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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.

Anonymous Referee #2

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A strength of this paper is the presentation of cloud in-situ observations that addresses the question of how convective clouds are influenced by changing the concentration of aerosols. The authors have determined that the height of rain initiation given by D_i is approximately 5 * the number concentration of cloud droplets at cloud base, N_d. This type of study is needed for developing parametrisations. It is difficult to obtain such a wide range of values of N_d using a single location for the study.

There are a number of problems with the paper however.

1. There are no details about the environment of the clouds, the effect of different cloud bases, or the dynamics and history of the clouds. For example, there should be information for each cloud pass about the distance from cloud top, the horizontal

distribution along the track of the vertical wind and the location where different measurements were made. Also, it is important to know the history of the cloud before that pass. Does the cloud and the aircraft indeed follow the pattern illustrated in Fig 2? None of the diagrams in Supplementary Fig 1 seem similar to Fig 1b. An example is Figs. 7b-c and 8. How far was the pass below the cloud top? Supercooled raindrops would only be observed in the updraft region.

2. Are there multiple thermals? These can be important for the development of raindrops?

3. Turbulence enhancement of collision and coalescence and the enhancement of droplet growth due to entrainment and mixing are not considered in the analysis. Both of these processes would change the simple relationship between D_i and N_d. Giant and ultra-giant aerosols are mentioned, but likewise the analysis does not consider them carefully.

4. The initiation of ice particles in convective clouds is complicated. The analysis presented does not discuss the critical aspects of the problem.

5. The paper is poorly constructed. The font is far too small and diagrams used in discussions are in different documents. Also there are too many similar figures that show very little and are not properly discussed, while more detailed analysis and accompanying figures are missing.

Specific comments.

1. Abstract. "Rain initiation" is a loose term. The authors should be more precise.

"Initiated as ice hydrometeors". The word "initiated" is confusing.

Does "polluted conditions" include biomass burning?

Say why smaller cloud droplets froze at lower temperatures compared to the larger [cloud?] droplets in the [un - or less?] polluted cases. And give the sizes.

"Entrainment and mixing almost completely inhomogeneous". It is the mixing that is either homogeneous or inhomogeneous. A value of r_e close to the r_ea value is not sufficient information to conclude that the mixing process is inhomogeneous. There could be only dilution and no evaporation.

"Secondary nucleation". It's not true that this process will necessarily inhibit the formation of rain, and may indeed enhance it by providing more small droplets for collisions. Secondary nucleation does not mean that the larger cloud droplets are absent necessarily. Addition of more smaller droplets shifts the value of r_e to smaller sizes.

2. p2, lines 87-90. It is incorrect to assume that the mixing processes is completely inhomogeneous. And if it was, the vertical profile of the cloud drop effective radius would most likely not follow an idealized adiabatic parcel; there would be broadening.

3. Line 95. I am not in favour of the wording "raindrops start to form" or "rain initiation" (discussed throughout the paper) since it is a stochastic process.

4. p3, lines 112-113. See #2 above, and furthermore, the vertical values of r_e would not necessarily be constrained by N_d at cloud base.

5. p3, line 138. Downdrafts can be significant. This is a good reason why the vertical velocity time series and the distance below cloud top should be shown.

6. p6, line 238. What was the wind direction and where were the clouds relative to the opening of the river in Fig1a?

7. p8, line 329. Is it possible to show the CDP size distribution just below cloud base?

8. p8, lines 339-340. The adiabatic parcel model would presumably use the aerosol size distribution, including the giant ccn, to initiate the cloud drops.

9. p8, line 344. Electrification is not part of this paper. This statement is conjecture.

10. p8, lines 345-346. It is not evident that downdrafts commence after rain starts. More detailed analysis is needed. 11. p8, lines 346-349. The sentence does not make

sense.

12. p8, lines 349-350. More evidence is required. The stronger updraft in Fig 3a of the supplement could be due to environmental conditions.

13. p9, line 358. There should be more discussion of Figs 13 and 14. Also, again, the images and size distributions should be presented in the context of the updraft structure. For example, the larger particle in the top panel of Fig 14a looks like a graupel particle. Has particle recognition been performed?

14. p9, line 360. As above, it is not right to have some of the figures used in the arguments in the supplementary material and others included in the paper. The diagrams are used in the same way as figures in the manuscript. The authors make a strong statement in the paper based on two cases with one of them shown in the supplementary material.

15. p9, line 360 Change the last word in the line (this) to "the" since Fig 5S shows plots constructed for two levels.

16. What is the evidence that the images in Fig 5aS are spherical? Some of the larger particles in Fig 5bS do look spherical, but it is not possible to tell for the smaller particles.

17. And now back to Fig 14b... Representative images from all parts of the cloud pass should be shown in all cases. What is the difference between almost spherical particles in Fig 14b and the same in Fig5S?

18. p9, lines 364 to 371. There is no discussion about the effect of the difference in cloud-base temperature. The main problem with the discussion, however, is there is no mention of the effects of inhomogeneous mixing, or even the decrease in N observed in the two flights: e.g. N_max at 10 deg is the same (500/cc) in AC18 (Fig 4aS) as in AC09 (Fig 13a).

19. p9, lines 371-374. It is not clear what is meant by the association with vertical

velocities? The rate of condensational growth does not depend on vertical velocity.

20. p9, lines 382 - 384. It is not a relative increase. It is perhaps more surprising that there is such a decrease in N_max between the lowest and next level in AC07 (Fig 5). More analysis should be shown to support suggestions made about secondary activation.

21. p9, lines 387 - 388. Should it not be the reduced rate of production of raindrops due to lower collision and coalescence efficiencies? There is no process of inhibition or suppression in the cloud.

22. p9, lines 388 - 389. 300 m is not a great distance when making aircraft passes. Is the result significant? What is the explanation?

23. p10, lines 393 - 394. As with so many other statements in the paper, more analysis should be presented to support the statement. What is the variation of CCN and updraft speeds at cloud base, for example?

24. I believe the discussion and conclusions should be edited based on the referee comments.

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