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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol concentrations determine the height of warm rain and ice initiation in convective clouds over the Amazon basin" *by* Ramon Campos Braga et al.

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Interactive response to reviewer's comment on "Aerosol concentrations determine the height of warm rain and ice initiation in convective clouds over the Amazon basin" by Ramon Campos Braga et al.

The structure is response after the quoted text of the reviewer.

The authors thank Darrel Baumgardner for the general comments and advices. Furthermore, the advices of the referee are highly appreciated as well as the very valuable and constructive suggestions to increase the quality of the manuscript. We tried to address the points requested by the reviewer to the paper be considered for publication.

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Discussion paper

Reviewer’s text: The authors have presented the case that cloud active aerosols at cloud base are responsible for determining the cloud depth at which precipitation forms. As pointed out in the introduction, this is not a new discovery and has been investigated in many regions by many researchers other than the ones that are heavily referenced in this paper. Although the failure to be more inclusive in mentioning these other studies is not a fatal flaw in this paper, it does weaken its overall premise and conclusions. There are more serious issues that I would like addressed before this study is published.

A: Four references to rain initiation were added.

Instrument issues

I could not find in either this paper or the Braga et al (2016) sufficient discussion on the processing of spectrometer measurements. In particular:

1) Coincidence corrections. Lance (2012) clearly shows that the CDP (unmodified with secondary mask) and CAS seriously undercount at > 500 cm-3. Lance (2012) says nothing about interarrival times and coincidence. Interarrival is used for shattering, so I don’t understand the justification for not correcting the concentrations. Many of the concentrations reported > 1000 cm-3 will likely be at least 50% larger which will seriously impact the derived LWC and subsequent Na.

A: Both instruments have different set ups compared to the configuration described in Lance et al. Specifically for the CAS, the pin hole in front of the sizing detector was changed to a smaller diameter. This significantly reduced the number of coincident particles. The analysis addressing coincidence was done following the paper by Lance (2012). We compared the LWC from the hotwire and the PSDs assuming spherical particles. If coincidence occurs in the sampling volume, larger but fewer particles would have been detected, thus the LWC from the particle probes would be higher for higher number concentrations. The CAS showed rather lower LWC than the hotwire at higher number concentrations (> 1000 cm-3), which stands in contrast to the observations by Lance et al. Furthermore, we looked at the Poissonian probability density function of the inter arrival times at high number concentrations (2000 cm-3). If coincidence occurs then a significant fraction of the inter arrival times should be at the lower end of the distribution (short inter arrival times) or even beyond the time resolution of the instrument. We could not find a significant fraction (< 5 %) at the lower end of the inter arrival time distribution. The CDP additionally measures the transit time. The transit time did not increase (unlike like the CDP in Lance et al. did) with the number of particles detected up to number concentrations of 1500 cm-3. Further, the good agreement of CAS and CDP regarding the number concentrations shows, if coincidence was an issue, it would be of similar magnitude for both instruments, which is very unlikely. The three independent analysis methods in addition to the good comparison of the probes proves that there are no indications of coincidence in our measurements.

2) In the images from the CIP, there are many out of focus droplets (donuts). The Korolev (2007) correction has to be done, otherwise the derived water content will be an overestimate and the height of precipitation might be incorrect. A: For the data processing of the CCP measurements, ice was assumed as the predominant particle phase in the mixed-state cloud conditions that were mainly given throughout the ACRIDICON CHUVA campaign. The ice assumption causes all images of droplets and ice particles to be treated and considered as particles (apart from shattering-induced particles) but the Poisson spot correction is then excluded. The Korolev correction is defined for liquid drops only and the SODA image processing disables this correction process once the ice-phase is selected. The assumption of ice density instead of water density implies a slight overestimation ($\sim$10 %) of the calculated rain water content for particles greater than 75 μm. This will be highlighted in the manuscript.

3) Was the PCASP operated with a heated inlet? If so, corrections are needed to size distribution.

A: No. PCASP was not operated with a heated inlet. We add a comment about this in the instrument description.

4) A fair amount of the paper is devoted to illustrating that the CAS and CDP compare within expected uncertainties. Given that this has already been done in the Braga et al. (2016), this is redundant and doesn't add much new information to the results.

A: We did not compare effective radius (re) calculated with CAS-DPOL and CCP-CDP as a function of mean volume radius and precipitation probability for at Braga et al. (2016). The results show that even with agreement the threshold of re for rain initiation is about 1 μm smaller for CAS-DPOL in comparison with CCP-CDP.

Science Issues

5) Modify title please. The current title is misleading and not correct. It currently implies that all aerosols determine the depth of precipitation initiation. The results do not support this strong of a statement. Some types of aerosols play a role in determining the height of warm rain initiation, i.e. CCN/IN and their concentration have an impact as is clearly shown in this paper. A more accurate title might be "Further evidence for the impact of cloud base CCN/IN on the height of precipitation initiation"

A: We have changed the title to: Further evidence for CCN aerosol concentrations determining the height of warm rain and ice initiation in convective clouds over the Amazon basin

6) The determination of Na needs much more explanation. The Na vs Precipitation depth is key to the conclusions and needs amplification. Why should the slope of the LWC vs Mv relationship with height provide a good estimate of Na? I understand that LWC = Nd*Mv but this is not discussed, nor is how Mv is derived. In addition, all the plots that determine Na should be shown. If they are anything like the one shown in Braga et al 2016, Fig. 14a, there can be a very large spread in values of LWC at each Mv and subsequent uncertainty in the Rea. Fig. 15 in Braga et al (2016) clearly show that there is a lot of dispersion when comparing Na and Nd. The best fit line in their Fig. 14a does not appear to fit the points and certainly can't justify reporting Na to such precision.

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Discussion paper

A: The reviewer wrote: "I understand that LWC = Nd*Mv". This is not quite so. The right expression is LWCa = Nda*Mva, where all are the adiabatic values. The whole idea of the methodology is that the actual re is similar to rea - the adiabatic effective radius, due to the nearly inhomogeneous nature of the mixing. The mixing does decorrelate LWC strongly from LWCa, while keeping re well correlated with rea. The methodology which use LWC vs. Mv relationship with height to estimate Na is well tested and validated at Freud and Rosenfeld (2011). The Na estimate is also explained and tested at Braga et al. (2016). Indeed there are uncertainties related to Na estimated mostly related when secondary nucleation takes place. The model does not predict that Nd increases with height, but decrease due to coalescence and inhomogeneous cloud mixing. The results suggest the occurrence of secondary activation with different strengths during flights AC08, AC12, AC13 and AC20 (see figures attached). Large updrafts were measured above cloud base during these flights which increase supersaturation inducing secondary activation. The increase of Nd with height was observed mostly when large aerosol amount was measured with PCASP and UHSAS above cloud base height. However, the estimation of Na have shown to be useful to discriminate clean from polluted environments and predict the height for rain initiation.

7) Nothing is said about the uncertainty in the determination of level of precipitation wrt to vertical motions and where the precipitation actually initiated, i.e. it could have actually been below the level of measurement before being lofted upwards. This uncertainty can be estimated using the measured vertical motions.

A: Doing that would require information that we don't have about the rate of rain formation with height, and will constitute a circular argumentation. The scatter in Figure 17 is the best that we can do for illustrating the uncertainty.

8) Nothing is said about the time it takes to make the measurements at the various cloud levels and how these levels were selected. This will give some idea of the time during which the cloud is growing an how long it took to initiate precipitation.

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A: Since the measurements were not following individual growing cloud towers, these times would not advance such knowledge.

9) Secondary nucleation is a very poor term because in a classical parcel model in an updraft, new particle nucleation occurs above cloud base until there are no more cloud active CCN at the level of SS. The implication here is that new CCN are being entrained and that is why the Nd increases with altititude, but this is likely not the case. When running a parcel model with a prescribed updraft and CCN spectra, the supersaturation increases in altitude as the parcel rises adiabatically and cools. The CCN will activate depending on their SS spectra and the available water. This needs revising.

A: The secondary CCN activation was observed mainly in cloud segments with updrafts that were much stronger than at cloud base. This supports the narrow definition of secondary activation as defined by the reviewer. However, we do not exclude the possibility of additional CCN being entrained and activated above cloud base.

10) The relationship Dr = 5*Na needs revising to take into account the data processing and uncertainties that I raise above, and needs an error bar.

A: The uncertainty of Na calculation with CDP (14 %) is now included in the linear relationship. The linear relationship including Na uncertainty is $D_r=(5\pm0.7)\cdot Na$.

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