

General comments:

1. Presentation and interpretation

1a. The writing style is unusually dense and, in my view, does not fit well with the style of this journal. The manuscript reads as though it was originally prepared for a high-impact journal and subsequently submitted here without substantial adaptation. This makes the paper unnecessarily difficult to follow.

For example, the paper contains only five figures, one of which is a schematic accompanied by a scatter plot that is not clearly explained or discussed. At the same time, the discussion of the proposed “mechanism” is very terse (e.g., lines 261–276 and 287–300), making it difficult to understand how the authors arrive at their interpretation. Adding intermediate figures that explicitly walk the reader through the different steps of the analysis would substantially improve the manuscript.

Response: We thank the reviewer for this valuable feedback on manuscript readability and stylistic presentation, and we fully acknowledge the concerns about dense narrative and insufficient graphical guidance through the analytical steps.

The work was initially submitted to npj journals, and was recommended by the handling editor there for submission to Atmospheric Chemistry and Physics given its thematic fit with the journal’s scopes. We had made preliminary adjustments before submission, but we recognize that further adaptation was needed to match the readability expectations and stylistic conventions of ACP. We have now carried out extensive revisions in direct response to your comments, as detailed below.

We believe these revisions substantially improve the readability and clarity of the manuscript, and better align the presentation with the style of Atmospheric Chemistry and Physics. We appreciate the reviewer’s guidance in helping us strengthen this aspect of the work.

1b. The tone of the manuscript is also too strong in several places, with conclusions that are more definitive than the analysis appears to support. For example, the first sentence of the abstract describes a “long-standing puzzle of the statistically weak linkage between the stratospheric polar vortex (SPV) and SC rainfall extremes.” I am not convinced that this constitutes a “long-standing puzzle.” It is certainly a worthwhile research question, but this wording overstates the motivation. Similarly, the use of “firmly established” on line 231 is stronger than warranted by the results.

Response: We thank the reviewer for pointing out this important issue regarding the

strength of our statements and we have carefully revised the wording throughout the manuscript to better align claims with the level of evidence. Specifically:

In the first sentence of the abstract, we have revised “a long-standing puzzle of the statistically weak linkage” to “a long-standing challenge in understanding the statistically weak linkage” between the stratospheric polar vortex and SC rainfall extremes (see Lines 19-20). This revised phrasing more accurately reflects that this is an outstanding and worthwhile research question, without overstating its status as a widely recognized “puzzle” in the field.

In line 237, we have replaced “firmly establishes” with indicates, to properly reflect that the CMIP6 multi-model analysis provides supporting evidence consistent with our proposed dynamical pathway, rather than offering definitive, unequivocal proof.

We believe these revisions substantially improve the rigor and appropriateness of the manuscript’s tone, ensuring that all conclusions are calibrated to the strength of the underlying analysis.

1c. More fundamentally, I do not think the analysis presented here constitutes a mechanistic explanation of the interaction between the MJO and SPV. Rather, it provides a diagnosis showing that the precipitation anomalies are dynamically and hydrologically consistent with changes in the subtropical circulation and moisture convergence. This is useful, but it is different from establishing a mechanism for how the MJO and SPV interact nonlinearly.

For example, the paper does not address several important physical questions: Why should an EESPV influence precipitation over South China in the first place? Why is the MJO-related wavetrain extending into the subtropics stronger during EESPV events? The authors do not necessarily need to answer these questions for the paper to be publishable, but the terminology should be adjusted accordingly. In particular, I would avoid referring to the diagnostics as establishing a “mechanism” (e.g., line 248) and instead describe them as evidence consistent with a particular dynamical pathway. The authors also invoke a downward arching of QBO winds from the subtropics. However, this effect is barely evident during December and January in reanalysis data (Garfinkel et al., 2026). Is the proposed pathway also evident when the analysis is restricted to DJF, or is it primarily present in February?

As an aside, Ning et al. (2026) recently examined how shifts of the vortex toward or away from Eurasia can affect precipitation using mechanistic model experiments. This study appears highly relevant to the interpretation presented here and should be

discussed.

There is also a concern regarding multiple testing. The authors effectively construct 16 different composites, and it is not surprising that some subsets show statistically significant differences. The authors should provide a stronger physical motivation for considering all of these combinations rather than presenting them primarily as an exploratory search. In addition, some of these composites must be based on relatively small sample sizes, given that reliable MJO observations are available only since approximately the 1970s. I think the authors should include an appropriate field-significance or multiple-testing correction that accounts for the number of hypotheses being tested.

Response:

① We sincerely thank the reviewer for this perceptive and constructive comment, which greatly helps us refine the rigor of our physical interpretation and academic wording. We have carefully revised the relevant terminology throughout the manuscript to address the physical questions raised. We have systematically calibrated our wording to accurately reflect the diagnostic nature of this study. Specifically, the phrase “synergistic dynamical mechanism” in line 260 has been revised to “synergistic dynamical pathway”.

② We also sincerely thank the reviewer for these constructive and insightful comments, and for drawing our attention to the highly relevant study by Ning et al. (2026). We have fully addressed both issues in the revised manuscript, with additional analysis and expanded discussion as detailed below:

We acknowledge the important point that the downward arching of QBO westerly winds from the subtropics is relatively weak in December and January, as documented in Garfinkel et al. (2026). To clarify whether our proposed dynamical pathway is evident throughout DJF or primarily in February, we have extended our analysis to compare the DJF seasonal mean and February-only patterns, and added this comparison as follows (see Revised Supplementary Fig.R1). The figures show correlation coefficients between the SPV index and zonal wind anomalies averaged over 30°W–150°E, for both DJF and February. Our results confirm that the downward-arching QBO westerly structure exists in both DJF and February.

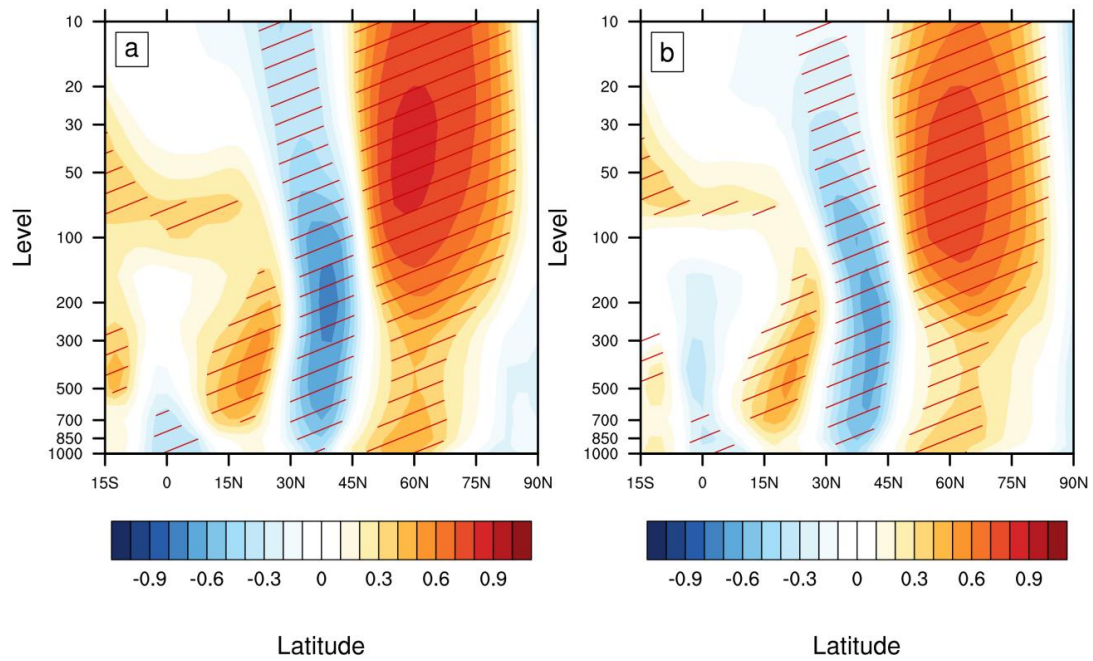


Figure R1. Pressure-latitude cross section of correlation coefficients between the (a) DJF and (b) February zonal wind anomalies averaged over 30°W-150°E and the ESSPV index. Red slashes indicate significant correlation coefficients at the 95% confidence level.

Following this suggestion, we have incorporated a discussion of this work in the Discussion section of the revised manuscript, together with the full citation. The added text reads (see Lines 425-428): “Existing literature has simulated the impacts of stratospheric polar vortex displacements on Eurasian precipitation using mechanistic model experiments (Ning et al., 2026), and the detailed dynamical pathways underlying this linkage warrant further investigation.” We have also updated the reference list to include Ning et al. (2026). We believe this addition strengthens the contextual framing of our results and properly acknowledges the close connection between prior modeling work and our observational findings.

Reference added:

Ning, W., Garfinkel, C. I., Cohen, J., White, I. P., & Rao, J. (2026). The tropospheric response to zonally asymmetric momentum torques: implications for the downward response to wave reflection and SSW events. *Weather and Climate Dynamics*, 7(1), 277-295. <https://doi.org/10.5194/wcd-7-277-2026>

③We sincerely thank the reviewer for raising this critical and constructive concern regarding multiple testing, sample size justification, and the physical rationale of our composite design. We have fully addressed each issue in the revised manuscript, with detailed clarifications and figure modifications as described below.

First, we explicitly report the number of daily samples for each composite group, and have added these sample sizes to the top-left corner of each panel in revised Figure 2b for full transparency:

- Under the ESSPV regime: 121 (MJO Phase 1), 136 (MJO Phase 2), 146 (MJO Phase 3), 120 (MJO Phase 4), 115 (MJO Phase 5), 119 (MJO Phase 6), 126 (MJO Phase 7), 109 (MJO Phase 8) days.
- Under the EWSPV regime: 196 (MJO Phase 1), 172 (MJO Phase 2), 214 (MJO Phase 3), 206 (MJO Phase 4), 163 (MJO Phase 5), 161 (MJO Phase 6), 186 (MJO Phase 7), 178 (MJO Phase 8) days.
- For the combined Phase 2–5 composites analyzed in Figure 3: 517 days (ESSPV), 755 days (EWSPV), and 2043 days (all NDJFM MJO Phase 2–5, as the climatological reference).

As the reviewer correctly noted, the sample size is fundamentally constrained by the availability of reliable satellite-observed outgoing longwave radiation (OLR) data. We use the NOAA daily OLR dataset spanning 1979–2022 (44 boreal winter seasons, NDJFM), which is the standard data length for MJO research in the peer-reviewed literature; consistent, high-quality OLR measurements for MJO index calculation are not available before the late 1970s.

Importantly, sample sizes on the order of 100–400 days per composite are widely accepted and standard in studies of intraseasonal oscillation and stratosphere-troposphere interaction (e.g., Chen et al., 2020; Chen et al., 2021; Kim et al., 2020; Zheng et al., 2020). These sample sizes provide sufficient statistical power to detect robust intraseasonal circulation and precipitation signals, as evidenced by the large body of published work using comparable or even smaller sample sizes.

References

- Chen, X., Li, C., Li, L., Yu, P., & Yang, M. (2020). Interannual variations of the influences of MJO on winter rainfall in southern China. *Environmental Research Letters*, 15(11), 114011.
- Chen, Y. L., Sui, C. H., Chang, C. P., & Tseng, K. C. (2021). Effect of the MJO on East Asian winter rainfall as revealed by an SVD analysis. *Journal of Climate*, 34(24), 9729-9746.
- Kim, H., Son, S. W., & Yoo, C. (2020). QBO modulation of the MJO-related precipitation in East Asia. *Journal of Geophysical Research: Atmospheres*, 125(4), e2019JD031929.
- Zheng, Y., Zhang, Q., Luo, M., Sun, P., & Singh, V. P. (2020). Wintertime

precipitation in eastern China and relation to the Madden-Julian oscillation: Spatiotemporal properties, impacts and causes. *Journal of Hydrology*, 582, 124477.

2. Definition of EESPV and EWSPV

The authors classify entire winters as EESPV or EWSPV. However, the morphology of the polar vortex can change substantially within a winter, and it is entirely possible for one month to be characterized by an EESPV configuration while the following month is characterized by an EWSPV configuration. Would the results be more robust or physically meaningful if the SPV classification were performed at the monthly rather than seasonal timescale?

Given that the MJO is already defined using daily data, using monthly EESPV/EWSPV classifications should not require a major change to the methodology. At minimum, I think the authors should test whether their main results are sensitive to this choice.

Response: We thank the reviewer for this very insightful and constructive methodological suggestion. We fully agree that the polar vortex exhibits pronounced intraseasonal variability, and its morphology can shift substantially between months within a single winter. It is indeed necessary to test whether our main conclusions are sensitive to the timescale of SPV classification. As suggested, we have performed a full set of analyses using monthly ESSPV/EWSPV classification, and the results confirm the overall robustness of our findings.

Specifically, we first defined a monthly ESSPV index as the area-averaged 50-hPa geopotential height anomaly over the Eurasian sector (50°N–80°N, 20°E–100°E) for each individual month. Monthly ESSPV and EWSPV cases were then identified using the same threshold as the seasonal classification (standardized index $< -0.7\sigma$ for ESSPV and $> 0.7\sigma$ for EWSPV), ensuring direct comparability with the original results. As the reviewer noted, this adjustment only modifies the temporal dimension of the SPV grouping, and the entire analytical framework (compositing, regression, and MJO phase partitioning) remains unchanged.

The results from the monthly-scale classification are largely consistent with those from the seasonal-scale analysis, indicating that our core conclusions are not sensitive to the choice of SPV classification timescale.

Spatial pattern of the SPV–precipitation linkage The regression pattern of SC R90 onto 50-hPa geopotential height at the monthly timescale still exhibits a structure corresponding to a Eurasian-shifted polar vortex. The location of the significant correlation region is largely consistent with the seasonal result, although the

amplitude of the correlation coefficient is slightly reduced due to enhanced intraseasonal noise at the monthly scale. The overall spatial structure, however, remains robust (see Fig R2).

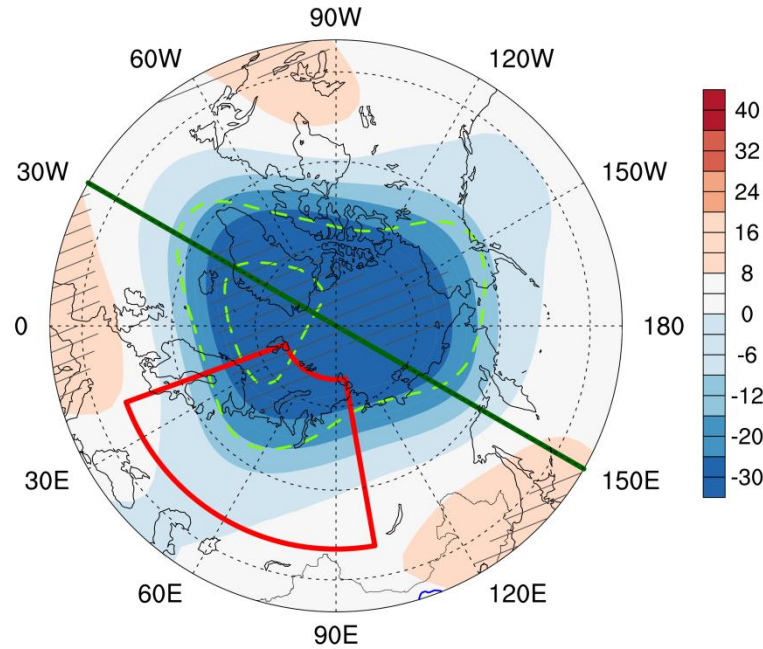


Figure R2. Regressed monthly 50-hPa geopotential height anomalies (shading, unit: gpm) and correlation coefficients (green dashed contours; interval -0.05 , starting from -0.1) onto the regionally averaged R90 anomaly over SC during NDJFM. White slashes indicate regressed geopotential height anomalies significant at the 95% confidence level.

Phase-dependent MJO modulation The composite precipitation patterns across MJO phases 1–8 under monthly ESSPV/EWSPV backgrounds closely resemble the seasonal results (see Fig R3). Under the monthly ESSPV regime, MJO phases 2–5 still produce significantly enhanced precipitation over South China, while phases 6–8 and phase 1 show no consistent enhancement. Under the monthly EWSPV regime, the precipitation response during MJO phases 2–5 is substantially weakened and even reversed to negative anomalies. This phase-dependent characteristic is highly consistent with the original seasonal-scale analysis.

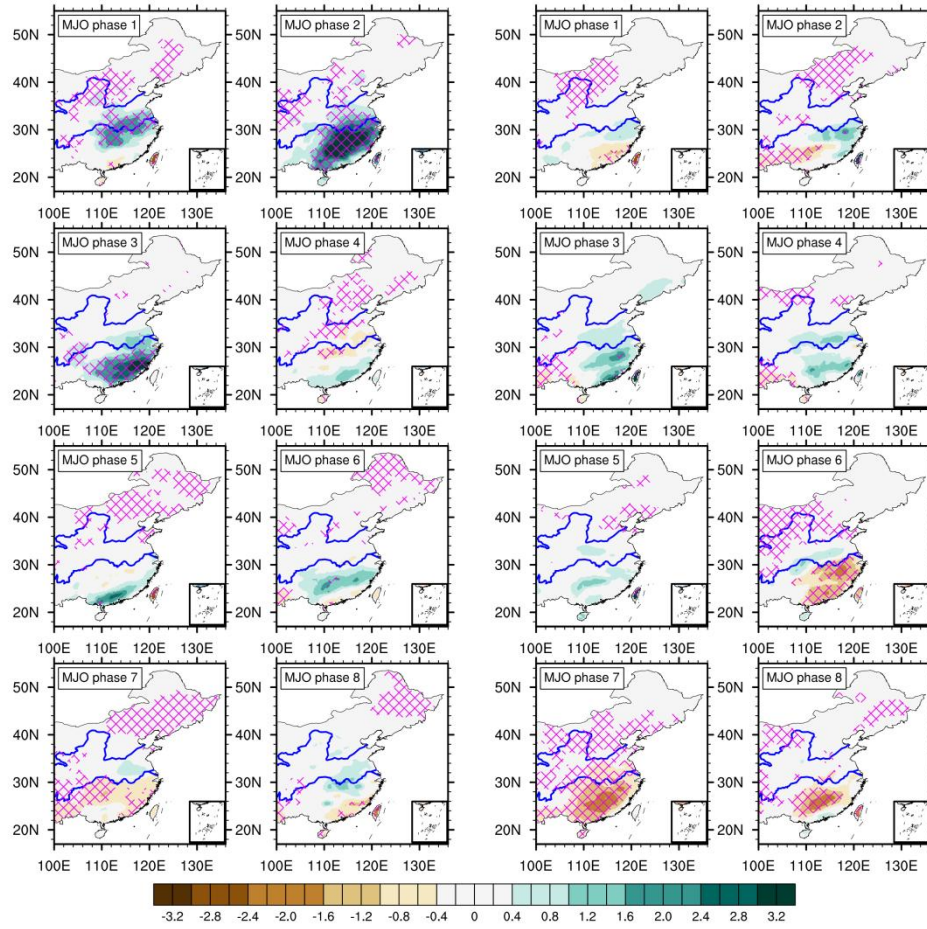


Figure R3. Composite evolution patterns of MJO phases 1-8 precipitation anomalies (unit: mm day^{-1}) associated with ESSPV (left) and EWSPV (right) months. Meshes indicate significant precipitation anomalies at the 95% confidence level.

Probability distribution of regional precipitation The probability density distributions of regionally averaged SC precipitation show the same qualitative behavior: during MJO phases 2–5, the 90th, 75th, and 50th percentiles of daily precipitation are all higher under monthly ESSPV conditions than under EWSPV conditions. The skewed distribution toward higher precipitation in ESSPV winters remains evident at the monthly timescale (see Fig R4).

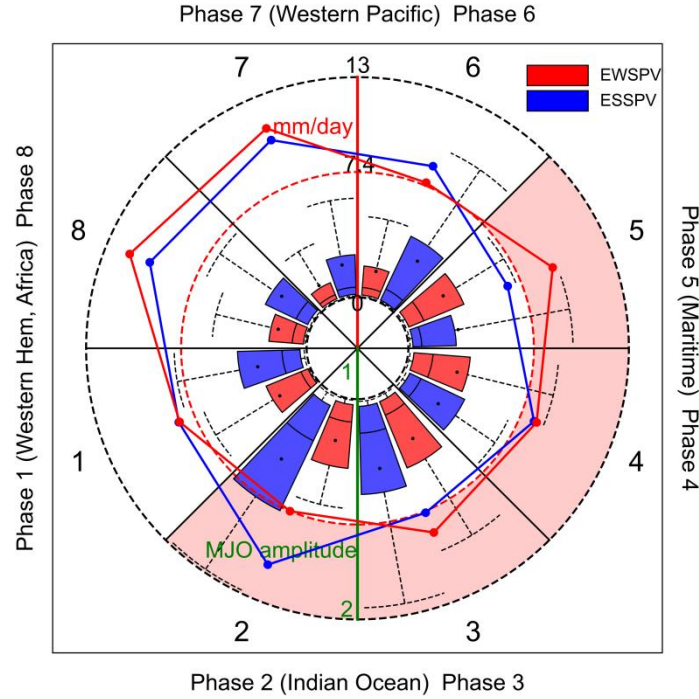


Figure R4. Probability density distributions of daily precipitation in SC during MJO phases 1-8 in ESSPV (blue box) and EWSPV (red box) months. The box bounds indicate the 10th, 25th, 50th, 75th, and 90th percentiles, and dots indicate the mean. The solid dot octagons denote the average amplitude of MJO in ESSPV (blue) and EWSPV (red) months. The red dashed line indicates the wintertime 90th percentile threshold for SC precipitation (about 7.4 mm day^{-1}).

From a physical perspective, the seasonal-scale ESSPV characterizes the mean zonal displacement of the polar vortex throughout the winter, reflecting the large-scale background state. The monthly classification captures shorter-term polar vortex meanders within the season. The consistency between the two timescales suggests that the phase-locked modulation of the SPV–precipitation linkage by the MJO operates across both interannual and intraseasonal SPV variations, reflecting the robustness of this dynamical coupling. We also note that the statistical significance is slightly weaker at the monthly scale, which is expected because monthly data contain more synoptic-scale noise. The seasonal average acts as a low-pass filter that better isolates the large-scale modulation effect of the polar vortex displacement.

We sincerely appreciate the reviewer’s constructive comments. The above analyses and sensitivity tests confirm that our core findings are not sensitive to the choice of SPV classification timescale, whether the ESSPV/EWSPV classification is performed at the monthly or seasonal timescale.

3. The MJO–precipitation relationship needs to be examined more thoroughly

The authors first establish a relationship between SPV configuration and South China precipitation and then investigate how the MJO modifies this relationship. However, what is largely missing is an equivalent analysis of the MJO–South China extreme precipitation relationship independent of the stratosphere.

The top row of Figure 3 provides some analysis of this possibility, but the treatment is substantially less extensive than the analysis of the SPV–precipitation relationship, which occupies essentially all of Figure 1. As a result, the paper currently feels somewhat imbalanced. A more thorough characterization of the MJO–precipitation relationship independent of SPV state would help establish what is genuinely nonlinear about the combined response.

Response: We thank the reviewer for this insightful and constructive comment. We fully agree that a thorough characterization of the MJO–South China extreme precipitation relationship independent of stratospheric forcing is essential to clarify the nonlinear nature of the combined SPV–MJO interaction, and it also greatly improves the structural balance of the manuscript. We have supplemented the relevant analysis and added corresponding textual description in the revised manuscript.

Specifically, at the beginning of the section describing the synergistic dynamical pathways (originally near line 215, before presenting the composite results under different SPV backgrounds), we have added a paragraph to characterize the climatological standalone MJO–precipitation linkage (see Lines 217–221): “The MJO convection over the Indian and western Pacific Oceans propagates eastward as coupled Kelvin–Rossby waves, which robustly modulate rainfall and surface temperature across low and mid-latitudes. During MJO phases 2–5, winter precipitation over SC is significantly enhanced, whereas it is markedly suppressed during phases 6–8 and 1 (Fig. S6 and S7) (Long et al.,2025; Song et al., 2020).”

This addition presents the independent modulation of MJO on South China precipitation under climatological conditions, which forms a symmetric narrative structure with the independent SPV–precipitation analysis detailed in Figure 1. More importantly, with this baseline reference, the genuinely nonlinear component of the combined response becomes evident.

We believe this revision substantially improves the balance of the manuscript and strengthens the logical rigor of the nonlinear interaction argument.

References

Long, Y., Ling, J., Li, C., Chen, G., Zhang, G., Tian, Q., ... & Zhu, Y. (2025).

Evolution of MJO structures during eastward propagation. *Theoretical and Applied Climatology*, 156(10), 528.

Song, L., & Wu, R. (2020). Distinct Eurasian climate anomalies associated with strong and weak MJO events. *International Journal of Climatology*, 40(15), 6666-6674.

4. Model anomaly calculation

Line 108: Why are the model anomalies calculated relative to the observed/ERA5 climatology rather than the climatology of each individual model? The methodology as currently described seems likely to introduce or amplify the influence of model mean-state biases. Unless there is a specific physical reason for using the observational climatology, I think the authors should calculate anomalies relative to each model's own climatology and explain the implications of this choice.

Response: We thank the reviewer for raising this critical methodological question. The choice of observational climatology as the common reference is physically motivated by the core objective of this study. The primary goal of this study is to evaluate how well CMIP6 models reproduce the observed dynamical linkage between the ESSPV, MJO, and SC extreme precipitation, rather than to assess their simulation of the mean climatological state. If anomalies were calculated relative to each model's own climatology, the definition of the ESSPV index threshold, the reference spatial pattern of the MJO, and even the 90th percentile threshold for extreme precipitation (R90) would all differ across models. These differences in baseline state and threshold definition would introduce confounding factors into the inter-model comparison, making it difficult to disentangle differences in the model's ability to simulate the dynamical linkage from differences in their intrinsic mean-state biases.

By using the observational 1981–2010 climatology as a unified reference frame, we place all models on an equal footing for inter-model comparison. This approach allows us to more cleanly isolate each model's fidelity in reproducing the physical coupling process.

5. Journal suitability

Overall, I think the paper is more naturally suited to *Weather and Climate Dynamics* than *Atmospheric Chemistry and Physics*. However, I will leave the final decision on journal suitability to the editor.

Response: This work focuses on the coupling of the troposphere and the stratosphere on extreme events, which is within the scope of APC, such as *Exploring impacts of troposphere–stratosphere coupling processes and challenges of forecasting extreme*

events in the Asian summer monsoon in a changing climate; Altitude Range : Troposphere, Stratosphere, Mesosphere, and Dynamics: Large-scale circulation of the troposphere, stratosphere and mesosphere. Weather system dynamics, including the evolution and behaviour of cyclones and fronts. Mesoscale and boundary-layer dynamics. Interaction of dynamics with atmospheric constituents, including clouds, precipitation, aerosols and gases. Transport and dispersion of atmospheric constituents, including trace gases, aerosols and water vapour.

Specific comments:

Lines 165–167: Several papers have discussed the downward arching of QBO winds into the subtropics in considerable detail (e.g., Garfinkel and Hartmann, 2011; Gray et al., 2018). These studies should be cited here.

Response: We appreciate the valuable feedback provided by the reviewer, and we have added citations to the relevant sections of QBO in the manuscript (e.g., Line 167).

References:

Garfinkel, C. I., and Hartmann, D. L.: The influence of the quasi-biennial oscillation on the troposphere in winter in a hierarchy of models. Part I: Simplified dry GCMs, *J. Atmos. Sci.* 68, 1273–1289, <https://doi.org/10.1175/2011JAS3665.1>

Gray, L. J., Anstey, J. A., Kawatani, Y., Lu, H., Osprey, S. M., and Schenzinger, V.: Surface impacts of the Quasi Biennial Oscillation, *Atmos. Chem. Phys.*, 18, 8227–8247, <https://doi.org/10.5194/acp-18-8227-2018>

Lines 261–264: I do not understand the dynamical argument being made here. In particular, why should two adjacent ridges induce upward motion? Please explain the dynamical reasoning more explicitly, ideally with an accompanying figure.

Response: We sincerely thank the reviewer for this constructive comment, which points out that our dynamical reasoning was not sufficiently explicit. We have now clarified the physical mechanism in detail in the revised manuscript, and supplemented an illustrative schematic as well as vertical velocity verification to support this argument. The detailed explanation is as follows:

During MJO phases 2–5 in ESSPV winters, two anomalous anticyclones (i.e., ridges) coexist in the low-troposphere (850 hPa). They are zonally adjacent (aligned along the East Asian sector) but meridionally separated, and their collocation induces upward motion over South China through the following dynamical process:

The mid-high-latitude anomalous anticyclone, forced by the ESSPV, is centered over eastern Siberia ($\sim 55^\circ\text{N}$, 110°E). Its southern flank is dominated by anomalous northeasterly flow, which drives cold air to surge southward.

The low-latitude anomalous anticyclone (the western North Pacific anticyclone, WNPAC), excited by MJO convective heating, is centered over the western North Pacific ($\sim 20^\circ\text{N}$, 130°E). Its northwestern flank is dominated by anomalous southwesterly flow, which transports warm moist air northward.

These two opposing flows, the northeasterly from the Siberian ridge and the southwesterly from the WNPAC ridge, meet and produce horizontal convergence over South China ($20^\circ\text{--}30^\circ\text{N}$, $110^\circ\text{--}120^\circ\text{E}$). In accordance with the atmospheric continuity equation, low- and mid-tropospheric horizontal convergence must be accompanied by compensating upward vertical motion, which provides the dynamical lifting condition favorable for extreme precipitation over South China.

To verify and visualize this mechanism, we have made two additions to the figures: We superimposed contours of 500 hPa vertical velocity (ω) anomalies on the original Figure 3b (Fig. R5). Negative ω values (blue shading, corresponding to upward motion) are clearly visible over South China, and their spatial distribution is highly consistent with the wind convergence zone between the two ridges, directly confirming the proposed dynamical linkage.

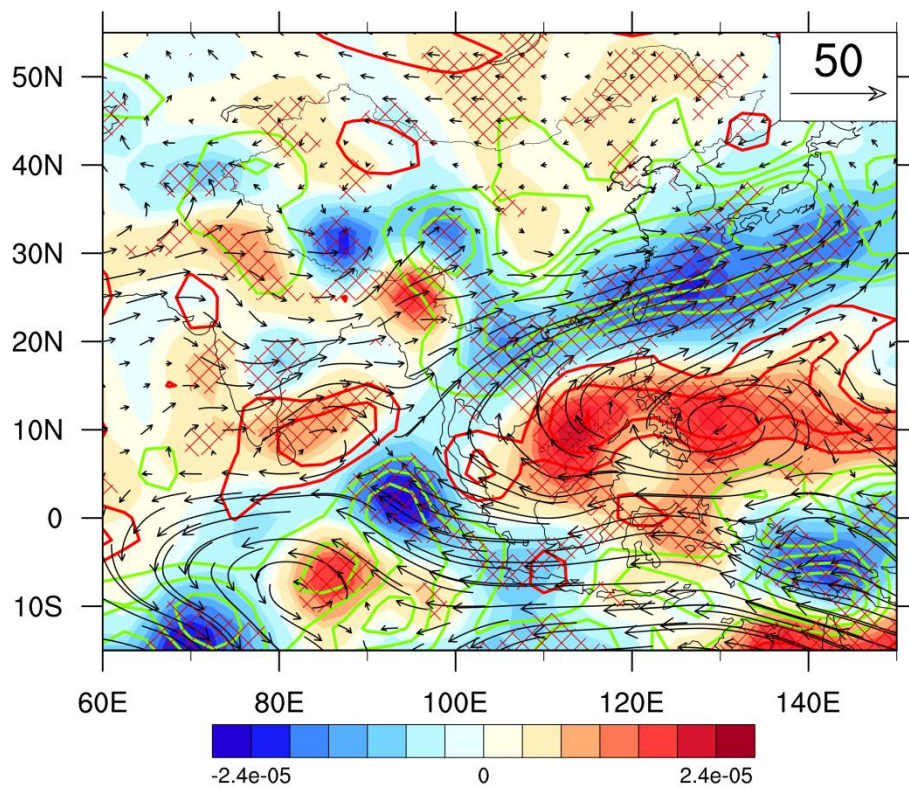
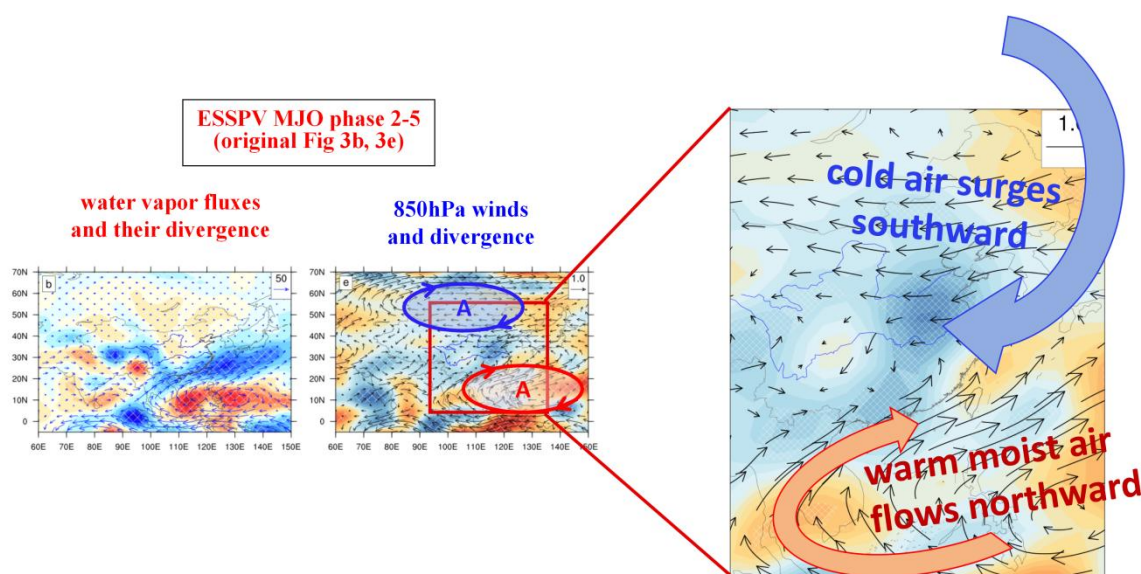


Figure R5. Composites of vertically integrated (surface to 300 hPa) water vapor fluxes (vectors, unit: $\text{kg m}^{-1} \text{s}^{-1}$), their divergence (shading, unit: $\text{kg m}^{-2} \text{s}^{-1}$) and 500hPa omega (contours, unit: Pa s^{-1} ; with contour intervals of 0.05; green indicates negative values, zero values are omitted, and red indicates positive values) during MJO phases 2–5 for ESSPV winters.

We added an illustrative schematic (shown below) that intuitively demonstrates the circulation pattern: cold air surging southward from the high-latitude ridge and warm moist air flowing northward from the low-latitude ridge, with their convergence driving ascending motion over South China.



Correspondingly, we have added contours of 500 hPa vertical velocity (omega) anomalies on the original Figure 3b (see Supplementary Figure S10), and have cited the updated figure panels in the text. We believe these revisions make the physical argument much clearer and more convincing.

Line 286: The word “validate” is inappropriate here. CMIP6 models cannot be used to “validate” an observed relationship. They can provide additional samples and therefore help address sampling uncertainty, but the models have their own biases, making it difficult to use them to independently validate a specific observed relationship.

Response: We will uniformly replace all occurrences of "validate" in the full text with "corroborate," "support," or "provide additional evidence for."

Lines 310–316: The MME1 and MME2 bars appear almost identical. Are the

differences between them statistically significant and robust? Please provide quantitative evidence rather than relying on the visual differences in the figure.

Response: We thank the reviewer for this careful check and for raising the important issue of statistical robustness of the MME comparison. We agree that visual inspection alone is insufficient to establish the significance of the differences, and we have supplemented additional spatial diagnostics to verify the robustness of the results.

To demonstrate the spatial coherence and robustness of this contrast, we have generated supplementary composite maps of the precipitation difference field (MME1 minus MME2, for the ESSPV minus EWSPV composite during MJO phases 2–5) for both the SPV-MME and MJO-MME grouping strategies. As shown in Fig. R6, the enhanced precipitation anomaly over South China is significant at the 95% confidence level based on two-tailed Students' t test in both groupings. The significant region is collocated with the core precipitation response area identified in the observational analysis, confirming that the inter-MME difference is spatially coherent and physically meaningful rather than dominated by local noise.

In the revised manuscript, we will add the quantitative significance statement to the figure captions of Figures 4b, for example: “The difference in mean precipitation between MME1 and MME2 is statistically significant at the 95% confidence level according to two-tailed Students' t test (see Supplementary Fig. S14).” The full spatial significance maps will be included in the supplementary material to provide complete statistical evidence.

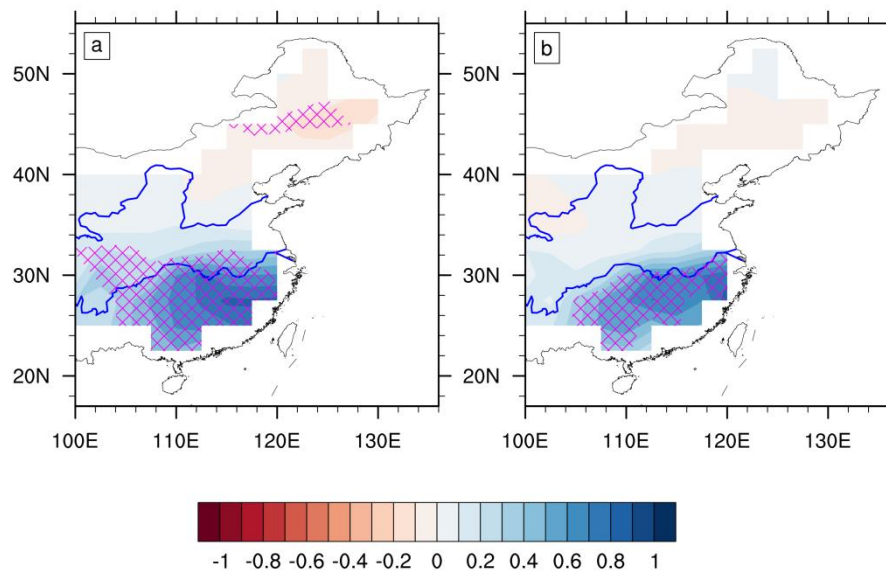


Figure R6. (a) Composite differences of precipitation (shading, unit: mm day⁻¹) between ESSPV and EWSPV winters during MJO phases 2-5 for MJO-MME1 minus

MJO-MME2 and (b) for SPV-MME1 minus SPV-MME2. Meshes indicate significant anomalies at the 95% confidence level.

Lines 321-325: Is the difference in the subtropical high and moisture convergence between MME1 and MME2 essentially imposed by the way the two groups are defined? If so, the logic of this section appears somewhat circular. The authors should clarify what is being independently demonstrated here versus what follows directly from the definition of the two groups.

Response: We thank the reviewer for raising this important logical question, which helps us clarify the design rationale of the multi-model ensemble (MME) grouping and further strengthen the rigor of our argument.

We first acknowledge that the difference in the western North Pacific anticyclone (WNPAC) and associated moisture convergence between MJO-MME1 and MJO-MME2 is, by construction, directly tied to how the two groups are defined. The MJO-MME groups are explicitly ranked and selected based on the simulation fidelity of the MJO phases 2–5-associated WNPAC. Therefore, the contrast in WNPAC intensity and moisture transport between the two ensembles is an expected outcome of the grouping strategy, rather than an independently emergent result.

This grouping design is deliberate and physically motivated. As demonstrated in the observational analysis, the WNPAC during MJO phases 2–5 serves as the critical low-latitude moisture carrier for South China winter precipitation. The core question we aim to address with the MJO-MME grouping is: does the ability to simulate this specific circulation feature determine a model's capacity to reproduce the observed ESSPV–SC extreme precipitation linkage?

Importantly, the MJO-MME1 and MJO-MME2 groups show no substantial difference in their simulation of the ESSPV background itself (as shown in Fig. 4a). If the SPV background simulation were the dominant factor, two groups with comparable SPV fidelity should produce similar SPV–precipitation linkages. However, we observe a pronounced divergence in the strength of the ESSPV–SC R90 correlation between MJO-MME1 and MJO-MME2. This contrast directly demonstrates that it is the simulation fidelity of the WNPAC, rather than the SPV background itself, that governs whether a model can capture the SPV–extreme precipitation linkage.

To further rule out circular reasoning and independently verify this conclusion, we constructed a second set of MME groups — SPV-MME1 and SPV-MME2 — defined purely by the strength of the simulated ESSPV–SC R90 linkage, completely

independent of WNPAC performance. Remarkably, the SPV-MME groups exhibit a very similar contrast in WNPAC simulation fidelity: models that better reproduce the SPV–precipitation linkage (SPV-MME1) consistently show higher skill in simulating the MJO phases 2–5-associated WNPAC, and vice versa.

The consistency between these two independent grouping strategies confirms that our conclusion is not circular. Both approaches converge on the same physical conclusion: the simulation of the MJO-associated WNPAC is the decisive bottleneck for reproducing the ESSPV–SC extreme precipitation relationship, while the simulation of the SPV background state alone is not the limiting factor.