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

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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 $\epsilon = \frac{\dot{P}}{\mathrm{CWP}}$, where $\dot{P}$ 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.

Specific comments

- ϵ calculation: Based on your definitions, $\frac{1}{\epsilon_{\mathrm{CWP}}} = \frac{1}{\epsilon_{\mathrm{l}}} + \frac{1}{\epsilon_{\mathrm{i}}}$, or efficiencies obey harmonic addition. This would help the reader contextualize some of the information in the paper, and would be worth highlighting.

- 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.

- 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.

- Condensate residence times ($\frac{1}{\epsilon}$: 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.

Specific points

- 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?
- 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.)
- Figure 3: ϵ_{i} in particular seems to be high near eastern coasts. Could you comment on this?
- Figure 3. Uses the difference in ϵ, $\Delta\epsilon$. Given ϵ and $\frac{1}{\epsilon}$ both have meaning, would a ratio between different ϵs not be more relevant?
- 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.
- 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.
- Section 3.1.2. What is the correlation between IWP from CCIC and ERA5?
- 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.
- 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?
- L280: Could you comment on the reasons why the cold anvil is more efficient than the warm anvil?
- 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.
- 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.
- 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.)
- 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.

Technical corrections

- L10: MCS → MCSs
- L18: Is "quickly" the right choice of word?
- L23: entrainment *and detrainment*
- L28-9: Circular, suggest: ~~, further highlighting the importance of understanding ϵ across different cloud types and weather systems~~

- L30: “denser” in what sense?
- L55: 1 $\rightarrow$ 2 – few GSRMs run with 1 km grid spacings.
- L73: Redefine CWP
- Figure 1: perhaps cloud ID instead of number? Cloud number makes me think of counts.
- L133: Mention TCs (and atmospheric rivers) filtered out in PyFLEXTRKR? First seems important for this region.
- L150: tb $\rightarrow$ Tb
- 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.
- L194: 15 and 30 $\rightarrow$ 10 and 20 (I think?)
- L227: skewedness $\rightarrow$ skewness
- L229: variability *likely* reflects
- L232: decreases $\rightarrow$ is lower
- 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.
- Figure 5: title for (b) should be Kukulies et al. (2026).
- L265: State this is from the Spearman rank correlation (as in caption for Figure 5).
- L275: morphologies $\rightarrow$ 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.
- L277: $\leq \rightarrow \geq$
- L278: median or modal? Table 1 gives median, and here you state modal (and they match).
- L287: 16.6 $\rightarrow$ 16.4
- L288-91: If you state that there are multiple studies, I would cite multiple. Or just make singular and cite Gupta 2023.
- L291: altogether $\rightarrow$ together
- 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).
- 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.
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- L326: reflects $\rightarrow$ reflecting
- L336: with MCS depth, which corresponds to a decreasing Tb $\rightarrow$ with decreasing Tb, which corresponds to increasing MCS depth.
- L365: previous section, as $\rightarrow$ previous section: as
- L416 examine the *diurnal and* seasonal dependence of

Supplement

- Figure S4: missing full stop.