nature portfolio

Peer Review File

The unexpectedly short Holocene Humid Period in Northern Arabia

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

31st Aug 21

Dear Dr Engel,

Please allow me to apologise for the long delay in sending a decision on your manuscript titled "The unexpectedly short Holocene Humid Period in Northern Arabia". It has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In particular, please ensure the revised manuscript meets the following editorial thresholds:

Provide a robust high-resolution record of lake conditions in the Tayma basin during the Holocene Humid Period and a compelling case that the wet phase in this region was significantly shorter than that recorded elsewhere

Fully justify your interpretations including an explanation of the inferred mechanisms responsible where necessary

Tone down the strength of your claims to ensure they are in line with the associated level of uncertainty

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for

resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Joe Aslin

Associate Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

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All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)

- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors present a very high resolution and well dated record, which enables the authors to draw novel claims that the wet phase in N Arabia is considerably shorter than the commonly defined HHP, with an especially humid time around 8.2ka when it is drier in adjacent regions. They are able to suggest possible changes in atmospheric circulation that could account for this. The use of multiple proxies, and robust interpretation of them, results in convincing conclusions. This paper moves forward thinking in the field. Figures are great e.g. Figure 3 is a really nice plot and demonstrates the regional heterogeneity well. Overall, a fantastic dataset, great interpretation and very clearly and succinctly written.

I have a few suggestions for improvements - mainly the need to clarify some points. If these clarifications are suitably made then I recommend this paper for publication in this journal based off the novel findings the authors have made.

1. Can you clarify in methods exactly what you are measuring for d18Ocarb? Is it the aragonite, mg-calcite and biogenic calcite combined? Or ostracod calcite because in methods you just talk about measuring aragonite valves and ostracod calcite, but then in the main text you don't talk about d18Oostracod - in the main text you just talk about δ18O of “single primary aragonite laminae” and “bulk carbonates”? If you are measuring bulk carbonate samples (i.e. biogenic calcite, inorganic

calcite and inorganic aragonite combined), do you have varying proportions of biogenic vs inorganic carbonate between samples? If so it would be good if you could justify why you do not see it as a problem that you are measuring biogenic calcite alongside inorganic carbonate – are there definitely not strong vital effects in the biogenic calcite that could be causing significant changes to the isotope signal independent of evaporation influence? I don't think there is an issue esp given the similarly of the d18Ocarb and d18Oarag on Figure 2 where there is overlap, but you need to discuss what is in carb measured for d18Ocarb in more detail and justify why having (presumably) varying amounts of biogenic and inorganic between samples is not an issue.

2. Figure 1a seems blurry (to me at least) in the middle – check this won't be the case on the final version.

3. You say "distinct change in varve composition from evaporation-driven aragonite varves to productivity-fuelled diatom-aragonite varves" – can you justify your interpretations here i.e. why are you sure the aragonite varves are evap driven and the diatom-aragonite ones productivity driven? At least cite some sources to justify.

4. What evidence do you have that the varves are indeed annual laminations?

Reviewer #2 (Remarks to the Author):

Sediments from a Sabka north of Tayma deliver a precisely-dated and highly-resolved Holocene record from Northern Arabia, a region were high-quality records are basically absent. Thus, this new record helps to fill a large spatial gap of high-resolution and adds important information on the duration of peak humid conditions in northern Arabia. The chronology of the new record is very well constrained as it is based on numerous radiocarbon dates, varve counting and cryptothephra. In addition, numerous parameters were measured to develop a multi-proxy record, whereas the interpretation of the climate proxies appears to be plausible. Based on this new record from Tayma, the authors present convincing evidence for a rather short humid period which lasted between 8,800-7,900 years BP, much shorter compared to southern Arabia. It is possible that tropical plumes rather than the African or Indian monsoons were the main source of moisture during this time, and not a southward shift in rain bearing depressions from the Mediterranean and Atlantic. Overall, I can recommend this manuscript for publication after some minor revisions were made (See comments below).
The interpretation of δ18O records, in particular those coming from speleothems, in the Eastern Mediterranean is complex, and they are not only influenced by the amount effect but also by a change in δ18O of the surface waters in the Eastern Mediterranean as a result of greatly enhanced Nile discharge. It is therefore not entirely correct to use the Jeita and Soreq Cave δ18O records as proxy for the amount winter rainfall. On this note, Cheng et al. (GRL, 2015) state that the Jeita Cave δ18O record is influenced by the "amount" and "source effect". The authors should address this issue in their manuscript. Interestingly, the Soreq Cave δ13C record documents an abrupt positive isotopic shift between approx. 8,600 and 7,000 yrs BP that resulted from a substantial increase in rainfall and associated change in the recharge conditions ("deluge events") above Soreq Cave. Thus, I suggest to include the Soreq Cave δ13C in figure 3.
The figures are of very high-quality, but some records shown in the figures are hardly mentioned in the main text. This could be done more effectively to support the main conclusions.

Figure 1a is slightly confusing as the colors denote annual rainfall which, however, does not coincide with the isohyets (white lines) for 200 and 500 mm? The text in the figure caption does not contain any explanations.
Lines 59 – 61: The authors cite a couple of articles on the intensification and extension of the African monsoon into Arabia. They forgotten to cite the original publication (Rosenberg et al., Quaternary Science Reviews, 2013), which was the first article that discussed the importance of African monsoonal rainfall for humidity in the Nefud desert and the surrounding areas. All cited articles were published after Rosenberg et al., 2013, so please add this reference here.

Reviewer #3 (Remarks to the Author):

The manuscript by Neugebauer et al. using a multi-indicator approach incorporating geochemical analysis and varve counts on paleolake deposits in order to reconstruct paleoenvironmental changes in northern Arabia at high temporal resolution across the HHP. This is very meaningful data from a poorly studied region during an interval of time that has important implications for understanding climatic processes during a period very different from today. Further, the combination of multiple lines of evidence from geochemistry and precise, high-resolution dating makes it possible to better understand potential teleconnections of climate changes to drivers.
I think this is an important study from an important region; however, there are a number of weaknesses in the manuscript that serve to make the message and data less clear that should be improved prior to publication. In particular, important arguments that stem from the interpretation of the data are 1) disentangling both groundwater vs rainfall moisture as well as 2) seasonality of rainfall, but the organization of data presentation and interpretation is not clear in the text. There is little discussion of the interpretation linking the measured observations to processes at this site, with the text often following often giving an observation then skipping right to an environmental interpretation without much discussion of the process in this specific lake system. Given how site specific many paleoenvironmental indicators may be (ex. gypsum formation can indicate different things in different systems), more context for previous work at the site or elsewhere would help us understand the basis for environmental interpretations in this record. There is also some confusion in how the geochemical data is referred to throughout (ex. see below comments about leaf waxes and oxygen isotopes). Additionally, it isn't clear from the section about separating the groundwater and rainfall signals how that is accomplished. Presumably leaf waxes tells us about rainfall (though the conversion of δDwax to δDp is not described), but it's unclear where the groundwater interpretation comes from? Is it just whenever δDwax and oxygen isotope signals are at odds? There are numerous places noted in my detailed comments below where more specific interpretations or a conceptual model are needed to make them clear. The text also includes many complicated run-on sentences that likely lead to the lack of clarity in many instances. Additionally, the interpretation of the length of the HHP is based on the duration of the varved phase, however there are few details included from the previous publication about controls on varve formation at this site that help us understand how only the varved phase relates to the HHP locally. Finally, the TP interpretation is quite novel but seems conjectural without stronger linkages to the presented data or other dynamical or empirical evidence. I think it can be presented but describing it as "proved" seems premature.
Detailed comments:
Abstract: does not include much in terms of broader impacts and significance for a publication of this level.

Line 31: “rainfall”

Line 32: “well-confined” does this mean well constrained? in space or time? Also, probably better to spell out “north” and “south”.

Line 39-40: unclear sentence

Line 47: unclear wording, "became the focus"?

Line 53: spell out approximately

Paragraph that ends at line 64: unclear how winter and southern moisture will be disentangled

Line 65: “currently unraveled” to “is being currently investigated by” or something similar. Previous wording is awkward.

Line 76: simplify the sentence starting in this line. Many sentences throughout are run-ons or include too much information and should be broken up and simplified.

Line 81: “proven” is used frequently. I suggest changing this throughout, particularly when conclusions include some conjecture to make uncertainties clearer.

Line 85-86: More process-based discussion needed on how the climate signal recorded by each individual indicator relates to an influence of both rainfall (directly related to climate) and groundwater (indirectly with a lag related to climate) at this site.

Line 86 “precise determination of the timing of the lake phases…”

Line 99: “basin were deposited..”

Line 103: write out pore-water and rainwater

“Groundwater vs rainfall signal” section: very unclear what was measured and how/why each is related to climate via rainfall or groundwater. There is no information about how leaf wax isotopes in particular is converted to δDp, which is necessary for the reader to understand the interpretation of how these are disentangled with the data presented. Also in line 108-109, -11‰ also seems like it is the maximum (high value) rather than “minimum” in comparison to -28‰.

“Evolution of the Tayma paleolake” section: It is generally hard to follow the organization here. The presentation of phases through time makes sense, but within each phase, the interpretation is presented before the data and the data are presented in a way that seems to vary a lot throughout the section. This makes following the arguments confusing. Part of this is that the individual proxies interpretations weren’t made clear for this site, and some abbreviations are mentioned that have not yet been defined (ex. δDnC29, nC31; Sometimes it δ18Ocarb sometimes δ18Ocarb/arag is used, are these the same or different).
-no description of the lithology of the section and its unclear how the environmental interpretations were made and based on which data? Notionally δDwax is the rainfall signal and the bulk isotopes can tell you about groundwater, but these aren't disentangled here or explicitly stated in relation to

what is known about the site. Make this more clear.

Line 127: Unclear how δDwax is related to seasonality here and how that inference is supported.

Line 134: this is a good example where a distinction between rainfall and groundwater inputs is interpretated that isn't clear how its arrived at. It is not yet presented why you interpret δ18Ocarb to indicate dry season evaporation (I think this makes sense, but it should nonetheless be justified with prior literature here or elsewhere) or why the leaf waxes here also indicate enhanced seasonality rather just higher rainfall.

Line 136-140: another example of a very complicated run-on that should be simplified for clarity. Also in line 140 "highest lake stand" is this higher than the 17m high stand mentioned before and how does this tie to the shoreline record mentioned previously?

Line 147: "first appearance of gypsum", this is the first-time gypsum has been mentioned, so a process based explanation citing literature would be useful for understanding how this connects at this site to a "rapidly declining lake".

Line 149: "TOC" and what it signifies in this site in this record have not been defined yet.

Line 154-159: two more examples of overly complicated sentences that need to be simplified and more thoroughly discussed

Line 158 : "aridisation" to "aridification"

Discussion: general comment, your mechanism cited here is very interesting, but it does seem a bit conjectural without other supporting evidence from other records or model output suggesting this is a plausible scenario for the period given the boundary conditions of the time. This interpretation would be supported by citing prior modeling studies of the HHP to determine whether circulation of this interval might be more conduce to TP events as well as to rule out other mechanisms (which are not much described here) related to groundwater recharge, summertime tropical moisture or winter rainfall associated with a more southward jet stream during a NH cold event at 8.2ka. This is where being very clear about the interpretations of the rainfall seasonality at each point would be very useful; however, since the TPs deliver moisture during the winter, it would also be useful to discuss how you can distinguish winter TP moisture from winter jet stream moisture.

Line 161-162: This is an example where I think "prove" is not quite the right word choice given uncertainties. Also, the duration for the HHP cited here is constrained by the varved portion of the record. Since the controls on varve formation are not talked about here, what other constraints on lamination formation, like wind or temperature or water chemistry, play into this determination of only linking to moisture? Its also unclear to me what this means with respect to rainfall vs groundwater discharge, since HHP peak should imply rainfall is high.

Dear Editor, dear Reviewers,

We are thankful for the evaluation of the manuscript. The reviews have acknowledged the high significance of the data but also, legitimately, point to some weaknesses. In this document we provide responses to each of the reviewers' queries:

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors present a very high resolution and well dated record, which enables the authors to draw novel claims that the wet phase in N Arabia is considerably shorter than the commonly defined HHP, with an especially humid time around 8.2ka when it is drier in adjacent regions. They are able to suggest possible changes in atmospheric circulation that could account for this. The use of multiple proxies, and robust interpretation of them, results in convincing conclusions. This paper moves forward thinking in the field. Figures are great e.g. Figure 3 is a really nice plot and demonstrates the regional heterogeneity well. Overall, a fantastic dataset, great interpretation and very clearly and succinctly written.

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→ We are thankful for this comment and understand that our approach of C and O isotope analysis was not sufficiently well explained, and we identified some inconsistencies in data presentation that were corrected. We reworded the chapters "Groundwater vs. Rainfall signal in the Tayma record (now: "Changes in precipitation and evaporation signals in the Tayma record"), "Evolution of the Tayma palaeolake" and the Methods section (chapters "Tayma sediment cores" and "Stable oxygen and carbon isotope measurements") to be more clear about the distribution of different carbonates in the sedimentary record and what exactly has been measured.
For this paper, C and O isotopes were measured in (i) aragonite from the annually laminated (varved) section of the core ($\delta^{18}O_{arag}$ and $\delta^{13}C_{arag}$) and (ii) bulk carbonates (i.e., all fractions combined) that are available from the entire core ($\delta^{18}O_{carb}$ and $\delta^{13}C_{carb}$). We did indeed also measure C and O isotopes from ostracod valves from some of the palaeo-shoreline sites

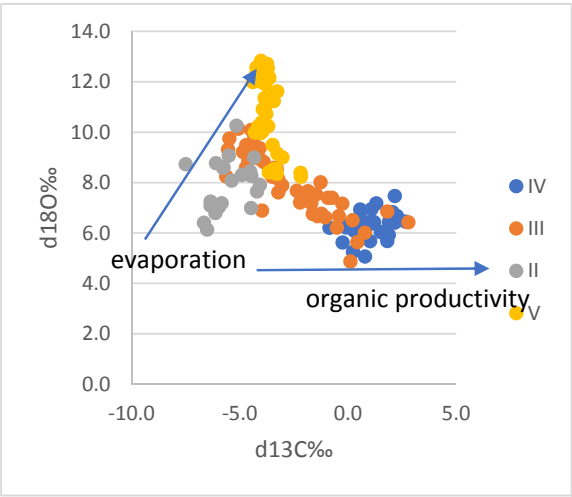
(Supplementary Figs. 4, 5), even though this data was not used in the current paper. We understand that mentioning the ostracod-based isotope measurements in the Methods section might have led to confusion and, therefore, deleted them (as the data was not used here).
We are confident regarding the bulk carbonate data, despite the varying ratios between aragonite, Mg-calcite and biogenic calcite in the core, as: (i) the correlation with the $\delta^{18}O_{arag}$ and $\delta^{13}C_{arag}$ over the varved core section is excellent as shown in Fig. 2, and (ii) the amount of biogenic calcite is very low at the core site (their influence on isotope data of bulk carbonate is considered negligible). Ostracod valves, foraminiferal tests, gastropod shells and barnacle fragments are very abundant along the shorelines of the palaeo-lake (Supplementary Figs. 4, 5), but only occur in low concentrations in the centre of the basin, where the mastercore was taken.

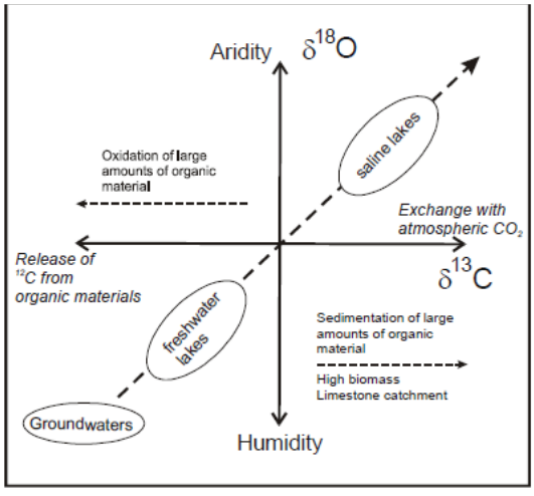
2. Figure 1a seems blurry (to me at least) in the middle – check this won't be the case on the final version.

→ There was an issue with the JPEG version inserted into the text file. The TIFF version of Fig. 1a is fine, though.

3. You say "distinct change in varve composition from evaporation-driven aragonite varves to productivity-fuelled diatom-aragonite varves" – can you justify your interpretations here i.e. why are you sure the aragonite varves are evap driven and the diatom-aragonite ones productivity driven? At least cite some sources to justify.

→Aragonite precipitation driven by evaporation is a straightforward interpretation given the geographic location in the tropic of Cancer with strong summer insolation and current annual evaporation rates of up to 2000 mm/yr (hPET model in Singer et al., 2021). Based on Leng & Marshall (2004; in particular summed up in their Fig. 2b, see below), the correlation between $\delta^{13}C_{carb}$ and $\delta^{18}O_{carb}$ during phases II and V shows aridity driven by evaporation, while a disproportional increase of $\delta^{13}C$ in III and in particular IV relates to phytoplankton bloom (controlled by ^{12}C depletion by photosynthetic uptake of CO_2). We added Leng & Marshall (2004) as a reference for this interpretation in the main text. Regarding similar correlations between $\delta^{13}C_{carb}$ and $\delta^{18}O_{carb}$ observed in the Dead Sea and reproduced in lab experiments, Ben Dor et al. (2021) established the following environmental interpretation: "peak temperatures and increased evaporation during summer resulted in increased aragonite saturation by the effect of evaporation and CO_2 degassing, resulting in abrupt aragonite precipitation." We agree with and follow this interpretation and added this new reference, too.





Left: Biplot of $\delta^{18}O_{carb}$ and $\delta^{13}C_{carb}$ values from this study. Right: Stable isotope interpretation from lake sediments as proposed by Leng & Marshall (2004; their Fig. 2b on page 813)

4. What evidence do you have that the varves are indeed annual laminations?

→ Every single varve was counted using thin sections. 650 +/- 40 varves were counted in total. Based on the age model of the core (based on 38 radiocarbon ages of pollen concentrates, *Ruppia* seeds, non-pollen palynomorphs, charred plant particles and two mollusc samples, as well as a tephrochronological anchor identified close to the base of the Tayma sediment record as the central Anatolian 'S1'-tephra dated at 8,983 ± 83 cal yr BP in the Dead Sea; Neugebauer et al., 2017), varve formation started at 8,549 cal yr BP (8,470–8,605 cal yr BP for the 95.4% probability range). The sedimentation rate based on the assumption of annual varves overlaps with the one derived from the age model for the varved section within errors, justifying the assumption of annual lamination.

Reviewer #2 (Remarks to the Author):

Sediments from a Sabka north of Tayma deliver a precisely-dated and highly-resolved Holocene record from Northern Arabia, a region were high-quality records are basically absent. Thus, this new record helps to fill a large spatial gap of high-resolution and adds important information on the duration of peak humid conditions in northern Arabia. The chronology of the new record is very well constrained as it is based on numerous radiocarbon dates, varve counting and cryptothephra. In addition, numerous parameters were measured to develop a multi-proxy record, whereas the interpretation of the climate proxies appears to be plausible. Based on this new record from Tayma, the authors present convincing evidence for a rather short humid period which lasted between 8,800-7,900 years BP, much shorter compared to southern Arabia. It is possible that tropical plumes rather than the African or Indian monsoons were the main source of moisture during this time, and not a southward shift in
rain bearing depressions from the Mediterranean and Atlantic. Overall, I can recommend this manuscript for publication after some minor revisions were made (See comments below).

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→ We are well aware that the Jeita and Soreq Cave $\delta^{18}O$ records are not only influenced by the amount effect, but also by the source effect and we have added some more explanation in the manuscript and the reference to Cheng et al. (2015). In Figure 3c, we marked a period of 'excess winter rains' at Soreq Cave between ca. 8000 and 7000 cal yr BP, which has been calculated in the sapropel study of Rohling et al. (2015) based on a comparison with the marine LC21 record, and where the source effect was considered in the calculation. Overall, Rohling et al. concluded that winter rainfall in the eastern Mediterranean was somewhat, but not substantially higher than average during the early to mid-Holocene, and only during the one millennium of 'excess winter rains'.

Interestingly, the Soreq Cave δ13C record documents an abrupt positive isotopic shift between approx. 8,600 and 7,000 yrs BP that resulted from a substantial increase in rainfall and associated change in the recharge conditions ("deluge events") above Soreq Cave. Thus, I suggest to include the Soreq Cave δ13C in figure 3.

→ As explained above, when the source effect is considered, rainfall was likely not that much higher. Adding the Soreq Cave $\delta^{13}C$ record instead of $\delta^{18}O$ would require a detailed discussion of the Soreq record, which we cannot do in this journal format. Also, in Figure 3, we intend to show a regional gradient using the same proxy ($\delta^{18}O$) and, therefore, we did not change the figure.

The figures are of very high-quality, but some records shown in the figures are hardly mentioned in the main text. This could be done more effectively to support the main conclusions.

→ We have now taken care that all panels in Figs. 2 and 3 are referred to in the text.

Figure 1a is slightly confusing as the colors denote annual rainfall which, however, does not coincide with the isohyets (white lines) for 200 and 500 mm? The text in the figure caption does not contains any explanations.

→ We agree about this confusing aspect. Explanations for the 200 mm and 500 mm isohyets are indeed missing. They do not represent modern annual values of rainfall, but were digitized from the georeferenced map of the wettest simulation of the 6000 BC COSMOS climate model in Guagnin et al. (2016) in order to indicate the maximum potential of differences between the HHP and now. Explanations were added to the captions.

Lines 59 – 61: The authors cite a couple of articles on the intensification and extension of the African monsoon into Arabia. They forgotten to cite the original publication (Rosenberg et al., Quaternary Science Reviews, 2013), which was the first article that discussed the importance of African monsoonal rainfall for humidity in the Nefud desert and the surrounding areas. All cited articles were published after Rosenberg et al., 2013, so please add this reference here.

→ We agree and added Rosenberg et al. (2013).

Reviewer #3 (Remarks to the Author):

The manuscript by Neugebauer et al. using a multi-indicator approach incorporating geochemical analysis and varve counts on paleolake deposits in order to reconstruct paleoenvironmental changes in northern Arabia at high temporal resolution across the HHP. This is very meaningful data from a poorly studied region during an interval of time that has important implications for understanding climatic processes during a period very different from today. Further, the combination of multiple lines of evidence from geochemistry and precise, high-resolution dating makes it possible to better understand potential teleconnections of climate changes to drivers.

I think this is an important study from an important region; however, there are a number of weaknesses in the manuscript that serve to make the message and data less clear that should be improved prior to publication.

In particular, important arguments that stem from the interpretation of the data are 1) disentangling both groundwater vs rainfall moisture as well as 2) seasonality of rainfall, but the organization of data presentation and interpretation is not clear in the text.

→ We agree and tried to improve data presentation, in particular in terms of moisture sources. We did so in the chapter "Groundwater vs. Rainfall signal in the Tayma record (now: "Changes in precipitation and evaporation signals in the Tayma record") and "Reconstruction of palaeo-moisture source and lake-water evaporation" in the Methods section. More detailed responses follow for the more specific comments below, also regarding the interpretation of seasonality inferred from δD and $\delta^{18}O$ data.

There is little discussion of the interpretation linking the measured observations to processes at this site, with the text often following often giving an observation then skipping right to an environmental interpretation without much discussion of the process in this specific lake system. Given how site specific many paleoenvironmental indicators may be (ex. gypsum formation can indicate different things in different systems), more context for previous work at the site or elsewhere would help us understand the basis for environmental interpretations in this record.

→ We agree that the processes leading to sedimentation and erosion in this basin are described only briefly in this paper. Therefore, we added information to the section "Regional setting of Tayma and its sabkha" in the online supplement, where sedimentation processes are explained. Furthermore, some relevant details have been outlined in previous publications that are now additionally referred to in this new section. As the processes of the sedimentary archives have been explained elsewhere in previous papers, there is no need to reiterate them in the main text of this manuscript. However, we added explanations on the formation processes of the varves and their implications to the section "Evolution of the Tayma palaeolake", see response to comment below.

There is also some confusion in how the geochemical data is referred to throughout (ex. see below comments about leaf waxes and oxygen isotopes). Additionally, it isn't clear from the section about separating the groundwater and rainfall signals how that is accomplished. Presumably leaf waxes tells us about rainfall (though the conversion of δDwax to δDp is not described), but it's unclear where the groundwater interpretation comes from? Is it just whenever δDwax and oxygen isotope signals are at odds?

→ We agree, the partially inconsistent reporting of isotope data has been corrected both in the main text and Methods section and should be clearer now. In the Methods section "Reconstruction of palaeo-moisture source and lake-water evaporation" the interpretation of groundwater and rainfall signals is explained. We show that sabkha groundwater close to the surface and surface water from modern wetland puddles both are highly enriched in heavier O, while δD is also high compared to water from the aquifer. This is interpreted as a strong evaporation signal (Supplementary Fig. 8). Therefore, we agree that we should reword the sentence in the caption of Supplementary Fig. 8 that now emphasizes the evaporation control. The modelled isotope data of the lake and wetland phases (in particular $\delta^{18}O$) (see Methods section on how this was done) are extremely heavy. We cannot clearly separate between groundwater and rainfall signals, and therefore changed the subheader in the main text. We can, however, unequivocally demonstrate the evaporation control based on modern isotope data from the study site, in particular those from Bir Haddaj.
We disagree, however, that the conversion of δD_{wax} to δD_p is not described. The Methods section „Stable hydrogen isotopes of leaf-wax n-alkanes" explains how these data were

measured on samples from the sediment. It is explained that δD_{wax} is interpreted as indicator for the variability in precipitation, humidity and vegetation type following the reasoning in Bowen (2010) and Sachse et al. (2006). It is stated that $\delta D_{nC29, nC31}$, of terrestrial leaf wax nC_{29} and nC_{31} n-alkanes, were used to calculate δD_p according to Sachse et al. (2012) and Collins et al. (2013). We then explain that precipitation rates were inferred from δD_p values as in Tierney et al. (2013). Any interpretation on groundwater signal comes from modern groundwater samples that we took inside the historical Bir Haddaj well in December 2015 that is only marginally affected by evaporation (if at all) and that shows striking similarity with stable isotope data from within the Saq aquifer accessible in literature (Alyamani, 2001; Al-Sagaby & Moallim, 2001).

There are numerous places noted in my detailed comments below where more specific interpretations or a conceptual model are needed to make them clear. The text also includes many complicated run-on sentences that likely lead to the lack of clarity in many instances.

→ Technically, there are no run-on sentences in the manuscript, as we always use proper punctuation. However, yes, some sentences are too long, and we tried to shorten/split them.

Additionally, the interpretation of the length of the HHP is based on the duration of the varved phase, however there are few details included from the previous publication about controls on varve formation at this site that help us understand how only the varved phase relates to the HHP locally.

→ We agree, added explanation to the section "Evolution of the Tayma palaeolake" and refer to the process-based model of varve formation at Tayma first developed in Ginau et al. (2012).

Finally, the TP interpretation is quite novel but seems conjectural without stronger linkages to the presented data or other dynamical or empirical evidence. I think it can be presented but describing it as "proved" seems premature.

→ Yes, we toned down the strength of the claim and extended the discussion towards the East African Summer Monsoon.

Detailed comments:
Abstract: does not include much in terms of broader impacts and significance for a publication of this level.

→ We disagree: We point out that the period that this paper has a focus, the early to mid-Holocene humid period, was the last period of substantially increased moisture availability, denser vegetation and human migrations into the desert environments of the Arabian Peninsula. We clearly state that this palaeoenvironmental record fills a long-standing geographic gap, which has been acknowledged by both Reviewers 1 and 2. This prominent period has been investigated in the wider region, i.e. the Saharo-Arabian belt, and published in very high-impact journals. Here, we not just reiterate the finding of the HHP, but we clearly demonstrate its short duration with almost unprecedented precision for the sub-region under study. Moreover, this phase overlaps with a climate event of large significance across the Holocene, i.e. the 8.2k event, which often is reported to be a dry-cool anomaly. Our evidence is contradictive, but solid. All this is stated in the abstract and represents a level of novelty and added value that in our opinion justifies publication in a high-impact journal.

Line 31: “rainfall”

→ Yes, changed.

Line 32: “well-confined” does this mean well constrained? in space or time? Also, probably better to spell out “north” and “south”.

→ Yes, we changed to well-constrained. We refer to time for a specific space, that is explicitly mentioned (two specific sub-regions of the Saharo-Arabian belt, i.e. North Africa and southern Arabian Peninsula). Directions are now always spelled out, in the entire paper.

Line 39-40: unclear sentence

→ We are not sure what exactly is unclear. The sentence is sound and was again checked from several different persons. The sentence states that we found a short period of peak humidity of only ca. 200 years, that coincides with the centennial-scale dry anomaly around 8,200 years BP interrupting the HHP in adjacent regions. This is one of the core messages of the paper. We leave it as it is until there is more information as to what exactly is unclear.

Line 47: unclear wording, "became the focus"?

→ The sentence was reworded.

Line 53: spell out approximately

→ OK, changed.

Paragraph that ends at line 64: unclear how winter and southern moisture will be disentangled

→ Indeed, this is yet unresolved. But no claim has been made that the present study will solve this question. Therefore, it is unclear to us why such an explanation has to be given at exactly this position in the manuscript.

Line 65: “currently unraveled” to “is being currently investigated by” or something similar. Previous wording is awkward.

→ OK, reworded.

Line 76: simplify the sentence starting in this line. Many sentences throughout are run-ons or include too much information and should be broken up and simplified.

→ As mentioned above, we tried to shorten long sentences, including this one.

Line 81: “proven” is used frequently. I suggest changing this throughout, particularly when conclusions include some conjecture to make uncertainties clearer.

→ We agree, changes were made.

Line 85-86: More process-based discussion needed on how the climate signal recorded by

each individual indicator relates to an influence of both rainfall (directly related to climate) and groundwater (indirectly with a lag related to climate) at this site.

→ We agree and added a section on sedimentation processes in the sedimentary archive to the Supplement chapter "Regional setting of Tayma and its sabkha", and also refer to previous publications on the palaeoenvironmental record. The sentence in Line 85-86 now has a reference to this new section.

Line 86 "precise determination of the timing of the lake phases…"

→ OK, wording modified.

Line 99: "basin were deposited.."

OK, wording modified.

Line 103: write out pore-water and rainwater

OK, wording modified.

"Groundwater vs rainfall signal" section: very unclear what was measured and how/why each is related to climate via rainfall or groundwater. There is no information about how leaf wax isotopes in particular is converted to δDp, which is necessary for the reader to understand the interpretation of how these are disentangled with the data presented.

→ For the sake of clarity we changed the subheader to "Changes in precipitation and evaporation signals in the Tayma record" and reworded some parts of the section. Furthermore, we adjusted the Methods section. The combined information from the section "Changes in precipitation and evaporation signals in the Tayma record" and the Methods section "Reconstruction of hydroclimatic conditions". Regarding the measurement of C and O isotopes and how these data relate to climate, we refer to our response to the 1st query of Reviewer 1.
We disagree that there is no information about how leaf wax isotope data were converted to δDp and refer to our 3rd reponse to Reviewer 3 above. Changes in precipitation are indicated by δD of terrestrial plant n-alkanes nC$_{29}$, nC$_{31}$, which show a close relationship to the surface water isotope composition (Garcin et al., 2012; Sachse et al., 2012; Aichner et al., 2019).

Also in line 108-109, -11‰ also seems like it is the maximum (high value) rather than "minimum" in comparison to -28‰.

→ We erroneously reported the calculated δD_p values here (as used in Supplementary Fig. 8), but it should be the $\delta D_{nC29,\ nC31}$ values as shown in Fig. 2 and used for interpretation. The text was changed accordingly.

"Evolution of the Tayma paleolake" section: It is generally hard to follow the organization here. The presentation of phases through time makes sense, but within each phase, the interpretation is presented before the data and the data are presented in a way that seems to vary a lot throughout the section. This makes following the arguments confusing.

→ The condensed format of the research paper prompts the combined presentation of core data and interpretation. The reviewer criticizes that interpretation is always presented before

the data (which is not true), but also that data presentation varies a lot. We slightly reorganized this section to describe lithology and further data before an interpretation. We do not use the same sentence structure and order of reported parameters, followed by interpretation for each time slice, as the indicative parameters vary. Based on our understanding of empirical data presentation in the text, this should be selective and focus on indicative patterns only, rather than to schematically report the whole dataset, in this case, core section by core section.

Part of this is that the individual proxies interpretations weren't made clear for this site, and some abbreviations are mentioned that have not yet been defined (ex. δDnC29, nC31; Sometimes it δ18Ocarb sometimes δ18Ocarb/arag is used, are these the same or different).

→ We agree, and have clarified and homogenized the abbreviations in the text. More extended explanations of the abbreviations and the individual stable isotope parameters are given in the Methods section. Please see also our 1st response to Reviewer 1.

-no description of the lithology of the section and its unclear how the environmental interpretations were made and based on which data? Notionally δDwax is the rainfall signal and the bulk isotopes can tell you about groundwater, but these aren't disentangled here or explicitly stated in relation to what is known about the site. Make this more clear.

→ We disagree, there are descriptions of lithology in the section "Evolution of the Tayma palaeolake". We agree, however, that more lithology presentation is needed and we added it for the core section where it was lacking before, in particular the varve formation. As mentioned above, further explanations on the interpretation of δD_{wax} and C and O isotope data are now given in the Methods section, Line 403 ff., and the caption of Supplementary Fig. 8.

Line 127: Unclear how δDwax is related to seasonality here and how that inference is supported.

→ See response to following comment. Text was added at the end of this section to better explain the interpretation of enhanced seasonality.

Line 134: this is a good example where a distinction between rainfall and groundwater inputs is interpretated that isn't clear how its arrived at. It is not yet presented why you interpret δ18Ocarb to indicate dry season evaporation (I think this makes sense, but it should nonetheless be justified with prior literature here or elsewhere) or why the leaf waxes here also indicate enhanced seasonality rather just higher rainfall.

→ Again, the interpretation of $\delta^{18}O_{carb}$ to indicate evaporation is straightforward and explained in the Methods section. However, we added references, e.g. Leng & Marshall (2004), and tried to make the reasoning more clear – both in the text and the Methods section. The relationship between δD_{wax} and δD_p is explained in the Methods section, where we also refer to Collins et al. (2013). Enhanced seasonality is inferred from two specific indicators: (i) a general high variability of $\delta D_{nC29,\ nC31}$ (cf. Sachse et al., 2012; Collins et al., 2013); (ii) intensified evaporation reflected by $\delta^{18}O_{carb}$ and a stable negative trend of $\delta D_{nC29,\ nC31}$ (despite strong variability between singular measurements) at the same time. This pattern indicates a moisture surplus during the rainy season (this is the season that is mostly reflected by $\delta D_{nC29,\ nC31}$; cf. Collins et al., 2013), which was able to compensate intensified evaporation represented by high $\delta^{18}O_{carb}$ around 8.4 kyr. We reworded this section for clarification, and added a reference.

For the calculation of the evaporation line we used a simple Rayleigh equation (Dansgard, 1964) under the assumption of lacking humidity and a mean temperature of 25°C: $\delta^{18}O_{evaporated\ water}=\delta^{18}O_{initial\ water}-(1000*(f^{(1/\square-1)}-1))$ at 25°C. Additional information is given in the Methods section.

Line 136-140: another example of a very complicated run-on that should be simplified for clarity. Also in line 140 "highest lake stand" is this higher than the 17m high stand mentioned before and how does this tie to the shoreline record mentioned previously?

→ This is not a run-on sentence per definition; its structure is actually rather simple. It simply lists indicators for the wettest conditions and highest lake stand. However, we tried to distribute these indicators over two sentences. The "highest lake stand" refers to the minimum of 17 m that has been inferred from the vertical distribution of Holocene shoreline deposits, as indicated in Line 86 already. In Line 88 we change "the" into "this" to make entirely clear that the depth of >17 m represents the peak lake phase which is equal to the "highest lake stand" during the Holocene. We are, however, not going to reiterate that we use the upper boundary of the shoreline deposit to infer a minimum peak lake level. This is referred to in Line 86 and in Supplementary Figs. 3–5.

Line 147: "first appearance of gypsum", this is the first-time gypsum has been mentioned, so a process based explanation citing literature would be useful for understanding how this connects at this site to a "rapidly declining lake".

→ Gypsum is an established indicator of lake level fall and lower supply of freshwater in arid regions, as demonstrated, e.g., by Torfstein et al. (2008) at a larger scale in the Dead Sea. At Tayma, the first gypsum formed due to enrichment of Ca^{2+} in the water column due to aridification and lake-level fall after the short HHP, and sulphate contribution dissolved in the groundwater inflow. Therefore, the occurrence of gypsum is interpreted to reflect lake-level fall and, later after 6800 cal yr BP, wetlands with only shallow, even ephemeral water cover.

Line 149: "TOC" and what it signifies in this site in this record have not been defined yet.

→ TOC methodology and significance were added to the Methods section (subsection "Tayma sediment cores").

Line 154-159: two more examples of overly complicated sentences that need to be simplified and more thoroughly discussed

→ Sentences were split. The transfer of shrinking water body, concentration of brine (this aspect was added) and increased precipitation of gypsum, that is observed in XRF data and thin sections, is straightforward and does not require further discussion, in our opinion.

Line 158 : "aridisation" to "aridification"

→ OK, changed.

Discussion: general comment, your mechanism cited here is very interesting, but it does seem a bit conjectural without other supporting evidence from other records or model output suggesting this is a plausible scenario for the period given the boundary conditions of the time.

→ We agree, and toned down the strength of this interpretation. We furthermore extended the discussion towards the EASM, as we believe both mechanisms contributed to enhanced rainfall during the HHP.

This interpretation would be supported by citing prior modeling studies of the HHP to determine whether circulation of this interval might be more conduce to TP events as well as to rule out other mechanisms (which are not much described here) related to groundwater recharge, summertime tropical moisture or winter rainfall associated with a more southward jet stream during a NH cold event at 8.2ka. This is where being very clear about the interpretations of the rainfall seasonality at each point would be very useful; however, since the TPs deliver moisture during the winter, it would also be useful to discuss how you can distinguish winter TP moisture from winter jet stream moisture.

→ We agree that, in addition to tone down the TP interpretation (and which we still believe has played a significant role in our short HHP), there is a need to discuss competing mechanisms, that have been proposed before. This is, in particular, contribution of the EASM, for which a further northeast penetration into the Arabian Peninsula during the early Holocene has been demonstrated by different climate models (e.g. Kutzbach & Liu, 1997; Jennings et al., 2015; Guagnin et al., 2016; Claussen et al., 2017).
Unfortunately, we cannot clearly identify the main season of rainfall or the source, based on the isotope data (see last part of the Methods section). On the one hand, the evaporation line calculated for our heavily δD-depleted isotopic signature for both, modern $\delta^{18}O_{water}$ and fossil $\delta^{18}O_{carb}$ rather fits the African monsoonal isotopic data reported from Khartoum (Sudan), on the other hand the enhanced seasonality identified for phase III based on combined $\delta^{18}O$ and δD evidence and the clear distinction of annual varves (season of surface discharge vs. season of evaporite[aragonite] formation) may indicate winter rainfall. Technically, only based on the isotope data, we cannot distinguish between Mediterranean winter rains or TP. But the fact that, in contrast to Tayma, the 8.2 kyr dry anomaly has been identified at sites fed by Mediterranean winter rains, we suggest the regionally confined TPs. Furthermore, climate models suggest that winter rains in Northern Arabia during the HHP were even less intense than today (Guagnin et al., 2016).
We extended the discussion in this regard. A definite answer cannot be given yet, though.

Line 161-162: This is an example where I think "prove" is not quite the right word choice given uncertainties. Also, the duration for the HHP cited here is constrained by the varved portion of the record. Since the controls on varve formation are not talked about here, what other constraints on lamination formation, like wind or temperature or water chemistry, play into this determination of only linking to moisture? Its also unclear to me what this means with respect to rainfall vs groundwater discharge, since HHP peak should imply rainfall is high.

→ We agree and reworded to tone down the strength off the interpretation. Varve formation is one indicator of more humid conditions, along with other lithological features, TOC, XRF and isotope data.
More explanation and referencing on varve formation have been added to the chapter "Evolution of the Tayma palaeolake", see also response above.
We are not able to disentangle groundwater and surface discharge signals, but there is evidence for both to have supplied more water to the basin during the HHP. Increased fluvial discharge is indicated by the clastic part of the varves that in some cases shows normal grading, i.e. wadis delivering sediment into the water body, where the clastic sediment settles from suspension (Ginau et al., 2012). The underlying groundwater pillow was necessary to

sustain the lake all year around, but its exact contribution to the lake for the HHP and the changes compared to the previous period are extremely difficult to quantify.

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21st Dec 21

Dear Dr Engel,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "The unexpectedly short Holocene Humid Period in Northern Arabia". It has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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https://www.nature.com/commsenv/
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors present a well justified rebuttal and have addressed my concerns with the original manuscript through their changes. I now recommend this for publication.

Reviewer #2 (Remarks to the Author):

The authors tried to address all comments and suggestions made by all three reviewers, including my main concerns regarding the interpretation of oxygen isotope records in the Eastern Mediterranean. In my opinion, the authors were able to address all points and the revised version is suitable for publication. I am confident that this study will make an important contribution as it presents the first high-resolution multi-proxy paleoenvironmental record in this region. This is a truly unique record from a key-area.

Reviewer #3 (Remarks to the Author):

The description of measurements, site, and context is much improved; however, the mechanistic interpretation is still unclear. Additionally, a few other small changes are suggested.

Minor comments:
Line 64: this abbreviation (EASM) is commonly used for the East Asian Monsoon System which could

be very confusing for keyword searches.
Line 106: does the calibration error also factor into this uncertainty?
Supplement: radiocarbon calibration says Intcal 13, should be IntCal20 now.

Interpretation:
Lines 225-228 need a reference, how do we know that “cool poles – dry tropics” lead to southwards penetrating troughs?
Paragraph beginning 232: the map in Figure 1 shows all of these features as separate feature (EASM, TPs, jets, etc), and the previous paragraph makes an argument for a TP mechanism for more winter moisture. I am then not following this additional paragraph. Perhaps I don’t understand because it is not clear to me how the EASM is related to TPs and it would intuitively seem like EASM moisture should be summer (since it’s a monsoon) rather than winter. The EASM is also described as “weakened” in line 241, which seems contradictory. I am unsure if multiple mechanisms are being proposed to explain increases locally in seasonal moisture in both winter and summer, or if the authors are making a link between TPs and EASM. More clarity is needed in this interpretation.

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Reviewer #3 (Remarks to the Author):

The description of measurements, site, and context is much improved; however, the mechanistic interpretation is still unclear. Additionally, a few other small changes are suggested.

→ We tried to make the climatological interpretation and causal relationships regarding the atmospheric processes clearer in the revised version.

Minor comments:

Line 64: this abbreviation (EASM) is commonly used for the East Asian Monsoon System which could be very confusing for keyword searches.

→ We agree and changed to African Summer Monsoon in the text and Fig. 1, as in many other published studies, and changed the abbreviation to ASM.

Line 106: does the calibration error also factor into this uncertainty?

→ Yes, it does, as mentioned in brackets. We clarify and put “calibration” instead of “measurement”.

Supplement: radiocarbon calibration says Intcal 13, should be IntCal20 now.

→ Yes, the age model was constructed using IntCal13, at a time when IntCal20 was not yet available.

Interpretation:

Lines 225-228 need a reference, how do we know that “cool poles – dry tropics” lead to southwards penetrating troughs?

→ There is a reference given [21; Tubi et al., 2017, *Q. J. Roy. Meteorol. Soc.*].

Paragraph beginning 232: the map in Figure 1 shows all of these features as separate feature (EASM, TPs, jets, etc), and the previous paragraph makes an argument for a TP mechanism for more winter moisture. I am then not following this additional paragraph. Perhaps I don’t understand because it is

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→ We agree that the structure of the Discussion is not entirely straightforward. We reorganised. Now, first the processes during the unexpectedly short HHP at Tayma/NW Arabia are discussed, mostly narrowed down to intensification and additional rainfall contribution of the ASM. Then we discuss the possible, local contribution of TPs to compensate for the temporarily weakened summer monsoons around the 8.2 kyr time window in NW Arabia. Both processes are not directly related. We hope this is clearer now.