Dear Editor Professor Hans Linderholm,

Thank you very much for your great editorial work. We are also very grateful to the two reviewers for their constructive comments and suggestions, which help to improve the quality of this manuscript significantly. We have revised the manuscript according to both reviewers' comments and suggestions. The following is the point-to-point response to the reviewers' comments. Correspondingly, the specific changes in the manuscript and supplementary material are highlighted in red and can be found in manuscript-markchanges.pdf and supplement-markchanges.pdf, respectively.

Best wishes,

Tao Wang and co-authors

Reply to Reviewer #1

We would like to thank the anonymous referee for his careful reviews and constructive comments and suggestions, which help to improve the quality of this manuscript significantly. We have revised the manuscript according to the referee's comments and suggestions. The following is the point-to-point response to the referee's comments.

Summary: The objective of the manuscript is to disentangle the precipitation variability in Asia over the past millennium by analyzing data from climate

model simulations driven by different forcing combinations and data from the last Millennium reanalysis Project. This latter data set results from polio data off-line assimilation into simulations with Earth-system models.

The main conclusion of the study is that a citation at multi-annual time stairs in this region displays a typo structure with northern Asia experiencing trial conditions and central and monsoonal Asia experiencing what conditions and vice versa. This structure is detectable in almost all climate simulations in the forcing and in the proxy-driven analysis. The authors conclude that this structure results from internal climate variability and is not associated with external forcing. The conclusion of the analysis is that this type of structure is associated with the Interdecadal Pacific Oscillation and with the impact of their sea surface temperature anomalies. Nevertheless, the authors also detect that this precipitation pattern is affected by the transition between the medieval climate anomaly and the Little Ice Age.

Recommendation:

The manuscript is very well written, the structure is very clear, and the analyses are all meaningful. Therefore, I am recommending the publication, but I do have a few comments that the authors may want to consider.

Main point:

1) The conclusion that the precipitation dipole is not affected by external forcing is not as solid as the authors believe. it is true that this structure appears in all empirical function analyses of almost all simulations regardless

of the external forcing. However, it is possible that the precipitation dipole, despite being produced by internal climate variability, might still be affected by the external forcing so that its time variations could be affected by phases of strong or weak volcanism or strong or weak solar output. To some extent, the study leaves this possibility open when the authors found that the time evolution of the precipitation dipole is affected by the Little Ice Age.

The model setup used by the alphas could also be used to ascertain the hypothesis that external forcing also affects the time of evolution of the precipitation dipole, and the ensemble of simulations that the authors have used there are several driven by all forces if the forcing affects the possibilation dipole it can component of the empirical function, namely, the principal component should display some correlation across all simulations driven by all forcings. If the forcing has no impact, then this correlation across the simulations should be very small. Therefore, there is a relatively easy way to support the initial conclusion of the study.

Response: We agree on your point. We calculated the correlations across the time series of the leading decadal precipitation mode (i.e., the principal components) simulated by CESM-LME 12 all-forcing simulations (Table R1). Except for autocorrelations for each principal component, the other correlations range from -0.06 to 0.35, and only 13.6% correlations are significant at the 95% confidence level. These relatively small correlations indicate the impacts of external forcings on the time variations of the leading decadal precipitation mode are very weak. The several

significant correlations suggest that, to a limited extent, the time variations of the leading decadal precipitation mode could be affected by external forcings (e.g., volcanic eruptions and solar radiation) (Ning et al., 2020; Xue et al., 2023). Thus, internal variability played a dominant role in shaping the time variations of the leading decadal precipitation mode. We have added some related discussion in the discussion section of revised manuscript. Thanks very much for your information and suggestion!

Tabel R1. The correlations across the time series of the leading decadal precipitation mode simulated by CESM-LME 12 all-forcing simulations.

Cor	**#002**	**#003**	**#004**	**#005**	**#006**	**#007**	**#008**	**#009**	**#010**	**#011**	**#012**	**#013**
#002	1.00	—	—	—	—	—	—	—	—	—	—	—
#003	0.10	1.00	—	—	—	—	—	—	—	—	—	—
#004	0.14	0.15	1.00	—	—	—	—	—	—	—	—	—
#005	0.17	0.17	0.19	1.00	—	—	—	—	—	—	—	—
#006	0.16	0.14	0.16	0.11	1.00	—	—	—	—	—	—	—
#007	0.16	0.17	0.20	0.21	0.19	1.00	—	—	—	—	—	—
#008	0.20	0.19	0.26*	0.24*	0.18	0.25*	1.00	—	—	—	—	—
#009	0.12	0.14	0.15	0.11	0.08	0.12	0.22*	1.00	—	—	—	—
#010	0.16	0.19	0.26*	0.23*	0.12	0.31*	0.35*	0.08	1.00	—	—	—
#011	0.03	0.19	0.24*	0.16	0.16	0.12	0.17	0.10	0.16	1.00	—	—
#012	-0.06	0.04	0.01	0.02	-0.06	-0.02	-0.02	0.05	0.04	0.04	1.00	—
#013	0.09	0.08	0.15	0.20	0.05	0.16	0.11	0.03	0.14	0.09	0.07	1.00

* denotes significant correlation at the 95% confidence level, except for autocorrelations.

Reference:

Ning, L., Chen, K., Liu, J., Liu, Z., Yan, M., Sun, W., Jin, C., and Shi, Z.: How do volcanic eruptions influence decadal megadroughts over eastern China? J. Climate, 33, 8195–8207, https://doi.org/10.1175/JCLI-D-19-0394.1, 2020.

Xue, J., Ning, L., Liu, Z., Qin, Y., Chen, K., Yan, M., Liu, J., Wang, L., and Li, C.: The combined influences of solar radiation and PDO on precipitation over eastern China during the last millennium, Clim. Dynam., 60, 1137–1150, https://doi.org/10.1007/s00382-022-06372-4, 2023.

Particular points:

2) 'Additionally, considering the superior performance of the Community Earth System Model (CESM) series in simulating Asian climate (Mishra and Aadhar, 2021; Ning et al., 2020; Xue et al., 2023),'

I would recommend rephrasing the sentence in a more specific way. In which sense is the CESM model superior? Does it produce better spatial patterns of precipitation or temperature or other recent trends, small realistic etc.. The judgements about the general superiority of a model about the models are usually unfair.

Response: Firstly, we apologize for the unfair judgement in the first version of manuscript. In the revised manuscript, we have replaced that sentence by "Additionally, to further explore the possible mechanisms underlying the linkage and the potential impacts of different external forcing factors, the Community Earth System Model Last Millennium Ensemble (CESM-LME, Otto-Bliesner et al., 2016) is also utilized because of its good performance in simulating Asian precipitation and summer monsoon (e.g., Hu et al., 2023; Mishra and Aadhar, 2021; Shi et al., 2018) and the availability of multiple samples forced by different forcing factors." (please

see lines: 102–108). In the revised sentence, we do not compare the performance of CESM-LME with the performance of other models anymore. The revised sentence emphasizes the good performance of CESM-LME in model evaluation. Thanks very much for your comment and suggestion!

Reference:

Hu, Y., Sun, W., Liu, J., Chen, D., Ning, L., and Peng, Z.: Decadal variability of precipitation over the Tibetan Plateau modulated by the 11-year solar cycle over the past millennium, Front. Earth Sci., 11, https://doi.org/10.3389/feart.2023.1137205, 2023.

Mishra, V. and Aadhar, S.: Famines and likelihood of consecutive megadroughts in India, npj Clim. Atmos. Sci., 4, 59, https://doi.org/10.1038/s41612-021-00219-1, 2021.

Shi, J., Yan, Q., and Wang, H.: Timescale dependence of the relationship between the East Asian summer monsoon strength and precipitation over eastern China in the last millennium, Clim. Past, 14, 577–591, https://doi.org/10.5194/cp-14-577-2018, 2018.

3) line 166 : measures the supply of soil water to the atmosphere. ‘

Evapotranspiration does not really measure the supply of soil water to the atmosphere but the atmospheric demand of water. This demand might be supplied if the soil is wet enough, but not necessarily

Response: Firstly, we apologize for the wrong statement in the first version of

manuscript. In the revised manuscript, we have replaced “the supply of soil water to the atmosphere” by “the atmospheric demand of water”. Thanks very much for your information and suggestion!

4) 164 ‘A larger aridity index indicates that relatively more moisture remains in the land, whereas a smaller aridity index represents drier condition’

This definition of the aridity index is real because the reader may assume that a larger ability in the index would indicate trial conditions and vice versa.

Response: In general, it is supposed that a larger value of an aridity index would indicate a drier condition. However, a larger value of the aridity index used in this study indicates a wetter condition. The aridity index here is defined with reference to Middleton and Thomas (1997), and this aridity index is widely used to produce the map of arid regions (e.g., Huang et al., 2016; Liu et al., 2019). To avoid misunderstanding, we wrote the above explanation (i.e., “A larger aridity index indicates that…”) right after the definition of this aridity index in the first version of manuscript. Thanks very much for your comment!

Reference:

Huang, J., Yu, H., Guan, X., Wang, G., and Guo, R.: Accelerated dryland expansion under climate change, Nat. Clim. Change, 6, 166–171, https://doi.org/10.1038/nclimate2837, 2016

Liu, S., Jiang, D., and Lang, X.: Mid-Holocene drylands: A multi-model analysis using Paleoclimate Modelling Intercomparison Project Phase III (PMIP3)

simulations, Holocene, 29, 1425–1438, https://doi.org/10.1177/0959683619854512, 2019.

Middleton, N. J. and Thomas, D. S. G.: World atlas of desertification, 2nd edn, Edward Arnold, London, The United Kingdom, 1997.

5) Their ensemble pattern was also consistent with the reconstruction (Fig. 1b).'

how was the ensemble pattern calculated? is the sample pattern the average of all leading EOF patterns or was it calculated by concatenating all simulations in time ?

Response: The ensemble pattern is the average of all leading EOF patterns simulated by CESM-LME 12 all-forcing simulations. In specific, we first calculated the leading EOF patterns in CESM-LME 12 all-forcing simulations, and then calculated the arithmetic mean of these 12 leading EOF patterns. In the first version of manuscript, we wrote the sentence "The analyses for the all-forcing simulations and the six subsets of single-forcing simulations were all based on the arithmetic mean of multiple members, which was the final step in the analyses." (This sentence can be seen in lines 139–141 of the revised manuscript) to explain the calculating processes. Thanks very much for your comment!

6) Conclusions discussion

In the present version of the manuscript, the discussion section comes after

the conclusions, which is strange. Usually, the conclusion section is the last section in the manuscript. Also, the discussion section is rather limited. I address just the difference between the Little Ice Age and other periods regarding the precipitation dipole. This is a Small Part of the analysis, and the discussion's main points should be actually devoted to the issues of the precipitation dipole, internal availability, and external forcing.

Response: We agree on your point. As mentioned in the previous response, we have added some discussion about the relative impact of internal variability and external forcings on the time variations of the leading decadal precipitation mode in the revised manuscript. And we have exchanged the order for the discussion section and conclusions section. Thanks very much for your comment and suggestion!

Reply to Reviewer #2

We thank the reviewer for his careful reviews, constructive comments and suggestions, which are important for us to improve this manuscript. We have revised the manuscript according to the reviewer's comments. The following is the point-to-point response to the reviewer's comments.

Summary:

Based on reanalysis and simulations of the last millennium, the existence of the linkage between decadal changes in precipitation in arid central Asia and humid Asian monsoon regions was ascertained in this paper. The decadal

linkage is characterized by the same changes in precipitation in arid central Asia and southern China, which were the opposite of those in the South Asian monsoon region and most of northern China. This paper also found that the internal variability associated with the Interdecadal Pacific Oscillation (IPO) plays a dominant role in connecting the decadal variations in precipitation between arid central Asia and monsoonal Asia by modulating the precipitation of their respective major rainy seasons. Besides, this decadal linkage of precipitation variation causes a similar decadal linkage between moisture changes in central Asia and monsoonal Asia.

Recommendation:

I think this paper is well written, well organized, and well diagramed. And this paper tried to ascertain and explain the observed decadal linkage between precipitation changes in Asian arid regions and monsoonal regions during the current period based on longer data (i.e., reanalysis and simulations of the last millennium), which is meaningful and interesting. However, I still have some comments. I think it is publishable after some comments in the following are considered.

Response: Thanks very much for your support and suggestions!

Main comments:

(I) The “time period 850–2005” in captions of several figures (e.g., Fig. 7 and 9)

is inaccurate, because the simulations forced by ozone and aerosols only cover the time period 1850–2005.

Response: Firstly, we apologize for the inaccurate statement. For the caption of Fig. 7, the brief title sentence for the whole figure (i.e., "The leading decadal precipitation mode for the time period 850–2005 in the control and single-forcing simulations.") has been replaced by "The leading decadal precipitation mode for the time period 850–2005 in the control and single-forcing simulations, with the exception of leading mode for the time period 1850–2005 in experiment forced by ozone and aerosols." in the revised manuscript.

For the caption of Fig. 9, the brief title sentence (i.e., "The simulated leading decadal aridity index mode for the time period 850–2005.") has been replaced by "The simulated leading decadal aridity index mode for the time period 850–2005 in all the experiments, with the exception of leading mode for the time period 1850–2005 in experiment forced by ozone and aerosols.".

Similar inaccurate statement also existed in the caption of Fig. S10. The brief title sentence for the whole figure (i.e., "The simulated leading decadal soil moisture mode for the time period 850–2005.") has been replaced by "The simulated leading decadal soil moisture mode for the time period 850–2005 in all the experiments, with the exception of leading mode for the time period 1850–2005 in experiment forced by ozone and aerosols.". Thanks very much for your comment!

(II) Section 3.3 Processes of the IPO modulating the leading precipitation

pattern is relatively long. It seems that this section is organized by the "Processes of the IPO modulating precipitation of major rainy seasons in central Asia" and "Processes of the IPO modulating precipitation of major rainy seasons in monsoonal Asia". It would be easier to follow if the authors subdivide this section into two further subsections by adding subsection titles.

Response: The section 3.3 has been divided into two parts (i.e., "3.3.1 Arid central Asia" and "3.3.2 Asian monsoon regions") in the revised manuscript. Thanks very much for your comment and suggestion!

(III) It is interesting that the IPO plays a dominant role in connecting the decadal variations both in precipitation and in moisture between arid central Asia and monsoonal Asia. Besides, the variations in moisture conditions result from the combined effect of precipitation and PET, as indicated by the aridity index (AI). Then I wonder how IPO affects the PET and whether the impact of IPO on PET positively contributes to the decadal linkage of moisture changes in central Asia and monsoonal Asia or not.

Response: Figure R1 shows the PET anomalies during the positive phases of the IPO in all the experiments. The PET anomalies associated with the positive IPO in all the experiments showed negative anomalies in arid central Asia and southern China and positive anomalies in the South Asian monsoon region and most of northern China. These PET anomalies contribute to wetter conditions in arid central Asia and southern China and drier conditions in the South Asian monsoon region and most of northern

China, which is consistent with the contributions of precipitation anomalies associated with the positive IPO. Thus, the impact of IPO on PET also positively contributes to the decadal linkage of moisture changes in central Asia and monsoonal Asia. However, the variations in PET can be determined by many factors (i.e., near-surface temperature, available energy, relative humidity, wind speed) according to the Eq. (2). This suggests that the impact of IPO on PET, especially the processes of IPO modulating the PET changes needs more in-depth analyses. We will try to do these analyses in detail in another study. Thanks very much for your comment and suggestion!

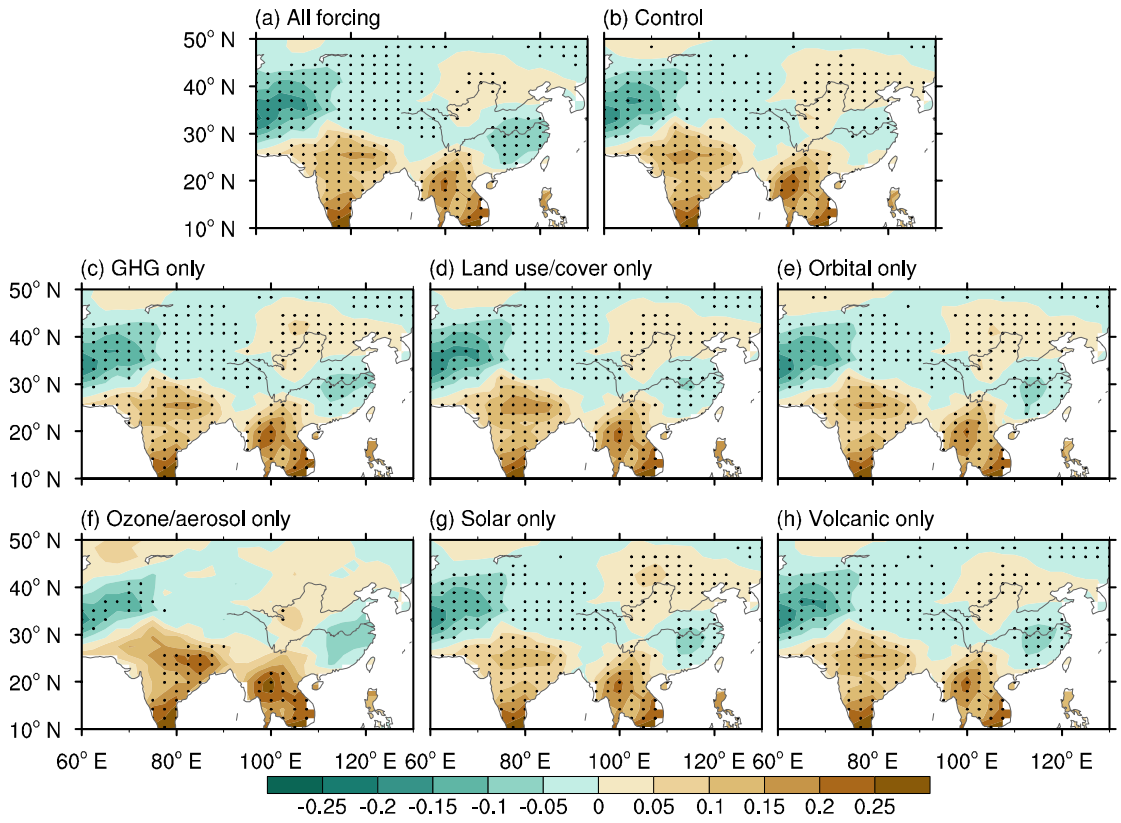

Fig. R1. Simulated PET anomalies during the positive phases of the IPO. The PET anomalies (units: mm day^{-1}) regressed onto the time series of the IPO index in the **(a)** all-forcing simulations, **(b)** control simulation, and **(c–h)** six subsets of the

single-forcing simulations. The dots in part **(b)** show significant anomalies at the 95% confidence level and the dots in parts **(a, c–h)** denote that at least two-thirds of the members simulate significant changes (at the 95% significance level), and these significant changes agree on the sign of the average value.

Line by line comments:

Line 21 ('output' can be "outputs")

Response: Modify accordingly! Thanks very much for your suggestion!

Line 66 ('EOF1' can be "the first leading mode (EOF1)")

Response: Modify accordingly! Thanks very much for your suggestion!

Line 67 ('LMR' can be "Last Millennium Reanalysis (LMR)")

Response: Modify accordingly! Thanks very much for your suggestion!

Line 104 ('this study will also utilize CESM…' can be "this study also utilizes CESM…")

Response: In the revised manuscript, we have replaced that sentence by "Additionally, to further explore the possible mechanisms underlying the linkage and the potential impacts of different external forcing factors, the Community Earth System Model Last Millennium Ensemble (CESM-LME, Otto-Bliesner et al., 2016) is also utilized because of its good performance in simulating Asian precipitation and

summer monsoon (e.g., Hu et al., 2023; Mishra and Aadhar, 2021; Shi et al., 2018) and the availability of multiple samples forced by different forcing factors.” (please see lines: 102–108). The future tense of that initial sentence has been changed to present tense. Thanks very much for your suggestion!

Line 254 (‘Last Millennium Reanalysis dataset’ can be “LMR”)

Response: Modify accordingly! Thanks very much for your suggestion!

Line 492 (‘abovementioned’ can be “aforementioned”)

Response: Modify accordingly! Thanks very much for your suggestion!

Line 558 (‘above-mentioned’ can be “aforementioned”)

Response: Modify accordingly! Thanks very much for your suggestion!