

Reviewer 1

I am still not satisfied by the authors' responses to my previous comments about the choice of K-means clustering, and Markovian dynamics.

Answer: We thank the reviewer for their time and effort. We understand that the issues raised were due to wording and conceptual framing rather than the underlying methodology. We have revised the manuscript clarifying terminology and we hope that the reviewer will find it acceptable in the current form (Section 2.5).

a)

Regarding K-means, the authors have responded that "each grid point serves as an independent axis in this high-dimensional space", but I don't think that each grid can be considered independently of its neighbors in case of spatial data. In fact, this problem will be there not only for K-means, but also for the other clustering approaches mentioned by the authors.

Answer: Thank you for your comments. We would like to clarify that the methodology does not assume independence between grid cells and we have taken care to ensure that the revised manuscript does not mention it. The clustering is performed on gridded rainfall fields represented as vectors in a high-dimensional Euclidean feature space, and spatial autocorrelation is inherently preserved within this representation (Anderberg, 1973).

We apologize that in our response to the reviewer, the feature-space description may have been phrased in a way that could be misinterpreted as implying independence of grid cells. Our interpretation of the data and clustering methodology is consistent with standard applications of k-means and related clustering approaches in geophysical data analysis (e.g. Rossow et al., 2005; Bernard et al., 2013; Raut et al., 2014; Straus, 2022). Hope this clarifies the concern raised by the reviewer.

Anderberg, Michael R. "Chapter: The broad view of cluster analysis." Cluster analysis for applications 1.1 (1973)

Bernard, Elsa, et al. "Clustering of maxima: Spatial dependencies among heavy rainfall in France." Journal of climate 26.20 (2013): 7929-7937.

Raut, Bhupendra A., Christian Jakob, and Michael J. Reeder. "Rainfall changes over southwestern Australia and their relationship to the Southern Annular Mode and ENSO." Journal of Climate 27.15 (2014): 5801-5814.

Rossow, William B., et al. "Tropical climate described as a distribution of weather states indicated by distinct mesoscale cloud property mixtures." Geophysical Research Letters 32.21 (2005).

b)

Of course, K-means will give some clustering result, but we do not know if the clusters found this way are "suitable" - for this the authors need to justify the results considering inter-class and intra-class distances, and Euclidean distance will not be very suitable in this case- the authors may need to consider a more suitable measure to compare spatial data.

Answer: We appreciate the reviewer's concern. However, we would like to clarify that cluster goodness metrics are intended as guidance and do not validate scientific results and each metric has known limitations. For a detailed methodological discussion, we refer to *Cluster Analysis for Applications* by M R Anderberg (cited above). In practice, changing standard cluster-goodness criteria usually does not alter the overall conclusion.

In this study, we used the Elbow method on within-cluster and between-cluster criteria (Figure 2), consistent with our earlier work where this approach was shown to be robust (Raut et al., 2021; Jackson et al., 2023). Many prior studies reported reasonably consistent behavior across these common cluster-selection methods. To explicitly demonstrate that cluster-selection criteria need not agree on a single exact value, we include this plot.

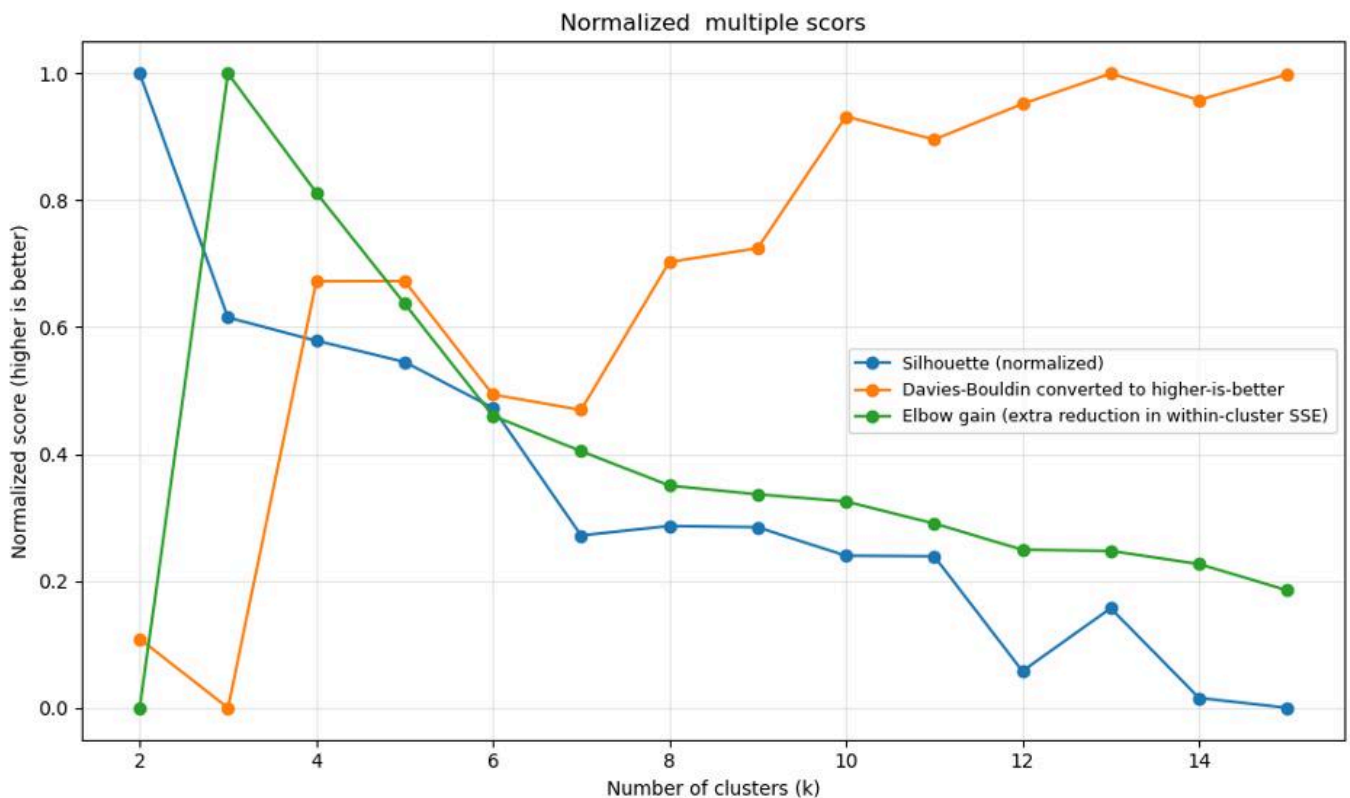


Figure: The plot contains three normalized and cluster-quality scores (DB score inverted for interpretations) versus number of clusters (k):

- Silhouette (higher is better)
- Davies-Bouldin converted to higher-is-better
- Elbow gain (reduction in within-cluster SSE when increasing k)

As one can see, individual metrics do not give a single K. The curves show strongest collective support in between 10 to 13 clusters (also shows support for K= 3 and 4). The best criteria is to check physical interpretability of the clusters. Therefore, we have compared them to synoptic patterns which in the authors' opinion have more value than any of the above tests.

Markovian Dynamics

In the case of Markovian Dynamics, the response by the authors does not provide the answer to my basic question - why should we consider a Markovian transition system at all in this case? What is the physical basis to justify the Markovian hypothesis that the cluster on day (t+1) depends only on the cluster on day (t), with some probability distribution? What should be the order of the Markov Chain? To answer this question, the authors may try to summarize the most prominent transition patterns (including self-transitions), and associate them to some well-known dynamics of the monsoon. However, since these are well-known anyway, the authors must also justify what are the new transition patterns that this approach suggests, and what is the physical explanation of such new patterns?

Answer: We thank the reviewer for this comment. We used the reference to "Markov chain" as a convenient descriptor for the day-to-day transition matrix between rainfall regimes. As it was also mentioned in the manuscript, no assumption of a full stochastic predictive Markov modeling was intended as this is not a forecasting application.

We recognize that this terminology may have introduced a conceptual ambiguity regarding process assumptions and its applications and therefore we have removed reference to "Markov chain" which was only in the Section 2.5 Transition Probabilities. The analysis itself is unchanged and remains strictly based on empirical transition frequencies between consecutive daily clusters, used as a diagnostic tool to summarize regime persistence and sequencing. Please, note that this revision is terminological rather than methodological to provide a clear description of our methodology.

Reviewer 2

Thank you for addressing my comments. I am largely satisfied with the responses. I have few remaining comments.

The manuscript does not reflect some of the responses. For clarity and transparency the manuscript should be revised corresponding to these responses For example, "As per our response on groupings in above reply, one should not assume that the clusters in the same

groups should have same trends." : I could not find any corresponding changes in the manuscript. "The Figure 8 shows the contribution of clusters in total monthly rainfall while Figure 10 shows the contribution of clusters in total change in rainfall . Thus, the cluster that contributes maximum rainfall doesn't have to be the one that changed the most." : I could not find any corresponding changes in the manuscript. These are important information that should be clearly mentioned in the manuscript.

Answer:

- Section 3.4 paragraph 1 has been modified to include a better description of Figure 8.
- Section 3.4 paragraph 4 explains the difference between figure 8 and figure 10.

A part of my earlier comments that is still unaddressed: "The authors also did not describe Figure 9 well. What is "Seasonal frequency of occurrence of cluster"? Is it N_i in Equation 3?"

Answer: Thank you for the comment. We have added reference of the figure and section in method sections and reference of method section in results section and figure caption. Figure 9 is not computed using equation 3. It is just counting the frequency per season for each cluster. Additionally, the description of figure 9 has been modified for a better explanation (Line 382 onwards).

Minor Comments

Line#40: JASS -> JJAS

Answer: Done. It is corrected.

Figure#8 caption: K&T -> T&K

Answer: Done. It is changed.

Code availability statement: the link https://github.com/RBhupi/monsoon_clusters is broken

Answer: The link has been updated to the given one - DOI: 10.5281/zenodo.20099064

Reviewer 3

Review of "A regime-based diagnosis of transition probabilities and changes in frequency and intensity of Indian Summer Monsoon rainfall" by Raut et al.

Recommendation: Major revisions

The editor asked me to review this paper after previous rounds of revisions and the editor also asked me specific aspects to comment on.

Answer: Thank you for your suggestions. Reviewers' comments were useful in understanding the concerns mainly due to the terminology of our responses.

This study investigates monsoon rains in India and tries to identify preferred regime states via k-means clustering. K-means is widely used in atmospheric sciences. I have some concerns about the author's use of k-means. Using gridded data has the problem of spatial autocorrelation which can affect the clustering. Typically one could do a EOF/PCA analysis to get independent data but that is probably not advisable for precipitation. So I would suggest including the longitudes and latitudes into the k-means analysis; maybe also including topography might be advantageous. That might improve the k-means clustering analysis.

Answer: We agree with the reviewer's assessment and understood the suggestions, however adding latitude and longitude (spatial coordinates) to k-means clustering will transform it into a superpixel segmentation algorithm (Wang et al., 2017, Sasmal and Dhal, 2023) which will not cluster the similar rainfall patterns into the cluster but the homogeneous regions of the rain fields into the segmentations. This is not the goal of the current analysis. Therefore we did not include spatial coordinates into the clustering.

Wang, Murong, et al. "Superpixel segmentation: A benchmark." *Signal Processing: Image Communication* 56 (2017): 28-39.

Sasmal, Buddhadev, and Krishna Gopal Dhal. "A survey on the utilization of Superpixel image for clustering based image segmentation." *Multimedia Tools and Applications* 82.23 (2023): 35493-35555

Fig. 2 shows the WCSS but to me the change would occur at cluster number 4 or 5. Not 11. Later in the manuscript the authors also group the 11 clusters into 4. 4 clusters would be consistent in my view with Fig. 2. It would also be useful to see a silhouette plot.

Answer: Within cluster (between cluster) distance gradually decreases (increases) with the increasing number of clusters until they reach a plateau at cluster number 11 and 12 (Figure 2). Therefore the cluster 11 is the point where the gradual decline clearly pauses which makes it the optimal k.

The grouping of the clusters is mainly done for brevity and better reading flow. The clusters grouped together are influenced by the same weather systems but in different geographic regions, hence the grouping is not replacing the clustering and must not dilute the clustering results like trends etc.

The authors show that there is significant change in precipitation between the 1st and 2nd half of the data period. K-means assumes stationarity of the data. Are the clusters stable when k-means is applied to the 1st and 2nd half of data? Such an analysis would make the study also more interesting.

Answer: Thank you for raising this question. To avoid the issue of stationarity the k-means algorithm is not applied to different periods separately but computed for all days, hence the clustering is valid over the entire period of the study. However, if we use the same cluster boundaries on future data the issues of stationarity would have affected the results. Therefore the clusters are not applicable for predictive tasks.

The authors then compute transition probabilities assuming the Markov property. It is necessary to show that the Markov property is valid. What does the partial autocorrelation function look like? Or a transition frequency analysis between $t+1$ and $t-1$ could be performed to see whether they are almost independent.

Answer: We would like to emphasise that we are not modelling or predicting transitions (stationarity will be an issue as correctly pointed out by the reviewer) in predicting future periods, but we only want to use this to compute the transition probabilities of the transitions from day-to-day basis for historical period, because that explains and quantifies some known dynamical properties underlying the monsoon rainfall and how they have changed over the study period. This not only validates our own methodology but can also be used to compare the transition probabilities in the model data to validate if they match with the observed values.

The use of such terminology may introduce unnecessary conceptual ambiguity regarding Markov assumptions and process order, whereas our analysis was intended purely as a diagnostic characterization of regime persistence and preferred transitions between rainfall states. To avoid further confusion for readers, we have removed the reference to "Markov chain" terminology only occurring in the subsection 2.5 and now describe the framework more directly as a transition-probability analysis between rainfall regimes.

We hope this clarifies the reviewers' concerns.