nature portfolio

Peer Review File

Reversed latitude dependence on the cyclicity of the Quaternary East-Southeast Asian hydroclimate

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

Parts of this Peer Review File have been redacted as indicated to remove third-party material where no permission to publish could be obtained.

26th May 22

Dear Dr Zhao,

Your manuscript titled "Reversed latitude dependence on the cyclicality of the Quaternary East-Southeast Asian hydroclimate" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

In the following, we list our main editorial thresholds:
• Demonstrate that the conclusion pertaining to the latitude-dependence of cyclicity in precipitation is robust taking into account the uncertainties outlined by reviewers.
• Provide strong justifications to support the application of K/Al ratio as a precipitation proxy at site U1429.
• Provide more details on the transient model set-up and more in-depth analysis of its output.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will 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.

We are committed to providing a fair and constructive peer-review process. Please do not 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 reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any 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 **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Sze Ling Ho, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-4898-9036

Joe Aslin
Senior Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

In this manuscript Zhao et al. present a weathering intensity reconstruction from the South China Sea and explore the relative importance of glacial-interglacial vs. precessional variability in the Maritime Continent, southeast Asia and East Asia in both data and model simulations. The weathering intensity reconstruction shows strong precessional variability with minimal glacial-interglacial variability; based on the authors' conclusion that the sediments are derived from the Yellow River, they interpret these data as reflecting a dominant precessional control on precipitation in Northern China. The authors then go on to highlight the relatively greater influence of glacial-interglacial cycles on precipitation in southern China and the Maritime Continent. For southern China, they relate this difference to the importance of spring and fall rainfall, and suggest that the amount of spring and fall rainfall increases during glacial periods.

This manuscript contains several distinct elements—the weathering reconstruction, the data comparison across eastern Asia and the Maritime Continent, and model analysis. Each element on its own is quite interesting, but in trying to do all three, I am concerned that the paper does not cover them in sufficient depth. I'll offer some specific thoughts below, but my overall recommendation is that this paper be split into two or more different papers to offer robust discussion of each topic, and that the authors restrict their focus to northern vs. southern China rather than trying to also cover southeast Asia and the Maritime Continent.

For the weathering reconstruction, the clear dominance of precessional variability over glacial-interglacial variability is an important new finding. There are many reasons that you could imagine glacial-interglacial variability being expressed in this record—changes in vegetation density and thus weathering intensity in soils due to pCO2 changes; changes in sediment delivery due to sea level changes; changes in sediment supply due to sediment mobilization on the loess plateau and other deserts—but remarkably there is very little glacial-interglacial variability, even in the raw data. That said, I found myself wanting substantially more detail about the record. For instance, I would like to know why the K/Al range in the sediment core does not overlap with the Yellow River sediments studied for the modern calibration. Also related to the modern calibration, I would like to see multivariate regression rather than two individual regressions for temperature and precipitation--if temperature correlates with precipitation and roughly the same variance in K/Al is explained by each variable, then it will not be possible to remove the effects of temperature. I would also like to see justification about using model output rather than data to estimate glacial-interglacial temperature changes in the region.

The data compilation also needs more detail. I think it is too much to try to include data from the Maritime Continent all the way up to northern China; it would be much clearer to just focus on southern vs. northern China. It is also confusing to be comparing records that have such varied relationships to local precipitation—precipitation isotope records may reflect upstream rainout or moisture source, speleothem trace element records may reflect soil moisture rather the precipitation amount, and other records may reflect different aspects of hydroclimate (e.g., P-E) rather than strictly precipitation amount. Very often I wanted more detail about the proxy records being discussed, and I also wanted more detailed criteria for choosing the particular records the authors focus on.

Lastly, the discussion of model output and dynamics also needs much more detail. While the authors talk about changing winds and moisture sources, they do not examine winds or water vapor fluxes in the model output, focusing only on precipitation amount in different seasons. The authors also link the relatively wetter glacials in southern China to prolonged spring conditions due to a delayed northward progression of the westerly jet, connecting to the work of Chiang et al.—but they do not note or try to resolve the clear contradiction here, that a shorter summer rainfall season should lead to glacial drying in northern China, but this is not observed. Put another way, what do the models tell us about why northern China doesn't experience glacial-interglacial precipitation variations, despite the glacial-interglacial variations in westerly jet position and moisture supply from the Pacific that the authors discuss?

I thus find much of this paper to be very interesting, and I applaud the breadth of the authors' work, but I find this paper to lack depth in a number of key areas, and I don't think it rises to the level I would expect of this journal.

Smaller details (by line):

Title, other places Change "cyclicality" to "cyclicity"
39-46 This lays out the central problem the paper is addressing, and it should be much clearer.
82, other places Change "liner" to "linear"
130-131 Where is the support for the statement that shelf exposure is an important control on precipitation isotopes in the loess plateau region?
148-154 I got lost in this long sentence.
171-172 "relatively little…rainfall fell" when? The LGM or 10 ka?
224-225 Isn't the shelf exposure hypothesis the same as "climatic responses to sea level changes"?
286 I assume the rate of change of orbital and GHG parameters was multiplied by 100, not the orbital parameters and GHG levels themselves.
Supplementary material The text here has more grammatical and word choice errors and needs more careful editing.

Reviewer #2 (Remarks to the Author):

Comments on Debo Zhao et al.

This is an interesting paper because of the following reasons. First, the authors found strong precession cycles in the variation of chemical weathering degree in the Yellow River drainage basin, which is different from the glacial-interglacial pattern seen in the Loess records. Second, the results of the authors' transient simulation of precipitation and precipitated water isotopes can explain the precipitation records from north China and the Cave isotope records from south China, potentially solving the debated issues raised in the paleo-monsoon community. Thus, this paper is worthy of publication if the interpretations of the K/Al proxy and transient simulation are well documented.

I have major questions below.
The authors propose that the K/Al ratio reflects surface soil's average degree of chemical weathering in the Yellow River basin. The ratio shows linear regressions with temperature and precipitation in the modern sample set, reflecting summer monsoon precipitation. The ratio in Site U1429 shows a strong precession cycle, which contrasts with the glacial-interglacial pattern of the soil-loess sequence in the Chinese Loess Plateau. The authors interpreted that the signals of the soil-loess sequence were influenced by changing dust input

(greater input in glacials), suggesting that their K/Al record is a real precipitation record. However, the dust input to the Yellow River basin may have affected the K/Al ratio by increasing fresher dust to surface soil(?) If this is true, MagSus in the CLP and the K/Al ratio in the Yellow River basin should vary parallel. But, in fact, the K/Al ratio at the East China Sea Site U1429 shows a different pattern. I request the authors to explain this difference.

The clay fraction in Site U1429 is delivered by riverine discharge and aeolian transport. Aeolian transport may bring clays from further drier areas such as the Taklimakan and Gobi deserts. The contribution of such deserts expectedly alters the K/Al signal. Japan Sea records suggest the position of the westerly jet varied in response to precession (e.g., Nagashima et al., 2007 P-3).

The authors' transient simulation results are very interesting for paleoclimatologists because the results can contribute to the interpretations of proxy records being debated intensively. Because of its importance, this paper should be reviewed critically by skilled modelers. I request the authors to interpret the model results in more detail. For example, explain what kinds of physical mechanisms generate the spatial patterns of precipitation in the same way as in modeling papers. Figs 1B, C, and A show very interesting results. Readers want to know the interpretation of the spatial distribution and its significance. Why is the response so spatially heterogeneous? This perspective is quite different from that expected before.

Small comments:
Line 82 The high LSR during glacials probably reflects the shorter distance from the coast rather than the physical erosion degree in the Yellow River drainage basin.
Fig. 1 Was the sea level change (shelf exposure) included in the calculation? Did strong eccentricity cycles in Borneo derive from shelf exposure?
Fig. 2 Describe the method of spectral analysis.
Fig. 2 Indicate the correlation coefficients between proxy and model records.

Reviewer #3 (Remarks to the Author):

Zhao et al. aim to address the debate which the precipitation changes in East Asia monsoon region latitude-dependently respond to different external and internal forcings. They reconstruct a rainfall record over the past 400 ky using the proxy of K to Al ratio (K/Al) from a marine sediment core in the northern East China Sea, after removing the temperature effect on K/Al. The authors found a dominant 23-ky cycle in this record, which comes from precession, in comparison with a 100-ky cycle, which results from glacial-interglacial ice volume, dominated in the hydroclimate records of the southern China. They then applied transient climate models (comprising orbital, greenhouse gas, and ice sheet forcings) through the last 300 ky to study the precipitation distribution and variability in northern and southern China. Finally, they suggest that the dominance of the precession cycle in northern China is probably due to enhanced land-sea thermal contrast resulted from surface heating more over lands than over oceans. The manuscript topic is suited for the scope of the journal, but not novel, although it is of interest to seeing more proxy records with 23-ky cycle in mid-high latitudes. Therefore, I would suggest a resubmission after the authors address some major concerns.

1. To my understanding, I would regard the locations of Hulu and Sanbao caves as southern China, not northern China. Since one of the key points of the manuscript is the 23-ky cycle in northern China, in addition to the K/Al-derived precipitation record, I suggest the authors to find out another convinced record. Moreover, in line 93, I am not sure the authors' description of more significant 23-ky cycle in the loess 10Be record.

2. The authors argue that the Yellow River is the major supplier of sediments in the study region even if the distance between the river mouth and deposit site is more than 1000 km apart. The evidence shown in the supplementary fig. S2 is less convincing, so needs more explanation.

3. Lines 116-143: This paragraph is used to discuss the explanation of the depleted δ2H values in southern China and South China Sea during glacial period. I was lost halfway to the end, and therefore recommend to re-structure and re-write this part.

Minor comments along the text

Line 82: …liner?...

Line 119: The meaning of the clause starting with "as well as" is not clear.

Line 143: 11 ka or 10 ka?

Line 162: …11 ka… or 10 ka?

Line 172: …(Fig. 3A, E and I)…

Line 174: …(Fig. 3A and I)…

Line 176ff: This sentence is a bit weird here, since early in the paragraph the authors talk about "full forcing".

Lines 178-180: This sentence is awkward.

Line 183: …liner?...

Line 193: Don't use double negatives.

Line 201: aforementioned rainfall proxies… Delete "aforementioned".

Line 208: …with insolation playing a secondary role. Delete "with insolation playing a secondary role".

Line 521ff: Please define the latitude range of northern, southern China, and southeastern Asia

Line 542: Why does the K/Alrainfall record give a negative value?

Fig. 1: I suggest to color-code all the symbols. For instance, one color for records in northern China and another one for records in the south. The records of Lz908, Chinese loess, and Tengger Desert don't appear in the main text and figures, but only in the supplementary. Therefore, their sites could be removed from Fig. 1.

Fig. 2: (E) The colors of IODP U1429 and ODP 1146 are too similar to be distinguished.
Line 550: …orbital periodicities… Why no obliquity cycle marked?

Fig. S5: I suggest to re-arrange all the records at right-hand panel according to the north-south order, as well as to unify the color of site symbols and records.

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Thanks for these questions. During the silicate weathering processes, weathering rates can be controlled by multiple factors, such as the mineralogy of the rocks exposed, the reactive surface area of these minerals, the supply of water and its residence time in the weathering zone, the abundance of organic acids, and the temperature of soil solutions (e.g., Kump et al., 2000). Crucially, chemical weathering rates can be enhanced by high temperature and humidity (e.g., White and Blum, 1995; West et al., 2005). As a result, orbital scale of chemical weathering rates are believed to have been slower during colder and drier glacial periods compared to warmer, wetter interglacials previously (e.g., Foster and Vance, 2006). But whether chemical weathering dominated by temperature or rainfall is a long-standing debate. Our chemical weathering record suggests a prominent 23-ka cycle even before the calibration. It's easy to exclude the first control of temperature, which exhibits a dominant 100-ka cycle.

Enhanced loess or desert materials supply during glacial periods probably has introduced more low weathered sediments into the Yellow River. Here we analyzed the element composition of sediments from a loess section in Yellow River upper reach (Fig. R1c). We provided a comparison of K/Al ratio between this loess section and U1429 sediments. Relative high K/Al ratios and thus low weathering degree in loess during entire glacial period can be observed (Fig. R1a). It is contrast with the strong precession fluctuation in K/Al ratios of U1429 (Fig. R1b), suggesting the loess sediments experienced strong precession-scale of chemical weathering controlled by rainfall during the transport from Yellow River upper to lower reaches in glacial period. Other factors such as sediment delivery due to sea level changes are also characterized by glacial-interglacial variability. Vegetation density mainly regulates the breakdown and erosion of the rocks, thus probably exerts small effects on the dominant unconsolidated sediments in Yellow River drainage basin.

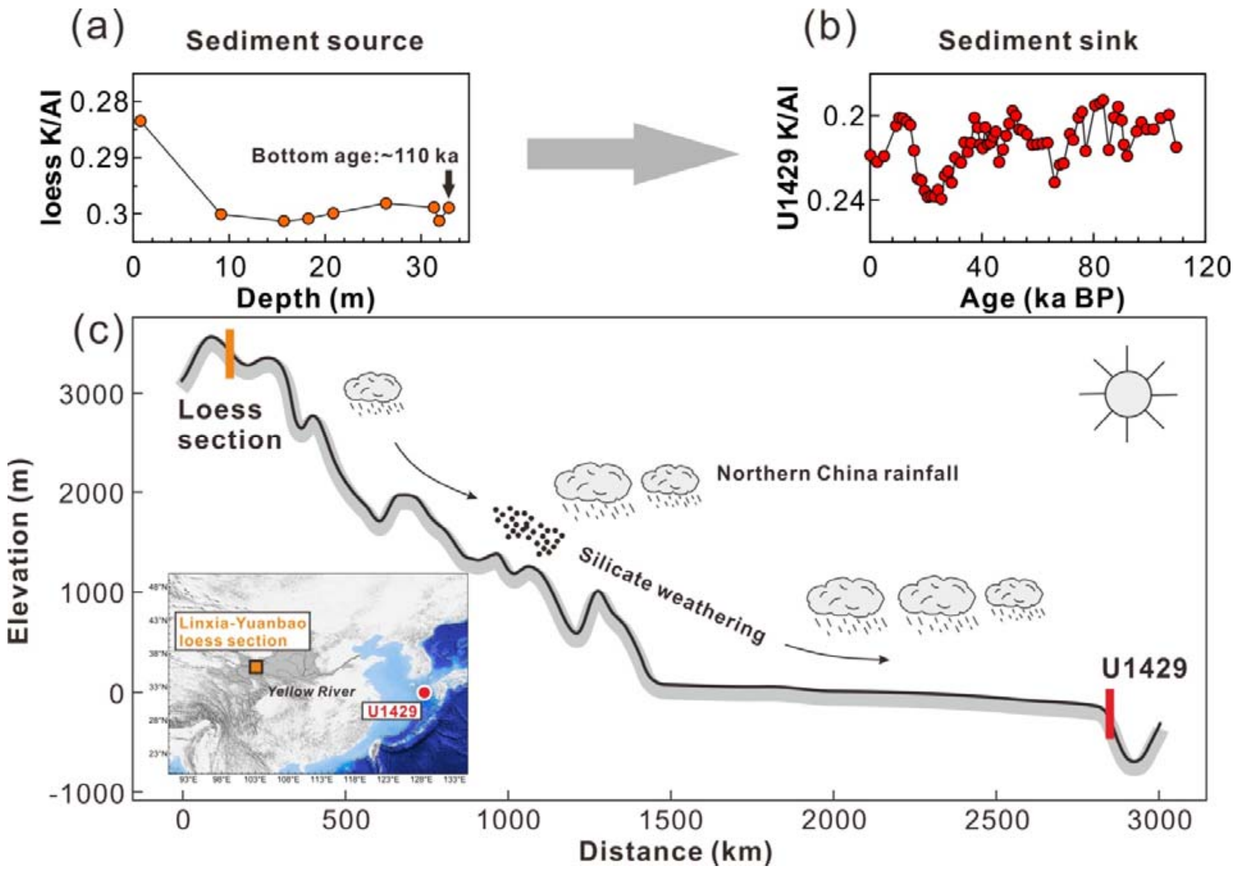

Fig. R1 Comparision of K/Al ratios in clay-sized sediments between loess section (unpublished data) (please see the detailed information of this loess section in Guo et al. (2021)) in Yellow River upper reach and U1429 since the last glaciation. Profile from western Chinese Loess Plateau to northern East China Sea in (c) was generated from GeoMapApp (www.geomapapp.org).

References

Foster, G. L., & Vance, D. (2006). Negligible glacial-interglacial variation in continental chemical weathering rates. Nature, 444(7121), 918-921.

Guo, F., Clemens, S. C., Wang, T., Wang, Y., & Sun, Y. (2020). Monsoon variations inferred from high-resolution geochemical records of the linxia loess/paleosol sequence, western chinese loess plateau. Catena, 198, 105019.

Kump, L. R., Brantley, S. L., & Arthur, M. A. (2000). Chemical weathering, atmospheric CO_2, and climate. Annual Review of Earth & Planetary Sciences, 28(1), 611-667.

West, A. J., Galy, A., & Bickle, M. (2005). Tectonic and climatic controls on silicate weathering. Earth & Planetary Science Letters, 235(1-2), 211-228.

White, A.F. & Blum, A.E. (1995). Effects of climate on chemical_weathering in watersheds. Geochimica et Cosmochimica Acta, 59(9), 1729-1747.

That said, I found myself wanting substantially more detail about the record. For instance, I would like to know why the K/Al range in the sediment core does not overlap with the Yellow River sediments studied for the modern calibration.

Such difference of K/Al ratio largely depends on the seasonal difference of chemical composition in river sediments. The Yellow River bed sediment samples in this manuscript were collected during the dry season of spring. The spring low rainfall amount and temperature (figure S1 in supplementary materials) resulted in low weathering degree of river sediments, and thus relatively higher K/Al ratio. In contrast, the U1429 sediments are the mixture of sediments with relatively high and low weathering degree supplied from all the seasons. Therefore this probably leads to relatively higher weathering degree and lower K/Al ratio than the spring river sediments.

In the first version of manuscript, the K/Al ratio of Yellow River bed sediments shown in figure S1 (in supplementary materials) was calculated based on the oxide contents of K_2O and Al_2O_3 directly. However, the K/Al ratio of U1429 shown in figure S3 (in supplementary materials) was calculated based on the element K and Al contents. To keep it consistent, we re-calculated the K/Al ratio of Yellow River bed sediments with element contents in this revised manuscript. Generally, K/Al ratios range from 0.25-0.29 for Yellow River sediments, and 0.19-0.24 for U1429 sediments.

In this work, our river sediments were collected in a single season. We can't make comparison for their chemical composition among different seasons. However, the advantage is that these samples from Yellow River upper to lower reaches are distributed in different climate zones. The temperature and rainfall increase synchronously from the upper to lower reaches of this river. Thus we can check the response of weathering degree of each sample to mean annual rainfall and temperature recorded by nearby meteorological station. Please note that our regression cannot make a quantitative calculation of rainfall changes, but try to eliminate the temperature effect on the sediment chemical weathering, and obtain a rainfall variation "trend". Despite the absolute values of sediment K/Al ratio of U1429 and Yellow River are different, it will not influence our trend analysis in this work.

Also related to the modern calibration, I would like to see multivariate regression rather than two individual regressions for temperature and precipitation, if temperature correlates with precipitation and roughly the same variance in K/Al is explained by each variable, then it will not be possible to remove the effects of temperature.

Linear correlation analysis between temperature and precipitation suggests they are highly correlated (figure. S1D in supplementary materials). Here we use "t" and "p" to represent temperature and precipitation, respectively. Thus their regression equation of temperature and precipitation is:

$t = 0.016p + 4.30$

Regression equation of K/Al and temperature is:

$Y_{K/Al-t} = -0.004t + 0.32 = -0.004\ (0.016p + 4.30) + 0.32 = -0.000064p + 0.148$

It is different with the regression equation of K/Al and precipitation:

$Y_{K/Al-p} = -0.00007p + 0.30$

Therefore we can use these equations to remove the temperature effect in K/Al.

I would also like to see justification about using model output rather than data to estimate glacial-interglacial temperature changes in the region.

This annual mean temperature used in proxy calibration is derived from HadCM3 model of Sun et al. (2019). Here, we show the annual mean temperature of eastern China with our 300-ka simulation based on NCAR-CCSM3; strong glacial-interglacial changes can also be identified (Fig. R2). However, we still use the HadCM3 model results to calibrate our 400-ka weathering record, because the age limitation (lacking 400 to 300-ka) of our simulation.

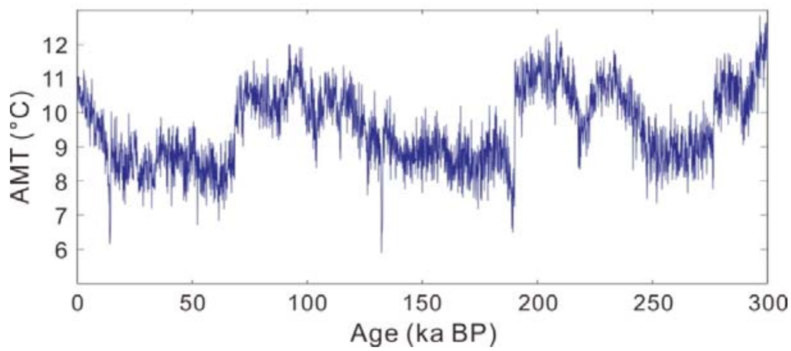

Fig. R2 Simulated annual mean temperature of eastern China (20-40°N, 110-120°E) during the last 300 ka.

<u>References</u>

Sun, Y., Yin, Q., Crucifix, M. et al. (2019) Diverse manifestations of the mid-Pleistocene climate transition. Nature Communications, 10, 352.

The data compilation also needs more detail. I think it is too much to try to include data from the Maritime Continent all the way up to northern China; it would be much clearer to just focus on southern vs. northern China.

Thank you very much for this kind suggestion. We recognized that rainfall discussion focuses on southern and northern China will be more straightforward, but we didn't remove the Maritime Continent part due to the following reasons. Firstly, our main purpose is to reveal the different orbital cycles of rainfall from mid-latitudes to tropical regions, and how they respond to high- and low-latitude forcing interactions. If we only focus on the southern and northern China, tropical rainfall response to high-latitude forcing will be ignored. Secondly, rainfall variation in Maritime Continent is a nice contrast to southern China, even they are all largely controlled by ice sheet forcing and have strong glacial-interglacial cycles; their anti-phased rainfall changes during glacial period highlight their different response to single forcing mechanism. Thus we didn't remove the Maritime Continent part after careful consideration. In the revised manuscript, more details of proxies and forcing dynamics have been added to make it more in-depth.

It is also confusing to be comparing records that have such varied relationships to local precipitation—precipitation isotope records may reflect upstream rainout or moisture source, speleothem trace element records may reflect soil moisture rather the precipitation amount, and other records may reflect different aspects of hydroclimate (e.g., P-E) rather than strictly precipitation amount. Very often I wanted more detail about the proxy records being discussed, and I also wanted more detailed criteria for choosing the particular records the authors focus on.

We provide more details about the referred rainfall proxy records for southern China-South China Sea and western Maritime Continent in the revised manuscript. Please see "Results" section in lines 75-180 in the revised manuscript. We show the details about proxy records below as well.

Southern China:

Yangtze River runoff proxy (U1429 seawater $\delta^{18}O$):

The northeastern East China Sea surface salinity can be strongly influenced by the Yangtze River diluted water discharge. Modern rainfall in the Yangtze River Valley is highly correlated to salinity at northeastern East China Sea (Clemens et al., 2018). Thus local seawater $\delta^{18}O$ (the parameter varies linearly with salinity) has been used to reconstruct the Yangtze River Valley rainfall changes (e.g., Clemens et al., 2018; Kubota et al., 2015). In Clemens et al. (2018)'s article, they argued that the impact of evaporation on the surface salinity in northeastern East China Sea is minimal because precipitation plus runoff dominates over evaporation.

South China Sea rainfall proxy (251PC seawater $\delta^{18}O$):

This sea surface salinity record has been used as a rainfall indicator in Zhou et al. (2021). They attributed sea surface salinity decreases during last glacial period to the enhanced monsoon and convective activities, resulting in extreme tropical precipitation. Variations in this sea surface salinity record show high consistency with U1429 seawater $\delta^{18}O$ in East China Sea. This should be not a coincidence but most likely implies the response of them to local rainfall variations.

Southern China rainfall isotope record (ODP 1146 leaf wax δ^2H):

This record was proposed by Thomas et al. (2014). They only focused on the precession-scale of δ^2H variation and attributed depleted δ^2H to the precipitation derived from depleted maritime water vapor in spring, autumn and winter, and/or the longer transport paths of water vapor under strong hemispheric insolation contrasts. Here our iTraCE simulation results suggest that positive annual, JJA, spring and autumn rainfall anomalies in southern China during glacial period corresponding to negative $\delta^{18}O$ loadings (Figure S4 in manuscript), suggesting the rainfall isotope in southern China probably can be largely interpreted as rainfall amount.

The maritime water vapor supply probably also plays a role in δ^2H variation. However, expansive glacial ice sheets could strengthen the pressure gradient between East Asian continent and Pacific Ocean, as well as enhance the Western North Pacific Subtropical High, and thus increased the amount of moisture advected from nearby South China Sea and West and North Pacific Ocean to East China (Cai et al., 2015). This can also be observed from our new model results of water vapor flux during spring and autumn (Fig. 5 in the revised manuscript or Fig. R4). This maritime water vapor should lead to the enrichment of precipitation isotopes, rather than depleted glacial δ^2H observed in this record. As a result, we proposed that rainfall amount, rather than water vapor supply, is the dominant control of the leaf wax δ^2H in this record.

Speleothem trace element record

In Zhang et al. (2018)'s article, they considered the trace element content is dominated by variations in the chemical composition of the cave drip-water. They mentioned the possible sources of elements from soil but didn't show the detailed discussion. Here we agreed with this comments of soil moisture can also influence the speleothem trace element. The soil moisture can be regulated by multiple factors such as rainfall, temperature and vegetation covers. Thus we remove this record in fig. S5 for prudential reasons.

Maritime Continent

Borneo stalagmite $\delta^{18}O$

Borneo stalagmite $\delta^{18}O$ record exhibits strong 100-ka cycles. Our annual and JJA simulation between 20 ka and 10 ka suggest positive $\delta^{18}O$ loading corresponding to decreased rainfall in Sumatra and southern Borneo; however, most regions of Sumatra and Borneo all suggest decreased spring and autumn rainfall corresponding to negative $\delta^{18}O$. This simulated rainfall-isotope patterns have also been found by Windler et al. (2020). They attributed the rainfall isotope changes to a result of regional increases in moisture convergence in the low-to-middle troposphere. Carolin et al. (2016) interpreted the observed glacial-interglacial cycles in Borneo stalagmite $\delta^{18}O$ as the global ocean $\delta^{18}O$ variance with glaciation. There are also some works interpreted rainfall isotope records in surrounding regions as both effects of rainfall amount and water vapor pathway (e.g., Parker et al., 2021; Wurtzel et al., 2018). We can't really constrain the response of rainfall isotopes clearly with existing data and model results. Thus here we remove this record for prudential reasons.

MD98-2152 $\delta^{13}C_{wax}$ record

Variations in this leaf wax $\delta^{13}C$ record mainly depend on the transition between C_3 and C_4 plant in Sumatra (Windler et al., 2019). C_4 plants are more tolerant of high temperatures and arid climates than C_3 plants, so they are often utilized as an indicator of past hydroclimate. For instance, Borneo is wet year-round, and dominated by C_3 tropical rain forests and so ocean sediments in this region have $\delta^{13}C$ wax values of approximately -32.5‰. In contrast, northwest Australia, with an extreme dry season from April to November (large seasonality), hosts a large expanse of C_4 grasses, and ocean sediments offshore have $\delta^{13}C$ wax values of approximately -25‰ (Dubois et al., 2014). Therefore, in Windler and some other's works in this region, leaf wax $\delta^{13}C$ proxy has been used to reconstruct the local rainfall seasonality. Enriched $\delta^{13}C$ values during glacial periods indicate more C_4 plants and higher rainfall seasonality in Sumatra; this also suggests more dry seasons occurred in a year-round. This is consistent with our annual rainfall simulation results in this region (Fig. 2H in the revised manuscript).

References

Cai, Y., Fung, I. Y., Edwards, R. L., An, Z., Cheng, H. & Lee, J., et al., Variability of stalagmite-inferred Indian monsoon precipitation over the past 252,000 y. Proc Natl Acad Sci U S A, 10(112), 2954-2959.

Carolin, S. A., Cobb, K. M., Lynch-Stieglitz, J., Moerman, J. W., Partin, J. W., & Lejau, S., et al. (2016). Northern borneo stalagmite records reveal west pacific hydroclimate across mis 5 and 6. Earth and Planetary Science Letters. 439, 182-193.

Clemens, S. C., Holbourn, A., Kubota, Y., Lee, K. E., Liu, Z., & Chen, G., et al. (2018). Precession-band variance missing from east asian monsoon runoff. Nature Communications, 9(1), 3364.

Dubois, N., Oppo, D. W., Galy, V. V., Mohtadi, M., Sander, V., & Tierney, J. E., et al. (2014). Indonesian vegetation response to changes in rainfall seasonality over the past 25,000 years. Nature Geoscience, 7(7), 513-517.

Kubota, Y., Tada, R., & Kimoto, K. (2015) Changes in East Asian summer monsoon precipitation during the Holocene deduced from a freshwater flux reconstruction of the Changjiang (Yangtze River) based on the oxygen isotope mass balance in the northern East China Sea. Clim. Past, 11, 265-281.

Parker, S. E., Harrison, S. P., Comas-Bru, L., Kaushal, N., Legrande, A. N., & Werner, M. (2021). A data-model approach to interpreting speleothem oxygen isotope records from monsoon regions. Climate of the Past(3).

Thomas, E. K., Clemens, S.C., Prell, W. L., Herbert, T. D., Huang, Y., & Liu, Z., et al. (2014). Temperature and leaf wax 2h records demonstrate seasonal and regional controls on asian monsoon proxies. Geology, 42(12), 1075-1078.

Windler, G., Tierney, J. E., Dinezio, P. N., Gibson, K., & Thunell, R. (2019). Shelf exposure influence on indo-pacific warm pool climate for the last 450,000 years. Earth and Planetary Science Letters, 516, 66-76.

Windler, G., Tierney, J. E., Zhu, J., & Poulsen, C. J. (2020). Unraveling glacial hydroclimate in the indo cc acific warm pool: perspectives from water isotopes. Paleoceanography and Paleoclimatology, 35(12).

Wurtzel, J. B., Abram, N. J., Lewis, S. C., Bajo, P., Hellstrom, J. C., & Troitzsch, U., et al. (2018). Tropical indo-pacific hydroclimate response to north atlantic forcing during the last deglaciation as recorded by a speleothem from sumatra, indonesia. Earth & Planetary Science Letters, 492, 264-278.

Zhang, H., Griffiths, M.L., Chiang, J.C.H., Kong, W., Wu, S., Atwood, A., Huang, J., Cheng, H., Ning, Y., Xie, S. (2018). East Asian hydroclimate modulated by the position of the westerlies during Termination I. Science, 362(6414), 580-583.

Zhou, Q., Yin, J., Yang, X., Chen, Q., & Zhang, H. (2021). Planktic foraminiferal $\delta^{18}O$ values indicate precipitation variability in the southeastern south china sea over the last 175 ka BP. Quaternary Science Reviews, 253(1), 106745.

Lastly, the discussion of model output and dynamics also needs much more detail. While the authors talk about changing winds and moisture sources, they do not examine winds or water vapor fluxes in the model output, focusing only on precipitation amount in different seasons. The authors also link the relatively wetter glacials in southern China to prolonged spring conditions due to a delayed northward progression of the westerly jet, connecting to the work of Chiang et al.—but they do not note or try to resolve the clear contradiction here, that a shorter summer rainfall season should lead to glacial drying in northern China, but this is not observed. Put another way, what do the models tell us about why northern China doesn't experience glacial-interglacial precipitation variations, despite the glacial-interglacial variations in westerly jet position and moisture supply from the Pacific that the authors discuss?

Thanks for this thoughtful comment. We added more details of the model set-up and model validation in terms of simulated monsoon rainfall evolution. The analysis for the forcing mechanisms has been expanded and included the vertically integrated water vapor flux, which more clearly presents how the motion of moisture and it's converging/diverging for the whole atmosphere column, and can be related to precipitation patterns.

Our reconstructed northern China rainfall record shows highly consistent trend with Chinese cave stalagmite $\delta^{18}O$ record, which has been considered as an indicator of summer monsoon intensity and is driven by upstream depletion of moisture in an increasing number of works based on data-model approaches in recent years (e.g., Liu et al., 2014; Wen et al., 2016; He et al., 2021; Cheng et al., 2021; Zhang et al., 2021). This suggests the linear response of rainfall in northern China to East Asian summer monsoon wind intensity across the glacial-interglacial cycles. Their relationship can also be observed in the modern hydroclimate, which shows high correlation coefficients between northern China rainfall and monsoonal wind intensity (e.g., Liu et al., 2014; Sun et al., 2015). Our simulated rainfall and moisture flux differences between 56 ka (a high insolation stage in glacial period) and present show stronger JJA rainfall and moisture transport to the northern China, providing a strong support of summer monsoon winds could still penetrate northern China and bring rainfall deep inland during high insolation stage in the glaciation, thus suggesting its insolation control (Fig. R3B).

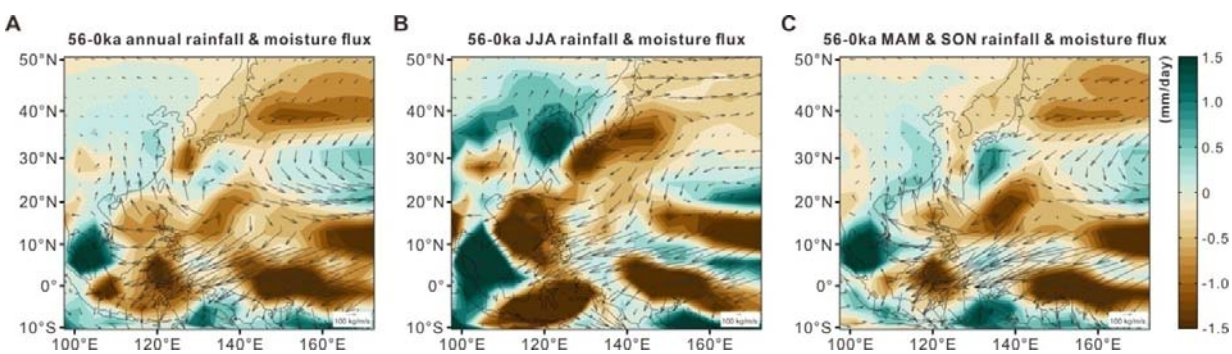

Fig. R3 Full forcing simulation of rainfall and moisture flux difference between 56 ka and present.

For the southern China, we found dominant 100-ka cycles in annual rainfall but 23-ka cycles in summer rainfall. Thus we considered the dominant 100-ka cycle in annual rainfall should largely depend on the intervention of spring and autumn rainfall. During glacial periods, the expansion of high-latitude ice sheets intensified the Siberian High, resulting in anomalous northwesterly winds, which brought cold and dry air to southern China and the South China Sea and enhanced convergence with warm-wet air masses transported by southwesterly winds in the spring and autumn months, thus strengthening rainfall in these regions (Fig. R4C).

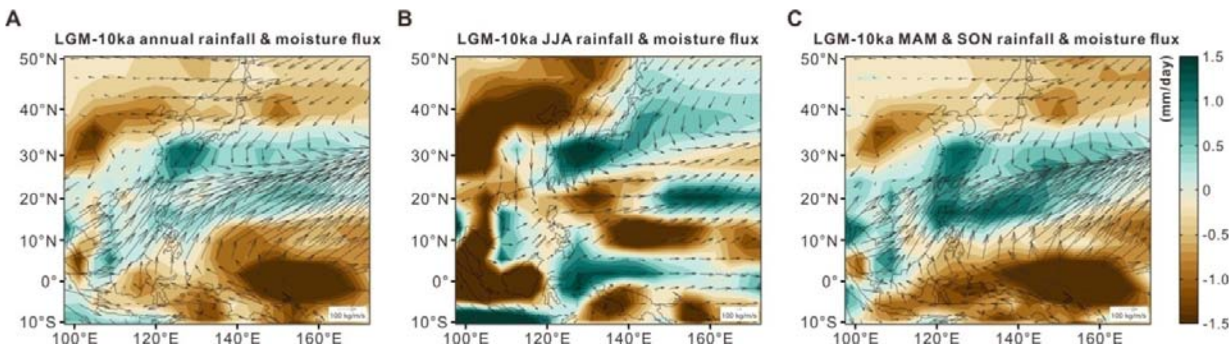

Fig. R4 Full forcing simulation of rainfall and moisture flux difference between LGM and 10 ka.

Please see the section of "Discussion" in lines 181-255 in the revised manuscript.

References

Cheng, H., Zhang, H., Cai, Y., Shi, Z., Liang, Y. I., & Deng, C., et al. (2021). Orbital-scale asian summer monsoon variations:paradox and exploration. Science China Earth Sciences 64, 529-544.

He, C., Liu, Z., Otto-Bliesner, B. L., Brady, E., & Bao, Y. (2021). Hydroclimate footprint of pan-asian monsoon water isotope during the last deglaciation. Science Advances, 7, eabe2611.

Liu, Z., Wen, X., Brady, E. C., Otto-Bliesner, B., Yu, G., & Lu, H., et al. (2014). Chinese cave records and the east asia summer monsoon. Quaternary Science Reviews, 83, 115-128.

Sun, Y., Kutzbach, J., An, Z., Clemens, S., Liu, Z., Liu, W., et al. (2015). Astronomical and glacial forcing of East Asian summer monsoon variability. Quaternary Science Reviews,115, 132-142.

Wen, X., Liu, Z., Wang, S. et al. (2016). Correlation and anti-correlation of the East Asian summer and winter monsoons during the last 21,000 years. Nature Communications, 7, 11999.

Zhang, H., Zhang, X., Cai, Y., Sinha, A., Christoph Spötl, & Baker, J., et al. (2021). A data-model comparison pinpoints holocene spatiotemporal pattern of east asian summer monsoon. Quaternary Science Reviews, 261(3), 106911.

I thus find much of this paper to be very interesting, and I applaud the breadth of the authors' work, but I find this paper to lack depth in a number of key areas, and I don't think it rises to the level I would expect of this journal.

Smaller details (by line):

Title, other places Change “cyclicality” to “cyclicity”

Changed. Please see the revised manuscript.

39-46 This lays out the central problem the paper is addressing, and it should be much clearer.

Details have been added. Please see lines 41-56 in the revised manuscript.

82, other places Change “liner” to “linear”

Changed. Please see the revised manuscript.

130-131 Where is the support for the statement that shelf exposure is an important control on precipitation isotopes in the loess plateau region?

Here we use rainfall isotopes in Chinese Loess Plateau to rebut the view of shelf exposure effect on rainfall isotopes proposed by Clemens et al. (2018). They attributed the glacial depleted δ^2H in southern China to the long moisture transport path over the emergent continental shelf. If this is true, other δ^2H records in northern China (e.g., in loess plateau region) should also have such depleted values and strong glacial-interglacial variations. However, the Loess Plateau δ^2H does not exhibit dominant glacial-interglacial cycles and relatively light isotope values during glacial periods. Thus the shelf exposure effect can be excluded. We realized that this negative example in the far northern regions might give rise to misleading. We removed this record in the revised manuscript.

148-154 I got lost in this long sentence.

This sentence has been reorganized. Please see lines 154-159 in the revised manuscript.

171-172 “relatively little…rainfall fell” when? The LGM or 10 ka?

The “Discussion” section has been rewritten, please see lines 181-255 in the revised manuscript. The paragraph includes this sentence has been removed.

224-225 Isn’t the shelf exposure hypothesis the same as “climatic responses to sea level changes”?

This section has been reorganized. Please see the section of “Forcing of the dominant glacial-interglacial cyclicity in southeast Asian rainfall changes” in lines 236-255 in the revised manuscript.

286 I assume the rate of change of orbital and GHG parameters was multiplied by 100, not the orbital parameters and GHG levels themselves.

Yes. We have added more details in the section of materials and methods on the model set-up to avoid confusion of the acceleration technique. Please see lines 311-313 in the revised manuscript.

Supplementary material The text here has more grammatical and word choice errors and needs more careful editing.

Grammatical and word choice errors have been edited. Please see the Supplementary materials.

Reviewer #2 (Remarks to the Author):

Comments on Debo Zhao et al.

This is an interesting paper because of the following reasons. First, the authors found strong precession cycles in the variation of chemical weathering degree in the Yellow River drainage basin, which is different from the glacial-interglacial pattern seen in the Loess records. Second, the results of the authors' transient simulation of precipitation and precipitated water isotopes can explain the precipitation records from north China and the Cave isotope records from south China, potentially solving the debated issues raised in the paleo-monsoon community. Thus, this paper is worthy of publication if the interpretations of the K/Al proxy and transient simulation are well documented.

Thanks for these encouraging comments.

I have major questions below.

The authors propose that the K/Al ratio reflects surface soil's average degree of chemical weathering in the Yellow River basin. The ratio shows linear regressions with temperature and precipitation in the modern sample set, reflecting summer monsoon precipitation. The ratio in Site U1429 shows a strong precession cycle, which contrasts with the glacial-interglacial pattern of the soil-loess sequence in the Chinese Loess Plateau. The authors interpreted that the signals of the soil-loess sequence were influenced by changing dust input (greater input in glacials), suggesting that their K/Al record is a real precipitation record. However, the dust input to the Yellow River basin may have affected the K/Al ratio by increasing fresher dust to surface soil(?) If this is true, MagSus in the CLP and the K/Al ratio in the Yellow River basin should vary parallel. But, in fact, the K/Al ratio at the East China Sea Site U1429 shows a different pattern. I request the authors to explain this difference.

Enhanced dust or desert materials supply during glacial periods probably have introduced more low weathered sediments into the Yellow River, and thus affected the K/Al ratio in the upper to middle reaches of Yellow River. However, these low weathered materials should be strongly weathered when they were transported toward the sea, due to intensified rainfall during high insolation stages in glacial periods.

Here we provide a comparison of K/Al ratio between a loess section in Yellow River upper reach and U1429 sediments (Fig. R5). Relative high K/Al ratios and thus low weathering degree in loess during glacial period can be observed (Fig. R5a). It is contrast with the strong precession fluctuation in K/Al of U1429 (Fig. R5b), suggesting the loess sediments experienced strong precession-scale of chemical weathering controlled by rainfall during transport from Yellow River upper to lower reach in glacial period.

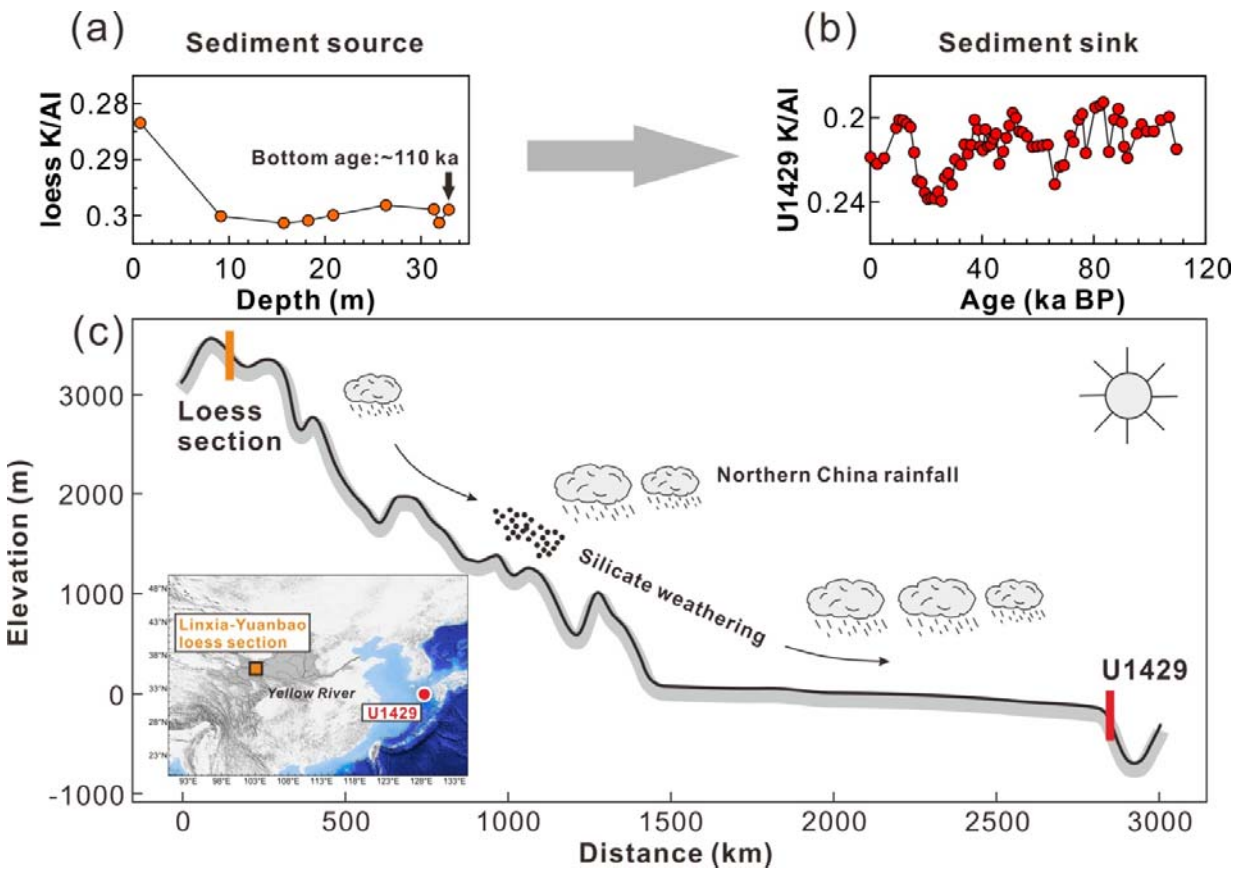

Fig. R5 Comparison of clay-sized sediment K/Al ratios between loess section (unpublished data) (please see the detailed information of this loess section in Guo et al. (2021)) in Yellow River upper reach and U1429 since the last glaciation.

Both our K/Al and corrected K/Al (after eliminated the temperature effects) records shows a strong precession cycle, consistent with the simulated rainfall changes in northern China. This indicates the sensitive response of chemical weathering proxies to rainfall changes in Yellow River basin, particularly in the lower reach of this river.

By contrast, whether MagSus in soil-loess sequence can be used directly to indicate rainfall has been questioned. The pedogenic process occurs mainly in the near surface soil horizon, where soil moisture and temperature are primary factors regulating the generation of magnetically susceptible grains (e.g., Long et al., 2016). The warm/damp environment in near-surface depths of soil is conducive to their generation. Thus it’s difficult to exclude the temperature effects on the MagSus in loess-soil sequence. During glacial period, magnetic minerals formation probably can be largely restrained by low temperature, despite high rainfall amount. Besides, it has also been suggested that MagSus can be strongly affected by the dilution effect of the dust sedimentation rate (e.g., Cheng et al., 2021; Kong et al., 2020). Higher sediment accumulation rates dilute the concentration of magnetic minerals, meaning that two different periods with the same rainfall rate but different sediment accumulation rates will reveal different MagSus. That’s the reasons why the loess-soil MagSus variation only has the dominant 100-ka cycle, which is different with our chemical weathering record of K/Al ratio.

References

Cheng, H., Zhang, H., Cai, Y., Shi, Z., Liang, Y. I., & Deng, C., et al. (2021). Orbital-scale asian summer monsoon variations:paradox and exploration. Science China Earth Sciences 64, 529-544.

Guo, F., Clemens, S. C., Wang, T., Wang, Y., & Sun, Y. (2020). Monsoon variations inferred from high-resolution geochemical records of the linxia loess/paleosol sequence, western chinese loess plateau. Catena, 198, 105019.

Kong, X., Zhou, W., Beck, J. W., Xian, F., & An, Z. (2020). Loess magnetic susceptibility flux: a new proxy of east asian monsoon precipitation. Journal of Asian Earth Sciences, 201(2), 104489.

Long, X., Ji, J., V Barrón, & Torrent, J. (2016). Climatic thresholds for pedogenic iron oxides under aerobic conditions: processes and their significance in paleoclimate reconstruction. Quaternary Science Reviews, 150, 264-277.

The clay fraction in Site U1429 is delivered by riverine discharge and aeolian transport. Aeolian transport may bring clays from further drier areas such as the Taklimakan and Gobi deserts. The contribution of such deserts expectedly alters the K/Al signal. Japan Sea records suggest the position of the westerly jet varied in response to precession (e.g., Nagashima et al., 2007 P-3).

Thanks for this suggestion. However, we argued that eolian dust is not an important component in U1429 clay-sized sediments.

Firstly, the Site U1429 locates in the northern Okinawa Trough, which sediments are mainly supplied from East Asian big rivers and surrounding islands (e.g., Beny et al., 2018; Zhao et al., 2017; 2018; 2019). In contrast, the Japan Sea sediments are mainly transported as eolian dust from the arid central Asia, and a small part is derived from Japanese island and/or Chinese big rivers by the Tsushima Warm Current (Nagashima et al., 2007; Nagashima et al., 2013; Shen et al., 2017). Thus the sedimentation rates of these two areas exhibit large difference. For instance, the IODP Site U1425 locates in the central Japan Sea; it's average sedimentation rate is only about 3.9 cm/ka since ~9.5 Ma (Zhai et al., 2021). This is far less than that at our study site of U1429 (~50 cm/ka). That means eolian dust in our study site is largely overshadowed by such huge riverine sediment input. Thus we considered eolian dust in U1429 sediments is not significant.

Secondly, Nagashima et al. (2007) proposed that the coarser grain-size population (2-15 μm) in sediments of Japan Sea covers the information of eolian dust. Our study site U1429 locates closed to the Japan Sea. Thus the grain-size population of eolian dust in U1429 sediments should also be close to 2-15 μm. However, our K/Al record is analyzed based on the clay-sized sediments (<2 μm). Thus this avoids the influence of eolian dust to our proxy reconstruction.

References

Beny, F., Toucanne, S., Skonieczny, C., Bayon, G., & Ziegler, M. (2018). Geochemical provenance of sediments from the northern east china sea document a gradual migration of the asian monsoon *belt over the past 400,000 years. Quaternary Science Reviews, 190, 161-175.*

Nagashima, K., Tada, R., Tani, A., Toyoda, S., Sun, Y., & Isozaki, Y. (2007). Contribution of aeolian dust in japan sea sediments estimated from esr signal intensity and crystallinity of quartz. Geochemistry, Geophysics, Geosystems, 8(2), Q02Q04.

Nagashima, K., Tada, R., & Toyoda, S. (2014). Westerly jet-east asian summer monsoon connection during the holocene. Geochemistry Geophysics Geosystems, 14(12), 5041-5053.

Shen, X., Wan, S., France-Lanord, C., Clift, P.D., Tada, R. & Révillon, S., et al. (2017). History of Asian eolian input to the Sea of Japan since 15Ma: Links to Tibetan uplift or global cooling? Earth and Planetary Science Letters, 474, 296-308.

Zhai, L., Wan, S., Colin, C., Zhao, D., Ye, Y., Song, Z., et al. (2021). Deep-water formation in the North Pacific during the late Miocene global cooling. Paleoceanography and Paleoclimatology, 36, e2020PA003946.

Zhao, D., Wan, S., Clift, P. D., Tada, R., Huang, J., & Yin, X., et al. (2018). Provenance, sea-level and monsoon climate controls on silicate weathering of yellow river sediment in the northern okinawa trough during late last glaciation. Palaeogeography, Palaeoclimatology, Palaeoecology, 490, 227-239.

Zhao, D., Wan, S., Song, Z., Gong, X., Zhai, L., Shi, X., & Li, A. (2019). Asynchronous variation in the Quaternary East Asian winter monsoon associated with the tropical Pacific ENSO-like system. Geophysical Research Letters, 46, 6955-6963.

Zhao, D., Wan, S., Toucanne, S., Clift, P. D., Tada, R., & S Révillon, et al. (2017). Distinct control mechanism of fine-grained sediments from yellow river and kyushu supply in the northern okinawa trough since the last glacial. Geochemistry Geophysics Geosystems, 18, 2949-2969.

The authors' transient simulation results are very interesting for paleoclimatologists because the results can contribute to the interpretations of proxy records being debated intensively. Because of its importance, this paper should be reviewed critically by skilled modelers. I request the authors to interpret the model results in more detail. For example, explain what kinds of physical mechanisms generate the spatial patterns of precipitation in the same way as in modeling papers. Figs 1B, C, and A show very interesting results. Readers want to know the interpretation of the spatial distribution and its significance. Why is the response so spatially heterogeneous? This perspective is quite different from that expected before.

Thanks for this constructive comment. We added more details of the model set-up and model validation in terms of simulated monsoon rainfall evolution. The analysis for the forcing mechanisms of the heterogeneous response has been expanded and included the vertically integrated water vapor flux, which more clearly presents how the motion of moisture and it's converging/diverging for the whole atmosphere column, and can be related to precipitation patterns.

Generally, spatial patterns of rainfall cyclicity shown in Fig 1B and C suggest high-latitude regions are dominated by strong 100-ka cycles, whereas low-latitude is dominated by 21-ka cycles, indicating they are mainly forced by ice sheet/CO_2 and insolation, respectively. However, just as this comment mentioned, the spatial patterns of rainfall cyclicity are different from that expected before; strong 100-ka cycles have also been observed in low-latitude regions, such as Southeast Asia and eastern Africa, and strong 21-ka cycles have been observed even in the Arctic regions. This probably mainly depends on the complex air-sea interactions under internal and external forcings. For example, model simulations have shown that changes in surface winds, sea surface temperature, and sub-surface temperature in the Indian Ocean play a critical role in amplifying the response to shelf exposure in surrounding Southeast Asia, dramatically altering rainfall patterns over East Africa, the Maritime Continent, and the western Pacific (e.g., DiNezio et al., 2013; 2018; Windler et al., 2019). Thus rainfall changes in these regions suggest strong response to high-latitude ice sheet forcing. The insolation can also regulate climate oscillation in high-latitude regions. A recent study by Barker et al. (2022) found large scale ablation of northern high-latitude ice sheets typically coincided with the increase to maximum summer insolation as a function of precession throughout the past 1.7 Ma, indicating persistent influence of precession on Northern Hemisphere high-latitude climate variability. Thus the observed heterogeneous response of rainfall changes in our model results can also be interpreted as the complex interactions between internal and external forcings.

However, it is unlikely to cover and discuss the rainfall forcing mechanisms on a global scale which is beyond the scope of our manuscript. Our simulation results in Fig 1 can provide a reference for future studies of rainfall variability in other regions or on a global scale. Here, we mainly focused on the cyclicity of rainfall changes in East-Southeast Asia, i.e., the eastern part of Asian monsoon region and surrounding tropical regions (Fig. R6 below or Figure 1B in the revised manuscript). They are typical examples of rainfall responses to the interactions between high- and low-latitude climate, as well as internal and external forcings. We provided the detailed discussion about the heterogeneous rainfall cyclicity in these regions in the revised manuscript. Please see the "Discussion" section lines 181-255 in the revised manuscript.

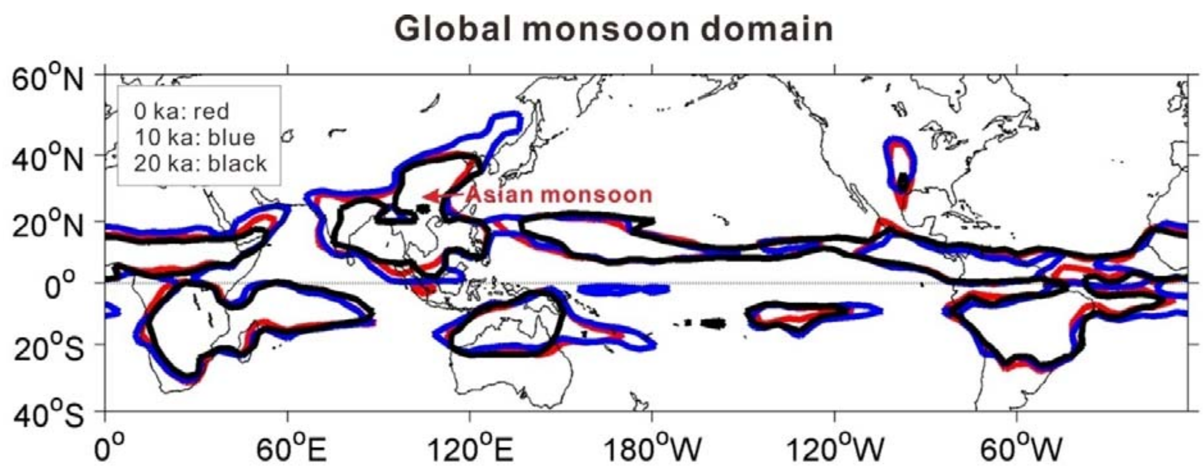

Fig. R6 Simulated global monsoon domain during 20 ka, 10 ka and 0 ka using CCSM3 model. The monsoon domain definition is from Wang and Ding (2008).

References

Barker, S., Starr, A., van der Lubbe, J., Doughty, A., Knorr, G., Conn, S., et al. (2022). Persistent influence of precession on northern ice sheet variability since the early Pleistocene. Science 376 (6596), 961-967.

DiNezio, P.N., Tierney, J.E. (2013). The effect of sea level on glacial Indo-Pacific climate. Nature

Geoscience, 6, 485-491.

DiNezio, P.N., Tierney, J.E., Otto-Bliesner, B.L., Timmermann, A., Bhattacharya, T., Rosenbloom, N., Brady, E. (2018). Glacial changes in tropical climate amplified by the Indian Ocean. Science Advances, 4, eaat9658.

Wang, B., & Ding, Q. (2008). Global monsoon: Dominant mode of annual variation in the tropics. Dynamics of Atmospheres and Oceans, 44(3-4), 165-183.

Windler, G., Tierney, J. E., Dinezio, P. N., Gibson, K., & Thunell, R. (2019). Shelf exposure influence on indo-pacific warm pool climate for the last 450,000 years. Earth and Planetary Science Letters, 516, 66-76.

Small comments:

Line 82 The high LSR during glacials probably reflects the shorter distance from the coast rather than the physical erosion degree in the Yellow River drainage basin.

Thanks for this suggestion. Yes, the high LSR during glacials probably reflects the shorter distance from the coast to study site. Actually, the physical erosion degree of Yellow River sediments largely depends on the river runoff, which is mainly regulated by rainfall in the drainage basin. Enhanced rainfall corresponding to increased erosion degree, and thus can restrain the chemical weathering intensity. However, our decreased K/Al ratio (increased chemical weathering degree) corresponds well with simulated high rainfall amount, suggesting the physical erosion degree is not a significant effect on chemical weathering in the Yellow River drainage basin. We re-interpreted this physical erosion effect. Please see the section of "Reconstruction of rainfall proxy in the Northern China" in Supplementary materials.

Fig. 1 Was the sea level change (shelf exposure) included in the calculation? Did strong eccentricity cycles in Borneo derive from shelf exposure?

The ICE forcing has been included in the simulation. We considered the high-latitude ice volume is the major forcing of global sea level change. Thus this boundary condition in the simulation should cover the effects of sea level change on climate interactions. In our manuscript, we attributed the observed strong eccentricity cycles in Southeast Asia to changes of moisture supply and air convergence/divergence in response to the ice sheet expansion and shelf exposure induced by sea level change. Please see the section of "Forcing of the dominant glacial-interglacial cyclicity in southeast Asian rainfall changes" in lines 236-255 in the revised manuscript.

Fig. 2 Describe the method of spectral analysis.

Method of spectral analysis has been added. Please see lines 676-677 in the revised manuscript.

Fig. 2 Indicate the correlation coefficients between proxy and model records.

Thanks for this suggestion. Correlation coefficients between proxy records and model results in northern and southeast Asia are 0.38 and 0.44, respectively, suggesting their moderate correlation.

The correlation coefficient in southern China is weak (only 0.02). Thus we didn't show their correlation coefficients in the manuscript. However, we noted this weak relationship probably result from the differences between the domain of the model box and the region that can be covered by these rainfall proxies, as well as from the age bias between modeling and rainfall reconstructions. Please see lines 154-159 in the revised manuscript. Generally, although the correlations between proxy and model records are moderate to weak, their same orbital periodicities and variation trends in glacial-interglacial cycles indicate their reliability to indicate local rainfall changes.

Reviewer #3 (Remarks to the Author):

Zhao et al. aim to address the debate which the precipitation changes in East Asia monsoon region latitude-dependently respond to different external and internal forcings. They reconstruct a rainfall record over the past 400 ky using the proxy of K to Al ratio (K/Al) from a marine sediment core in the northern East China Sea, after removing the temperature effect on K/Al. The authors found a dominant 23-ky cycle in this record, which comes from precession, in comparison with a 100-ky cycle, which results from glacial-interglacial ice volume, dominated in the hydroclimate records of the southern China. They then applied transient climate models (comprising orbital, greenhouse gas, and ice sheet forcings) through the last 300 ky to study the precipitation distribution and variability in northern and southern China. Finally, they suggest that the dominance of the precession cycle in northern China is probably due to enhanced land-sea thermal contrast resulted from surface heating more over lands than over oceans. The manuscript topic is suited for the scope of the journal, but not novel, although it is of interest to seeing more proxy records with 23-ky cycle in mid-high latitudes. Therefore, I would suggest a resubmission after the authors address some major concerns.

Thanks for these encouraging comments.

1. To my understanding, I would regard the locations of Hulu and Sanbao caves as southern China, not northern China. Since one of the key points of the manuscript is the 23-ky cycle in northern China, in addition to the K/Al-derived precipitation record, I suggest the authors to find out another convinced record. Moreover, in line 93, I am not sure the authors' description of more significant 23-ky cycle in the loess ^{10}Be record.

Thanks for this suggestion. In our manuscript, the referred composite Chinese cave stalagmite δ^{18}O record (derived from Hulu, Dongge and Sanbao Caves) (Cheng et al., 2016) has been used to indicate the East Asian summer monsoon intensity, rather than rainfall. Records of Lz908 in Yellow River month and Tengger Desert have been adopted as the northern China rainfall records. We recognized that the climate interpretation of changes in Chinese cave δ^{18}O remains a subject of intense debates, such as changes in rainfall amount, land rainfall recycle, moisture source temperature, and changes in the moisture source region and transport pathway (e.g., Parker et al., 2021). However, an increasing number of works in recent years based on data-model approaches interpreted Chinese cave δ^{18}O as summer monsoon intensity and is driven by upstream depletion of moisture (e.g., Liu et al., 2014; Wen et al., 2016; He et al., 2021; Cheng et al., 2021; Zhang et al., 2021).

We compare our reconstructed rainfall proxy to this indicator of summer monsoon intensity. Highly consistent trends between them indicates that rainfall in the northern China is linearly dependent on the summer monsoon intensity, thus highlights the rainfall in the northern China mainly occurs in summer during glacial-interglacial cycles, and thus exhibits strong 23-ka cycle. Please see details in the discussion section.

The significant 23-ky cycle in the loess ^{10}Be rainfall record has been proposed by Cheng et al. (2021). Here we repeated the spectral analysis for this record, and found the 23-ky cycle is really strong but still a little bit weaker than 100-ka cycle, however, the confidence level of 100-ka peak fails to reach 80% (Fig. R7). In Beck et al. (2018), the loess ^{10}Be rainfall record has been firstly calibrated to remove the dust flux and geomagnetic effects, as well as the recycled ^{10}Be. However, we noted that ^{10}Be concentration in loess can also be strongly influenced by sediment grain size; most of them are absorbed by fine particles (<4 μm), which account for ~58-74 % in loess layers and 74-92% in soil layers (Gu et al., 1996). The large grain size fluctuations of loess sediments between glacial and interglacial periods probably also be a large contribution to the significant 100-ka cycle observed in the ^{10}Be rainfall record. We mentioned this grain size effect on loess ^{10}Be rainfall proxy. Please see lines 102-104 in the revised manuscript.

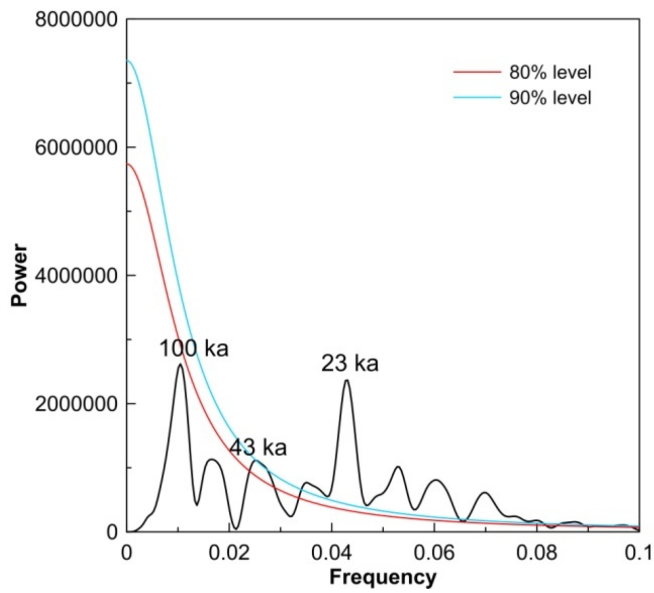

Fig. R7 Spectral analysis of loess ^{10}Be rainfall record with *PAST* software. The window function is Rectangle, number of oversample and segments is 9 and 3, respectively.

References

Beck, J. W., Zhou, W., Li, C., Wu, Z., White, L., & Xian, F., et al. (2018). A 550,000-year record of East Asian monsoon rainfall from ^{10}Be in loess. Science, 360(6391), 877.

Cheng, H., Edwards, R. L., Sinha, A., Sptl, C., Yi, L., & Chen, S., et al. (2016). The asian monsoon over the past 640,000 years and ice age terminations. Nature, 534, 640-646.

Cheng, H., Zhang, H., Cai, Y., Shi, Z., Liang, Y. I., & Deng, C., et al. (2021). Orbital-scale asian summer monsoon variations:paradox and exploration. Science China Earth Sciences 64, 529-544.

Gu, Z. Y., Gu, Lal, D., Liu, T. S., Southon, J., Caffee, M. W., Guo, Z. T., et al. (1996). Five million year^{10}Be record in Chinese loess and red-clay: climate and weathering relationships. Earth & Planetary Science Letters, 144, 1-2, 273-287.

He, C., Liu, Z., Otto-Bliesner, B. L., Brady, E., & Bao, Y. (2021). Hydroclimate footprint of pan-asian monsoon water isotope during the last deglaciation. Science Advances, 7, eabe2611.

Liu, Z., Wen, X., Brady, E. C., Otto-Bliesner, B., Yu, G., & Lu, H., et al. (2014). Chinese cave records and the east asia summer monsoon. Quaternary Science Reviews, 83, 115-128.

Parker, S. E., Harrison, S. P., Comas-Bru, L., Kaushal, N., Legrande, A. N., & Werner, M. (2021). A data-model approach to interpreting speleothem oxygen isotope records from monsoon regions. Climate of the Past(3).

Wen, X., Liu, Z., Wang, S. et al. (2016). Correlation and anti-correlation of the East Asian summer and winter monsoons during the last 21,000 years. Nature Communications, 7, 11999.

Zhang, H., Zhang, X., Cai, Y., Sinha, A., Christoph Spötl, & Baker, J., et al. (2021). A data-model comparison pinpoints holocene spatiotemporal pattern of east asian summer monsoon. Quaternary Science Reviews, 261(3), 106911.

2. The authors argue that the Yellow River is the major supplier of sediments in the study region even if the distance between the river mouth and deposit site is more than 1000 km apart. The evidence shown in the supplementary fig. S2 is less convincing, so needs more explanation.

Thanks for this constructive suggestion. This may seem contradictory given the long distance between the Yellow River mouth and study site (Fig. R8), but this mainly depends on the sediment load of rivers from potential sources. The modern Yellow River delivers 1100 Mt (before 1980s) of suspended sediments annually to the eastern marginal seas (Milliman and Farnsworth, 2011). Then the fine-grained particles of these sediments have been transported to the Okinawa Trough by cross-shelf currents (e.g., Yuan et al., 2008). This finding has been tested repeatedly by sediment source tracing works using clay minerals, Sr-Nd-Pb isotopes and rare earth elements in past few years (e.g., Beny et al., 2018; Zhao et al., 2017; 2018; 2019).

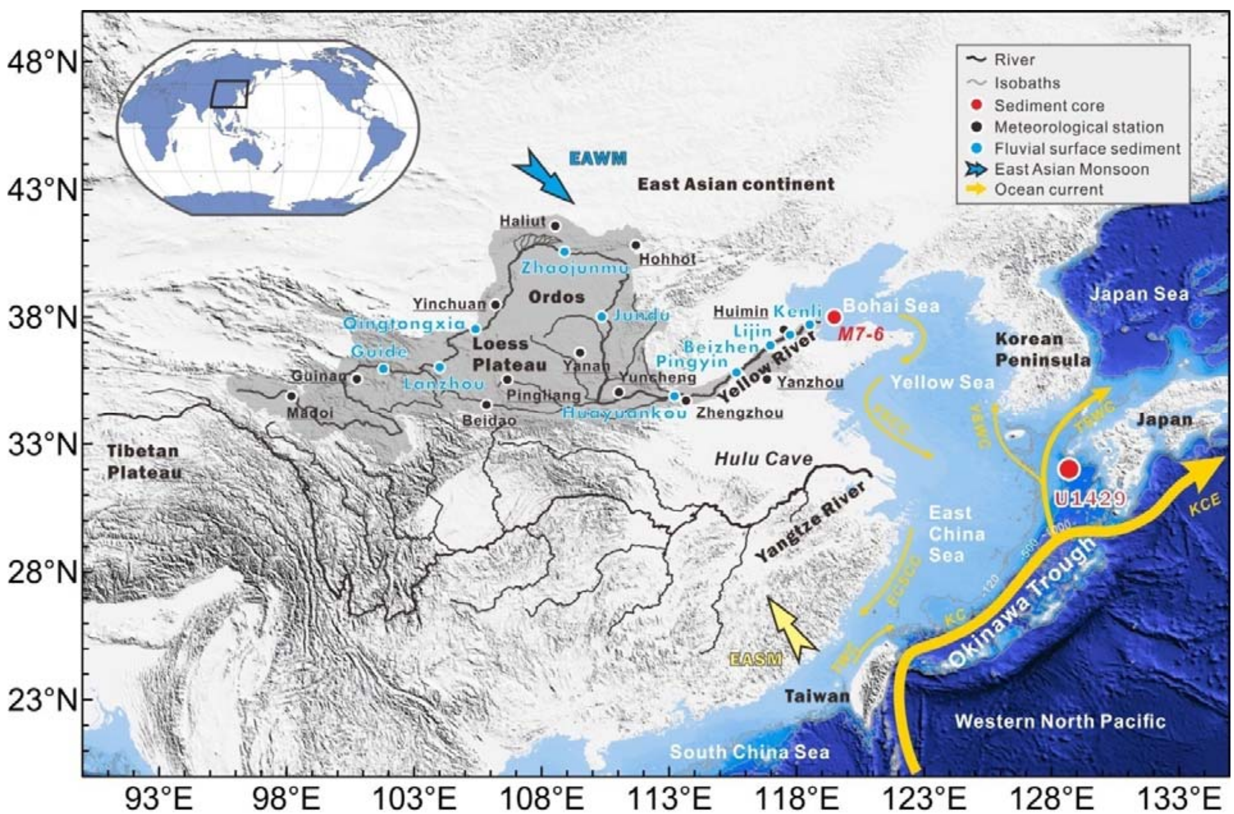

Fig. R8 Locations of potential source regions of sediments at IODP Site U1429. Yellow arrows show the ocean currents in surrounding oceans. YSCC, Yellow Sea Coastal Current; YSWC, Yellow Sea Warm Current; TSWC, Tsushima Warm Current; ECSCC, East China Sea Coastal Current; KC, Kuroshio Current; KCE, Kuroshio Current Extension; TWC, Taiwan Warm Current.

The nearby Japanese island supplies 1.8 Mt of suspended sediments annually to East China Sea via the Chikugo River (Milliman and Farnsworth, 2011). However, sediment source tracing works found the grain size population of Kyushu sediments deposited at U1429 is much coarser (30-63μm) than clay size (<2 μm) (e.g., Beny et al., 2018; Zhao et al., 2020). This is probably due to the short distance of sediment transport and plentiful rainfall on Kyushu, which facilitate the quick delivery of the primary products of land erosion and weathering to the study region (e.g., Sidle and Masahiro, 2004). In this study, our K/Al record is analyzed based on the clay-sized sediments. Thus we considered the effects of Kyushu sediments should be very minor.

The modern Yangtze River delivers 470 Mt of suspended sediments annually to the East China Sea (Milliman and Farnsworth, 2011). However, most of the Yangtze River sediments are thought to accumulate off the river mouth and adjacent coastal area to the south, due to strong southward coastal current (e.g., Dong et al., 2020); where only a small portion of fine-grained particles is transported offshore, and further to the middle and southern regions of the Okinawa Trough (e.g., Diekmann et al., 2008; Zheng et al., 2014).

Rivers in the southern Korean Peninsula including the Seumjin and Nakdong, deliver about 10 Mt of suspended materials annually to the Tsushima Strait (Milliman and Farnsworth, 2011). However, there is almost no sediment input in the study region from the Korean Peninsula (e.g., Beny et al., 2018; Zhao et al., 2017; 2018; 2019). This probably due to the strong blocking effect of Tsushima Warm Current and/or East Korean Warm Current and Korean Coastal Current, which transport sediments from southern Korean Peninsula into the Japan Sea (e.g., Chun et al., 2015).

Besides, Taiwanese rivers collectively discharge about 200 Mt of sediments annually into the surrounding ocean (Milliman and Farnsworth, 2011). However, more than half of them (~120 Mt) are delivered into the South China Sea and the Taiwan Strait (Milliman and Farnsworth, 2011). Previous studies have indicated that Taiwan-derived sediments have only been transported to the southern (e.g., Diekmann et al., 2008; Dou et al., 2016) and middle Okinawa Trough via the Kuroshio Current (Zheng et al., 2016).

We added more explanation in the source tracing section. Please see the section of “Sediment source tracing” in Supplementary materials.

References

Beny, F., Toucanne, S., Skonieczny, C., Bayon, G., & Ziegler, M. (2018). Geochemical provenance of sediments from the northern east china sea document a gradual migration of the asian monsoon belt over the past 400,000 years. Quaternary Science Reviews, 190, 161-175.

Chun, J. H., Kim, Y., Bahk, J. J,. et al. (2015). Late Holocene distal mud deposits off the Nakdong delta, SE Korea: evidence for shore-parallel sediment transport in a current-dominated setting.

Geo-Mar Lett, 35, 475-485.

Diekmann, B., Hofmann, J., Henrich, R., DK Fütterer, Röhl, U., & Wei, K. Y. (2008). Detrital sediment supply in the southern okinawa trough and its relation to sea-level and kuroshio dynamics during the late quaternary. Marine Geology, 255(1-2), 83-95.

Dong, J., Li, A., Liu, X., Wan, S., & Shi, X. (2020). Holocene climate modulates mud supply, transport, and sedimentation on the East China Sea shelf. Journal of Geophysical Research: Earth Surface, 125, 10, e2020JF005731.

Dou, Y., Yang, S., Shi, X., Clift, P. D., Liu, S., & Liu, J., et al. (2016). Provenance weathering and erosion records in southern Okinawa Trough sediments since 28 ka: Geochemical and Sr-Nd-Pb isotopic evidences. Chemical Geology, 425, 93-109.

Milliman, J. D., & Farnsworth, K. L. (2013). River Discharge to the Coastal Ocean: A Global Synthesis.

Sidle, R. C., & Chigira, M.. (2004). Landslides and debris flows strike kyushu, japan. Eos Transactions American Geophysical Union, 85(15), 145.

Yuan, D., Zhu, J., Li, C., & Hu, D. (2008). Cross-shelf circulation in the Yellow and East China Seas indicated by MODIS satellite observations. Journal of Marine Systems, 70(1-2), 134-149.

Zhao, D., Wan, S., Clift, P. D., Tada, R., Huang, J., & Yin, X., et al. (2018). Provenance, sea-level and monsoon climate controls on silicate weathering of yellow river sediment in the northern okinawa trough during late last glaciation. Palaeogeography, Palaeoclimatology, Palaeoecology, 490, 227-239.

Zhao, D., Wan, S., Lu, Z., Zhai, L., Feng, X., Shi, X. & Li, A. (2020). Response of heterogeneous rainfall variability in East Asia to Hadley circulation reorganization during the late Quaternary. Quaternary Science Reviews, 247, 106562.

Zhao, D., Wan, S., Song, Z., Gong, X., Zhai, L., Shi, X., & Li, A. (2019). Asynchronous variation in the Quaternary East Asian winter monsoon associated with the tropical Pacific ENSO-like system. Geophysical Research Letters, 46, 6955-6963.

Zhao, D., Wan, S., Toucanne, S., Clift, P. D., Tada, R., & S Révillon, et al. (2017). Distinct control mechanism of fine-grained sediments from yellow river and kyushu supply in the northern okinawa trough since the last glacial. Geochemistry Geophysics Geosystems, 18, 2949-2969.

Zheng, X., Li, A., Wan, S., Jiang, F., & Johnson, C. (2014). ITCZ and ENSO pacing on East Asian winter monsoon variation during the Holocene: sedimentological evidence from the Okinawa Trough. Journal of Geophysical Research: Oceans. 119, 7, 4410-4429.

Zheng, X., Li, A, Kao, S., Gong, X., Frank, M. & Kuhn, G. et al. (2016). Synchronicity of Kuroshio Current and climate system variability since the last glacial maximum. Earth & Planetary Science Letters, 452, 247-257.

3. Lines 116-143: This paragraph is used to discuss the explanation of the depleted δ^2H values in southern China and South China Sea during glacial period. I was lost halfway to the end, and therefore recommend to re-structure and re-write this part.

Thanks for this suggestion. This part has been rewritten. Please see lines 120-144 in the revised manuscript.

Minor comments along the text

Line 82: …liner?...

Changed to linear. All the typos about this word throughout this manuscript have been changed. Please see the revised manuscript.

Line 119: The meaning of the clause starting with "as well as" is not clear.

This sentence has been reorganized. Please see lines 125-127 in the revised manuscript.

Line 143: 11 ka or 10 ka?

Thanks for this reminder. It has been changed to 10 ka. Please see line 144 in the revised manuscript.

Line 162: …11 ka… or 10 ka?

It has been changed to 10 ka. Please see line 167 in the revised manuscript.

Line 172: …(Fig. 3A, E and I)…

The "Discussion" section has been rewritten, please see lines 181-255 in the revised manuscript. The paragraph includes this sentence has been removed.

Line 174: …(Fig. 3A and I)…

The "Discussion" section has been rewritten, please see lines 181-255 in the revised manuscript. The paragraph includes this sentence has been removed.

Line 176ff: This sentence is a bit weird here, since early in the paragraph the authors talk about "full forcing".

The "Discussion" section has been rewritten, please see lines 181-255 in the revised manuscript. The paragraph includes this sentence has been removed.

Lines 178-180: This sentence is awkward.

This sentence has been reorganized. Please see lines 191-193 in the revised manuscript.

Line 183: …liner?...

Changed to linear. All the typos about this word throughout this manuscript have been changed. Please see the revised manuscript.

Line 193: Don't use double negatives.

The paragraph includes this sentence has been reorganized. Please see lines 211-226 in the revised manuscript.

Line 201: aforementioned rainfall proxies… Delete "aforementioned".

The paragraph includes this sentence has been reorganized. Please see the section of "Forcing of the dominant glacial-interglacial cyclicity in southern China rainfall changes" in lines 211-235 in the revised manuscript.

Line 208: …with insolation playing a secondary role. Delete "with insolation playing a secondary role".

Deleted. Please see lines 224-226 in the revised manuscript.

Line 521ff: Please define the latitude range of northern, southern China, and southeastern Asia.

The latitude and longitude ranges have been added. Please see lines 651-654 in the revised manuscript.

Line 542: Why does the K/Al rainfall record give a negative value?

The river sediments have relatively higher K/Al ratio than U1429 sediments, and thus lower weathering degree. This is mainly due to the Yellow River sediment samples were collected during the dry season of spring, and U1429 sediments are the mixture of sediments with relatively high and low weathering degree supplied from all the seasons. Thus the calculated corresponding K/Al ratios of delta T (the correction methods are shown in Supplementary material) with the equation regressed from river sediments are relatively large. Then we subtract these K/Al ratios of delta T from total K/Al of U1429, and thus get the negative value. Our regression cannot make a quantitative calculation of the rainfall change, but try to eliminate the temperature effect on the sediment chemical weathering, and to obtain a rainfall variation "trend". Thus we considered these negative values will not influence our trend analysis.

Fig. 1: I suggest to color-code all the symbols. For instance, one color for records in northern China and another one for records in the south. The records of Lz908, Chinese loess, and Tengger Desert don't appear in the main text and figures, but only in the supplementary. Therefore, their sites could be removed from Fig. 1.

Thanks for this suggestion. Because the records of Lz908, Chinese loess, and Tengger Desert have been mentioned in the main text of section "Dominant precession cyclicality of the hydroclimate in northern China", we didn't remove these records in Figure 1. We changed the color of their symbols in different regions. Please see Figure 1 in the revised manuscript.

Fig. 2: (E) The colors of IODP U1429 and ODP 1146 are too similar to be distinguished.

Changed. Please see Figure 2E in the revised manuscript.

Line 550: …orbital periodicities… Why no obliquity cycle marked?

Obliquity cycle has been marked. Please see Figure 2 in the revised manuscript.

Fig. S5: I suggest to re-arrange all the records at right-hand panel according to the north-south order, as well as to unify the color of site symbols and records.

Thanks for the suggestion. The records have been re-arranged. Please see Figure S5 in Supplementary materials.

16th Sep 22

Dear Dr Zhao,

Please allow us to apologise for the delay in sending a decision on your manuscript titled "Reversed latitude dependence on the cyclicity of the Quaternary East-Southeast Asian hydroclimate" 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.

In the following, we list our main editorial thresholds:
• Provide compelling evidence including standard multivariate statistics to support the use of the K/Al ratio of marine sediments as a proxy for chemical weathering intensity and fully describe your methodology and approach.

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.

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,

Sze Ling Ho, PhD
Editorial Board Member
Communications Earth & Environment
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Joe Aslin
Locum Chief Editor
Communications Earth & Environment

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- 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.

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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 have improved the manuscript in many ways. They have offered a clearer explanation of the purpose and goals of the study. They have also rewritten the description of model results to offer clearer insights into the mechanisms of glacial-interglacial vs. precessional variability in each region. I appreciate the effort they've put into revisions, and I believe the revised study offers an important template for understanding climate variability across East Asia that appears robust both in data and model simulations--it has certainly affected my own thinking about this region's climatic history.

I have one important remaining concern. I am unsatisfied with the authors' response to my request for them to use multivariate linear regression to remove the effects of temperature on K/Al rather than two separate regressions; their rebuttal to this item just offers more univariate regressions. As they show, temperature and precipitation are very highly correlated in their modern calibration dataset—meaning that in using a univariate linear regression to remove the effects of temperature, they also remove much of the effect of precipitation. This is a very common problem across many sciences (having two predictor variables that are correlated), and there are robust techniques for controlling for one predictor in order to examine the effect of the other; these techniques should be applied here.

Finally, there are many small errors with subject-verb agreement, missing articles, etc. In most cases, these don't interfere with the reader's understanding, though sometimes they distract the reader's attention (e.g., "should be resulted" in line 216 ought to be changed to "should result"). One area in which things sometimes get confusing is when the authors are discussing anomalies—it is sometimes unclear which time period is being subtracted from which. For example, in lines 142-144 the authors state that iTRACE results show “positive…rainfall loadings…accompanied by negative d18O loadings between the last glacial maximum (LGM) and 10 ka.” Here “loadings” should be replaced by “anomalies”, and the text should be modified to make clearer that 10 ka has greater precipitation and more negative d18O values relative to the LGM.

Reviewer #2 (Remarks to the Author):

Comments on Debo Zhao’s manuscript

I can not follow the authors’ interpretation of the K/Al ratio in Site U1429. It is unclear whether the gradient of the K/Al ratio in the Yellow River basin (Fig. S1) reflects either the gradient of temperature, precipitation or others. I strongly recommend adding more description and interpretation. In the reply letter, the authors show a concept of chemical weathering changing the K/Al ratio in the Yellow River basin (Fig. R5), but there is no supporting data from the Yellow River basin, I am thus not convinced with this concept and think that it is speculative. The K/Al ratio seems to decrease from upstream to downstream (Fig. S1). Because there is no description of the method and results of the river sediment analysis, I cannot judge what this dataset means. The authors should show the K/Al values in river sediments at each location. Did you separate the clay fraction and determine the K/Al ratio in the clay fraction? How about the Loess record shown in Fig. R5? I wonder if the K/Al decrease downstream is associated with a decrease in grain size. K is a major element of K-feldspar which exists as coarse grains in river sediments. Fining of sediment grains should decrease the K/Al ratio. If the data are not obtained from the clay fraction, the relationship between the K/Al ratio should be discussed in this paper. Also, it is noted that the K/Al ratio in Site U1429 (0.20-0.24) is lower than the minimum K/Al ratio in the Yellow River sediments (0.24). The difference suggests further alteration of the K/Al ratio during the transportation from the river mouth to the U1429 site, which cannot be attributed to chemical weathering but other factors such as grain size fractionation or sediment source.

I do not understand why the chemical weathering process could completely alter the fresher sediments from the Loess Plateau in the Yellow River basin and diminish its glacial-interglacial signal. If so, the K/Al ratio in the Yellow River basin would reach the minimum limit (0.20 seen in Site U1429).

The correction of the temperature effect on the K/Al ratio could be wrong because it assumes that the K/Al ratio is 100% determined by temperature.

The proposal of the K/Al ratio in marine sediments as a chemical weathering intensity in the sediment source area is challenging. Thus it can be a central issue of the manuscript if your data indicate it is reliable. I hope to see thorough descriptions of samples, methods, results and interpretations in a single paper focusing on this issue .

The discussion on the comparison with the WPWP region is not based on the dataset shown in this paper but mainly on the authors’ speculation. This part is unsuccessful.

Readers know there are various model outputs, and correspondence of results between proxy and model results can convince readers only if readers regard both results as reliable. I hope to see the authors describe the results much more thoroughly in a separate paper, which will be reviewed by modelers.

Reviewer #3 (Remarks to the Author):

I am generally satisfied with the authors' explanation on my previous comments. However, the concern about the results of simulated precipitation is not fully resolved. For instance, the seasonal rainfall difference in Fig. 3, a large portion of the southern China and South China Sea were wetter during LGM than 10 ka. I am not an expert in modeling, but this result is contradictory to many proxy records, even the Hulu speleothem record which the authors used. Hence, I hope this paper should be carefully reviewed by experienced modelers.

Minor comments along the text

Lines 41-45: This sentence is too long. Pls rewrite it.

Lines 224-228: It is interesting that the cyclicities of simulated annual and summer rainfall are different. However, it is not a convincing deduction jumping directly from annual vs. summer rainfall to spring and autumn rainfall.

Fig. 2E caption: Pls indicate that the circles are ODP 1146 and the rectangles are 251PC.

Fig. S3D: Please check the plot of Fig. S3D. The data points look more than the raw K/Al data (Fig. S3C).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have improved the manuscript in many ways. They have offered a clearer explanation of the purpose and goals of the study. They have also rewritten the description of model results to offer clearer insights into the mechanisms of glacial-interglacial vs. precessional variability in each region. I appreciate the effort they've put into revisions, and I believe the revised study offers an important template for understanding climate variability across East Asia that appears robust both in data and model simulations--it has certainly affected my own thinking about this region's climatic history.

Thanks for these encouraging comments.

I have one important remaining concern. I am unsatisfied with the authors' response to my request for them to use multivariate linear regression to remove the effects of temperature on K/Al rather than two separate regressions; their rebuttal to this item just offers more univariate regressions. As they show, temperature and precipitation are very highly correlated in their modern calibration dataset—meaning that in using a univariate linear regression to remove the effects of temperature, they also remove much of the effect of precipitation. This is a very common problem across many sciences (having two predictor variables that are correlated), and there are robust techniques for controlling for one predictor in order to examine the effect of the other; these techniques should be applied here.

Thanks for this constructive suggestion, which provide a quantitative insights for our rainfall reconstruction with this weathering proxy.

Because the high correlation between temperature and rainfall, the collinearity diagnosis was made firstly on the regression model, to test if there was collinearity between the independent variables. The variance inflation factor (VIF) is 6.322, which is large than 1, suggesting a collinearity problem. Thus before linear regression, the dimensionality of the data is reduced with the Principal Component Analysis (PCA). Highly correlated independent variables have been transformed into mutually independent variables without linear relationship. These transformed variables can reflect most of the information of the original data. Then linear regression has been conducted on the variable of weathering proxy and transformed variable, to obtain a regression model covers K/Al, temperature and rainfall. As a result, this model can be used to predict the result of one variable (rainfall) given the two other variables (K/Al and temperature).

Based on this regression model, the calculated mean annual rainfall during the last 400 ka ranges from ~470-717 mm. This is close to the modern annual rainfall (~562-648 mm) in the Yellow River middle to lower reaches, as well as simulated annual rainfall (~407-854 mm) in northern China during the last 300 ka. Thus we considered our reconstructed quantitative rainfall variation is reliable.

Please see the details of data analysis in lines 94-124 in the Supplementary materials.

Finally, there are many small errors with subject-verb agreement, missing articles, etc. In most cases, these don't interfere with the reader's understanding, though sometimes they distract the reader's attention (e.g., "should be resulted" in line 216 ought to be changed to "should result"). One area in which things sometimes get confusing is when the authors are discussing anomalies—it is sometimes unclear which time period is being subtracted from which. For example, in lines 142-144 the authors state that iTRACE results show "positive…rainfall loadings…accompanied by negative d18O loadings between the last glacial maximum (LGM) and 10 ka." Here "loadings" should be replaced by "anomalies", and the text should be modified to make clearer that 10 ka has greater precipitation and more negative d18O values relative to the LGM.

Thanks for these suggestions. We checked the grammar and references, as well as other errors. Some confusing sentences have been modified. Please see lines 140-143, 219-220 and other parts in the revised manuscript.

Reviewer #2 (Remarks to the Author):

Comments on Debo Zhao's manuscript

I can not follow the authors' interpretation of the K/Al ratio in Site U1429. It is unclear whether the gradient of the K/Al ratio in the Yellow River basin (Fig. S1) reflects either the gradient of temperature, precipitation or others. I strongly recommend adding more description and interpretation.

Thanks for this suggestion. Due to the high correlations between K/Al and rainfall, K/Al and temperature, and rainfall and temperature, it's difficult to conclude which factor (rainfall or temperature) is the major control on this chemical weathering proxy. Because the temperature and rainfall can all influence land weathering processes. Here we applied multiple statistical analyses to obtain a relationship covers these three variables, and figure out one variable (rainfall) on the basis of other two variables (K/Al and temperature). Please see the details in lines 94-124 in the Supplementary materials.

In the reply letter, the authors show a concept of chemical weathering changing the K/Al ratio in the Yellow River basin (Fig. R5), but there is no supporting data from the Yellow River basin, I am thus not convinced with this concept and think that it is speculative.

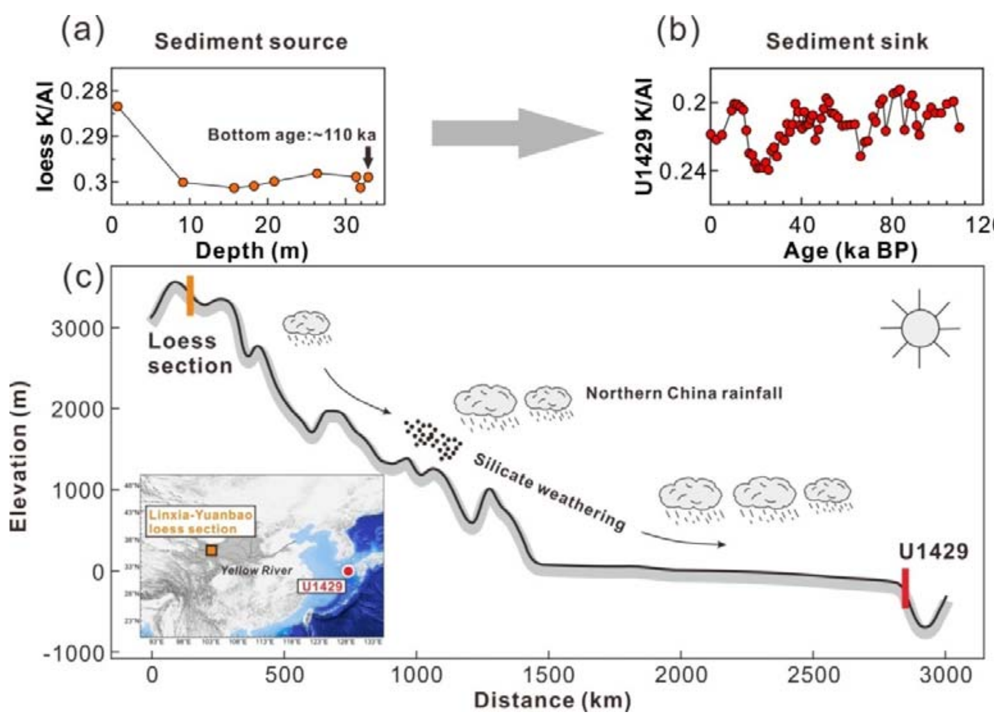

Fig. RR1 Comparison of clay-sized sediment K/Al ratios between loess section (unpublished data) (please see the detailed information of this loess section in Guo et al. (2021)) in Yellow River upper reach and U1429 since the last glaciation.

This model is valid mainly depending on the Yellow River sediments are dominated by the Loess Plateau sediments. It has been proposed that ~90% sediments in Yellow River lower reaches are supplied from the Loess Plateau (e.g., Ren and Shi, 1986; Yang et al., 2004). This can be proved by similar Nd isotopic composition between sediments in Yellow River and Loess Plateau (e.g., Beny et al., 2018). Then these sediments are transported to Site U1429. Thus chemical weathering occurs during the sediment transport from Loess Plateau to Site U1429 (Fig. RR1).

The main logic of the reconstruction of past land chemical weathering flux with marine sediments is to compare the weathering degree between sediment source and sink. Weathering proxy data in the river basin is not required (e.g., Lupker et al., 2013). The differences of weathering proxies between source and sink reflect the sediments undergo chemical weathering during their transport (e.g., Lupker et al., 2013). The Chinese Loess Plateau K/Al record (source) shows strong glacial-interglacial cycle, which is distinct with the U1429 K/Al ratio (sink) with strong precession cycle. Thus we considered such strong precession fluctuations in U1429 K/Al ratio were mainly forced by the precession fluctuations of rainfall in Yellow River middle to lower reaches, which regulates chemical weathering of sediments transport from Loess Plateau to Site U1429. We hope we could convince the reviewer with these explainations.

We added more interpretations about sediment weathering process in lines 66-74 in the Supplementary materials.

References

Beny, F., Toucanne, S., Skonieczny, C., Bayon, G., & Ziegler, M. (2018). Geochemical provenance of sediments from the northern East China Sea document a gradual migration of the Asian monsoon belt over the past 400,000 years. Quaternary Science Reviews, 190, 161-175.

Lupker, M., France-Lanord, C., Galy, V., Lavé, J. et al. (2013). Increasing chemical weathering in the Himalayan system since the Last Glacial Maximum, Earth and Planetary Science Letters, 365, 243-252

Ren, M., and Y. Shi (1986), Sediment discharge of the Yellow River (China) and its effect on the sedimentation of the Bohai and the Yellow Sea, Cont. Shelf Res., 6(6), 785-810.

Yang, S., Jung, H.S., & Li, C. (2004). Two unique weathering regimes in the changjiang and huanghe drainage basins: geochemical evidence from river sediments. Sedimentary Geology, 164(1/2), 19-34.

The K/Al ratio seems to decrease from upstream to downstream (Fig. S1). Because there is no description of the method and results of the river sediment analysis, I cannot judge what this dataset means. The authors should show the K/Al values in river sediments at each location.

Thanks for this suggestion. The K/Al values have been added to the Fig. S1.

Did you separate the clay fraction and determine the K/Al ratio in the clay fraction? How about the Loess record shown in Fig. R5? I wonder if the K/Al decrease downstream is associated with a decrease in grain size. K is a major element of K-feldspar which exists as coarse grains in river sediments. Fining of sediment grains should decrease the K/Al ratio. If the data are not obtained from the clay fraction, the relationship between the K/Al ratio should be discussed in this paper.

Yes, the K/Al ratios of sediments in U1429, Yellow River and Loess record are all derived from clay-sized sediments. We show this information in the caption of Fig. R5 in the previous response letter. Also see it in Fig. RR1. Please see lines 291-295 in the revised manuscript.

Also, it is noted that the K/Al ratio in Site U1429 (0.20-0.24) is lower than the minimum K/Al ratio in the Yellow River sediments (0.24). The difference suggests further alteration of the K/Al ratio during the transportation from the river mouth to the U1429 site, which cannot be attributed to chemical weathering but other factors such as grain size fractionation or sediment source.

All the K/Al ratios of sediment samples in U1429, Yellow River and Loess record are derived from clay-sized fractions. The grain size fractionation effect can be excluded. Our source tracing results also clearly show the Yellow River is the dominant source of Site U1429 sediments (Fig. S2). This can also be proved by the previous studies (e.g., Beny et al., 2018). Thus the sediment source is also not the reason of different K/Al values between Yellow River and U1429. Please see the description about clay-sized fraction extraction in lines 291-295 in the revised manuscript. Sediment source discussion can be found in lines 24-63 in the Supplementary materials.

Generally, K/Al ratios range from 0.25-0.29 for Yellow River sediments, and 0.19-0.24 for U1429 sediments. However, such higher value of K/Al of Yellow River surface sediments is mainly depends on the sample season. The Yellow River bed sediment samples in this manuscript were collected during the dry season of spring. The spring low rainfall amount and temperature (figure S1 in supplementary materials) resulted in low weathering degree of river sediments, and thus relatively higher K/Al ratio. In contrast, the U1429 sediments are the mixture of sediments supplied from all the seasons and probably reflect their average. Therefore this probably leads to relatively higher weathering degree and lower K/Al ratio in U1429 sediments than the spring river sediments. This has also been proved by some seasonal scale of river sediment weathering studies, which suggest lower weathering rate during spring than that during summer and annual average (e.g., Yu et al., 2019). For the river sediments, our main purpose is to obtain an equation about K/Al (element behaviors under different rainfall and temperature, not different seasons), rainfall and temperature. Thus such seasonal factor of river samples should exert small effect on our result. We hope we could convince the reviewer with these explainations.

References

Beny, F., Toucanne, S., Skonieczny, C., Bayon, G., & Ziegler, M. (2018). Geochemical provenance of sediments from the northern East China Sea document a gradual migration of the Asian monsoon belt over the past 400,000 years. Quaternary Science Reviews, 190, 161-175.

Yu, Z., Keys, L., & Wu, G., et al. (2019). Seasonal variation of chemical weathering and its controlling factors in two alpine catchments, Nam Co basin, central Tibetan Plateau. Journal of Hydrology, 576, 381-395.

I do not understand why the chemical weathering process could completely alter the fresher sediments from the Loess Plateau in the Yellow River basin and diminish its glacial-interglacial signal. If so, the K/Al ratio in the Yellow River basin would reach the minimum limit (0.20 seen in Site U1429).

Sediments transported from the Loess Plateau haven't been chemical altered completely. They just have been altered to some extent. Change of cycles from glacial-interglacial cycle in loess record (sediment source) to precession cycle in U1429 (sediment sink) suggest the precession fluctuations of rainfall in Yellow River drainage basin regulate chemical weathering of sediments transport from Loess Plateau to Site U1429. We understand this comment to be why the K/Al ratio in U1429 (0.19-0.24) is lower than K/Al ratio in the Yellow River sediments (0.25-0.29). The sediments of loess record and U1429 cover mixing signals of all the seasons. However, the Yellow River sediment samples were collected from single dry season. This probably leads to relatively lower weathering degree and higher K/Al ratio in river sediments. Please see the previous response.

The correction of the temperature effect on the K/Al ratio could be wrong because it assumes that the K/Al ratio is 100% determined by temperature.

Thanks for this suggestion. In the revised manuscript, we applied multiple statistical analyses to solve this problem. Please see the details in lines 94-124 in the Supplementary materials.

The proposal of the K/Al ratio in marine sediments as a chemical weathering intensity in the sediment source area is challenging. Thus it can be a central issue of the manuscript if your data indicate it is reliable. I hope to see thorough descriptions of samples, methods, results and interpretations in a single paper focusing on this issue.

Thanks for this kind suggestion. We are analyzing Li isotope of U1429 clay-sized sediments recently. Li isotope is a powerful proxy for silicate weathering reconstruction (e.g., Pogge von Strandmann et al., 2015). This isotope results will be compared with element proxy proposed in this manuscript. Combining with the loess record mentioned above (unpublished data), we will further prepare a paper focusing on reconstruction of chemical weathering history and calculation of weathering flux in this regions.

References

Pogge, V., Frings, P. J., & Murphy, M. J. (2017). Lithium isotope behaviour during weathering in the Ganges Alluvial Plain. Geochimica et Cosmochimica Acta, 198, 17-31.

The discussion on the comparison with the WPWP region is not based on the dataset shown in this paper but mainly on the authors' speculation. This part is unsuccessful.

Thanks for your concern. But we would like to clarify that for the WPWP region, we provided evidence of the rainfall changes, rainfall isotope and water vapor transport paths based on our model simulation, not simply speculation. These model results have been compared with reconstructed result of MD98-2152 $\delta^{13}C_{wax}$ record: our simulated rainfall changes during the last 300 ka shows large consistency with this $\delta^{13}C_{wax}$ record, suggesting strong glacial-interglacial cycle signals. We also discussed the forcing mechanisms of this cyclicity based on our simulated water vapor transport and rainfall isotope results, and found that enhanced moisture divergence during glacial periods in western Maritime Continent caused such strong glacial-interglacial cycles of rainfall changes. We have provided the complete chain of evidence from the external forcings (e.g. sea-level and land-sea mask differences prescribed in the model) linking to the forcing mechanisms, the precipitation periodicity as revealed by both model and data, as well as an explicit model-data comparison. Thus we argued that our discussion about this section was not mainly based on speculation.

Readers know there are various model outputs, and correspondence of results between proxy and model results can convince readers only if readers regard both results as reliable. I hope to see the authors describe the results much more thoroughly in a separate paper, which will be reviewed by modelers.

Thanks for this kind suggestion. Indeed we used multiple model output for different purposes in this study, particular model-data comparison (300-ka simulations are preferred due to data lengh and larger forcing signals), mechanism discussion (TraCE simulations are preferred to elucidate the individual forcing effects, iTraCE involves water isotope processes and enables a direct model-data comparison). Note that the models used in this manuscript including NCAR-CCSM3 (for 300-ka rainfall simulation and TraCE-21ka simulation) and iCESM (for iTraCE simulation) have been used frequently in previous paleoclimatology and paleoceanography studies (e.g., Clemens et al., 2018; Liu et al., 2009; 2014; Lu et al., 2019; Zhao et al., 2021; Brady E et al., 2019; He et al., 2021a, 2021b). Model results in this manuscript are analyzed in similar statistical ways as previous studies. Given the overall good agreement between model and data in terms of periodicity, we assume that our model simulation approaches sound and results reliable. Besides, our rainfall reconstruction use weathering proxy is also reliable based on robust sediment source tracing and element analysis focusing on the uniform sediment grain size. We will consider preparing a separate paper for these data combined with some new results in the next step following the reviewer’s suggestion.

References

Brady E, et al. (2019). The Connected Isotopic Water Cycle in the Community Earth System Model Version 1. Journal of Advances in Modeling Earth Systems 11, 2547-2566.

Clemens S, et al. (2018). Precession-band variance missing from East Asian monsoon runoff. Nature communications 9, 3364.

He C, et al. (2021). Deglacial variability of South China hydroclimate heavily contributed by autumn rainfall. Nature Communications 12, 5875.

He C, et al. (2021). Hydroclimate footprint of pan-Asian monsoon water isotope during the last deglaciation. Science Advances 7, eabe2611.

Liu Z, Lu Z, Wen X, Otto-Bliesner BL, Timmermann A, Cobb KM. (2014). Evolution and forcing mechanisms of El Niño over the past 21,000 years. Nature 515, 550-553.

Liu Z, et al. (2009). Transient Simulation of Last Deglaciation with a New Mechanism for Bølling-Allerød Warming. Science 325, 310-314.

Lu Z, Liu Z, Chen G, Guan J. (2019). Prominent Precession Band Variance in ENSO Intensity Over the Last 300,000 Years. Geophysical Research Letters 46.

Zhao, D., Wan, S., Lu, Z., Zhai, L., Feng, X., Shi, X., & Li, A. (2021). Delayed collapse of the North Pacific Intermediate Water after the glacial termination. Geophysical Research Letters, 48, e2021GL092911.

Reviewer #3 (Remarks to the Author):

I am generally satisfied with the authors' explanation on my previous comments. However, the concern about the results of simulated precipitation is not fully resolved. For instance, the seasonal rainfall difference in Fig. 3, a large portion of the southern China and South China Sea were wetter during LGM than 10 ka. I am not an expert in modeling, but this result is contradictory to many proxy records, even the Hulu speleothem record which the authors used. Hence, I hope this paper should be carefully reviewed by experienced modelers.

Thanks for this comment. Yes, Fig. 3 shows large regions of the southern China and South China Sea were wetter during LGM than 10 ka. However, we would argue that this feature is consistent with, rather than contradictory to a large number of proxy records.

Large numbers of the East Asian rainfall records covering the period since the LGM to the present are mainly distributed in the northern China. For example, the magnetic susceptibility, carbonate carbon isotope and beryllium isotope of Loess Plateau records and many pollen records in northern Chinese lakes. All of them suggest drier LGM than early Holocene, including our proxy and model results in this manuscript. In our explanation, we attributed this to the linear response of northern China rainfall to southerly monsoon wind intensity, which driven by the insolation. However, the point is this consistency probably give us a misleading picture to some extent that rainfall in the whole region of East Asia are weaker during LGM than that during early Holocene.

Another point is the interpretation of Chinese stalagmite oxygen isotope record. All the cave records in different regions of East Asia also suggest consistent trends. Despite these stalagmite records may be controlled by multiple factors, such as rainfall amount, changes in the moisture source region and transport pathways, they have been cited widely as a rainfall proxy for a long time. This also probably gives us a misleading that rainfalls in the whole region of East Asia are weaker during LGM than that during Holocene. In recent years, an increasing number of works based on data-model approaches tend to interpret Chinese stalagmite $\delta^{18}O$ as summer monsoon intensity and is driven by upstream depletion of moisture (e.g., Cheng et al., 2016, 2019, 2021; Liu et al., 2014; Liu et al., 2020; Wen et al., 2016; He et al., 2021; Zhang et al., 2019; Zhang et al., 2021). This may draw the paleoclimate researcher's attention that it's better to avoid comparing Chinese cave $\delta^{18}O$ as a rainfall proxy. In our manuscript, we cited this record as a monsoon wind proxy. We found our reconstructed rainfall record in northern China is consistent with it, and thus proposed the linear response of northern China rainfall to southerly monsoon wind intensity, which driven by the insolation.

Here, the question is whether the southern China rainfall was stronger during LGM than early Holocene. The number of southern China and South China Sea rainfall record covering LGM to present is much less than the northern China. We collected several records more directly linked to precipitation changes compared to speleothems, including Yangzte River runoff record, Pearl River drainage basin leaf wax record, South China Sea surface salinity record, Dajiuhu pollen record, Dahu swamp magnetic record and Xialu peatland pollen record (Fig. 2 and S5 in manuscript). Almost all the records show stronger rainfall during LGM than Holocene (also note that records of Yangzte

River runoff and Dahu swamp suggest LGM rainfall was approximately equal to the Holocene). There was also a paper reviewed a large numbers of Chinese pollen records since LGM, and indeed found a wetter southern China during LGM (Wu et al., 2019) (Fig. RR2).

[image redacted]

Fig. RR2 Rainfall anomalies at the LGM relative to the mean climate during 1951-2001. Left is mean annual rainfall, right is summer rainfall. This figure is from Fig. 4e and f in Wu et al. (2019).

From a modelling perspective, there are also limited focus on 10 ka. PMIP timeslice simulations only cover the LGM, the mid-Holocene (6 ka) and the PI, so a multi-model comparison of the precipitation changes between the LGM and 10 ka is not available. In addition to our transient simulations, another independent modelling work based on the Norwegian Earth System Model also reveals wetter southern China during LGM (Dai et al., 2021) (Fig. RR3). They found the same results with our model output, suggesting stronger summer and annual rainfall in the southern China and South China Sea during 22 ka than 10 ka. We added this evidence to the revised manuscript. Please see lines 216-217.

[image redacted]

Fig. RR3 Simulated summer and annual rainfall patterns for 10 ka minus 22 ka. This figure is from Fig. 3c and d in Dai et al. (2021).

Thus based on the proxy reconstruction and model evidence mentioned above, we hope we could convince the reviewer and the readers with our conclusions.

References

Cheng, H., Edwards, R.L., Sinha, A., Spötl, C., Yi, L., Chen, S., Kelly, M., Kathayat, G., Wang, X., Li, X., Kong, X., Wang, Y., Ning, Y., Zhang, H., 2016. The Asian monsoon over the past 640,000 years and ice age terminations. Nature 534, 640-646.

Cheng, H., Zhang, H., Cai, Y., Shi, Z., Yi, L., Deng, C., Hao, Q., Peng, Y., Sinha, A., Li, H., Zhao, J., Tian, Y., Baker, J., Perez-Mejías, C., 2021. Orbital-scale Asian summer monsoon variations: Paradox and exploration. Science China Earth Sciences 64, 529-544.

Cheng, H., Zhang, H., Zhao, J., Li, H., Ning, Y., Kathayat, G., 2019. Chinese stalagmite paleoclimate researches: A review and perspective. Science China Earth Sciences 62, 1489-1513.

Dai, G., Zhang, Z., Otterå, O. H., Langebroek, P. M., Yan, Q., & Zhang, R. (2021). A modeling study of the tripole pattern of East China precipitation over the past 425 ka. Journal of Geophysical Research: Atmospheres, 126, e2020JD033513.

He, C., Liu, Z., Otto-Bliesner, B. L., Brady, E., & Bao, Y., 2021. Hydroclimate footprint of pan-asian monsoon water isotope during the last deglaciation. Science Advances, 7, eabe2611.

Liu, X., Liu, J., Chen, S., Chen, J., Zhang, X., Yan, J., Chen, F., 2020. New insights on Chinese cave δ18O records and their paleoclimatic significance. Earth-Science Reviews 207, 103216. doi: 10.1016/j.earscirev.2020.103216.

Liu, Z., Wen, X., Brady, E. C., Otto-Bliesner, B., Yu, G., & Lu, H., et al., 2014. Chinese cave records and the east asia summer monsoon. Quaternary Science Reviews, 83, 115-128.

Wen, X., Liu, Z., Wang, S. et al., 2016. Correlation and anti-correlation of the East Asian summer and winter monsoons during the last 21,000 years. Nature Communications, 7, 11999.

Wu, H., Li, Q., Yu, Y. et al. (2019). Quantitative climatic reconstruction of the Last Glacial Maximum in China. Sci. China Earth Sci. 62, 1269-1278.

Zhang, H., Ait Brahim, Y., Li, H., Zhao, J., Kathayat, G., Tian, Y., Baker, J., Wang, J., Zhang, F., Ning, Y., Edwards, R.L., Cheng, H., 2019. The Asian summer monsoon: Teleconnections and forcing mechanisms-A review from Chinese speleothem δ18O records. Quaternary 2, 26. doi:10.3390/quat2030026.

Zhang, H., Zhang, X., Cai, Y., Sinha, A., Christoph Spötl, & Baker, J., et al., 2021. A data-model comparison pinpoints holocene spatiotemporal pattern of east asian summer monsoon. Quaternary Science Reviews, 261(3), 106911.

Minor comments along the text

Lines 41-45: This sentence is too long. Pls rewrite it.

Rewritten. Please see lines 41-45.

Lines 224-228: It is interesting that the cyclicities of simulated annual and summer rainfall are different. However, it is not a convincing deduction jumping directly from annual vs. summer rainfall to spring and autumn rainfall.

Thanks for this suggestion. We reorganized these sentences in this paragraph to make it more logical. Please see lines 230-238.

Fig. 2E caption: Pls indicate that the circles are ODP 1146 and the rectangles are 251PC.

Description has been added in the caption. Please see lines 701-702.

Fig. S3D: Please check the plot of Fig. S3D. The data points look more than the raw K/Al data (Fig. S3C).

Thanks for this suggestion. Fig. S3D has been replaced with a new calibrated rainfall curve. Please see Fig. S3D in Supplementary materials.

8th Dec 22

Dear Dr Zhao,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "Reversed latitude dependence on the cyclicity of the Quaternary East-Southeast Asian hydroclimate". It has now been seen again 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, provided you add to the main manuscript text a comprehensive discussion of the uncertainty of your K/Al ground-truthing dataset, as questioned by Reviewer #2.

If appropriate, we will publish your manuscript 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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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have addressed my concern with the removal of temperature effects on K/Al. Their use of PCA appears to deal with the covariation of temperature and precipitation and to provide more robust inferences of past precipitation from K/Al ratios. I recommend acceptance of the paper.

Reviewer #2 (Remarks to the Author):

The main problem of this paper is that the descriptions of methods, results, and interpretations are not enough for the review judgment. This is probably because three major topics (K/Al proxy and U1429 interpretations, hydroclimate of the WPWP, and model results) are packed into a single paper. I am unfamiliar with model results. Also I think the WPWP issue needs a much more comprehensive discussion on various proxy records from the area. In this review, I thus focus on the K/Al issue.

The zonal gradient of the K/Al ratio should be discussed intensively. In the reply letter, the authors assume that the mixing of fresher dust input to chemically-weathered sediments enhances the K/Al ratio values in riverine sediments because the samples were collected in a dry season, to answer my question why the riverine sediments have higher K/Al ratios than U1429 sediments. If this is true, the zonal gradient of the K/Al ratio can be explained by the eastward decreasing dust input. In other words, the K/Al ratio can be interpreted as reflecting the proximity of the dust source rather than temperature and precipitation.

The above assumption is not tested. The data of river particles collected in a rainy season are necessary to test.

The above assumption can explain the zonal gradient of the K/Al ratio in dry-season river sediments and the difference in the K/Al ratio between the Yellow River and U1429 sediments. But the authors should note that the obtained relationship between the K/Al ratio and precipitation/temperature cannot be applied to the reconstruction of the paleoenvironment in the Yellow River basin because the rainy season Yellow River sediments having different K/Al ratios should be transported to the Site U1429.

Also, the changing position of westerlies in response to precession may have changed the dust input and the K/Al ratio in the Yellow River basin, which needs another test.

Further, The grain size effect is still a potential factor, and the grain size data help us understand this effect.

Fig. S2A REE pattern is sensitive to background geology and samples (grain size, river sediments? etc). Describe more about samples and locations of referenced samples other than Yellow River sediments. Without such information, readers cannot be convinced that the Yellow River is the only source (line 76).

To calibrate the K/Al ratio with climatic values, the authors performed the PCA. I wonder how they performed on which variables.

Reviewer #3 (Remarks to the Author):

I appreciate that the authors carefully respond to my concerns on their simulation results. Now I am convinced and agree with the explanation on the wet LGM over the southern China. Regarding the multivariate linear regression between temperature, precipitation and K/Al, the authors made a transformation for all the three variables before linear regression (lines 94-124 in the supplementary). I am satisfied with this method and this indeed improved the reliability in rainfall reconstruction. However, I am not sure if the regressions shown in Fig. S1 are updated. Pls check it. When this part is also confirmed, I am happy to see its acceptance.

Reviewer #2 (Remarks to the Author):

The main problem of this paper is that the descriptions of methods, results, and interpretations are not enough for the review judgment. This is probably because three major topics (K/Al proxy and U1429 interpretations, hydroclimate of the WPWP, and model results) are packed into a single paper. I am unfamiliar with model results. Also I think the WPWP issue needs a much more comprehensive discussion on various proxy records from the area. In this review, I thus focus on the K/Al issue.

Thanks for your concerns. For the part of Southeast Asia rainfall, records covering the glacial-interglacial cycles are still scarce. It can hardly to make a comprehensive comparison with various proxy records in this region. In this manuscript, we selected one record which can indicate local rainfall variation robustly and compared it with our model results, and further discussed the different orbital cycles of rainfall variations compare to southern and northern China. Our results provide a wider perspective to discuss the high-low latitude climate forcing on the East-Southeast Asian hydroclimate. We will conduct the case study of the WPWP rainfall reconstruction in the future works.

More details of proxy dependability of K/Al ratio can be found in lines 69-96 in the revised manuscript. We also added some discussion about this issue in the Supplementary Information, please see below response.

The zonal gradient of the K/Al ratio should be discussed intensively. In the reply letter, the authors assume that the mixing of fresher dust input to chemically-weathered sediments enhances the K/Al ratio values in riverine sediments because the samples were collected in a dry season, to answer my question why the riverine sediments have higher K/Al ratios than U1429 sediments. If this is true, the zonal gradient of the K/Al ratio can be explained by the eastward decreasing dust input. In other words, the K/Al ratio can be interpreted as reflecting the proximity of the dust source rather than temperature and precipitation.

Thanks for this question. However, sediments in the middle to lower reaches of Yellow River are dominantly transported from the Loess Plateau, rather than from eolian dust. The Yellow River in northern China has long been regarded as a typical large river influenced by intense catchment erosion. It is globally known for its very high sediment flux (~11 billion ton/yr, before 1980s) sourced dominantly from the Chinese Loess Plateau (CLP) (~90 %) in the middle reaches of the river (Milliman and Farnsworth, 2011; Wang et al., 2010; Wang et al., 2016). In contrast, the dust flux deposit in the northern China is very small. The observation data from 2007-2014 suggests that dust fluxes (average dust column burdens during dust events) in the cities of Lanzhou, Yinchuan, Erenhot, Taiyuan, Zhengzhou and Beijing in northern China are only 0.24, 0.25, 0.28, 0.21, 0.21 and 0.25 g/m^2, respectively (Wang et al., 2018). Clearly, such huge difference between Yellow River sediment eroded from Loess Plateau and dust flux suggests that the chemical composition of Yellow River sediments is dominated by the loess materials, rather than eolian dust.

Besides, the dust grain-size is much coarser than clay-sized particles. It has been reported that even in the farther east part of the Asian dust transport route, i.e., Japan Sea, the dust grain-size population in sediments is 2-15 μm (Nagashima et al., 2007). In this work, all of our sediment samples of Yellow River and Site U1429 are analyzed on the clay-sized fraction (<2 μm). Thus we are confident that the dust disturbance, i.e., the eastward decreasing of dust input to the Yellow River, on the K/Al ratio can be ignored. More discussion about sediment source constrain has been added in lines 48-51 and 87-91 in Supplementary Information.

References

Milliman, J. D., & Farnsworth, K. L. (2013). River Discharge to the Coastal Ocean: A Global Synthesis.

Nagashima, K., Tada, R., Tani, A., Toyoda, S., Sun, Y., & Isozaki, Y. (2007). Contribution of aeolian dust in japan sea sediments estimated from esr signal intensity and crystallinity of quartz. Geochemistry, Geophysics, Geosystems, 8(2), Q02Q04.

Wang, H. J. et al. (2010). Recent changes in sediment delivery by the Huanghe (Yellow River) to the sea: Causes and environmental implications in its estuary. J. Hydrol. 391, 302-313.

Wang, S., Fu, B., Piao, S. et al. (2016). Reduced sediment transport in the Yellow River due to anthropogenic changes. Nature Geosci 9, 38-41.

Wang, X., Liu, J., Che, H. et al. (2018). Spatial and temporal evolution of natural and anthropogenic dust events over northern China. Sci Rep 8, 2141.

The above assumption is not tested. The data of river particles collected in a rainy season are necessary to test. The above assumption can explain the zonal gradient of the K/Al ratio in dry-season river sediments and the difference in the K/Al ratio between the Yellow River and U1429 sediments. But the authors should note that the obtained relationship between the K/Al ratio and precipitation/temperature cannot be applied to the reconstruction of the paleoenvironment in the Yellow River basin because the rainy season Yellow River sediments having different K/Al ratios should be transported to the Site U1429.

Thanks for this concern. Yes, our samples collected from the dry season, their K/Al ratios have different values with the rainy season. However, the advantage of our samples is that they are distributed in different climate zones. The Yellow River has a length of 5464 km and a catchment area of 7.5×10^5 km^2. The basin has an arid to semi-arid continental climate, being more arid and cold in the upper and middle reaches, and more humid and temperate in the lower reaches. The essence of the variations in river sediment K/Al ratio is that they can response to different temperature and rainfall. Thus we argued that our river sediment samples distributed in different climate conditions can be used to obtain the relationship among different temperature, rainfall and K/Al ratios, despite they were collected in a single season. More descriptions have been added in lines 122-125 in Supplementary Information.

Also, the changing position of westerlies in response to precession may have changed the dust input and the K/Al ratio in the Yellow River basin, which needs another test.

Please see the response of above. We argued that the dust disturbance on K/Al ratio of Yellow River sediments can be ignored.

Further, The grain size effect is still a potential factor, and the grain size data help us understand this effect.

Thanks for your concern. In this work, all of our sediment samples of Yellow River and Site U1429 were analyzed on the clay-sized fraction (<2 μm). This avoids the grain-size effect on their chemical composition. Please see the description in lines 102-104 in Supplementary Information.

Fig. S2A REE pattern is sensitive to background geology and samples (grain size, river sediments? etc). Describe more about samples and locations of referenced samples other than Yellow River sediments. Without such information, readers cannot be convinced that the Yellow River is the only source (line 76).

Thanks for this suggestion. Information of samples in potential source regions has been added. We also changed some interpretations of source tracing based on REE. Please see lines 78-81 and 179-180 in Supplementary Information.

To calibrate the K/Al ratio with climatic values, the authors performed the PCA. I wonder how they performed on which variables.

We provided detail processes of the multiple statistical analyses. Please see lines 127-152 in Supplementary Information.

Reviewer #3 (Remarks to the Author):

I appreciate that the authors carefully respond to my concerns on their simulation results. Now I am convinced and agree with the explanation on the wet LGM over the southern China. Regarding the multivariate linear regression between temperature, precipitation and K/Al, the authors made a transformation for all the three variables before linear regression (lines 94-124 in the supplementary). I am satisfied with this method and this indeed improved the reliability in rainfall reconstruction. However, I am not sure if the regressions shown in Fig. S1 are updated. Pls check it. When this part is also confirmed, I am happy to see its acceptance.

Thanks for this reminder. We added the updated regression plot. Please see Supplementary Figure 1e.