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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical distribution of the phase state of particles in tropical deep-convective clouds as derived from cloud-side reflected solar radiation measurements" *by* Evelyn Jäkel et al.

Anonymous Referee #1

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General comments: The analysis and modeling of deep convective clouds (DCC) and their vertical distribution of certain parameters is an ongoing research topic. The manuscript describes many methods and aspects of the possible parameters derived by optical measurements in a special configuration measured sideways from aircraft. The phase state of the cloud particles are derived by a method already published by the author. Nothing new about that. The only new part is the geometric retrieval.While this configuration is good for this kind of case studies it is not a standard configuration compared to satellite instruments or other airborne applications. Hence this case study can give precious information if thoroughly compared and validated to standard

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retrievals and model comparisons.

Questions: - The combination of geometric retrievals and hyperspectral measurements is new and can give additional cloud structure information. In addition its an important information to analyze angle dependent reflectance properties of the DCC relative to the Sun, but the derived parameter of cloud distance from the geometric retrieval is only one single parameter. The observed area by the imaging instrument depends on the field of view, cloud structure and distance and could lead to a 3D cloud structure, but the simple assumption of a homogenous vertical cloud area within the field of view of a single imaging pixel might lead to errors in the analysis of cloud scattering effects.

- A cloud masking procedure is introduced to distinguish between directly reflected areas of the cloud and diffuse shadow regions. The analysis of the manuscript is restricted to directly reflected areas only, which in fact is a sum of direct and diffuse light.

- Why does the described method of the distribution of the cloud phase does work only for direct reflected light of the Sun?

- What is the influence of the diffuse light?

- It would be nice to see some more direct and detailed comparisons to the methods of Marshak (2006, reference missing) or Zinner (2008) , MODIS, possibly Cloudsat and insitu. The description of Figure 9 could be in much more detail and as the major part from my point of view this is worth more than just one page.

Detailed comments: Page 2 line 5: A mixed phase state of water does not exist. I would rather describe it as an area of phase transition levels from existing state phases eg. from liquid to ice which can vary in temperature gradient, altitude and vertical depth (line 17 is here more precise than 5)

Page 2 line 13: . . . (more aerosol particles . . .)

Page 3 line 17: Why did Martins and Marshak use a different Wavelength? With Spec-

MACS it could be used as well and compared.

Page 4 line 14: Temperature profiles are mentioned and it would be very helpful to have some graphs. Sidewards looking IR-camera would be interesting.

Page 5 line 2: Again a comparison to the method of Marshak, ... is possible with SpecMACS.

Page 5 line 29: mixed phase layer ... → phase transition layer

Page 5 line 29: A retrieval of cloud particle size of the measurements would demonstrate this sentence Page 6 line 11: Please explain this statement, if its true. Please compare state of polarization with Mi-Theory.

Page 6 line 23 and 30: detection limit unclear. >1 cm-3 or 0.3 g/m-3

Page 7 line 3: Please explain the adjustment of the temperature, humidity, ... profiles

Page 7 line 11: adjustment of the aerosol profile?

Page 7 line 19: As mentioned before. Why is the diffuse light restricted to shadow regions or does it have the same amount in the other regions as well?

Page 7 line 21: Here we have some weak indications why the diffuse light in shadow regions are not used in this study. While a thoroughly radiative transfer simulation can include the influence of ground reflectance, surface albedo in this manuscript cant be taken into account because of this influence. Why is that and a view sentences later the influence of the surface cant be seen in the airborne data? The reasoning in this part of the manuscript is somehow very weak.

Page 8 line 29: Where does this formula and the constants come from? I would propose to use a spectrum to rgb conversion via a CIE 1931 color space. SpecMACS has a broad spectral range and a large number of spectral channels why not using them? This would reduce noise as well.

Page 8 line 1, ...: How is the histogram of the RGB values converted or evaluated to the frequency distribution? Please explain in more detail. Where does the relative and absolute frequency come from in fig. 3? Why are the simulated once in Fig 3b absolute and the measured once in Fig 3c relative? The calculation of a single RGB value with the formula is used to find the threshold of “directly !” illuminated pixels. What are the model simulations for if you dont use them?

Page 8 line 3: What is the max height of the model domain?

Page 8 line 16: What is a relative azimuth angle of exactly 68 degree with a changing attitude and Sun elevation during airborne missions?

Page 8 line 22,23: The simulation shows an increase in cloud particle size in Fig 4 for that region. What is wrong?

Page 8 line 28: How does this simple formula compare to the methods from Marshak, Martins and Zinner?

Page 9 line 5: Is the combined Ip profile a simulated or measured profile. I dont understand how the combined profile is calculated and where it comes from.

Page 9 line 7: three phases ?

Page 9 line 10: What is a pronounced absorption, of what?

Page 9 line 12: Each cloud height → The cloud vertical structure is ...

Page 9 line 14: To derive the particle size is first mentioned here. Is that the goal or what is the reason? A look up table would do as well, please look at AMT Zinner 2016.

Page 9 line 15: What is a more realistic cloud? Are the other clouds not realistic?

Page 9 line 17: What is the first case?

Page 9 line 18: The transition layer is characterized by a strong increase in particle size and change in the value of phase index. See Fig 4b (simulations) and Fig. 8

(measurements).

Page 9 line 24: I assume that we have a polluted and a clear case, but its not clear in this part of the manuscript. Here we have only two cloud cases, one with fixed microphysics and one with changing cloud properties. Please clearify.

Page 10: Geometry is Ok, but could be shorter. Except a real 3D cloud structure would be the final product.

Page 12 line 12: A profile and comparison of remote sensing and insitu droplet size would be interesting. A sharp transient of the droplet size shows the transition layer.

Page 12 line 30: mixed phase levels → phase transition levels or better layer

Page 13 line 20: Why are liquid water data from up to 8.7 km not shown

Page 14 line 2: A temp profile is missing.

Page 14 line 25: three phases ?

Page 14 line 29: Is there only one polluted case during the whole campaign?

Page 14 line 30 bottom: Low statistics? Are those 2 flights analysed in this study the only possible ones of the whole campaign?

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