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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical Dependence of Horizontal Variation of Cloud Microphysics: Observations from the ACE-ENA field campaign and implications for warm rain simulation in climate models" *by* Zhibo Zhang et al.

Anonymous Referee #3

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Title:

Vertical Dependence of Horizontal Variation of Cloud Microphysics: Observations from the ACE-ENA field campaign and implications for warm rain simulation in climate models

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Overview:

In addition to being interesting, I think this study is well thought out and the manuscript is well written. I found reading it to be a pleasure. In particular, I would like to compliment the authors on the quality of the introduction. That said, I do have one major concern (general comment #1, below) and several suggestions that might improve the manuscript.

Recommendation: Publish after major revisions

I have labeled this as a major revision, out of concern that responding to general comment #1 may require regenerating most of the figures in the manuscript and I want to give the authors ample time to do this, if necessary. This should not be taken to mean this is a poor work. I think it is excellent.

General Comments:

1) Bimodality in the relationship between qc and N

My only major concern with the analysis is that the bimodality in the relationship between qc and N has a significant impact on the results. The manuscript documents that this bimodality is associated with systematically different values for N measured when the aircraft was flying either along and across the wind. It seems to me that it is extremely unlikely that this would happen by random chance (that the aircraft just happened to observe a cloud field with two distinction regions with differing N that happen to align with the flight tracks) given that the same offset is found between legs occurring at different times and different altitudes. I can't think of any physical reason why this should occur. As such it seems to me that it is very likely that this is a measurement artifact. Accordingly:

(A) Discussion: Is it real?

Am I missing something simple? In general, I think the possible causes of this systematic shift need to be discussed.

(B) Subdivide the horizontal leg data between along and cross wind directions.

All of the analysis is based on analyzing full length of the horizontal legs, which if I understand correctly mean the outer scale is about 60 km. That is, the enhancement factors you present represent the enhancement in going from the $\sim$10 m scale of the FCDP measurement to a nominal grid scale of $\sim$60 km.

This is a choice you made. There is (as far as I can see) nothing that prevents you from dividing your hlegs into separate along and cross wind legs (I presume each will be about 30 km) and instead examine the variability on this smaller scale $\sim$ 30 km scale.

Obviously doing this will increase the sampling uncertainty associated with each leg, but you will have twice the number of points in most of your figures and more critically it will significantly reduce the impact of any measurement bias in N associated with the along or cross wind measurements.

2) Flying in and out of cloud?

How are data for those legs where the aircraft goes in and out of cloud being handled? I assume that you didn't include a lot of zeros in either qc and/or N when calculating the means and variances, or for that matter, the directly calculating E values (e.g., equation 9)? Please describe what was done and comment on the impact this might have on the results.

3) Inner Scale Dependence

One of the most interesting points in the paper is that the PDF of N is not lognormally distributed near the boundaries. It seems to me that this is likely due to entrainment preferentially evaporating the smallest cloud droplets first. The time series is very spikey in nature (spikes having low N values). This suggests to me that this effect

might be greatly reduced if you started from a larger inner scale (100 or 200 m) rather than the $\sim$ 10 m.

I think it would be a nice addition to your analysis to first average the FCDP data to a scale of 100 to 200 m (use 1 Hz data) and examine how this impacts the skewness of the distributions and the ability of the bivariate log normal to model the variability and enhancement factors.

4) Dependency rather than trend

I think the discussion is overly focused on whether there is a (linear) trend in the data. Let's look at Figure 9. To me, "trend" just doesn't seem like the right word to describe the situation in Figure 9a. I suggest a more apt description might be, "Figure 9a shows significant scatter in E with altitude, with smaller values and relatively little scatter near cloud top, and decidedly larger values near cloud base."

I understand entirely that the change with height projects on to a line and the correlation is significant. There is a vertical dependence. But it is not obvious to me that there is a linear dependence or trend.

Importantly I see Figure 9c has this same pattern, with larger En values (on average) at the bottom than the top, as does 9b for Eq with the exception of the two outliers near cloud top.

Somewhat similarly in 9d, the correlations are clearly stronger and there is less variability near cloud top. But it is not clear to me there is a linear dependence. I note the bimodal points are causing much of the variability mid-cloud.

I recognize this is partly philosophical, but I think it would be better to recast some of the discussion in terms of a there being a vertical dependence rather than a "trend", though perhaps with more points (general comment 1b) a trend will become clearer.

Specific Comments:

Line 213. What is the WCM-2000? Please provide a reference and perhaps add to table #1. Perhaps add a figure demonstrating agreement in qc, or at least quantify what "excellent" agreement means (e.g. Bias less than X and RMS difference less than Y% at 10 Hz?). I note here that this study is about variability and so ideally it would be good to establish variability in qc is the same from both sensor (not simply that the bias between two measurements is small).

Table #1. Perhaps change label "Accuracy" to "Particle Size Intervals" or something similar?

Line 214. A value of 20 microns for ðİŚ§* is about as small a threshold as might be chosen, and there are often a significant number of particles in the 20 to 50 micron size range for clouds that are described as "non precipitating". I expect particle concentrations in the 20 to 50 micron size range will co-vary with the concentration of particles less than 20, so I am not surprised that you see little sensitivity. Nonetheless I think it would have been better test of the sensitivity by choosing values for r* of 20 and 50, rather than 20 +/-5. In particular, you show later in the manuscript that N (with the cut off of r* of 20) is not well described by a lognormal near cloud-top or cloud-base. Is this result sensitive to the choice of r*?

Line 238. Do I understand correctly that only flight labeled as drizzling in table #2 were considered? If so, how were the groupings in Table #2 established? Does "non precipitating" in table #2 mean only particles less than 20 microns where present OR Only when drizzle is clearly falling from clouds (from radar and "pilots" on line 269)? Perhaps it doesn't matter for this paper, but I presume models will apply autoconversion rate parameterizations all the time and I think it would be useful to examine precipitation formation for cloud without obvious precipitation / virga falling from the bottom OR otherwise clearly establish the conditions under which your results apply.

Line 269. I don't fully understand this criteria or how it is being applied. Is this requirement applied to each horizontal leg or just for choosing cases (see comment for line

238)? If individual legs, are you focusing on the portion of the leg that occurs near the radar? Is there a reflectivity threshold? In general, you have CDP and 2DS data and so I don't understand the choice to rely on radar or pilots.

Line 274. Does "same region" here mean the hlegs must occur along roughly the same track, that is the same set of latitudes and longitudes? If yes, I don't understand the rationale for this. As long as you are sampling the same cloud field (i.e. it is reasonable to expect a representative measure of the variability) and you are using hlegs with the same total length, why do you care if the hlegs and nominally stacked?

Line 332. Previous studies? Please provide a reference.

Line 443. "slight"? It is not clear to me whether this difference would or would not have a significant impact on a model simulation. Perhaps just indicate the bivariate lognormal results in an EF value that is about 0.5 larger (be quantitative rather than qualitative).

Line 541. It seems natural to expect qc and N will be positively correlated, since stronger updraft mean both activating more droplets and condensing more water.

Line 503-12. The use of a "relative hleg altitude" seem to me to be "just as prone" or even "more prone" to misinterpretation as using cloud boundaries from either radar/lidar or the vlegs. Here you are effectively demanding that the highest and lowest legs in each flight should have the same relationship regardless of how close or far they are from the actual cloud top or cloud base. I guess the question is, does it matter? Based on your 4th criteria (line 281) I would hope not.

Minor Comments

Line 55-6. Replace ". . . at the spatial scales much smaller .. " with ". . . on spatial scales that are much smaller . . ."

Line 56-7. Remove the phrase " . . . making the simulation of these processes in GCMs highly challenging." This is entirely redundant with the "challenging" remark on the previous sentence.

Line 164. Perhaps rephrase as "… better understand the horizontal variations in ðİŚđðİŚŘ and ðİŚĄðİŚŘ, their covariation, and the dependence on the vertical height in cloud …"

Line 184. Replace "seasonable" with "seasonal"

Line 185. Perhaps add comma and remove "and" so the sentence reads " … the MBL (Dong et al., 2014; Rémillard 185 et al., 2012), to improving cloud parameterizations in the GCMs (Zheng et al., 2016), to validating the space-borne remote sensing products of MBL clouds (Zhang et al., 2017)."

Line 189. Change "is" to "was", and perhaps simplify to read "In 2013 a permanent measurement site was established by the ARM program on Graciosa Island, and is typically referred to as the ENA site (Voyles and Mather, 2013)."

Figure 1. I presume the green blobs are islands? Perhaps explain this in the caption along with note about location of the ARM site and likewise describe what the number in white boxes and colored stripes mean in radar panels? Also I suggest higher resolution figure would be helpful here.

Figure 9. The solid symbols seem a bit problematic here. I am pretty sure that in panel c, there is a red dot near cloud top that must be hidden under the other symbols – and this make me wonder if other symbols might also be hidden.

Line 537. I think you mean "Eq is larger than E", and you need to change the second occurrence of Eq in this sentence to be simply E.

Line 561. Perhaps change to read "... implications of subgrid variability as relates to the enhancement of autoconversion rates ..."

Line 566. Perhaps add "near cloud top" to the end of the sentence. I think it is reasonable to make the association with cloud-top entrainment but obviously, the study

doesn't define entrainment zone as best I recall.

Line 569. Change "we" to "our".

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