim “Some studies suggest that the monsoon fringe even expanded up to 31°N (Sha et al., 2019; Tierney et al., 2017) which would mean that the Nile-River delta (situated at 30-31°N; Figure 1) became influenced by the African summer monsoon.”. Both papers use paleoclimate archives from western Africa and several modelling studies have shown that eastern Africa remained much drier than the western part (e.g., Pausata et al., 2016; Dallmeyer et al., 2020*).

  *We re-wrote and extended the discussion about the northernmost extent of the WAM in this paragraph to clarify what models and data suggest regarding the northwestern and northeastern Sahara (lines 658-687).*

  Then in LL654-656 the authors wrote “However, in most climate simulations the northernmost position of the ITCZ is located at ~24°N (e.g. Pausata et al., 2016) which is also corroborated by proxy data (Hamdan and Brook, 2015; Cheddadi et al., 2021).” The authors are mixing up the ITCZ extension with the northern most location of the ITCZ (same in line 740 where they confuse the ITCZ for the WAM). The two sentences the authors wrote are not in contradiction: the very paper the authors cite for the ITCZ location at 24°N (Pausata et al., 2016) is the same showing the northern most extension of the WAM reaching up to 31°N, which is also cited in Tierney et al. 2017. Even with the WAM reaching up to 31°N in western Sahara, in the Nile Delta the summer rainfall in their model did not exceed 0.5 mm/day, i.e. less than 60 mm over the entire summer season.

*Thanks for pointing out these erroneous denotations. Of course, the northernmost position of the WAM was meant in the sentence in line* 654-656*. In the new version this is corrected (line 662). As indicated in our response to the previous comment, we re-wrote the paragraph in lines 658-687. In doing so (lines 658-687), we give details about different inferences regarding the northernmost extent of the WAM (having in mind the definition given in Pausata et al., 2016, i.e. the northernmost latitude that the monsoon reaches). In this paragraph we also mention the west-east gradient in humidity (lines 574-576) and conclude* *that the WAM has never reached the Nile Delta region during the AHP (lines 684-688). Moreover, we rephrased section 5.5.6 as we intended to more appropriately cite other studies (see our response to the second comment). Line 740 was changed to: "According to climate model simulations monsoonal precipitation may not have provided enough moisture to sustain vegetation beyond ~24°N given mismatches with proxy data (Chandan and Peltier, 2020; Braconnot et al., 2007; Perez-Sanz et al., 2014; Cheddadi et al., 2021)" (lines 793-795).*

The authors should also cite Hely et al. 2014 here.

*We cite Hely in line 792.*

> Finally, to get a better understanding of the relation between WAM and ITCZ I strongly suggest the authors to read this review article: Geen, R., Bordoni, S., Battisti, D. S., & Hui, K. (2020). Monsoons, ITCZs, and the concept of the global monsoon. Reviews of Geophysics, 58, e2020RG000700. https://doi.org/10.1029/2020RG000700.

*Thank you for mentioning this informative review paper.*

L147 In Alexandria does not rain only 118 mm/year! It rains almost twice about 190-200 mm/year. I then checked climate.data reference provided by the authors where one can see an annual average of 181 mm/year, which is more reasonable. Please correct it.

*Thank you for pointing out! We corrected the typo.*

L152 Mai --> May.

*Corrected.*

L168 For clarity I would repeat in the sentence in which areas "Warm and wet winters are generally associated with negative NAO-states while cold and dry winters occur during positive phases."

*Done.*

In Figure 1 panel A the NAO+ should be NAO- and vice versa.

*You are right, the NAO states were accidentally mixed up during the figure production. Thanks for pointing out! However, in the text the attribution is correct. We corrected Figure 1.*

Figure 4: the Green Sahara is also known as African Humid Period. I do not understand while the authors are giving them two difference period ranges.

The reference to 11-5 ka BP for the green Sahara was based upon e.g. Watrin et al., 2009*. However, in the revised version we reconsidered the interval and now attribute the green Sahara to 14.6-5 ka BP. In Figure 4 and 5 we marked the optimum of the AHP and green Sahara with the dark grey shading. The dashed lines have been removed.

North Africa refers to specific countries so it’s better to use northern Africa (the n is not capital). Moreover, the authors sometimes use capital letters (Northwest Africa in L77) sometime lowercase (northeast Africa in L78). I suggest using northwestern, northeastern, northern Africa to avoid confusion with the political definition of those regions.

*As recommended we use northwestern, northeastern, northern, southern, etc. throughout the manuscript.*

Westerly Jet, why jet is with a capital letter?

*Written in lower case letters, now.*

L691 the authors refer to a warm spell for the warming period in the EM between 10 – 7 years ago. However, warm spells are periods characterized by **several days** of very warm temperatures compared to local or regional averages. Warm spell is not the right term to use in a climate context.

*We replaced “spell” by “phase”.*

* Dallmeyer, A., Claussen, M., Lorenz, S., and Shanahan, T.: The end of the African humid period as seen by transient comprehensive Earth system model simulation of the last 8000 years, Clim. Past, 16, 117–120, https://doi.org/10.5194/cp-16-117-2020, 2020.

Watrin, J., Lézine, A. M., Hély, C., Cour, P., Ballouche, A., Duzer, D., Elenga, H., Frédoux, A., Guinet, P., Jahns, S., Kadomura, H., Maley, J., Mercuri, A. M., Pons, I. A., Reynaud-Farrera, I., Ritchie, J. C., Salzmann, U., Schulz, E., Tossou, M. G., Vincens, A. and Waller, M. P.: Plant migration and plant communities at the time of the “green Sahara,” Comptes Rendus - Geosci., 341(8–9), 656–670, doi:10.1016/j.crte.2009.06.007, 2009.