

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 #2

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REVIEW of “Vertical Distribution of the phase state of particles in tropical deep-convective clouds as derived from cloud-side reflected solar radiation measurements”

General comments: In this paper, the profile of thermodynamic phase of tropical deep convective clouds is derived from passive shortwave reflectometry with a lateral viewing geometry. Whereas the phase discrimination is directly determined from spectral imagery by way of a previously established near-infrared bi-spectral retrieval method, the altitude registration is established through an additional stereo inversion based on a standard RGB camera. The altitude-registered profiles of individual clouds are put

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into the context of in-situ measurements as well as satellite observations.

The main intent and take-home message remain a bit unclear. The introduction starts off by summarizing aerosol effects specifically on deep convection, emphasizing the location and width of the mixed-phase zone as an indicator for the potential influence of aerosols on deep-convective cloud development.

One of the lingering questions with respect to the observational dimension of this problem is whether time and space can indeed be regarded as interchangeable to study the problem with the so-called ensemble approach where single-image satellite snapshots of multiple clouds at different development stages are statistically linked to their temporal development. Detailed airborne observations could provide further evidence that this approach does indeed work, and thus justify the use of satellite observations in future studies. But it is unclear whether this study intends such work. Instead, it seems to seek consistency between retrievals from airborne imagery, in-situ data, and concomitant satellite images, without really drawing conclusions with regard to aerosol influences on vertical development due to the limited number of samples.

It would help the paper if the objectives were laid out a bit more clearly, perhaps through a number of questions that are answered in the conclusions. To a certain degree, the reader is left wondering about the distinct advantages of aircraft over satellite observations (aside from resolution). Why is a lateral viewing geometry superior to satellite imaging (primarily from above), and what is the advantage of resolving even the smallest convection events? What parameters can we get from aircraft observations that satellites do *not* provide – and that are yet critical to advance theory? Some of these questions may be answered implicitly, but if relevant, they should be addressed in the paper, preferably with support from the measurements.

I believe that minor modifications can go a long way towards addressing some of my general comments above. Below, I offer specific comments in sequential order, which are intended to improve the next version. I would be willing and happy to look at the

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revised manuscript.

Sequential comments:

p3,L5: I don't see the relevance of the cited paper (Cahalan, 1994) and the associated science (plane-parallel retrieval assumptions) in this context.

p3,L17+L30: These are places where the manuscript could outline how the specific work fits into the larger context that was set up previously. Currently, this page in particular looks like a list of work done by the authors and predecessors (e.g., Martins Rosenfeld), with only tangential connection to the motivation from the previous page(s). This is not a deal breaker for the manuscript, but it would be better to see how the listed work serves a number of outstanding questions related to the introductory comments earlier on.

p4,L4: In the description of the manuscript's structure, it seems that the goal is to compare remote sensing derived cloud profiles to MODIS and in-situ data, which would make the manuscript more appropriate for AMT than for ACP. If, however, there are some higher-level goals that address some of the questions brought up above, this should be made clearer.

p4,section 2.1: A table with flights and clouds cases would help.

p5,L12-18: The manuscript should elaborate on the stereo algorithm a little more. Also, L21, should this be "assign" instead of "allocate"?

p6,L23: The description of the aspherical fraction is a bit unclear; what is measured, and what is derived?

p6,L30: Why is the threshold this high? 0.3 g/m³ seems excessive, considering that a typical BL cloud top-level LWC is 1 g/m³.

p7,L1: Why is it necessary to perform 3D calculations? Only because of the geometry, or because deviations from standard 1D models are expected? If so, what are they

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(aside from shadows)?

p7,L6: Rayleigh scattering $\hat{\alpha}$ > molecular scattering?

p9: Here it becomes quite difficult to understand what the authors are after - the position/width of the mixed layer? Is that the purpose of the simulations? Are they done to only replicate the earlier study, or are they something new?

p10: The geometry retrieval description is rather cryptic. Would it help to cite work related to MISR, or is the method unrelated? What happens if the cloud moves during two consecutive images (used for the stereo method)? Also, elaborate on p10,L18-19, and state with respect to which coordinate system the "elevation angle" is provided.

p10: Others circumvented the whole (rather difficult) stereo algorithm by including an IR channel. Why was that not done here? Were these measurements simply not available? And why did the authors prefer the more complicated method to the simple IR imager?

p11, Applications section. What is it that the paper seeks to find out? Refer to the main question here, at least at the beginning? This whole section reads a little bit like a listing of results with no specific purpose. Quite surely there is one, and that should be clarified more.

p12: It is a bit unclear what the in-situ measurements really have to offer here if "the direct comparison of in situ and remote measurements is difficult". Really, the in-situ measurements should serve as validation for the remote sensing, but what do we learn if that doesn't work? Is it still worth using the in-situ data? If the comparison does not work out, what does that mean for the initial hypotheses (if there is one: perhaps a question about the interchangeability of pixel/time mentioned earlier?)

p13: The in-situ data puts ice higher than remote sensing. Which is right? What do we learn here about the representativeness of satellite data and *its* consistency with the aircraft measurements?

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p13,L34 (top): So what phase *does* MODIS get for 6km? The shown results are certainly not liquid drops, given the large size.

p13,L12-14: Here we get some potentially important conclusions, which should be expanded and elaborated on. What is the significance of this finding? What can the satellite-based ensemble method do, and what not? Do in-situ and remote sensing observations from the aircraft tell two different stories?

p14,L15: Does this explain the discrepancy between in-situ obs and remote sensing?

p14,L16/17: distinctive *change* in gradient, or simply “significant gradient” (change of gradient is a gradient. . .)

p14,L21: Here the question is again why this was chosen over IR imaging.

p14,L31: Earlier, the authors said that the aircraft measurements are not statistically significant to prove/disprove theory. This statement here is not meant to be the main finding of the manuscript, is it? Wouldn't satellite data be more suitable to put this on a statistical basis? If so, what would then be the purpose of the aircraft measurements? This may be obvious, but it would help the reader to understand this point.

p15,L38(top): The results from remote sensing and in-situ are not really consistent (as discussed earlier by the authors, and noted by the reviewer).

p15,L13: “invariance of space and time” does not seem an appropriate way to describe the assumptions of the ensemble method. It's really spatial statistics vs. temporal evolution, isn't it? Secondly, the manuscript now divulges that it did seek to study the aerosol effect on deep convection - or is this a statement that this was done (by others) using MODIS? Clarification is needed here what was done in the manuscript vs. prior work. Is it fair to say that the manuscript got closer to observational evidence for the validity of the interchangeability of spatial statistics and temporal evolution?

p15,L13: This seems like a fair statement, but how do we interpret it? For which purposes is the satellite-based method good enough, and for which problems do we

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need to use airborne or tower-based observations (as later suggested by the authors)?

Minor comments:

p2,L17: 1) Why is there a new paragraph? 2) Suggest re-wording “In particular. . .” as “The phase transition . . . is especially relevant for. . .” without a preceding indent/new paragraph.

p2,L25: remains > remain

p4,L30: “In further development of the scanning. . .” Something wrong with the language here and the conclusion of this sentence. p4,L12: add comma after “September”

p4,L21: The “degree” characters should be superscripts.

p5,L7: “by measuring monochromatic radiation from a monochromator” - revise language?

p8,L17: “inlay” > “inset”?

p9,L31 (top of page) “ grid cell in” > “grid cell at”

p9,L2: ranging between > from . . . to?

p9,L3: What is the “first cloud case”? At this point, the table suggested above would really be helpful.

p9,L6: “originated” > “originating”?

p9,L8: The phase index is significantly shifted to positive values > either it assumes positive values or not - what is the meaning of “significantly shifted to positive values” How about “shifted to positive values”?

p9,L9: Why “obviously”? Perhaps “apparently”? Meaning unclear.

p9,L14: “related” > “relative”?

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p9,l15: move "also" to after "is"

p9,l20-21: Too hard to understand. Try to improve language.

p9,l22: "as can be concluded" > how can this be concluded?

p10,l1: "showing" > "with"

p10,l2: "need to be taken" > move to right after "images" on l1.

p10,l2: explain "epipolar plane"

p10,l3: What's the "world" coordinate system?

p10,l6: Usage of the word "exemplarily" seems out of place throughout most of the manuscript. How about "for example"?

p10,l6-7: Unclear what this means - wrong axis perhaps?

p11,l25: 6b: are the distances in km?

p11,l27: "quite *a* homogeneous" (add "a")

p11,l14: "scientific" > "science"

p11,l16: "which" > "because it" ?

p12,l30 (top): "have been" > "were"

p12,l32 (top): "phase states. Mainly" > "phase states, mainly"

p15,L15: ATTO - introduce acronym somewhere.

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