This paper uses observations, re-analysis output and model simulations to examine interdecadal variability in the East Asian monsoon boundary zone, particularly in precipitation. The authors find that the cold phase of the Indian Ocean basin mode prompts anomalous cyclonic circulation over the north-eastern Indian Ocean, which ultimately enhances moisture transport from the Pacific Ocean to the boundary zone. I apologise to the authors for the delay in submitting this review.

Many thanks for your constructive and valuable comments, which have greatly improved our manuscript.

I have a few a few comments about this paper, primarily about the presentation and discussion. I recommend major revisions.

We have revised the manuscript based on your comments. The revisions are highlighted in red color in the revised manuscript. In the following, we summarize our point-by-point replies to your comments.

This paper uses far too many acronyms: I counted 30 in total. While some are fine to keep – if they are mentioned more than five or so times – others are used sparingly. Unfortunately, this makes large sections of the paper very difficult to follow: I spent a great deal of time trying to remember what each acronym was, or flicking backwards to look it up again. Consequently, I found the science message was often unclear. (And as something of an aside, a couple of acronyms were poorly chosen: EU commonly means European Union, and P-E could be confused with “precipitation minus evaporation”, particularly to an audience of atmospheric scientists.)

Reply: We have removed the sparingly used acronyms (e.g., NH, AWJ, WNP, and ENSO) and kept the frequently used acronyms in the revised manuscript. For easy reading and reviewing, we have included the “Glossary of acronyms” in the Supplementary File. Please see the Glossary of acronyms in the Supplement File.

Furthermore, as you proposed, a couple of acronyms (i.e., EU and P-E) were poorly chosen owing to unclear science message conveying. Therefore, we abandoned these acronyms in the revised version.

The coastlines plotted on the figures are very faint: it is difficult to pick out the important features when it’s unclear where they are. Also, axes and colour bars should be labelled on the figures as well as in the captions. And in Figure 6, hatching is used to indicate significance, whereas in other figures the authors use dots: please use just one for consistency.

Reply: We have modified associated figures for more conspicuous coastlines. For the consistency, we abandoned the hatching in **Figure 6**. Throughout the revised manuscript, we use grey dots to indicate significance. Please see the modified figures in the revised version.

Moreover, you mentioned that axes and colour bars should be labelled on the figures as well as in the captions. After checking the papers regarding the climate dynamics that published in ACP, we found that the layout of our figures is quite consistent with those papers. For example, we scrutinized the axes and colour bars in **Figure 3** in the Paper "Yu, L., Zhong, S., Vihma, T., Sui, C., and Sun, B.: A change in the relation between the Subtropical Indian Ocean Dipole and the South Atlantic Ocean Dipole indices in the past four decades, Atmos. Chem. Phys., 23, 345–353, https://doi.org/10.5194/acp-23-345-2023, 2023".

We thus think that the layout (i.e., axes and colour bars) of our figures could be suitable.

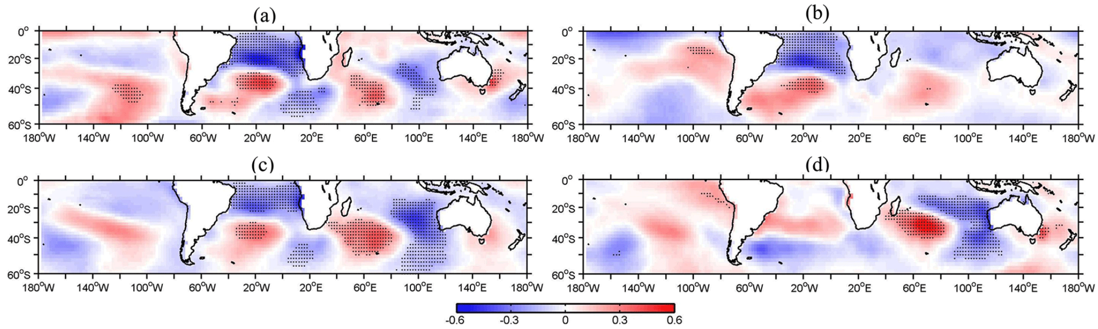

Figure 3 Regression maps of the SST anomalies (◦C) on the summertime indices of (a, b) SAOD and (c, d) SIOD over the periods of (a, c) 1979–1999 and (b, d) 2000–2020. Dots denote the regions where the confidence level is above 95 %. (from Yu et al., 2023, ACP)

I think the authors need to include more details about the model simulations described in Section 2.6 They subtract one set of simulations from the other, but it is not clear to me how this achieves the authors' stated goal (line 205). Please. Explain make clear how the two sets of simulations are different: why is internal variability arising from Indian Ocean SSTs unique to one simulation and not the other? And indeed – and I apologise if I have missed something – it is unclear how these models are used subsequently. I think it would be helpful to note, as the results are discussed, which data sets are being used at each point.

Reply: Thanks for your constructive comments and queries. Please see **Line 213-219** for the answers. More details about CESM1_LENS and CESM1_IOPES can be found in Kay et al. (2015) and Yang et al. (2020), respectively.

Line 213-219

"As indicated by Yang et al. (2020), the CESM1_LENS 35-member ensemble mean results can better provide an estimate of the influence of the external radiative forcing signals (e.g., greenhouse gas) on the climate system. Furthermore, the 10-member ensemble mean results in CESM1_IOPES contain the responses to both external forcings and the observed SST variations over the TIO domain (Yang et al., 2020). Therefore, by subtracting the CESM1_LENS ensemble mean from the

CESM1_IOPES ensemble mean, we can obtain responses of the climate system to the internal variability stemming from the time-varying TIO SSTAs, distinguishing the impact of external radiative force changes from the intrinsic variability driven by TIO SSTAs."

Reference:

Kay, J.E., Deser, C., Phillips, A., Mai, A., Hannay, C., Strand, G., Arblaster, J.M., Bates, S.C., Danabasoglu, G., Edwards, J., Holland, M., Kushner, P., Lamarque, J.F., Lawrence, D., Lindsay, K., Middleton, A., Munoz, E., Neale, R., Oleson, K., Polvani, L. and Vertenstein, M., 2015. The community earth system model (CESM) large ensemble project: a community resource for studying climate change in the presence of internal climate variability. Bulletin of the American Meteorological Society, 96(8): 1333-1349.

Yang, D., Arblaster, J.M., Meehl, G.A., England, M.H., Lim, E.-P., Bates, S. and Rosenbloom, N., 2020. Role of tropical variability in driving decadal shifts in the Southern Hemisphere summertime eddy-driven jet. Journal of Climate, 33(13): 5445-5463.

Furthermore, we used the model simulations (see the subsequent **Section 3.4**) to validate our proposed mechanisms of how the remote IOBM modulates the summertime EAMBZ precipitation at interdecadal timescales, aiming at providing more confident results. In other words, the statistical result has to be compared against the numerical model result. If they are consistent, we could confidently indicate that our proposed mechanisms are reliable.

The physical-based empirical model (Section 3.4) appears to be behind a key result of the paper: it is mentioned in the abstract. But this section feels rather brief. Could the authors perhaps discuss the implications of their model a bit further? They say it captures some of the observed interdecadal variability: what about its shortcomings? How is this result helpful?

Reply: Thanks for your insightful comments. We have extended the discussion concerning the shortcomings and the helpful aspect of our proposed physical-based empirical model in Section 3.5. Please see **Line 411-416** and **Line 461-466** in the revised version.

Line 411-416

"Although our proposed physical-based empirical model could confirm the concurrently intimately interdecadal relationship between IOBM and EAMBZ precipitation, we should acknowledge the shortcomings of the model. First, the amplitudes of the hindcast estimates are fairly lower, which cannot well capture the extreme precipitation years (e.g., years around 1960; Fig. 11). Second, the simultaneous signal of IOBM cannot be served as a predictor for summertime EAMBZ precipitation variations. As such, this model inherently lacks the ability to predict the interdecadal EAMBZ precipitation anomalies in advance."

Line 461-466

"Second, this study merely identifies the physical linkage between the interdecadal summer EAMBZ precipitation and the contemporaneous SST mode over the TIO basin from the tropical route. Nonetheless, the contemporaneous IOBM is not a predictor. According to many previous studies (e.g., Wang et al., 2015; Li et al., 2023), the physical-based empirical model based on multiple predictors may better improve the forecast skill. Thus, it is urgent to find out more salient precursor signals of the lower boundary anomalies [e.g., sea ice (Han et al., 2021)] and figure out associated mechanisms for interdecadal EAMBZ precipitation changes to construct an effective prediction model."

Reference:

Wang, B., Xiang, B., Li, J., Webster, P.J., Rajeevan, M.N., Liu, J. and Ha, K.-J., 2015. Rethinking Indian monsoon rainfall prediction in the context of recent global warming. Nature Communications, 6(1): 7154.

Li, J., Zheng, C., Yang, Y., Lu, R. and Zhu, Z., 2023. Predictability of spatial distribution of pre-summer extreme precipitation days over southern China revealed by the physical-based empirical model. Climate Dynamics, 61(5): 2299-2316.

Han, T., Zhang, M., Zhu, J., Zhou, B. and Li, S., 2021. Impact of early spring sea ice in Barents Sea on midsummer rainfall distribution at Northeast China. Climate Dynamics, 57(3): 1023-1037.

And related to the above point, I think the discussion (Section 4) could be improved. At present, it summarises the results – which are they summarised again in the Conclusion – but gives little sense of how the results fit into existing knowledge. How does this work move the field forward? What questions arise from it?

Reply: Thanks for your constructive comments. We modified the Discussion Section in the raw manuscript into Section 3.4 "Results from CESM1 simulations", with the aim of confirming our proposed mechanisms based on the statistical results. Notably, we have improved the Section 3.4 in the revised manuscript, avoiding the repeated summary of the results. Please see **Line 376-389** in the revised version.

Line 376-389

"3.4 Results from CESM1 simulations

In this subsection, we use the pacemaker experimental data based on the ensemble mean of CESM1_IOPES and CESM1_LENS to validate our proposed mechanisms regarding the modulation of IOBM cooling on the interdecadal enhancement of summer EAMBZ precipitation. Considering the predominant role of southerly anomalies over the key monsoonal southerly domain, we therefore emphasize the low-level (850 hPa) atmospheric anomalies at interdecadal timescales tied to the IOBM-like SST cooling, as depicted in Fig. 10. We can observe a clearly anomalous cyclonic circulation around the northeast corner of TIO, accompanied by local positive precipitation anomalies and easterly anomalies that stretch from SWP to its northern flank, which are generally resembled those in the observation (Figs. 7b and 9b). In this circumstance, a similar "north-low–south-high" meridional seesaw pattern over the Northeast China–SWP sector can be formed to spark and sustain the enhanced EAMBZ precipitation in boreal summer (Fig. 10). In summary, by and large, the ensemble mean composite results can well reproduce the observed anomalous circulation and precipitation driven by IOBM-related SSTAs, confirming the crucial role of IOBM cooling in driving enhanced summer precipitation over EAMBZ at interdecadal timescales."

As for your concerned questions, please see **Line 452-459** in the revised version.

Line 452-459

"The following two points deserve further discussion. First, although results from CESM1_LENS and CESM1_IOPES can reasonably confirm our proposed physical pathway of how IOBM cooling exerts a distant modulation on the interdecadal enhancement of summer precipitation over EAMBZ, we can still notice the weakness of the model simulations. That is, positive precipitation anomalies around the northeast corner of TIO and the easterly anomalies exhibit weaker magnitudes compared to the observations (Fig. 10 vs. 7b and 9b). Besides, systematic biases exist regarding the simulated positions of the upper (lower) tropospheric divergence (convergence) and negative (positive) RWS anomalies (Fig. S6), manifesting themselves in the eastward displacement tendency in contrast to those around the northeast corner of the TIO (Fig. 8)."