Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-835-RC2, 2020

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Cloud Phase Characteristics Over Southeast Asia from A-Train Satellite Observations" *by* Yulan Hong and Larry Di Girolamo

Anonymous Referee #3

Received and published: 28 January 2020

In this paper, the authors investigate cloud properties as seen by different A-train satellites over the Southeast Asia. They further divide clouds into 5 cloud types as a function of their cloud phase and different overlapping possibilities among the cloud-type layers. In the last part of the manuscript, they study possible links between these 5 types and MJO and ENSO conditions.

While the topic of this paper aligns with the scope of the journal, it is difficult to judge the novelty of the analysis since most of it is a kind of climatology rather than new results. However, I acknowledge a tremendous amount of work from the

authors. Yet, the paper is too long and descriptive, which makes it difficult to follow. In addition to trimming the manuscript, I have a couple more concerns to address before recommending this paper for publication. A more detailed explanation is provided below.

Main concerns

1) This paper is too long and descriptive. It is hard to follow and I often lost track of the goal of the sections. Every section should be reduced in size and I would recommend focusing on specific findings relevant to the topic of the study rather than describing every subplot of the figures as well as the behavior of each cloud types...

2) If I understand correctly there is no filtering of the data whatsoever to make them consistent with each other. I find this a little concerning. For example, in section 3.5.1 when comparing the spatial heterogeneity index with CloudSat-CALIPSO, all pixels are used including those where CC and MODIS cloud masks disagree. This may result in large biases as explained by the authors later on. It would be best to keep in the main analysis the pixels where CC and MODIS agree.

3) The authors use the version R04 of the 2B-CLDCLASS-LIDAR product. This version is not free of uncertainties in particular when it comes to detecting shallow cumulus clouds. It's been shown that this version overestimates the amount of shallow cumulus clouds (https://www.earth-syst-sci-data.net/11/1745/2019/essd-11-1745-2019.html). Similarly, nothing is said about any kind of uncertainty in the cloud phase retrieval of this product. For example, has this product been evaluated against other cloud phase dataset (ground-based, satellite or in situ?). I know that the cloud phase confidence considerably decreases when the lidar is totally attenuated and the decision tree only relies on the radar signal. I would suggest the authors to mention these at least.

4) Finally, the authors consistently mention that their results could be used for model evaluation but fail to explain how. I understand it is tempting to sell any observational result as a possible constraint for model, but if the authors want to do so, they need to explain how and why, which is not done here.

Minor comments

Throughout the manuscript, the authors use plural with the term cloud phase, I feel like most of the time, it would be better to use singular.

P2 L35: improve GCM performance ⇒ improve climate simulations
P4 L8: in the lower troposphere ⇒ below 8 km
P4 L8: in the upper troposphere ⇒ above 8 km
P4 L21: Please specify the version. From your table, I believe you use the version R04.
P4 second paragraph: You don't describe how the algorithm works when the lidar signal is completely attenuated. The cloud phase is then based only on Ze and T thresholds, which substantially decreases its confidence level. Since the region under study is dominated by convective clouds, this situation may occur very often.
P8 L13: I don't understand the unit of LTS, it's supposed to be in K (or ËŽC).
P8 L18-23: The CLDCLASS-LIDAR product provides a cloud fraction (between 0 and 1) per layer so how do you get cloud and sample numbers?
P8 L25: Large ⇒ large
Fig. 3: why do you use this particular cross section rather than a zonal mean.
P8 L41: "Also... of cirrus". Why do you mention MISR here out of the blue? Also this sentence is confusing.

P9 L1: Can you elaborate on this statement? DO you mean for that region?
P9 L19: Confusing sentence.
P11 L13: This should appear in the data section along with the other uncertainties related to the datasets.
P11 L22: CLIPASO ⇒ CALIPSO
P11 L22: attenuated by clouds with optical thickness greater than 3.
P12 L14: spatial ⇒ spatially
P12 L26: "due to the small optical thickness of the.."
P12 L27: It's unclear to me why the authors constantly refer to MISR for no reasons since MISR observations are used in this study.
P12 L36: This sentence needs re-wording.
P13 L1-2: I would strongly recommend excluding pixels in which CC and MODIS disagree in the main figures rather than only mentioning it as "not shown".
P13 L2-4: There is no main verb in this sentence, please re-word.
Also, are you referring to shallow cumulus clouds? In this case it would be rather easy to validate your hypothesis by focusing on a shallow Cu dominated region, such as the Barbados. However, depending on the MODIS product used, Pincus et al 2012 reported that a substantial amount of these clouds (partially-filled pixels) are excluded of the cloud product. Another important thing to note is that, R04 over-estimate shallow Cu cloud fraction (see main concern comments).
P13 L33: Proves seems a bit strong.
P14 L35: reflecting
P17: Why are you showing MJO phases? It's been documented in many many studies already. What does this bring to your study?
P17 L38: "suppressed"
P18 L3: I don't understand the meaning of this sentence and I don't see how this could be used to validate climate models.
P18 L8-20: It basically shows there are more convective clouds.
P19 L13-14: Another sentence without meaning, please re-word.

P19 L36: “preferentially occur in”
P19 L38: occur ⇒ are
P21 L10: a comma is missing after ENSO L11. Overall, I would suggest rephrasing this sentence because I don’t think the authors can claim the heterogeneity index captures MJO or ENSO. At best, it varies for the different ENSO/MJO phases, but it’s definitely not well correlated. For the second part of the sentence, unless the authors explain how one could use this for model evaluation, I’d recommend to remove.
P21 L14-16: Here again, there is no tool to compare this to models, at least to the best of my knowledge, so unless the authors elaborate on this statement, they should remove this statement. I can envision a qualitative comparison of heterogeneity at best.

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