Atmospheric
Chemistry
and Physics
Discussions

Open Access

ACPD

**Interactive
comment**

Interactive comment on "Estimating precipitation susceptibility in warm marine clouds using multi-sensor aerosol and cloud products from A-Train satellites" *by* Heming Bai et al.

J. Quaas (Referee)

johannes.quaas@uni-leipzig.de

Received and published: 7 November 2017

Bai et al. present a comprehensive calculation of statistics of precipitation vs. aerosol index/cloud droplet concentration from A-Train satellite retrievals. The document differences in the linear regression metrics when applying different (microwave vs. visible-near infrared) LWP retrievals; different precipitation retrievals (albeit all from CloudSat), different aerosol metrics (aerosol index from different retrievals vs. cloud droplet number concentration estimates), and different thresholds.

Although this work does not provide breakthrough science itself, documenting these differences in a consistent way is useful to the debate. The study is in general per-

formed diligently and is pertinent to ACP.

I have two main modifications I recommend, and several specific comments.

I have not highlighted semantic or orthographic mistakes since I assume the Copernicus copy editing will take care of this.

Main remarks

1) The authors should expand their discussion of the state of the art, especially they need to discuss the various other aerosol-precipitation interactions beyond the "lifetime effect". It is in particular necessary that the authors discuss the role of aerosol scavenging when interpreting the metrics they investigate.

2) I am a bit astonished on how rather poorly the figures are done. The authors should take care revising these so that the content is more readily understandable.

Specific remarks

P1 l15: Here and at a plenty of instances in the text, the relationship between aerosol and precipitation derived from the observations is overly readily interpreted in a cause-effect manner. If only this science was so easy, then a plenty of issues wouldn't exist. I urge the authors to thoroughly revise their text and imply causality only where they can prove it, or at least where they can corroborate cause-effect relationships. Why not interpret a negative aerosol index – POP relationship, for example, as showing the wet scavenging precipitation effect on aerosol?

P1 l26: I suggest the authors adapt to the IPCC AR5 language and define the radiative forcing due to aerosol-cloud interactions ("cloud albedo effect") and cloud adjustments (all subsequent modifications). It is necessary that the authors put the "cloud lifetime effect" hypothesis into context of the manifold other hypotheses.

P2 l4: Also, the relationship would need to be linear (or more generally, of known, universal, monotonic functional form).

P4 l17: But L3 is at 1°, so far from the stated 5 km resolution

p4 l25: again, why the two, and not only the L2 data?

P5 l1: The authors should report exactly how the colocation is done.

P5 l5: A discussion of Christensen et al. doi 10.5194/acp-2017-450 would be useful here

p5 l7: Wood and Hartmann is a good paper, but is it a pertinent reference here?

P5 l12: What is a "pixel" here? A 1-km MODIS cloud retrieval, or rather an aggregated CALTRACK 5 km grid box?

P5 l15: Is this statement tested/implemented? Or is it just taken for granted from the Kubar study?

P6 l29: It is nonsense that LTS is able to clearly distinguish cloud regimes (e.g. Nam and Quaas doi 10.1002/grl.50945). Klein and Hartmann only show that the seasonal cycles of cloud fraction and LTS correlate.

P6 l30: The term "unstable" is a misnomer. "unstable" would mean, a negative LTSS.

P7 l10: Why this choice and not deciles?

P9 l5: When using AMSR-E LWP, are the pixels selected overcast at AMSR-E footprint? Or is the AMSR-E interpolated to the CALTRACK grid cells?

P9 l17: Is this maybe due to the fact that AMSR-E LWP in fact is cloud fraction times in-cloud LWP, in combination with the fact that CDNC is positively correlated to cloud fraction (Fig. 2)?

Fig. 1: Since there are only ten bins in LWP, I suggest to label each bin center on the x-axis. Possibly the axis could be chosen irregular then. It would be good to indicate the total amount of data points in the caption. In (a) a zeroline would be helpful.

Fig. 4: The authors need to choose a different y-axis that spans only the range of data.

As it is now, no details can be distinguished. Again, a zeroline is necessary.

Fig. 5: zeroline would be good

Fig. 7: a, b, e, f: more x-axis tick marks necessary; e-f: more y-axis tick marks necessary

Fig. 10 b: zeroline necessary

Fig. 12: the color code is poorly selected. The colors should be centered around zero (light pink shouldn't indicate positive). Are the LTSS bins chosen so that each contains on average the same amount of pixels (that is the way it should be, else a PDF of LTSS would need to be shown).

Fig. 14: is it not possible to differentiate likelihoods, e.g. by putting equal weight on each curve entering the shaded area and then varying the color intensity/darkness?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-887, 2017.