

The revised manuscript shows the full SOM analysis in supplementary material but does not sufficiently address the previously raised concerns. Specifically, major questions are still pending regarding the SOM classification, which lies at the heart of the analysis. The authors refrain from providing any metrics to estimate the SOM robustness, nor do I better understand why the original 9-node SOM is not used. The authors do not share my concern regarding the evidently low correspondence between low-level geopotential heights and winds in the region of interest. While the subject of the study is important, and relevant for the scope of the journal, substantial issues arise from the present methodology. I conclude that the manuscript should still undergo **major revisions before being accepted**.

Major revisions:

- 1) The subjective formation of a 6-node SOM from an objectively defined 9-node SOM raises issues. The classified geopotential patterns' robustness (or confidence level) is unclear. It is important to account for each node's internal variability and to mark which regions of the flow are significant. From previous experience, the statistical significance of the SOM nodes may not include the entire domain, and it is important to clarify the regions where the pattern is indeed robust. This is helpful to determine the relevance of the mean node states displayed in figure 10, i.e., what is the standard deviation of the mean environmental conditions? If they largely overlap with that of MCS days, then the separation may not be as meaningful as suggested by the authors. Furthermore, no topological or quantization errors are displayed, nor is a Sammon map, making assessing the SOM's performance virtually impossible. Also, the relative frequency of each node is not given, though it is clear that the distribution is far from equal. This leads to peak Monsoon node 6 showing a reduced MCS frequency despite containing the largest amount of MCSs. This is problematic since the main frequency of each node is unclear. I.e., if node 6 is twice (or more) as frequent as node 5, it is reasonable to expect a lower MCS frequency that may be an attribute of inner cluster variability (or noise) rather than a dynamic feature.
- 2) There is a problem with the logic of the present analysis: Nodes are associated with monsoon phases mostly based on their seasonality. MCSs make up 30-80% of the monsoon precipitation, so they are clearly more common during peak monsoon stages. This suggests that pre-monsoon nodes will have significantly fewer MCSs and hence their response to MCS appears larger. As a result, seen in Figure 10, only node 1 seem to show a significant response to MCS, while for the other nodes, the mean state is well within the STD of the MCS conditions. This undermines the main point of the paper.
- 3) The MCS data should be better treated and displayed. As innovative as combining satellite data to such a large-scale perspective is, it eventually ends up only as a cloud indicating all MCSs within each node. It is worth visualizing the data in a manner that will ease the interpretation of the results. For instance, it is unknown what the MCS spatial distribution actually looks like for each node. Overlapping dots are invisible and may hide preferable locations for MCSs. The mean number of MCSs per MCS day per node is not given, thus it is possible for example that while MCS "daily" frequency peaks on node 5, the overall MCS frequency may be larger for node 6, simply by having more MCSs per MCS day. Such discrepancies in the MCS data should be addressed and studied within the present manuscript, including spatial variability.
- 4) Information on the SOM dynamics also seems relevant, e.g., how long does each node persist? are the nodes typically changing on a time scale of days or weeks? Which nodal transitions are frequent, which are rare, and how does this relate to the dynamical interpretation of the SOM as indicating the monsoon phases? Such information is valuable to evaluate the SOM's consistency in mapping consecutive days, and support the derived conclusions.

Minor comments:

L140: the heat low is not well captured relative to similar SOM analysis. I've commented more about that above.

L144: ts = typo?

L156: Q and T errors should be presented per cluster

L160: what led to the choice of 9 members? Was it purely a qualitative choice?

L210: Why is a heat low evident as a high-pressure area?

L212: "and linked southward retreat" – rephrase

L238: "weaker geopotential heights representative of high-pressure areas" – unclear. Rephrase.

L335: the use of the term "storm" is confusing. Stick to MCS.

L356 & 357: use "show/shown" instead of the passive "observe/observed".

L359: remove double dots

L364: "with frequent convective activities during peak monsoon" - Why is the same not true for node 5 with the most frequent MCS days?

L376: "... states and then examined..." = ... states and examined...