Response to Reviewer #1

General comment

This study proposes a novel synoptic clustering-based approach (NL26) using Self-Organizing Maps (SOM) to define the South China Sea Summer Monsoon (SCSSM) onset based on persistent large-scale circulation regimes. Evaluated using the ECMWF SEAS5 seasonal hindcasts, the author concludes that this regime-based definition yields systematic improvements over the conventional zonal wind-based criterion (W04) in deterministic and probabilistic skill metrics up to a 5-month lead time.

The manuscript addresses a highly relevant and challenging topic in seasonal monsoon prediction. The methodology is interesting, and the motivation aligns well with the scope of *Weather and Climate Dynamics*. However, before the manuscript can be recommended for publication, there are several major scientific concerns that need to be addressed. These primarily relate to the physical consistency of the new index, the validity of the "improved predictability" during extreme delayed years (e.g., 2018), and a lack of diagnostic evidence supporting the claims regarding subseasonal-to-interannual timescale interactions. I recommend a **Major Revision**.

Response

We sincerely thank Reviewer #1 for the careful reading of the manuscript and for the constructive and insightful comments. We appreciate the reviewer's positive assessment that the study addresses a relevant and challenging problem in seasonal monsoon prediction and that the methodology is of interest for *Weather and Climate Dynamics*. We also appreciate the reviewer's clear identification of the main concerns, particularly regarding the physical consistency of the proposed index, the interpretation of forecast skill in extreme delayed years, and the need for stronger support for the discussion of multi-timescale variability. In response, we have substantially revised the manuscript. The major changes include:

(1) adding new OLR and MTG diagnostics to assess thermodynamic and convective consistency;

(2) refining the NL26 onset definition to avoid unrealistically late onset dates in some years of the independent period (2017–2025);

(3) adding cross-index validation against the conventional W04 benchmark;

(4) including representative case studies in the Supplementary Material to clarify the physical differences between NL26 and W04; and

(5) revising the discussion and conclusions to present the role of subseasonal variability more cautiously and explicitly.

Major comments

Comment 1: Physical Consistency with Thermodynamic Metrics

Figure 2 shows a correlation of 0.74 between the NL26 and W04 indices. While the SOM clustering is designed to capture large-scale circulation regimes, the abrupt onset of deep convection and precipitation remains the most critical thermodynamic characteristic of the SCSSM onset. Since precipitation was not selected as an input variable for the clustering, it is vital to verify whether this pure circulation-based index remains physically consistent with actual convective activities. The author should include a comparative analysis between the NL26 index and key thermodynamic/convective indicators, specifically the Meridional Temperature Gradient (MTG) and Outgoing Longwave Radiation (OLR), to ensure that the identified circulation regimes robustly correspond to the onset of the monsoon rainfall.

Response

We agree that it is important to demonstrate that the proposed circulation-based onset definition is physically consistent with convective and thermodynamic monsoon establishment. In the revised manuscript, we added a new analysis based on pentad composites of outgoing longwave radiation (OLR) and meridional temperature gradient (MTG). This new analysis is presented as Figure 4 in the main text. The results show that, prior to onset, OLR over the South China Sea (SCS) remains relatively high and MTG over the SCS is weak or locally negative. From the onset pentad onward, area-averaged OLR decreases below 240 W m^{-2}, indicating enhanced deep convection, while MTG becomes positive over most of the SCS, indicating the establishment of a favorable large-scale thermal structure. These changes persist into the subsequent pentads. We therefore revised the manuscript to show more clearly that the circulation-based onset definition is not only dynamically grounded, but also thermodynamically and convectively consistent with monsoon establishment.

Changes in manuscript

Added OLR dataset information in Section 2.1.1; added new Figure 4 and associated OLR–MTG analysis in Section 3.2; revised the Discussion accordingly.

Comment 2: Logical Transition from Climatology to Interannual Forcing

There is a noticeable logical gap between the climatological evolution presented in Figure 3 and the interannual Sea Surface Temperature (SST) patterns associated with early/late onset years shown in Figure 4. The transition from mean state characteristics to specific interannual boundary forcings feels abrupt and lacks sufficient diagnostic bridging in the text. Consider moving Figure 4 to the Supplementary Material, or significantly expand the dynamical explanation in the text to justify the transition to SST forcing patterns at this stage of the manuscript.

Response

We agree with this comment. In the original version, the transition from climatological circulation evolution to interannual SST forcing was indeed too abrupt. In the revised manuscript, we restructured Section 3.2 to improve the logical flow. Specifically, we now retain the climatological circulation evolution in Figure 3 and add a new Figure 4 showing the corresponding evolution of OLR and MTG, which provides a direct convective and thermodynamic consistency check for the circulation-based onset definition. The SST and 850-hPa wind composites associated with early and late onset years have been moved to the Supplementary Material (new Figure S6), where they are discussed as background modulation of interannual onset timing rather than as an immediate continuation of the climatological evolution. This revision clarifies the distinction between mean seasonal evolution and interannual boundary forcing and provides a more natural diagnostic bridge in the main text.

Changes in manuscript

Revised Section 3.2 and moved the SST-based early/late-onset composites to Supplementary Figure S6.

Comment 3: Cross-Validation against Operational Benchmarks

In Section 4.1.1 (Forecast skill assessment), Figure 5 validates the W04 model forecasts against W04 observations, while Figure 6 validates the NL26 forecasts against NL26 observations. Although internally consistent, this comparison is insufficient to demonstrate the practical forecasting value of the new method. Given that W04 is widely applied as a standard benchmark in operational forecasting services, please add an evaluation comparing the NL26 model forecasts directly against the W04 observational values. This cross-index validation is necessary to quantify the actual added value of the NL26 approach in real-world operational contexts.

Response

We thank the reviewer for this important suggestion. In the revised manuscript, we added a cross-index validation (CIV) analysis in which the observed onset dates are defined using W04, while the forecast onset dates from SEAS5 are derived using NL26. This new analysis is presented as Figure 7 for the dependent period and Figure 11 for the independent period. The CIV results show that the improved skill of NL26 is not solely an artifact of matched-definition verification. In the dependent period (1981–2016), correlations remain statistically significant for all initialization months and are broadly comparable to or higher than those of W04. In the independent period, CIV correlations also remain positive and generally exceed those of W04. We agree that this additional comparison was necessary to demonstrate the practical forecasting value of the proposed definition against the conventional benchmark.

Changes in manuscript

Added cross-index validation (CIV) and interpretation in both the dependent and independent periods.

Comment 4: Forecast Skill Evaluation in Extreme Years (e.g., 2018)

In Section 4.2.1, the manuscript highlights that the correlation coefficient for the NL26 prediction is significantly higher than that of W04. However, a detailed comparison of Figures 8 and 9, alongside the lead-time performance (December to April), reveals a critical issue. The apparent improvement in NL26 is largely driven by its performance in specific years, such as 2018. In reality, 2018 featured an extremely late SCSSM onset. The NL26 index defines the "true" onset date for 2018 as May 8, which allows the model to register a "hit" at all lead times. However, this entirely misses the actual physical delay of the monsoon onset that year. The author must provide an in-depth discussion on extreme years where traditional ENSO-based seasonal forecasts typically fail (e.g., 2018 and 2019). It is essential to clarify whether the claimed "enhanced forecast robustness" is a genuine improvement in capturing anomalous monsoon dynamics or merely a mathematical artifact resulting from redefining the onset target.

Response

We thank the reviewer for this careful and important comment. We agree that the interpretation of forecast skill in extreme years needed to be more nuanced. In the revised manuscript, we substantially softened the wording regarding “enhanced forecast robustness” and now avoid implying that higher NL26 skill automatically means that the forecast system fully captured all aspects of anomalous monsoon dynamics in years such as 2018 and 2019. Instead, we emphasize that NL26 and W04 represent different physical aspects of onset. The revised text clarifies that apparent skill improvements under NL26 may partly reflect the fact that the clustering-based definition identifies the timing of the large-scale circulation regime transition, whereas W04 is more sensitive to delayed or transient threshold crossing in low-level zonal wind.

In addition, we expanded the year-specific analysis of differences between NL26 and W04 and added representative case studies in Figs. S4 and S5. These case studies illustrate how disagreement between the two definitions can arise from brief westerly surges, delayed coherent regime transitions, or other transient influences. We now state more cautiously that NL26 may better isolate the large-scale predictable component of onset timing, but does not necessarily resolve every aspect of anomalous monsoon evolution in extreme years.

We also note that, during revision, the NL26 definition was slightly refined by removing the former persistence threshold from the original version, because it occasionally yielded unrealistically late onset dates in the independent period (2017–2025). The revised definition remains regime-based, but produces onset dates that are more physically consistent with W04.

Changes in manuscript

Expanded the discussion of representative cases and revised the Discussion to soften the interpretation of extreme-year skill.

Comment 5: Evidence for Subseasonal Variability Claims

The abstract and discussion state that the improved predictability reflects "multi-timescale controls" and that "subseasonal variability triggers the onset transition." The author suggests that the NL26 index better isolates the predictable component when the ENSO-monsoon relationship is weak. However, the main text lacks concrete case studies or dynamical diagnostics to substantiate this. Existing literature shows that predicting the onset is more difficult in years dominated by subseasonal signals. To support the current claims, the author should include specific case studies [such as 2019 or other years with strong intraseasonal oscillation but weak ENSO forcing] to explicitly demonstrate how the NL26 index outperforms traditional indices in capturing the superimposition of subseasonal signals onto the large-scale circulation.

Response

We agree that the original wording overstated the evidence for subseasonal triggering. In the revised manuscript, we substantially softened the corresponding statements in the abstract, Section 3.2, and the Discussion. We now frame the multi-timescale interpretation as a physically plausible explanation rather than a formally demonstrated causal mechanism. Specifically, we state that slowly varying large-scale boundary forcing helps precondition the background circulation, while higher-frequency atmospheric disturbances may influence the exact timing of onset once the background state becomes favorable. We no longer claim that subseasonal variability has been explicitly diagnosed as the trigger in the present study.

At the same time, the revised year-specific discussion and added Supplementary case figures provide qualitative support for the idea that threshold crossing and coherent regime transition can diverge in years affected by transient disturbances. However, we now state clearly that a full diagnosis of subseasonal mechanisms is beyond the scope of this study and should be addressed in future work using dedicated intraseasonal diagnostics.

Changes in manuscript

Revised the abstract, Section 3.2, and the Discussion to soften the multi-timescale and subseasonal claims.

Other suggestions

Typographical Error in Figure 2

In the final row of Figure 2, the subplot labels should be corrected to C1, C3, and C6 to align with the SOM clustering nomenclature used throughout the manuscript.

Response

We thank the reviewer for catching this typo. We have corrected the labels in Figure 2 accordingly.

Changes in manuscript

We have corrected the labels in Figure 2 accordingly.

References

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