Reviewer 1 (RC1): Adway Mitra

MS No.: egusphere-2025-4585

> The paper focuses on identifying spatial patterns of Indian Monsoon rainfall using K-means clustering. These patterns are identified with specific phases of Indian Monsoon, such as the break phase with low overall rainfall, and several other phases, each of which is characterized by rainfall over some particular region like the Indo-Gangetic Plane, Northeastern India or Kutch and Thar. It is argued that these 10-odd clusters represent the spatial distribution of rainfall on most of the days in the study period, it is assumed that these patterns follow Markovian dynamics, and transition probabilities are worked out with some physical justifications. The change in the frequency of each of the clusters/patterns and the intensity of rainfall associated with them is studied across the entire study period, and such change is considered as a marker of climate change. I have the following observations about this paper:

We thank Dr. Mitra for his insightful comments. Author reply to each question is given below.

Comment 1: Originality

> This is not the first work that aims to identify a set of "canonical" patterns of rainfall. MISO or Monsoon Intra-Seasonal Oscillations (Suhas et al, 2013, "An Indian monsoon intra-seasonal oscillations (MISO) index for real time monitoring and forecast verification") and the binary Random Fields (Mitra et al, 2019, "Spatio-temporal patterns of daily Indian summer monsoon rainfall", Sharma et al, 2020, "Spatio-temporal relationships between rainfall and convective clouds during Indian monsoon through a discrete lens"). The patterns identified here have significant similarity with these patterns, and hence some sort of qualitative and quantitative comparison is needed, along with a justification of why at all more patterns like this are needed.

We thank the reviewer for bringing these studies to our attention. We think independent methodologies converging on similar patterns actually strengthens the case that these modes of monsoon rainfall variability are physically robust rather than artifacts of any particular technique.

We agree that rainfall-regime identification is not new in itself, and we have revised the Introduction to position our study more clearly relative to previous work. The novelty of this paper does not lie only in identifying spatial rainfall patterns, but in the post-clustering diagnostic framework built on those patterns. Specifically, this study (i) derives regimes directly from 58 years of daily 0.25° IMD rainfall without dimensionality reduction, (ii) associates each regime with composite synoptic anomalies of wind, sea-level pressure, and moisture, (iii) quantifies regime persistence and sequential evolution using transition probabilities, and (iv) decomposes regional rainfall changes into cluster frequency and intensity contributions. While some regimes are qualitatively similar to those identified in earlier studies, that convergence supports their physical robustness. We have now added discussion of Suhas et al. (2013) and Mitra et al. (2019) in the Introduction and clarified how the present framework differs in aim and analysis.

Comment 2: Choice of K-means

> It is not very clear what was considered as the feature vectors for clustering. Is it vectorized form of the rainfall values of each day? If so, we may be missing out on the spatial aspects due to vectorization (2D to 1D). Also, what is the nature of the space on which we are doing the clustering? What kinds of clusters are desired, and is K-means the ideal way to do it? What is the within-cluster variance and across-cluster separations?

The daily rainfall field is vectorized from 2D to 1D for clustering, which is purely a programming convention and does not result in any loss of spatial information. K-means operates in Euclidean space where distances between data points are independent of data dimensionality or spatial arrangement — each grid point serves as an independent axis in this high-dimensional space, and the distance between any two daily vectors therefore inherently reflects their spatial similarity across the entire rainfall field. This holds for most clustering methods unless spatial coordinates are explicitly encoded as additional features.

Regarding the choice of k-means: We understand that application-specific requirements influence the optimal choice which is why we do not advocate one clustering method over another. K-means was selected based on our prior experience demonstrating that it yields physically interpretable and stable rainfall clusters over Australia (Raut et al., 2014) and because it is extensible for climate model ensemble comparison (Raut et al., 2017), directly facilitating the model evaluation applications we envision for this dataset. We have also worked with SOMs, DBSCAN, and GMMs in other contexts, and in our experience k-means performs comparably to these methods for gridded rainfall fields.

Regarding within-cluster variance and between-cluster separation: these are directly and quantitatively addressed in the elbow plot (Figure 2) through WCSS and BCSS as explicit functions of k. The clear elbow at k = 11 confirms an optimal balance between intra-cluster compactness and inter-cluster distinctiveness. Cluster stability was further verified through repeated initializations with fewer than 0.1% of days changing cluster affiliation, providing strong evidence that the clusters are robust and well-separated.

Comment 3: Markovian Dynamics

> What is the basis of considering the first-order Markovian dynamics of the clusters? What is the physical justification of it? How many days does each cluster persist and why? The kind of

> transition dynamics that has been estimated - how can it be justified using known processes (oscillations etc) associated with the Monsoon?

Markov chain framework is used for computing the transition probability matrix and serves as a compact, quantitative diagnostic of regime persistence and sequencing (Franzke et al. 2008), one that can be directly compared against transition matrices derived from model simulations. This provides a rigorous methodology for evaluating whether models correctly reproduce not just the spatial patterns of ISM rainfall regimes, but also their sequential evolution, that mean-state comparisons alone cannot capture. Here it is not intended as a predictive tool.

The transition probability matrix is a way of showing repetition of each cluster and reveals both persistence and transience in a physically meaningful way. Break monsoon (Cluster 3) shows the highest self-transition probability (~0.75), consistent with observed break durations of several days to weeks while Depression-related clusters (11, 4, 9) show lower persistence (~0.4) with sequential transition 11, 4, 9, 3. This is a metastate as described in (Franzke et al. 2008) and mirrors the well-documented westward propagation of depressions from the Bay of Bengal through central India toward the northwest, followed by a break.

As per reviewer's request we did a quick computation that shows the mean duration of each cluster repetition (in days) as follows.

Cluster	Mean Duration (days)
1	1.54
2	1.53
3	3.93
4	1.61
5	1.74
6	2.06
7	2.22
8	2.07
9	1.64
10	1.78
11	1.73

Franzke, Christian, et al. "A hidden Markov model perspective on regimes and metastability in atmospheric flows." *Journal of Climate* 21.8 (2008): 1740-1757.

The paper may become suitable for acceptable and publication after these queries are answered and the paper is modified accordingly.

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 dosn'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.

Community Comments: Nima Zafarmomen

> The manuscript presents a regime-based analysis of Indian Summer Monsoon (ISM) rainfall for JJAS 1961–2018, using: IMD 0.25° daily rainfall, k-means clustering to identify 11 spatial rainfall

> regimes, NCEP–NCAR Reanalysis-1 to characterize circulation, pressure, and moisture anomalies for each cluster, Markov-chain style transition probabilities between regimes, A decomposition of rainfall change (1961–1989 vs 1990–2018) into frequency vs intensity contributions by regime and region. The study aims to link objectively derived rainfall regimes to synoptic drivers (depressions, trough position, breaks, etc.), examine their transition dynamics, and use these regimes to interpret long-term regional rainfall changes (particularly over Thar & Kutch, Indo-Gangetic Plains, and Northeast India).

We thank reviewer for the suggestions.

> The manuscript would benefit from a clearer articulation of what is genuinely new compared to prior regime-based studies of the ISM (e.g., Straus 2022, Catto et al. 2012–style decompositions, previous clustering of rainfall vs winds). Please more explicitly contrast your k-means rainfall regimes with wind-based regimes and IMD active/break diagnostics, and state what new physical insight emerges.

We thank the reviewer for this important point. This has been flagged by another reviewer, and we have highlighted the unique points of our study in the reply and made changes to the manuscript to highlight the same (See Reviewer 1, Comment 1).

> The Elbow method and five initializations provide only limited evidence that 11 clusters are "optimal," especially for high-dimensional rainfall fields. Please show more formal robustness tests (e.g., silhouette scores, explained variance, random subsampling, or clustering on sub-periods), and ideally a sensitivity example for k = 9 or 12.

Thank you for pointing this out. When k-means with independent random seeds (that's N_M_k random numbers) converges to the same cluster solution in two initializations, that alone is sufficient to demonstrate that the clusters are stable and did not arise by chance because the probability of two independent random initializations being the same is infinitesimally small. In that sense, running five initializations was already an over extension. We have therefore changed the description in the text to avoid giving the false impression to the future readers that more iterations is a better test.

We show within cluster and between cluster distances with the elbow method in this work. This is the most widely used method that shows compactness and separability. We are also familiar with the above mentioned tests to decide the optimal number of clusters and have used them in our other studies. We do not agree with the reviewer that these are somehow more formal tests than within cluster and between cluster elbow tests. In our experience, multiple statistical tests do not even agree on the precise number of clusters but help in finding the range of optimum number of clusters as a guidance. We agree with Anderberg's view in Cluster analysis for applications (1973), that the data from real systems may not have a fixed set of clusters, however the good clusters are more compact, separable and stable, which is demonstrated in this paper. However, the major test of any clustering methodology is their physical interpretability, which in this case is evident from the synoptic systems associated with the rainfall clusters and their transition dynamics. Hence, 11 clusters worked nicely for this dataset and problem at hand.

> The transition matrix is interpreted as a Markov chain, but daily rainfall regimes are highly autocorrelated and embedded in multi-day synoptic systems. Please discuss the validity and limitations of the Markov assumption, and consider including multi-day transitions (e.g., 2-day lag).

Autocorrelation and Markov structure are not mutually exclusive. In fact, a first-order Markov chain naturally produces autocorrelated sequences. If a cluster has a high self-transition probability, like Cluster 3 with P33 = 0.75, the resulting time series will be strongly autocorrelated, with long runs of the same state. In addition, we are not using the Markov chain to forecast the next rainfall regime, and we make no claim that the ISM state sequence is literally generated by a first-order Markov process. Rather, the transition matrix is compact summary of the sequential behavior of rainfall regimes.

> Several regimes are qualitatively associated with monsoon depressions, mid-tropospheric cyclones, and trough positions, but the linkage remains somewhat descriptive. Can you provide more objective metrics (e.g., depression track climatology, MTC detection, trough latitude index) to verify that specific clusters indeed correspond to those synoptic features?

We respectfully disagree that the synoptic associations are merely descriptive. Depressions, mid-tropospheric cyclones, and monsoon troughs are not subtle features, they are the most prominent synoptic systems in the Indian summer monsoon, with unambiguous signatures in pressure, wind, and moisture fields. Moreover, the composite anomaly maps in Figures 5 and 6 are computed over 58 years of data. Conducting full depression track climatology, MTC detection, and trough latitude index analyses as per the reviewer suggestion, is beyond the scope of this paper.

> I strongly recommend to consider "Analysis of historical global warming impacts on climatological trends for the partially gauged Hirmand river basin based on multiple data products and bias correction methods" in your paper.

This work is not relevant in our study.