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Interactive comment

Interactive comment on "Subgrid Variations of the Cloud Water and Droplet Number Concentration Over Tropical Ocean: Satellite Observations and Implications for Warm Rain Simulation in Climate Models" *by* Zhibo Zhang et al.

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General comments:

This paper uses Daily gridded Level-3 histograms of MODIS cloud retrievals to derive the small-scale variability in liquid cloud properties, specifically cloud liquid water path (lwp) and droplet number concentration(cdnc). This is the first study to address the variance of cdnc from satellite data. The variability is then used to diagnose the expected enhancement of the autoconversion process due to sub-grid scale distribution

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Discussion paper

of cloud fields in global models. The regional variation of the enhancement are shown. Surprisingly the enhancement due to variability in the cdnc is shown to be often larger than that due to lwp.

The largest enhancement due to number concentration variability is correlated with number concentration itself. This correlation is largely unexplained and a major result of the paper. There is a limited attempt to attempt to explain the unexpectedly large cdnc enhancement factor based on retrieval uncertainty in broken cloud scenes but the authors should consider physical mechanisms as well. I would suggest that thin detrained veil clouds near precipitating cumulus could be a physical mechanism for seeing this variability in the observations.

The science focus of this paper is novel and timely, the methodology is appropriate, and the presentation is generally good. I've included some additional references to add and specific comments below. In terms of additional analysis I would advocate quantifying the correlation between E_n and other cloud properties on various scales (correlate 1 degree grids (super pixel), correlate spatial patterns) to identify the controlling factors. This will help us better understand what variables might be influencing the high E_n (i.e. cloud fraction, low optical depth, CDNC, LWP, etc. A Table might work well to present these results.

-Matt Lebsock

Specific comments:

Lines 123-128: add Ahlgrimm et al., 2016 (https://doi.org/10.1002/qj.2783). They also use DOE data and create a parameterization of E based on cloud fraction.

Line 135: Add citation to Takahashi et al., 2017 (https://doi.org/10.1002/2016JD026404). They have shown that more advance parameterization, specifically a version of the Multi-scale Modeling Framework model is able to produce reasonable distributions of regional distributions of the cloud water heterogeneity when compared against the satellite observations (their figure 2).

Line 137: Somewhere in here you should point out that the estimate of variance depends on the spatial resolution of the observations. With satellite observations (even MODIS) we are using relatively coarse observations and therefore we cannot resolve variance on the smallest scales. So satellite observations will necessarily underestimate variance because of this effect, however, they should provide an accurate assessment of regional distributions of the microphysical process enhancement factors.

Line 146: I wouldn't say that the 'empty cloud' problem is a well defined term. I can guess what this means but I would state explicitly a diagnosis of the problem. Probably there is too much rain and clouds with very low or zero liquid water path at the end of the time step???

Line 246: I think that E_q should be E_N here and cloud water should be CDNC.

Figure 1B/Line 262/Line263, and elsewhere: What is plotted here is not the rain rate. It is rate of conversion cloud water to precipitation water (or the autoconversion process rate). Rain rate is the integral of over the precipitation drop size distribution multiplied by the density-dependent fall velocity for each drop radius. This should be corrected throughout the manuscript.

Line 318: You should point out that calculating the nu parameter in this way can be very sensitive to outliers as the sample size gets small (i.e. low cloud fraction) and there are other methods to calculate nu from the data (e.g. Oreopoulos and Cahalan, 2005) that will give different answers.

Figure 5: The caption says these are means, as does panel b. But the other panels say median as does the paper text. Which is it? Median I think. . ..

Line 327: Lebsock et al., 2011 (https://doi.org/10.1175/2010JAMC2494.1) also argue this about 3.7 micron re.

Figure 8: you should describe in the caption the difference in the fitting so the reader

doesn't have to go to the text and equations.

Figure 8: I do think it is useful to show that the parameterization of nu based on cloud fraction does not work because of the non-linearity in the process. However, can you explain why even the parameterization of the enhancement factor directly on cloud fraction under-predicts the direct calculation? That relationship show in 7b is fairly normally distributed so I can't understand why the parameterization would not get the median about right.

Eqs. 26/27 and related discussion: I don't like this parameterization of nu based on cloud fraction because it isn't well justified physically. Ideally both the cloud fraction and nu could be calculated from either prognostic or diagnostic distribution of the subgrid co-variability of total water and temperature. CLUBB in fact can do this so there should be no need to for such an ad-hoc representation. It is true that such relationships have been advocated in the past but they strike me as very unphysical. I wouldn't advocate this in the context of CLUBB, which is heavily referenced here.

Line 603: One physical interpretation of the MODIS retrievals of high effective radii in these broken cloud scenes is that they could be 'optically thin veil' clouds as described by O et al. (https://doi.org/10.1029/2018GL077084) to be extensive detrained anvil cloud from shallow cumulus with low liquid water content and very low CDNC -> thus potentially large radius. Indeed they are often seen by cloud radar (Wood et al., 2018). Now if this is the case in reality these clouds might contribute quite a bit to the variance in CDNC but shouldn't lead to any substantial increase in the autoconversion because the low CDNC pixels should also have very low liquid water path -> so the correlation should matter. In fact you show this exact correlation later on.

Line 603: I think it is important to explore and show some correlations between the E_N and various other parameters, such as CDNC, cloud fraction, number of pixels with cloud optical depth < 4. Clearly if some factor is influencing E_N (Like cloud fraction) you would expect to see some correlation between the variables. You could show either the regional correlations, or do this for individual 1 degree grids. It seems quite clear that there is not a good correlation between liquid cloud fraction and E_N which doesn't support the idea that cloud-fraction related retrieval artifacts have much to do with these results. If on the other hand the E_N mostly correlates with large CDNC, which I suspect it does, then there is a mystery yet to be explained.

Line 646: Again, I think that there may be a physical explanation for this correlation. Specifically that there are a lot of these low water, low N veil clouds around shallow convection.

Line 665: I would argue significantly better.

Line 683: Another example of parameterization that includes subgrid information is the EDMF approach (e.g. Sušelj et al., 2013, https://doi.org/10.1175/JAS-D-12-0106.1), variants of which are used in a number of models.

Technical comments:

Line 41 The phrasing 'clear cloud' might be confusing. Consider 'obvious' or 'demonstrable' instead of clear.

Line 94: superfluous 'on'

Line 369: the 2∼4 notation seems odd to me. I would use ∼2-4 COT and ∼10-12 um.

Line 402: 'dominate' -> 'dominant'

Line 458: missing 'of'

Line 480: grammar, missing word after more.

Line 493: 'product' -> 'production'

Line 496: second 6b should be 6a.

Line 497: 'tend' -> 'tends'

Line 508: ‘facts’ -> ‘fact’

Eqs. 26/27: parenthesis don’t match.

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