Dear authors,

Your revised manuscript has now been seen by one of the original reviewers. The reviewer suggests that more changes are needed before the manuscript can be published. In particular, the reviewer suggests that the climate dynamics interpretation is misrepresented in the manuscript, and in some cases even internally contradictory. Please provide a revised version of your manuscript that incorporates the additional reviewer comments.

One of the suggestions is to look at transient climate model simulations of the last deglaciation (the TraCE and/or i-TraCE simulations). In particular the isotope-enabled i-TraCE experiment could be compared directly to your observations (https://www.cesm.ucar.edu/working-groups/paleo/simulations/cesm1-itrace, contact person Chengfei He, cxh1079@earth.miami.edu). I agree with the reviewer that this would be a valuable addition to the manuscript. However, it would represent a large revision. While I would be supportive of adding such a model component, this is not a requirement for publication and I am happy to consider a revised manuscript that does not include it - provided you address the other reviewer concerns satisfactorily.

Good luck with the revisions, and I look forward to seeing your response. Please let me know if you have any questions.

Best regards, Christo Buizert (CP editor)

Dear Christo,

Thank you very much for inviting us to re-submit a revised version of our manuscript. We responded to all comments and suggestions by the reviewer. We agree that comparing TraCE or even i-TraCE outputs with our results would be very interesting but you are right that this would be a drastic revision. It would probably mean a drastic reorganization of the manuscript and probably would complicate and lengthen the story unnecessarily. As we also write in our response letter to the reviewer, such a comparison would probably provide enough material for a follow-up study and second manuscript. We decided to stick to the interpretation of our biomarker records as it already has several topics to discuss, i.e. the provenance and climate.

Kindest regards,

Vera Meyer and co-authors

Reviewer 1:

The revised version of the manuscript is slightly improved; however, it is not publishable yet, and the interpretation of the results needs to be rewritten based on a more accurate analysis of model outputs. It appears that the authors struggle with atmospheric dynamics and their implications in interpreting their proxy record, blinding referring to a study (without making sure their interpretation was correct) and then also contradicting themselves (colder climate moves the jet northward as well as a warmer climate). Moreover, I still see that the authors stated their conclusions as if they were facts; instead, most of their conclusions are based on hypothetical reasonings and speculation that were not substantiated. Furthermore, I still found some improper

referencing and incorrect understanding of the NAO impacts on the Mediterranean climate. All authors should proofread the article before submitting it rather than expect the reviewers to do that on their behalf! Simple software (like Grammarly or even Word itself) would have corrected many of the typos I found). I felt that the senior authors of this work should be more involved in the writing and proofing of the manuscript and its references.

Dear reviewer,

Thank you for your comments and suggestions to improve our manuscript. We addressed your concerns point by point below. Of course, all of the authors were involved in the writing and proofreading of the manuscript as well as the discussion of the data. Obviously, some typos and missing commas were not identified. For the new version, we paid extra attention to that issue.

Kind regards,

Vera Meyer and co-authors

• Regarding the climate dynamics aspects, I identified two main issues:
1. LL164-165"Warm and wet winters in the Mediterranean are generally associated with negative NAO-states while cold and dry winters occur during positive phases (Eshel and Farrel, 2000)."
Also in L661
Again, this is not correct! Only the southern part of the Mediterranean Sea will experience warm winters with NAO-, in particular northern African countries, while the rest of the Mediterranean will see average (central part) or below average (northern part) temperature.

We specified the region saying "southern Mediterranean" (lines 164-165; 661).

2. LL565-566 "According to Li et al. (2019), a weakening of the Atlantic Meridional Overturning Circulation (AMOC) during HS1 would push the westerlies northward."
In the conclusions, the authors wrote:
"First the climate became arid due to a northward shift of the storm track. At 16 ka BP wet conditions established as the storm track was pushed south. Afterwards the climate remained relatively dry until 11 ka BP as the westerly jet moved north in response to ice sheet retreat." (commas are again missing after First, At 16 ka BP,... Afterwards,)

It is unfortunate that Li et al. do say that, but it seems their interpretation of the model output (their figure 7) is not correct. Figure 7a shows a strengthening and narrowing of the zonal "jet" (although 500 hPa is not the geopotential height to look at the jet stream, it should be 200 hPa!), rather than a northward shift! This is not conducive to a positive NAO phase. Moreover, at higher altitudes, the easterly winds seem to increase, which is the opposite of an NAO+. Figure 9a [max (PI) – min (EH) precession] instead shows a northward shift of the zonal winds (I wonder at what elevation as the changes are tiny); NAO+ anomalies at 500 hPa (Fig. 9b, I assume it's at 500 hPa as the authors don't mention it, and also here we are talking about changes of max 1-3 dm [if the units they said (m2/s2) are indeed what they plot), which is also tiny], and a cooling of the Arctic (that could be dynamical simply because of less meridional exchange due to NAO+).
In another paper from Löfverström and Lora (2017) (https://doi.org/10.1002/2017GL074274), both atmospheric dynamicists looking at Trace simulations (the same as Liu et al., 2019) clearly show that the weakening of the AMOC, during the H1 leads to a narrow (due to the expansion of sea-ice, that's why the jet cannot go northward!) and stronger jet (see their figure 2). Also, the jet is less variable compared to warmer climates where the jet is more tilted, broader and weaker. See also an older

paper by Li and Battisti 2008 (DOI: 10.1175/2007JCLI2166.1).

Finally, the authors, with their interpretation based on Liu et al. 2019, were contradicting themselves as for the H1 with more sea ice and colder climate, the jet somehow was moving northward, but then when the ice sheet and sea ice retreated, the jet also moved northward!

Please re-interpret your data based on what actually the models are showing. If possible, I suggest using TraCE and checking whether a shift northward or southward impacts precipitation in northeastern Africa. Another option (or additional option) could even be to use ERA5 or observations and look at what happens at precipitation when the jet is northward and when the jet is southward. For the H1, if I recall correctly, the models, in general, show an overall aridification, so I am not sure why the authors' record shows 2 phases with the second wet; maybe because in the second phase, it is getting warmer? But then why would it get dryer again till 11 kyrs ago?

We were not aware of the controversy regarding the interpretation of the model output for HS1 in Liu et al. (2019) since none of us is an expert in paleo-climate modeling. Therefore, we are grateful to the reviewer for pointing this out. However, apart from the model, Li et al use grain size analysis of loess sequences in Central Asia to infer dynamics of the westerlies. These records suggest a stronger influence of the westerly jet at their study sites during Heinrich Stadials, which also supported their hypothesis of a northward movement. Besides, the review paper by Naughton et al. (2023) invokes northward and southward migrations of the polar front and associated Jet during HS1 to explain the two-phase pattern of HS1 described based on proxy data. In that data-based context, our inference regarding the movements of the polar front during HS1 seemed reasonable. However, considering other studies, this interpretation may be challenged. As we recall from the literature review, the response of the westerlies to AMOC slowdown during H stadials is debated. There are studies invoking a northward shift (Li et al., 2019) but others suggest the opposite (Nagashima et al., 2011). A recent study suggested that the jet axis remained stable but that the wave train was altered (Gai et al., 2023). In turn other studies indicated that the jet narrowed and strengthened at these times (e.g. Löfverström and Lora). So, just mentioning the hypothesis of the northward movement which, according to the reviewer, can be challenged from a modeler perspective, is probably not enough and one should also consider other hypotheses. At this point we agree with the reviewer and Editor that a detailed investigation of the development of the polar front in TraCE simulation and a comparison to proxy data in the Mediterranean-Eurasian realm would be a valuable and interesting endeavor to investigate whether the dynamics of the polar front can explain the hydrologic pattern in our record. However, we feel that this would be too much information for this manuscript as it would drastically lengthen it and the interpretations based on data and model might be rather complex. Such a comparison would probably be enough material for a follow-up study and publication. Therefore, we prefer to focus on the proxy data in this manuscript.

As a detailed investigation of the response of the jet to AMOC forcing is not the major focus of our manuscript, we decided to remove or rephrase sentences where shifts were mentioned. We talk about stronger or weaker influence of the westerlies in the new version. This leaves space for other dynamics than latitudinal movements of the jet axis including widening and narrowing of the westerly belt as well as strengthening or weakening of the winds as well changes in the wave pattern.

The following changes were made:

lines 21, 563-573 and 588-590: removed

513-516; 780-785: rephrased.

Gai, C., Wu, J., Roberts, A. P., Heslop, D., Rohling, E. J., Shi, Z., Liu, J., Zhong, Y., Liu, Y. and Liu, Q.: Heterogenous westerly shifts linked to Atlantic meridional overturning circulation slowdowns, Commun. Earth Environ., 4(1), 1–9, doi:10.1038/s43247-023-00987-z, 2023.

Nagashima, K. et al. Millennial-scale oscillations of the westerly jet path during the last glacial period. J. Asian Earth Sci. 40, 1214–1220 (2011).

Things are not as straightforward and clear as the authors make them seem to be! That's why the authors should always use conditional tenses when making those speculations and also refer to studies in which climate dynamicists and modellers are interpreting model outputs.
Here is a suggestion for one point in the conclusion (see also suggested correction in capital letters)

"Amplified winter precipitation MAY HAVE resulted from a combination of regional and large-scale factors. ENHANCED Local cyclogenesis (Cyprus Lows) MAY HAVE BEEN promoted by elevated HIGHER SSTs and southward displaced westerlies (negative NAO-like pattern) steering Atlantic storms into the Mediterranean AND intensifYING the moisture delivery and precipitation in the Nile-River delta region."

We added conditional tenses in the discussion and conclusion. Examples: lines 780-789; 736; 710.

• I lost count of how many times the authors mentioned in the text that the increase in precipitation during the AHP over northeastern Africa is due to an increase in winter precipitation, as the summer monsoon most likely did not go further north than 22 °N. Please be concise!

We shortened several paragraphs in the introduction and discussion to be more concise. Examples: lines 55-65; 625-639; 680-688; 691-693; 699-701.

We also reorganized section 5.1.

Additional comments

The authors said in their answer that they had corrected all instances, but I still found several places where they refer to North Africa rather than northern Africa: e.g., L 29, L32, L77, ...
The issues mentioned were changed.

Before "which" it usually goes a comma, e.g. L99. Again, the authors should use at least a software that corrects for such basic things (even Word would do it!).
L96 comma after "basin"...

L96: Changed. We searched for missing commas in the manuscript and added them.

L159 "Next to the Cyprus Lows the Red Sea Trough"... comma after "Lows"
there are so many of those instances that I cannot write them all here! Please do your job to proofread it and use the appropriate software to catch those mistakes.

Changed.

L143 change to read "along the coast".

Changed.

L246 "The concentrations of HMW n-alkanoic acids and HMW n-alkanes are shown in Figure 3." What is the point of such types of sentences in the main text? Just directly describe the results as done in the following sentence and then refer to the figure.

Sentences removed.

LL380-381 ". Bowen et al. (2005) define the growing season as months with mean temperatures >0°C." The authors cited Bowen et al. 2005 "Global application of stable hydrogen and oxygen isotopes to wildlife forensics" for the definition of growing season. This paper clearly does not seem to really focus on plants' growing season. I already asked the authors to check their references! Anyway, that definition makes no sense; most of central and southern Europe will have a growing season of 365 days or almost! One definition that is used as an indicator of the length of the growing season (https://www.epa.gov/climate-indicators/climate-change-indicators-length-growing-season) is the interval between the last and first frost, but there are others more accurate. This won't change the authors' conclusion or approach but further shows that senior authors should be more involved in the writing process. Otherwise, such mistakes cast doubts on the whole work.

We cited Bowen's dataset correctly and therefore did not change anything. Bowen et al., defined the growing season as days above 0°C and called their dataset dD of the growing season. We intended to correctly cite this published dataset by using its original name. We also state that in case for the Nile-River watershed this definition implies that the growing season equals the annual mean. This is also acknowledged by Bowen et al themselves with respect to the tropical regions. This is the information that matters as the annual mean is also what the plants capture in their isotopic signature. So, the dataset is applicable to our source apportionment approach, no matter if there are other definitions of the growing season that are incompatible with Bowen et al. By stating that the growing season definition implies that we are dealing with annual means is enough information for the readers to correctly understand how Bowen's dataset should be interpreted in our case. So, we do not understand why the reviewer has concerns about how carefully we checked the reference in this case. We cannot recon any misrepresentation here.

L532 the authors mean increase in evaporation.

Thanks for pointing out. Sentence removed to be more concise.

L437 L443 (and other places) either use the acronym EM or not (make sure to define it the first time).

We removed the acronym and work the full term throughout the new version of the manuscript. Figure 5 is an exception. We keep the acronym here to save space. In the caption the acronym is explained (line 581).

LL625-628 Again, this representation of the literature is incorrect!
First of all, if the models were showing 24°N and the proxy 23-28°N, there would be a good agreement.
Second, the authors cited modelling experiments in which the Sahara is vegetated, saying the models show the monsoon goes up to 24°N; however, Pausata et al.'s study is mentioned in the first sentence when the authors say the monsoon goes up only up to 24°N, but also, in the following one when they stated it goes up to 31°N). So, to my understanding, based on what the authors wrote, the models provide a range from 24 to 31°N and proxy records from 23 to 28°N, so it would seem a good agreement. Actually, the models seem even to overestimate the proxy values...

The confusion probably arose from the fact that PMIP simulations significantly underestimate the monsoon extension, if I recall correctly, doesn't even reach 18°N ... probably 16°N. However, those simulations should not be used to compare with proxies as they are missing necessary boundary conditions (such as vegetation changes). Hence, it would be a pointless exercise.
So please try to tell the story well, referring correctly to the literature; otherwise, the paper will lose credibility.

We are not entirely sure what the reviewer's point is. We are aware of the fact that PMIP-simulations do not include vegetation and dust feedbacks and that in many of these simulations the monsoonal extent is somewhere between 16-20°N. This underestimates what pollen and leaf wax data suggest. We did not cite the PMIP simulations along the lines and therefore do not understand why the reviewer is speculating about a confusion stemming from these simulations. We cite studies working with land-surface feedbacks which set the northernmost extent between 23-27°N. So yes, at least for some simulations, there is a relatively good match with pollen data as far as the northernmost extent of the monsoon is concerned. However, uncertainty remains regarding the consistency of models and data between ~27-31°N, where leaf-wax lipids indicate an increase in rainfall that cannot be reproduced by the majority of models (e.g. Chandan and Peltier, 2020; Thompson et al., 2019;2021). To our knowledge, there is only one simulation in Pausata et al which sets the boundary as far north as 31°N. Researchers speculate about other mechanisms driving the isotopic signal in the northern Sahara and the view of the monsoon fringe extending up to 31°N has recently been challenged by data from Morocco (Cheddadi et al., 2021). This is mentioned in lines 630-632.

As for the reviewers comment on the Pausata et al. citation, we cited the study in these two sentences as Pausata et al., provide a range of northernmost extents, which depend on different combinations of vegetation and dust feedbacks. The range is 26-31°N

For the new version, we removed the paragraph encompassing lines 625-639, which to a large extent, address the situation over northwestern Africa. We wanted to address the reviewer's recommendation of being more concise. Since the lines, are not relevant for the study site in the northeastern Sahara, we decided to remove them. The remaining paragraph focuses on northeastern Africa and the Nile delta region only. It is concluded that models and proxies agree that the monsoon fringe did not reach the Nile delta during the AHP. This is the important information.

L647 correct "north easternmost"
Changed to: "northeasternmost"

L650 "...most likely was responsible for the rain ..." it should be "was most likely responsible"; again, using basic grammar correction tools would have sufficed!

Changed.

LL655-658 "The warm SST would have enhanced evaporation over the Mediterranean Sea ... Additionally, warming would have strengthened the formation of Cyprus Lows over the Levantine Basin." The first part should be "warm SST enhanced" as it is a fact, whereas the second is hypothetical, so it should be "may have"... as warmer SST, if accompanied by more barotropic conditions, would not lead to more precipitation at all, but drier conditions.

Changed.

LL701-702 The verb is missing in that sentence!

Sentence says now: "Savannah was centred between 20-25°N partly reaching to ~28-31°N (Larrasoaña et al., 2013; Hély et al,. 2014; Giraudi et al., 2013; Hamdan et al., 2016)."

L709-710 avoid repeating the same word in the same sentence (enhanced).

Done.