Review of "An Observational Perspective on Precipitation Efficiency of Mesoscale Convective Systems over the Asian Monsoon Region"

General Comments

Analysis of 1321 tracked Mesoscale Convective Systems (MCSs) over the Asian Monsoon Region over a single month (August–September 2016) is performed by examining their precipitation efficiency, ϵ. ϵ is defined by *P/CWP* where *P* is the rain rate and CWP is the cloud water path. ϵ has dimensions of time^{-1}, and its inverse can be interpreted at a residence time. ϵ is further partitioned into its liquid- and ice-phase variants. MCS precipitation is found to be 50% more efficient than non-MCS for ϵ and the phase-partitioned metrics. The spatial patterns are analysed, both geographically and by longitudinal/latitudinal averaging, with increased ϵ seen over the Arabian Sea, Bay of Bengal, and towards the equator, and increases seen from north to south, and east to west. The spatial pattern is compared to a different, dimensionless way of calculating PE based on the positive tendency of ice water path (IWP), and results are comparable *for one of the phase-partitioned efficiency metrics*.

Regions within the MCS are analysed, with the core showing the highest ϵ, followed by the cold then warm anvils. ϵ increases with increasing MCS area up to 2×10^4 km^2, then decreases, which the authors attribute to a greater amount of ice in the cold cloud shield. It increases monotonically with decreasing MCS brightness temperature (increasing depth). It shows MCS lifecycle dependence, increasing up to 20% of the MCSs' lifecycle, before steadily decreasing, perhaps as a result of the area relationship they found.

Overall, I thought this paper was well written and the figures clear. Most of the results were well backed up by evidence. I am not wholly convinced on the benefits of performing the phase-partitioning of ϵ, some questions are below, but am happy with the overall results. I suggest it should be returned for minor revisions, with the points below addressed. I found some of the results discussion to be overly descriptive, and would have liked to see more referencing of the literature in this area to compare their results with previous studies. This study clearly lays the foundations for a further study comparing ϵ in observations and simulations, which I can see would be valuable.
We thank the reviewer for their time and effort spent reviewing and providing feedback on our work.

Specific Comments

ϵ calculation: Based on your definitions, efficiencies obey harmonic addition. This would help the reader contextualize some of the information in the paper, and would be worth highlighting.
Thank you for this helpful suggestion. In Section 2.3, we note that because CWP = LWP + IWP, the corresponding precipitation efficiencies satisfy the harmonic-addition relationship, $1/\epsilon = 1/\epsilon_\ell + 1/\epsilon_i$. This provides additional physical context for interpreting the total, liquid-phase, and ice-phase precipitation efficiencies throughout the manuscript.

ϵ calculation: Further, it seems that the ratio between the liquid- and ice-phase is the important piece of information you get from decomposing by water phase – did you look at this? I found the interpretation of the two efficiency phase-partitioned metrics difficult to make sense of – is this standard practice for precipitation efficiency analysis? Could you justify its use some more, stating what the value gained is and how to interpret the difference signals seen? I will leave it up to you as to whether you think the manuscript needs updating.
To Sections 2.3 and 3.1, we clarify the physical significance of these partitioned-values: "These values reflect the relative P produced per unit condensate, rather than an intrinsic microphysical conversion efficiency of ice-to-rain or liquid-to-rain processes."

ϵ calculation: Equation 1. I see you can either calculate it instantaneously or time-averaged. I am not clear when you have done each of these in your results. Please clarify.
Thank you for pointing this out. We have revised Section 2.3 to distinguish the two formulations of ϵ. Spatial maps and statistical distributions are computed from the ratio of time-averaged precipitation to time-averaged cloud water path, whereas the MCS lifecycle analysis is based on instantaneous ϵ calculated at each MCS time step and subsequently composited onto a normalized lifecycle coordinate.

Condensate residence times ($1/\epsilon$: I am not sure how to interpret this. Does a residence time of a few minutes imply that the condensate turns into rain in a few minutes? Or that the condensate turns into rain and reaches the surface in a few minutes? The latter is not possible – a 10 m s^{-1} would take more than 10 minutes to fall from above 6000 m.
Thank you for this important clarification. We agree that $1/\epsilon$ should not be interpreted as the physical fall time of hydrometeors. Instead, it represents a bulk condensate turnover (or depletion) timescale, i.e., the time required to convert condensate aloft and in-cloud to condensate sedimenting out of cloud . We clarified the interpretation of condensate residence time in Section 3.1 of the revised manuscript.

Specific comments
L44: Does the lower fall speed of lighter rain (smaller raindrops) also lead to more evaporation and lower ϵ? For that matter, what about the smaller size of raindrops with bigger surface area to volume ratio?
We thank the reviewer for this insightful comment. Lower fall speeds are not the only mechanism contributing to lower ϵ. As you note, smaller raindrops are more susceptible to evaporation during descent, given their larger surface-area-to-volume ratio. We have added a sentence to the Introduction about enhanced evaporation and lower ϵ for small drops.

L168: Only one year - I guess this is to match DYAMOND phase 1. Is this year affected by ENSO? Could you perform for longer to get better idea for representivity? (Not suggesting you do, but perhaps mention in future work if not there.)
Thank you for this helpful comment. We agree that using a single monsoon season limits the

assessment of interannual variability. August–September 2016 was selected to coincide with DYAMOND Phase I, as you note. We have added a statement to Section 2.3 acknowledging that future work should extend the analysis to multiple years and different ENSO phases to assess the robustness and representativeness of our findings.

Figure 3: ϵ_i in particular seems to be high near eastern coasts. Could you comment on this?
Thank you for this observation. In section 3.1 we noted that the elevated ϵ_i along the eastern coastal regions likely reflects efficient ice-phase precipitation production associated with frequent deep convection over the Bay of Bengal, South China Sea, and Maritime Continent, together with humid maritime environments that reduce sub-cloud evaporation and allow a larger fraction of ice-phase precipitation to reach the surface.

Figure 3. Uses the difference in ϵ, $\Delta\epsilon$. Given ϵ and $1/\epsilon$ both have meaning, would a ratio between different ϵ_s not be more relevant?
Thank you for this helpful suggestion. We agree that both ϵ and its inverse ($1/\epsilon$) are physically meaningful. We chose to present $\Delta\epsilon$ because it directly quantifies the absolute enhancement in condensate-to-precipitation conversion associated with MCSs. Ratio-based metrics can become disproportionately large in regions where the denominator is small, potentially exaggerating relative differences. We have added a brief explanation of this rationale in section 3.1.
Figure 3/5. Is it worth commenting on and/or investigating the low ϵ east of Sri Lanka? It seems like a very consistent feature, I guess caused by low rainfall there.
Thank you for pointing out this interesting feature. We agree that relatively low ϵ is consistently observed east of Sri Lanka in both Figures 3 and 5. Because it appears in both the MCS and non-MCS analyses, it likely reflects a regional characteristic rather than a process unique to MCSs. We have added a brief discussion noting that this feature may result from relatively low precipitation rates in this region.

Section 3.1.1. I am not sure how much value this section adds. A lot of it is descriptive, explaining how the geographic features lead to the observed changes in ϵ. These are clearer in my opinion in Figure 3.
We thank the reviewer for this feedback. We have shortened the subsection to focus on physical interpretation of observed spatial gradients in ϵ rather than description of geographic patterns. Specifically, we now relate the longitudinal variations to the climatological Asian summer monsoon moisture gradient and large-scale moisture transport.

Section 3.1.2. What is the correlation between IWP from CCIC and ERA5?
Thank you for this suggestion. We added the Spearman rank correlation between IWP in these two datasets ($\rho \approx 0.6$, $p \ll 0.01$) to this section. There is a statistically significant positive relationship between the two datasets despite differences in their retrieval and assimilation methodologies.

Section 3.1.2/Figure 5. This shows ϵ_i for non-MCS (I believe) and PE. First, state this in the section – it wasn't obvious to me and I had to compare with Figure 3 to determine this. Second, justify why you are using both the ice-phase version, and non-MCS version for this comparison.

Thank you for this helpful comment. We have revised Section 3.1.2 to explicitly state that Figure 5 presents ϵ_i computed using CCIC ice water path and non-MCS precipitation. We also clarified that only ϵ_i can be derived because CCIC provides ice water path retrievals only, and that the analysis is restricted to the non-MCS framework because PyFLEXTRKR tracking was not performed using the CCIC dataset.

L265-7: The stand out feature to me is the area of high values over the Arabian Sea in Figure 5a, which are not completely, but somewhat, replicated in Figure 5b. Could you comment and/or amend text?

Thank you for this observation. We agree that the high ϵ over the Arabian Sea is more pronounced in Figure 5a than in Figure 5b. We have added a couple sentences to Section 3.1.2 noting that, although both formulations show broadly consistent spatial patterns, this feature is stronger in the Li et al. (2022) calculation because it depends only on the precipitation-to-IWP ratio. In contrast, the Kukulies et al. (2026) formulation also includes the condensate tendency term, $\partial IWP/\partial t$, which reduces PE in regions where condensate is simultaneously accumulating and precipitating.

L280: Could you comment on the reasons why the cold anvil is more efficient than the warm anvil?

We have expanded the text in Section 3.2 to clarify why the cold anvil exhibits higher ϵ than the warm anvil. Specifically, we explain that cold anvils remain more directly connected to active convection and efficient ice-phase growth processes, whereas warm anvils represent older, more diluted anvil outflow characterized by weaker vertical motions and greater condensate retention. We have also added supporting references (e.g., Yuter and Houze, 1995; Houze, 2014) to strengthen this interpretation.

L294-304: Figure S4 is key to understanding this paragraph. I would put it in the main text if it is vital, or leave out/reduce this paragraph if not. I also find this paragraph and the reasoning confusing.

Thank you for this helpful suggestion. To keep the main manuscript concise and within the journal's length guidelines, we have retained the figure in the Supplementary Information while revising the accompanying text in section 3.2 to improve clarity and streamline the physical interpretation.

Figure 7: The area starts at 10^3 km^2. However, in Section 2.2, the minimum area of MCSs is 4×10^4 km^2, and "Both the CCS and PF criteria had to be satisfied for at least 4 consecutive hours to exclude short-lived convective clusters.". Can you reconcile this discrepancy? Are you including the PyFLEXTRKR tracks from before the systems get MCS status? This would be fine, but you should make it clear. N.B. 4×10^4 km^2 is a high threshold that selects large systems.

Thank you for pointing this out. Figure 7 includes the full tracked evolution of systems contained in the PyFLEXTRKR MCS database, including stages before they first satisfy the MCS identification criteria. Consequently, precipitation-feature areas smaller than $4 \times 10^4 \text{km}^2$ represent the developing stages of systems that later evolve into MCSs. We have clarified this in Section 3.2.1 and the Figure 7 caption to avoid confusion.

Section 3.3: Often when doing lifecycle analysis you can bin your MCSs into different lifetimes – e.g. short (4-8 hr), medium (8-12 hr), long (12+ hr) (normally pick so that there are roughly equal numbers in each bin). This lets you compare similar types of MCSs on an equal footing. I would be interested to see this for your data, as a reply to this review or perhaps in the Supplement. If it is not much work, you can do this, but likewise you could keep this in mind for future work. It might also be interesting to show other variables as a function of lifecycle, such as area, depth…, to see how ϵ varies in conjunction with these. (This might help clarify your points in paragraph L365-71.)
Thank you for this idea. In response, we repeated the lifecycle analysis by stratifying MCSs into short-, medium-, and long-lived systems and compositing each group on a normalized lifecycle coordinate using the median with 95% bootstrap confidence intervals. The resulting lifecycle composites (Figure S7) show that the overall evolution of precipitation efficiency is broadly similar across lifetime classes, although the timing and magnitude of the ice-phase efficiency (ϵ_i) peak vary among short-, medium-, and long-lived MCSs. These lifetime-stratified efficiencies are now in Figure S7 and we have added a brief discussion in Section 3.3. Investigation of additional structural variables (e.g., area and cloud depth) will be pursued in a subsequent modeling study.

L367: "shrinking system area" could be associated with increased ϵ (Figure 7). Implies that there may be some path dependence to area relationship if what you say here is true – if a system is growing a given area is associated with higher ϵ and shrinking lower. Something to think about.
Thank you for this insightful observation. We agree that the relationship between MCS area and ϵ may exhibit some path dependence. We have clarified that Figure 7 shows the statistical relationship across all systems and lifecycle stages, whereas the lifecycle analysis follows the temporal evolution of individual MCSs. Accordingly, we have softened the interpretation to note that the late-lifecycle increase in ϵ likely reflects changes in condensate conversion processes and system structure in addition to the reduction in MCS area. A more detailed investigation of potential hysteresis between MCS growth and decay phases is beyond the scope of the present study but represents an interesting direction for future work.

Technical corrections

L10: MCS → MCSs
We have added "s" here.

L18: Is "quickly" the right choice of word?

We have replaced "how quickly" with "timescales associated with".

L23: entrainment *and detrainment*
We have added "and detrainment" to this line.

L28-9: Circular, suggest: ~~, further highlighting the importance of understanding ϵ across different cloud types and weather systems~~
We have deleted the sentence as suggested.

L30: "denser" in what sense?
We have deleted "denser" to explicitly refer to clouds with "larger condensate mass (i.e., higher cloud water path)".

L55: 1 → 2 – few GSRMs run with 1 km grid spacings.
We have revised the text in the tracked version to reflect more realistic resolutions (~2–5 km).

L73: Redefine CWP
We have redefined CWP in full.

Figure 1: perhaps cloud ID instead of number? Cloud number makes me think of counts.
We have updated the figure and change title of subplot (d) to cloud ID as suggested.

L133: Mention TCs (and atmospheric rivers) filtered out in PyFLEXTRKR? First seems important for this region.
We have clarified in this section that PyFLEXTRKR identifies systems based on convective cloud and precipitation feature criteria and does not explicitly filter tropical cyclones or atmospheric rivers. However, the applied thresholds (e.g., cloud-top temperature, size, and duration) largely exclude non-convective or synoptic-scale systems.

L150: tb → Tb
We have capitalized the 't' here.

L163: Should probably use conservative interpolation for extensive quantity. Why did you interpolate the coarser grid to the finer? I suppose it makes comparison with MCS masks a lot easier – you could state this.
We chose not to regrid IMERG in order to preserve the native precipitation structure and avoid introducing artifacts into the observational dataset. Instead, ERA5 fields were interpolated to the IMERG grid to facilitate a direct comparison with the MCS masks derived at that resolution. You are correct that this approach does not strictly conserve extensive quantities and may introduce uncertainty. We note this in Section 2.3.

L194: 15 and 30 → 10 and 20 (I think?)

We have revised these numbers, thank you.

L227: skewedness → skewness
Word revised.

L229: variability *likely* reflects
Word added.

L232: decreases → is lower
Word revised.

L247-8: "These latitudinal fluctuations in ϵ metrics again reflect the land-ocean contrast and impact of topography on MCS condensate and precipitation." You could plot the mean land cover as a function of latitude, longitude, and also the mean orography, to make this statement more concrete. Perhaps add these as a figure in reply or in the supplement.
Thank you for this helpful suggestion. We have revised Figure 4 under section 3.1.1 to include the mean orography profile (black dashed line; right axis) as a function of longitude and latitude. The additional information provides geographical context for the observed spatial variations in ϵ and shows that several changes in ϵ coincide with major topographic features across the Asian monsoon region.

Figure 5: title for (b) should be Kukulies et al. (2026).
We have revised the title for Figure 5(b).

L265: State this is from the Spearman rank correlation (as in caption for Figure 5).
We have revised the sentence.

L275: morphologies → regions. And subsequently, e.g. L284. Morphology specifically references the shape of the MCS in my view. Also, on L315, morphology specifically refers to the extent and depth of the MCS. I would recommend only using it for this sense.
We have replaced "morphologies" with "regions" across the subsection.

L277: $\leq \rightarrow \geq$
Thank you for catching this. We revised the mathematical symbol.

L278: median or modal? Table 1 gives median, and here you state modal (and they match).
Thank you for noticing this. The values reported in Table 1 are median values, whereas the text incorrectly referred to them as modal values. We have corrected the text.

L287: 16.6 → 16.4
We have revised the values.

L288-91: If you state that there are multiple studies, I would cite multiple. Or just make singular and cite Gupta 2023.
Since Gupta et al. (2023) was the primary study of interest, we have revised the text to refer to a singular study.

L291: altogether → together
Word revised.

L312-3: "Together, these patterns suggest that anvils-—particularly their warm portions-—act primarily as regions of condensate storage punctuated by localized, phase-dependent precipitation events." Could you spell out the reasoning here? (Also, strange double dashes).
We thank the reviewer for this suggestion. We have revised the text to explain the physical reasoning behind this statement, relating the relatively low precipitation efficiencies in the warm anvil to longer condensate residence times and intermittent, localized phase-dependent precipitation production. We have also replaced the double dashes with standard punctuation for improved readability.

L315: "stratify" to me would mean that you break the results down to have bins of, e.g., small, medium and large MCSs. What you are doing is calculating the relationship between area and mean/max-rainrate. Suggest you reword.
We thank the reviewer for this comment. We have revised the text to avoid the term "stratify": "We examine the scaling relationships between ε and MCS area and depth as a function of lifetime mean and maximum rain rates."

L326: reflects → reflecting
Wording revised.

L336: with MCS depth, which corresponds to a decreasing Tb → with decreasing Tb, which corresponds to increasing MCS depth.
Sentence revised.

L365: previous section, as → previous section: as
":" added

L416 examine the *diurnal and* seasonal dependence of
Words added.

Supplement
Figure S4: missing full stop.
Full stop added.