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 previously 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.

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:

a. Feng et al., 2021 use a mean hourly precipitation threshold of 2 mm h-1 to define candidate MCSs, but you state using a 3 mm h-1 threshold (L129). Was this a typing error? If not, why use a different threshold and how would this impact your results?
b. L130: please state the exact lifetime-dependent thresholds used to categorise the precipitation features.
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?
d. How are the non-MCS cases designated? Are these also tracked in the same manner?
e. How were the warm anvil regions defined?
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.
g. The exact period being studied was not specified; "2016 monsoon period" (L157) is not sufficient.
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?
i. How is the instantaneous precipitation efficiency defined? As E = mean anvil (P) / mean anvil (CWP) or E = mean anvil (P / CWP)

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).

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.
    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.
    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?

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.

6. Conclusions.

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

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.

Technical corrections

L114: Please provide a reference for the CCIC dataset.
L140: “local maxima in P are collocated with…” Is this a description or a methodological step?
L151: “+ cloud anvil area” please revise to “and …”
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 oganisation “established”? This would nicely link the results in Fig. 9 to the true cloud lifecycles in your dataset.
Fig. 5: the year of Kukulies et al., in the figure does not match that referenced in the text.
Fig. 7 and 8: Perhaps you could specify that it is “maximum MCS area” and “minimum Tb”.
Fig. 4 and 9: Should E_i and E_l be shown on the same yaxis range?