

## **Review of “An Observational Perspective on Precipitation Efficiency of Mesoscale Convective Systems over the Asian Monsoon Region”**

### **General comments**

This study undertakes a statistical evaluation of precipitation efficiency in mesoscale convective systems (MCS) over the Asian Monsoon Region.

The authors use the PyFLEXTRKR database, which tracks MCSs globally using satellite brightness temperatures and GPM IMERG precipitation data. They combine this with collocated ERA5 estimates of column-integrated condensate to perform a Lagrangian analysis, providing a cloud-lifecycle perspective for the region.

The study assesses the spatial, structural and lifecycle variability of precipitation efficiency in the tracked MCS. They note a number of meaningful results. They find that precipitation efficiency in MCS is 50% greater than that of non-MCS in the region and find a strong positive relationship between precipitation efficiency and both MCS size and MCS depth. They show that precipitation efficiency is greatest in the vicinity of the convective core and during cloud development. They also show that precipitation efficiency is greater where large-scale moisture is expected to be greater and show a signal of increased precipitation efficiency in regions of high topography.

The overall quality of the paper is good, as the results are clearly presented and the analyses offer a meaningful contribution to the field. However, the description and robustness of the methods lack clarity, and the structure of the manuscript and interpretation of the results should be improved. Therefore, I recommend the paper for publication if the following specific comments can be addressed.

### **Specific comments**

1. Contextualising the results. It would be helpful to provide more specific context on the existing literature on precipitation efficiency in tropical convective clouds. The results of the predominantly “domain-averaged” literature (L34) are not indicated, and existing Lagrangian-based studies of precipitation efficiency in observations and their results are not clearly acknowledged. Additionally, the existing detail is somewhat confusing. The paragraph L36-46 in the introduction provides commentary on expected relationships, but lacks clear sources for the information given and fails to adequately indicate the knowledge gap being pursued. The author’s previous state that “the role of cloud morphology and convective organization in shaping precipitation efficiency remains understudied” (L35), but at L43 reference a result stating convective cores have greater precipitation efficiency while stratiform regions have lower values. I suggest revising this section to clearly state prior findings and motivate the study.

[We thank reviewer for this helpful comment. We have revised paragraph 2 of the Introduction to \(i\) summarize the key findings from previous Eulerian and Lagrangian studies, \(ii\) clearly identify what is already known about precipitation efficiency across cloud regimes and MCS](#)

components, and (iii) more explicitly state the remaining knowledge gap. In particular, we clarify that previous studies have largely focused on domain-averaged or storm-integrated precipitation efficiency, whereas observational analyses of the geographic variability, phase-partitioned efficiency, and lifecycle evolution remain limited. These revisions provide clearer motivation for the objectives of the present study.

2. The description of the results. While some aspects of the methods are clearly detailed, some key information and definitions are not provided. In particular, it is not completely clear to me whether you (i) use the PyFLEXTRKR database or (ii) perform the algorithm yourselves. Additionally:

We thank the reviewer for this important comment. In this study, we use the existing PyFLEXTRKR MCS tracking dataset developed following Feng et al. (2021), rather than performing the tracking ourselves. We have revised the manuscript in Section 2.2 to clarify.

a. Feng et al., 2021 use a mean hourly precipitation threshold of 2 mm h<sup>-1</sup> to define candidate MCSs, but you state using a 3 mm h<sup>-1</sup> threshold (L129). Was this a typing error? If not, why use a different threshold and how would this impact your results?

We thank the reviewer for identifying this error. The precipitation threshold to define MCSs was 2 mm h<sup>-1</sup>, following Feng et al. (2021). The value of 3 mm h<sup>-1</sup> reported in the manuscript was a typo and has been corrected in Section 2.2.

b. L130: please state the exact lifetime-dependent thresholds used to categorise the precipitation features.

Section 2.2 has been revised to report representative thresholds for precipitation feature area, mean rain rate, rain-rate skewness, and heavy-rain volume ratio following Feng et al. (2021).

c. L132: “centroid” is an unclear word choice. What object was the overlap and propagation direction test applied to? And what was the requirement exactly?

We have revised the text to clarify the tracked object (CCS masks) and how temporal continuity was established between consecutive time steps, replacing the term “centroid”.

d. How are the non-MCS cases designated? Are these also tracked in the same manner?

Non-MCS cases were not tracked independently. Instead, MCS precipitation was identified using PyFLEXTRKR MCS masks, and precipitation outside these masks was classified as non-MCS precipitation. We clarify this point in Section 2.2.

e. How were the warm anvil regions defined?

Warm anvil regions are characterized by brightness temperatures between 241 and 261 K. Although this definition was in Section 3.2, we mention this definition earlier within Section 2.2.1 now.

f. Fig. 1: Why are many of the cases in panels b and c are excluded from the final results in panel d? The caption states that all panels are filtered to the defined MCS.

Thank you for this observation. The discrepancy arose because panels (b) and (c) displayed the full precipitation and cloud fields, while panel (d) showed only pixels associated with tracked MCS objects after applying the FLEXTRKR identification criteria. We have updated the figure so that panels (b) and (c) are also masked to include only tracked MCS pixels, ensuring consistency across all panels.

g. The exact period being studied was not specified; “2016 monsoon period” (L157) is not sufficient.

We thank the reviewer for pointing this out. We have revised the manuscript to specify the exact analysis period from 10 August–10 September 2016.

h. L160: why is this sample “representative”, is this claim based only on the number of samples, or have you also tested that the distribution of cloud properties samples the true distribution of MCS observed in the region?

We agree that the term “representative” may imply formal testing of sample representativeness. Since such an assessment was not performed, we have revised the text to describe the dataset as a “large sample of monsoon-season MCS activity”.

i. How is the instantaneous precipitation efficiency defined? As  $E = \text{mean anvil } (P) / \text{mean anvil } (CWP)$  or  $E = \text{mean anvil } (P / CWP)$

Thank you for this important clarification. The instantaneous precipitation efficiency is calculated as the ratio of the MCS-area-mean precipitation rate to the MCS-area-mean cloud water path at each time step,

$$\epsilon(t) = \frac{\bar{P}(t)}{\bar{CWP}(t)}$$

where the overbar denotes averaging over all pixels belonging to the tracked MCS object. We take the ratio of averages, not the average ratio. We have clarified this distinction in Section 2.3.

3. Interpreting the partitioning of ice and liquid precipitation efficiency. An interesting angle of this study is the assessment of contributions from cloud liquid vs cloud ice to the total cloud precipitation efficiency. But I think the interpretation of these results may overstep. This partitioning essentially tells us about the relative amount of liquid and ice condensate under a given precipitation efficiency regime. And may not indicate a direct link to the precipitation removal tendency of each partition. For example, in a single column, when  $IWP > LWP$ ,  $P/IWP < P/LWP$ , and  $E_i < E_l$ . The greater “liquid water path efficiency” in this case could then result either from greater removal or merely from lower production (such as in high anvil cloud). Instead, the results and their description and discussion implicitly assume the  $E_i$  and  $E_l$  values indicate increased ice or liquid removal by precipitation, respectively.

Please either (i) change the terminology used or (ii) clearly state the meaning of and possible reasons for high/low ice/liquid “precipitation efficiency”. Please also either (i) reformulate the analyses to describe the proportion of cloud ice/cloud liquid

under each spatial, structural or lifecycle precipitation efficiency regime; or (ii) add results detailing the mean cloud ice and water that corresponds to the results (like nicely shown in Fig. S4). Please also clarify in each instance how best we can interpret the condensate-partitioned results, given what it exactly represents (i.e., when can we expect it may or may not actually correspond to ice or liquid increased precipitation efficiency).

We thank the reviewer for this thoughtful comment. We have revised Section 2.3 to clarify that  $E_i = P/IWP$  and  $E_l = P/LWP$  quantify precipitation normalized by the available ice and liquid condensate, rather than direct measures of ice-to-rain or liquid-to-rain conversion efficiency. We now emphasize that variations in these metrics may arise from differences in condensate abundance, residence time, or production, in addition to precipitation removal. Accordingly, we have revised the terminology and discussion to avoid implying that higher  $E_i$  or  $E_l$  necessarily reflects more efficient precipitation production from the corresponding condensate phase. We have also added a paragraph discussing this limitation and the appropriate interpretation of the partitioned efficiency metrics.

#### 4. Robustness of some results.

a. I am sceptical as to the robustness of the 2d spatial results as they are presented. This is because you stated a sample size of  $O(1000)$ , and different regions may be characterised by a particular cloud type or cloud lifecycle and your later results indicate a strong dependence on the size, depth and lifestage of the cloud. If there are spatial biases in these properties over the region that may instead explain some of the spatial patterns shown. Thus, it would be helpful if you could illustrate the frequency and characteristics of the MCS and non-MCS datasets in the region.

Thank you for this helpful comment. To demonstrate the robustness of the spatial analyses, we have added Supplemental Figure 3 showing the spatial distribution of MCS and non-MCS sample counts. The majority of grid cells across our domain have at least 100 samples.

b. Fig. S5 shows a 2d plot of the precipitation efficiency over the spatial axis and local time. However, the data appears noisy and is difficult to interpret. Additionally, you make little reference to the diurnal cycle in the text. I would remove this dimension and instead focus on the main result, which was the increase in precipitation efficiency with topography. On that. Please specify what the black line exactly represents in Fig. S5, as well as the prevailing propagation direction of the MCS.

We thank the reviewer for this helpful suggestion. Since the manuscript focuses on the spatial distribution of precipitation efficiency and its evolution over MCS lifecycle, we have removed Fig. S5 from the Supplementary Material.

c. A second key result in Section 3.1 was the latitudinal (land vs ocean) and longitudinal (monsoon moisture gradient) dependence of precipitation efficiency. It would be helpful to include the support (number of samples) in Fig. 4 in each bin, as Fig. 3 showed very few MCS and non-MCS coverage in the East. Additionally, please detail the expected characteristics of the monsoon moisture gradient. Does it vary during the period studied?

We thank the reviewer for this helpful suggestion. In Figure. S4, we show the number of MCS and non-MCS samples at each longitude and latitude. We have also expanded the discussion of the climatological Asian summer monsoon moisture gradient in Section 3.1.1. We explain that the west-to-east increase in moisture is a persistent large-scale feature during the mature monsoon season and acknowledge that its strength and position vary on synoptic and intraseasonal timescales.

5. Structure of the results. Most of the results presented in Section 3.1 and some other key results are contained within the appendix; please include these in the main text. Instead, Section 3.1.2 could be relegated to the appendix. Fig. S3 is almost identical to Fig. 4, it would be clearer if you combined these.

Thank you for this feedback. Following the reviewer's recommendation, we have removed Supplementary Figures S3 and S5 because of redundancy. The discussion has been streamlined to focus on key findings only. We have, however, retained Section 3.1.2 in the main text because it provides an independent observational evaluation of the ice-phase precipitation efficiency using the CCIC dataset, which we consider an important validation for the study.

## 6. Conclusions.

a. Please provide a comment on how your results fit into the existing literature.

We thank the reviewer for this suggestion. We have added a short paragraph to the Conclusions placing our findings in the context of the existing literature. We note that our results are broadly consistent with the studies discussed in the Introduction, including previous work demonstrating enhanced precipitation efficiency in organized convection.

b. It would be helpful to comment on whether there are relevant biases in the datasets used. For example, IMERG and ERA5 accuracy is highest where ground-based observations are most plentiful, and India is known to suffer from greater biases in both datasets compared to other regions. Do you think that would impact your results?

We have added a discussion in the Conclusions acknowledging the main sources of uncertainty in the observational datasets.

c. It would be helpful to comment on the regional context. How would your results translate to other regions? Are the comparisons between MCS and non-MCS representative, or highly specific to the region because of the characteristics of those that fail the PyFLEXTRKR MCS tests?

We thank the reviewer for this valuable suggestion. We have expanded the Conclusions to clarify the regional context of our findings.

d. Also, it may be worth commenting on whether it is organisation or merely system size that determined the contrast between MCS and non-MCS cases, or how this could be analysed in future studies that additionally consider characteristics of the large-scale environment.

We have revised the Conclusions to clarify that our results suggest the observed contrast between MCS and non-MCS precipitation efficiency is primarily associated with storm organization and its accompanying dynamical and microphysical processes, rather than system size alone.

e. Clarity of the written expression. There are several instances where the exact meaning of sentences is unclear or poorly expressed. I think you can improve the clarity of this text nicely by editing it again to ensure the point of each sentence is clear and expressed with specificity.

We thank the reviewer for this helpful comment. We have thoroughly edited the manuscript to improve the clarity and specificity of the writing. Several sections have been rewritten for improved readability, ambiguous wording has been clarified, and the manuscript has been carefully proofread in collaboration with the co-authors to ensure that the scientific arguments are presented clearly and consistently throughout.

### Technical corrections

L114: Please provide a reference for the CCIC dataset.

A reference for the CCIC dataset has now been added to the manuscript.

L140: “local maxima in P are collocated with...” Is this a description or a methodological step?

The statement was intended as a descriptive observation highlighting the association between precipitation maxima and MCS convective cores, rather than a methodological criterion. We have revised the text for clarity.

L151: “+ cloud anvil area” please revise to “and ...”

The text has been revised as suggested.

L355-360: This claim is not really supported. Could you please show the lifecycle of the MCS studied, i.e., when do they reach “maturity”, when is their organisation “established”? This would nicely link the results in Fig. 9 to the true cloud lifecycles in your dataset.

Thank you for this helpful suggestion. To better connect the precipitation-efficiency evolution to the physical lifecycle of the MCSs, we computed normalized lifecycle composites of MCS area, minimum cloud-top brightness temperature ( $T_b$ ), mean rain rate, and maximum rain rate (Figure S6). These diagnostics show that mean and maximum rain rates peak at normalized lifecycle values of approximately 0.26 and 0.37, respectively, while the coldest cloud tops occur near 0.42. Maximum MCS area is reached later, at approximately 0.58. Together, these results indicate that the strongest convective organization and deepest cloud tops occur during the middle portion of the normalized

lifecycle, providing an objective reference for the mature stage of the systems. We have added this figure and revised the discussion of Figure 9 accordingly.

Fig. 5: the year of Kukulies et al., in the figure does not match that referenced in the text. Thank you for identifying this inconsistency. The reference year for Kukulies et al. has been corrected for consistency.

Fig. 7 and 8: Perhaps you could specify that it is “maximum MCS area” and “minimum Tb”.

We have updated Figures 7 and 8 accordingly, specifying “maximum MCS area” and “minimum Tb” in the figure labels and captions to improve clarity.

Fig. 4 and 9: Should  $E_i$  and  $E_l$  be shown on the same yaxis range?

Thank you for this suggestion. To facilitate comparison between the  $E_i$  and  $E_l$  distributions, we have revised Figures 4 and 9 to use consistent y-axis ranges. The figures have been updated accordingly in the revised manuscript.