Reviewer 2

> I find the motive of identifying clusters important. However, I find the link between the clusters and the regional long-term trends muddy and not well established by the existing arguments. Overall, the narration requires sharpening.

We thank the reviewer for your comments. The links between clusters and regional trends are established in Section 3.4 ("Seasonality of Clusters and Long-Term Changes") by connecting cluster contributions to the three focus regions through Figure 8, and Figure 10 provides the quantitative decomposition of regional rainfall changes into cluster-specific frequency and intensity components. We have now modified this section to clarify this point.

> The manuscript first identifies seasonal mean rainfall changes over India over the duration 1961–2018 (Figure 3). It does so by splitting the whole duration into two periods, 1961–1989 (earlier period) and 1990–2018 (later period), and then subtracting the seasonal mean rainfall of the earlier period from that of the later period. By performing this analysis, the authors identify three regions that exhibit noticeable trends, namely Thar and Kutch (west north-western part of India), Indo-Gangetic plains, and the Northeast Indian region (in fact, the easternmost part of India). It is noteworthy that the reliability of the data the authors have used is debatable over the Northeast Indian region (Zahan et al., 2021).

We thank the reviewer for pointing this out. Zahan et al (2021) has compared multiple datasets to study the multidecadal variability of rainfall over northeast India, however they raise questions on the dataset for extreme rainfall. Our analysis does not focus on extreme events but on the overall pattern of rainfall over northeast India. Additionally, the dataset used by Pai et al., 2014 shows an out of phase active - break pattern consistent with previous studies as well as Zahan et al., 2021.

Any trends attributable to the changing rain gauge network are linear and statistically significant only over the long period 1901–2010, with a modest trend of −0.11 mm/day/decade (Pai et al. 2014) . The NEI drying we report emerges sharply in the late 1980s and represents a rapid decline in rainfall. Furthermore, if changing rain gauge density were responsible, it would affect all clusters equally and gradually, yet we observe the decline concentrated particularly for Cluster 2, rather than spread uniformly across all rainfall regimes. This cluster-specific and temporally abrupt nature of the trends can not be an artifact of the data. Also, the negative trend over NEI is independently confirmed by multiple studies using different datasets (Jain et al., 2013; Dhara et al., 2025; Zahan et al., 2021) now cited in the revised discussion section. We have added a note in the discussion acknowledging this uncertainty while making clear why we are confident the reported trend is real.

> Then the authors decompose JJAS rainfall over India into 11 clusters or spatial-patterns (Figures 4, 5, and 6) and analyze their transition probabilities (Figure 7). They group these 11 clusters into 4

> groups. Is this done only based on the transition probabilities? The grouping requires a better argument and quantification.

The purpose of grouping is to improve the flow of the writing by discussing the related clusters together. Therefore, grouping are done more subjectively based on transitions, location of rainfall and synoptic weather systems. These points have been mentioned in the manuscript.

> Further, they compute monthly contributions of these clusters to the three regions (Figure 8) identified in Figure 3. It is not clear how this was computed. From Figure 8, it seems the authors computed the seasonal mean of each cluster and then computed its percentage relative to the total seasonal mean, averaged over each region indicated by the boxes in Figure 3 (please explain this in detail in the relevant section of the manuscript).

Yes. the clusters have been used to quantify the changes in three regions (as shown by boxes in Figure 1). The motive of this is to highlight the changes happening in the three regions undergoing the most significant changes. This has been highlighted in the manuscript.

> The narration of Figure 8 is not transparent enough. There is considerable confusion regarding the interpretation of Figure 8. As stated, Figure 8 indicates considerable contribution from specific clusters to specific regions. However, despite finding that specific clusters contribute to specific regions, the authors analyzed the "Seasonal frequency of occurrence of cluster" for all 11 clusters. Interestingly enough, especially because some clusters earlier were grouped, the clusters of the same group exhibit different trends in Figure 9. For example, clusters 2, 5, 8, and 10 in Figure 9. The authors also did not describe Figure 9 well. What is "Seasonal frequency of occurrence of cluster"? Is it Ni in Equation 3?

As per our response on groupings in above reply, one should not assume that the clusters in the same groups should have same trends. They are grouped together because it is easier to write about them as a group rather than as indivudual 11 clusters with no linkage.

> In Figure 10, the last figure of the manuscript, the authors compute changes in rainfall corresponding to each cluster and decompose that change into intensity and frequency change (following Catto et al., 2012; the authors should refer to relevant citations while discussing the results, in addition to mentioning them in the introduction or data-and-methodology section).

We cited Catto et al (2012) only for the method to decompose the intensity and frequency changes of the cluster. They used radiosonde data over Darwin Australia and their results are not relevant to our dicussion.

> Like Figure 9, the description of Figure 10 is also muddy. The green bar corresponding to cluster 9 for the T&K region (that is mentioned as "Kutch and the Thar" in Figure 10; please make it consistent

> with the rest of the manuscript) goes past 40 mm. Then why do you mention "Cluster 9 frequency gains (+20 mm)" in the manuscript? Why do you not discuss negative contributions from cluster 2 for T&K whereas you do discuss those from clusters 2 and 8 over IGP?

We thank the reviewer for pointing out this inconsistent naming and a typo. The Figures 8 and 10 are now modified for consistency and the "Cluster 9 frequency gains (+20 mm)" is corrected to "Cluster 9 frequency gains (+40 mm)"

We have also added
`In T\&K, the $\sim$+30 mm (+15 \%) increase arises mainly from Cluster 9 frequency gains (+40 mm) together with slight intensity increases, and a $\sim$+10 mm contribution from Cluster 6 intensity rise. The cluster 2 on the other hand brings 20 mm declined due to frequency reduction.`

> For T&K, IGP, and NEI, the clusters that were found to contribute most in Figure 8 and Figure 10 are not exactly the same. How can we reconcile this? After performing the above analysis, the authors claim that: This study presents a diagnostic framework linking rainfall clusters to synoptic-scale drivers: I fail to see any mechanism or statistics in the analyses presented in the manuscript supporting this claim.

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.

The paper presented following post-clustering analysis framework 1. identifying synoptic patterns of winds and moisture variables associated with the rainfall clusters to study what type of weather regimes causese the rainfall patterns. Doing this analysis for model rainfall and comparing it with the current study will suggest if the model is producing the rainfall patters for correct weather regimes of not. 2. calculate the transition probabilities to study temporal relationship between the clusters and 3. decompose the intensity and frequency changes for attributing the changes observed in the rainfall.

> This study identified eleven distinct rainfall regimes: I also fail to understand how 11 distinct regimes were identified if the authors argue that some of the clusters are actually dynamically similar and can be put under one group, forming a total of 4 groups. Based on the above reasoning, I recommend that the manuscript requires major revision.

Figure 2 explains how the number of 11 clusters was identified. The 11 clusters were found to be optimal as per criteria of compactness and separability while also being stable under random initialization. In addition, the grouping of the clusters is for a more effective discussion of the regional overlaps of rainfall and synoptic drivers of those patterns. The clusters in the same group doesnt have to have same behavior in every aspect (i.e. they are not same analytically) which why there are separate clusters. The manuscript has been modified to explain the process better.

Detailed Comments

Comment 1: Title

> The title can be more conclusive.

Thank you for this suggestion. We have modified the title to better explain the motive of the study.

Comment 2: Abstract

> Unclear. Also, the authors ambiguously use the words "intensity" and "frequency" in the abstract. It is not clear if they mean these for rainfall or for clusters.

The abstract has been modified for better clarity.

Comment 3: Introduction

> It reads pedagogically rather than as an introduction to a research manuscript. Evidence of this is the consistent use of 50-year-old references. Paragraph #1 introduces the monsoon in general. The subsequent paragraphs introduce land–ocean contrast, the ITCZ, monsoon evolution, monsoon trough, depressions, mid-tropospheric cyclones, orographic effects, intraseasonal variability, and teleconnections. Then, in the paragraph starting at line #76, the authors very quickly mention a large number of features of the monsoon. In the paragraph at line #87, the authors direct attention to extremes: past evidence of their increase over India (central India), arguments for their projected continued increase (Clausius–Clapeyron logic), and finally discuss future projections of increasing extremes. In the final paragraph, the authors introduce the aims of this study. The three aims, nice and interesting as they are, are not related to the previous paragraphs of the introduction. The three aims mention links between rainfall and atmospheric circulation, their evolution, and their impact on monsoon rainfall trends. They also mention a future scope relevant to teleconnections and model biases. The bottom line is that the introduction is not sharp enough, not updated enough with relevant and recent references, and does not provide adequate scientific background.

We agree that the original Introduction reads too broad and pedagogical relative to the aims of the paper. In the revision, reduced descriptive material that is not directly used later in the paper. However, we kept details of the Indian summer monsoon system that we discussed in our results for the international readers.

The fifty year old references we refered to are landmark papers or memoirs that set the stage for our understanding of the monsoon synoptic systems. However, the introduction also has recent references to better explain the motive of the study. The introduction has been modified for a better flow.

Comment 4: Data and Methodology

> a) Why not use ERA5 data?

We have used reanalysis data to compute synoptic scale anomaly patterns to identify the large scale weather systems causing the rainfall in the clusters. These average features may not change between the different reanalysis products.

> b) Are the results consistent with Zahan et al. (2021) over NEI? It seems consistent. Nonetheless, please comment.

We thank the reviewer for pointing us to this study. Yes, our results are consistent with Zahan et al (2021) about decrease in frequency however, we did not isolated the extreme rain events. It has been included in our references and discussion.

Comment on Section 3.1 ("Spatial Analysis of Trends")

> Finds rainfall trends: T&K - increase: consistent with poleward migration of monsoon winds and more rain over desert regions. Indo-Gangetic plains - decrease: reported earlier in some studies. NE India - decrease. These are already reported for observations. Please cite relevant studies.

Done. We have added references to Jain et al (2013), Dhara et al (2025) and Zahan et al (2021) as per reviewer's suggestion.

Comment on Section 3.2 ("Characteristics of Rainfall Regimes")

> Grouping of clusters is debatable. Northeast rainfall, break phase (NE-B) [2, 5, 8, 10]: I agree 5 and 10 look similar. 2 seems to be somewhat different. Especially focussing on the low-level winds (Figure 5). Cluster 8 definitely looks different. Grouping of clusters will always remain debatable if it is based on visual inspection unless the authors can argue based on some matrix that can quantify the degree of association of clusters. Monsoon depressions, active phase (MD-A) [4, 11, 9]: 11 is over the T&K region and 9 is NE region. Are they the same? Your rainfall trend analysis says they are Not.

We appreciate this concern and have clarified the manuscript accordingly. The 11 clusters are the objective output of the k-means. The four broader categories introduced in Section 3.2 are not quantitative classes and are not used in any subsequent calculations. They are used only to improve narrative flow by discussing related rainfall patterns together. We now state this explicitly in the manuscript.

Comment on Section 3.3 ("Cluster Transition Dynamics")

> Transition probabilities discussed are agreeable. Noteworthy that authors mention "Cluster 8" acts as an independent category. It goes back to my comment on the debatable logic behind grouping of clusters. Another comment on the transition probabilities. Table 1 can be converted to a heat-map with warmer colors for higher values and cooler colors for smaller values to make it visually more communicative. I mean, the table would remain a table but each cell will have a color. The

> nonsignificant transition probabilities may be omitted or not colored in the heat-map. Also, Figure 7 can be omitted (or moved to supplementary).

We thank the reviewer for suggestion regarding the visualization of the transition probabilities. However, we are uncertain about the journal's policy regarding the use of colored tables in the manuscript. In the current table, we already distinguish stronger and weaker transition probabilities using bold and regular font styles, which provides a visual separation of the statistically significant and weaker probabilities. The Figure 7 is indeed the visual representation of the transition probabilities. Therefore, we have retained the existing format.

Comment on Section 3.4 ("Seasonality of Clusters and Long-Term Changes")

> (paragraph centered around L#340): These observations are consistent with the cluster spatial patterns. But at the same time, aren't these statements redundant since from the cluster spatial patterns (Figure 4) it is obvious that rainfall contributions over T&K, IGP, and NE would dominantly come from (6,9), (1,7), and (2,5,8,10) respectively? On a closer observation, I notice from Figure 8 that, IGP has a lot of contributions from Cluster-8 and also cluster-6 (in July) and I fail to see contributions from Cluster-1. Also, for NE I see a lot of contributions from Cluster-3. Maybe I am not reading the plot well. In my opinion, Pie Diagrams or Stacked Violin Plots might be a better option instead of a stacked bar plot. In any case, the narration requires a lot more transparency.

Figure 8 shows rainfall intensity multiplied by frequency, which conveys most information in the stacked bar diagram. For this reason, we have retained this depiction. Cluster 1 does not have a core over the IGP region. Also the frequency is low which is why it will not contribute too much. Cluster 6 has more rainfall intensity over IGP and it has a higher frequency compared to cluster 1, which is why it contributes more to IGP rainfall than cluster 1. The contributions of Cluster 6 to IGP in July and Cluster 3 to NEI are physically consistent with their spatial patterns described in Section 3.2 and have been explicitly acknowledged in the revised text.

> L#347: "consistent with the observed Northeast India drying": Please provide either evidence or reference.

Done.

Comment on Conclusions and Remarks

> Claims are unsupported by analyses presented. For example, "Our results show strong spatial heterogeneity in ISM variability and trends. Over Thar & Kutch (T&K), increased rainfall since 1990 is primarily linked to higher frequency of mid-tropospheric cyclones and westward-propagating systems (Clusters 6 and 9), which transport moisture into arid zones." Which analysis evidentially supports this claim? I only see your statement, "Another cluster that shows interesting transition

> behavior is Cluster 9 (northwest-focused active). It has a significant probability to transition to Cluster 3 ($P_{9,3}$ = 0.118), meaning after a rain event in the northwest (often due to a mid-level cyclone or dying depression), the monsoon likely goes into a break."

The synoptic composite for Cluster 9 in Figure 5 discussed in Section 3.2, which shows a cyclonic anomaly over Rajasthan and enhanced precipitable water over northwest India consistent with mid-tropospheric cyclone activity, and Figure 10 in Section 3.4, which shows that increased frequency of Clusters 6 and 9 is the dominant contributor to T&K rainfall increase.

Figure 7 and table 1 show the transition dynamics and analysis mentioned in the statement as well. This provides the necessary analytical and evidentary support.