Response to Reviewers

Response to Reviewer #1 Matthew Lebsock

I would like to thank Dr. Matthew Lebsock for his insightful and suggestive comments that helped us substantially improve the manuscript. Point-to-point replies to the comments are provided below (reviewer's comments in italic blue font).

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 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.

Reply: Thanks for the review and helpful comments. Following your suggestions, we made significant revisions to the paper. Major changes include:

- We added more discussions on the correlation between LWP and CDNC and its implications for enhancement factor.
- We also provide some possible physical explanation on the large E_N. Please see details below.
- Figure 5, 7, 10, 11 are updated.

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.

Reply: Thanks. The paper is added to the citation list.

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).

Reply: Thanks. The paper is added to the citation list.

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.

Reply: Good point. Some discussions on the limitations of satellite observations are added after the Lebsock (2013) study.

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?

Reply: You are right. "empty clouds" have near-zero cloud water which is caused by excessive rain (Song et al. 2018). This sentence is revised.

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

Reply: Thanks for catching this. It is revised.

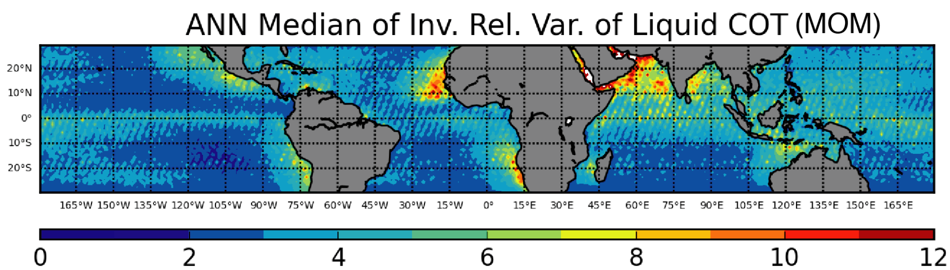
Figure 1B/Line 262/Line263, and elsewhere: What is plotted here is not the rain rate. It is rate of conversion cloud water to precipitation water (or the autoconversion process rate). Rain rate is the integral of over the precipitation drop size distribution multiplied by the density-dependent fall velocity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

Reply: Thanks for pointing this out. We should be more careful. In the revised manuscript, we use "autoconversion rate", instead of "rain rate" throughout the paper.

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.

Reply: Thanks for pointing this out. Indeed, the method we used in this study is the method of moment (MOM). The inverse relative variance can also be estimated using the maximum likelihood estimate (MLE). We pointed this out in the revised manuscript.

The MODIS level 3 product reports the logarithm mean of cloud optical thickness which enables us to use the MLE method to estimate the ν_{MLE} from Eq. 6 of Oreopoulos and Cahalan (2005). The results are shown below compared with the value from the MOM ν_{MOM}. Apparently, ν_{MLE} tends to be larger than ν_{MOM} especially over regions with low water cloud fraction, although the spatial pattern is similar. This is probably because, as you pointed out, the MOM is more prone to the impact of extreme values when cloud fraction is small. Nevertheless, the difference does not change any conclusions.



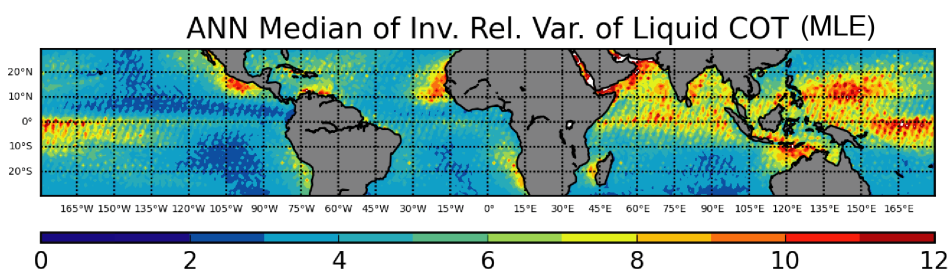


Figure 5: The caption says these are means, as does panel b. But the other panels say median as does the paper text. Which is it? Median I think. . ..

Reply: It's a typo and should be "Median". Corrected.

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

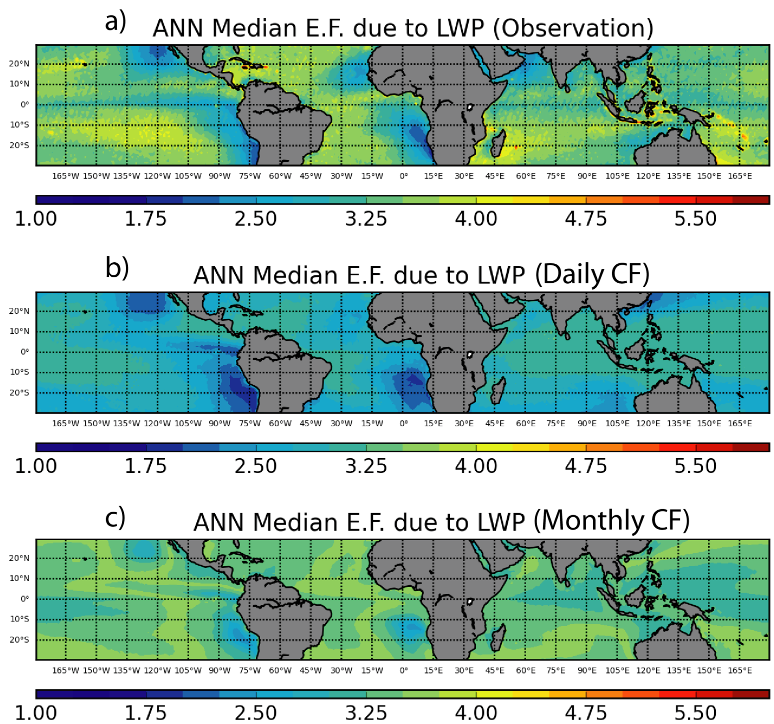
Reply: Thanks. This paper is cited in the revised version. Of course, the choice of coefficient for LWP computation does not matter in this study because it is a common factor in both numerator and denominator in the calculation of v.

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.

Reply: Good suggestion. We added the information.

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.

Reply: This is a very good question. To answer it, first let us explain how E_q in Figure 6 and Fig. 8 are obtained. The parameterization scheme in Eq. (27) and Fig. 7 are developed based on the relation between monthly-mean observation-based E_q and monthly-mean f_{liq} in the tropics (i.e., 10 years x 12months x 360 longitude x 60 latitude x fraction of ocean). The sample size would be too large if daily products were used. After we obtained the parameterization scheme (i.e., Eq. 27), we then used it to compute the daily E_q based on daily CF_{liq}. The daily E_q values are then temporally aggregated, weighted by daily f_{liq}, to first obtain monthly and then annual E_q in Fig. 8b in the same way as we obtain observed E_q in Fig. 6. Going back to your question, we think the underestimation of parameterized E_q (Figure 8b compared to Figure 6a) is due to the fact that the parameterization is developed based on monthly data but applied to daily f_{liq}. To test this, we applied the parameterization scheme to monthly f_{liq} . The results are significantly better. See below.



The lesson learned is that the simple parameterization scheme developed based on monthly f_{liq} cannot capture the day-to-day variation of E_q, which is not surprising. In our view, the parameterization scheme is only better than assuming a constant E_q in the sense that it can capture the cloud regime dependence. However, it would be unrealistic to hope that it can simulate the dramatic instantaneous variation. For that, we would have to rely on advanced scheme like CLUBB or MMF.

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.

Reply: We agree with your point about the parameterization of ν. The highly non-linear relation between ν and the enhancement factor makes the parameterization not so useful. It is shown here simply because some previous studies, e.g. Boulte et al. (2014), Xie and Zhang (2015), had tried to parameterize the ν directly. The unsatisfying results motived us to parameterize the enhancement factor directly.

On the other hand, we think the direct parameterization of enhancement factor is meaningful. It provides with a simple way for those GCMs without advanced sub-grid parameterization scheme to account for the impacts of cloud inhomogeneity on precipitation simulation. We agree that CLUBB presumably would do a better job than simple parameterization. Nevertheless, the results from this study, including the parameterization of enhancement factor, provide observational basis for evaluating the results from CLUBB.

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.

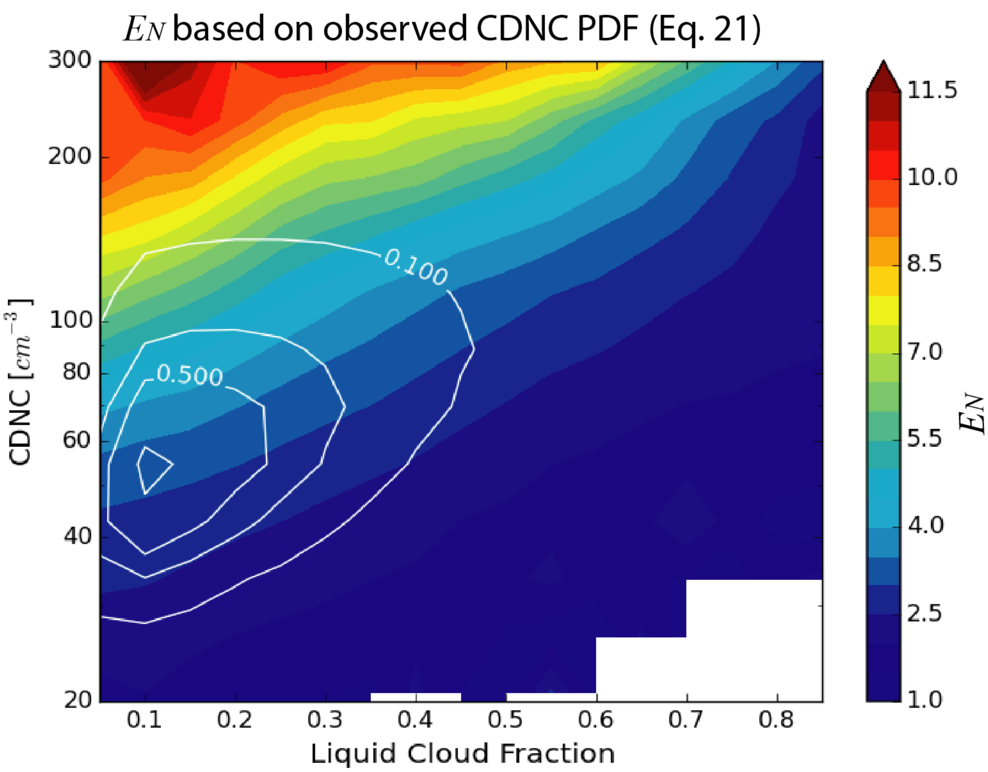
Reply: This is a very insightful comment and thanks for the references (we are aware of Wood et al. 2018 but not O et al.). Following your suggestions, we have de-emphasized the influence of retrieval error and focused more on the potential physical processes that lead to the large subgrid CDNC variance. These papers are now in Section 4 when discuss the new Figure 5 e about the correlation between LWP and CDNC and the its implications.

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.

Reply: Thanks for the great suggestions! We made several significant changes to the paper accordingly. We replaced original Figure 10 (which focuses on the retrieval artifacts) with an analysis of the dependence of E_N on liquid cloud fraction and CDNC. See below. As you suspected, E_N shows a stronger dependence on CDNC than cloud fraction, which seems to suggest that the dependence is largely due to some underlying physical mechanisms rather than retrieval artifacts. The largest E_N is usually found where CDNC is large and cloud fraction is small and it decreases with decreasing CDNC and to a less extent also with increasing cloud fraction. The strong dependence of E_N on CDNC might be explained by the following mechanism in which aerosol plays an important role: when aerosol loading is small, even weak updraft can activate most CCN. As a result, the subgrid turbulence and variance of thermodynamical conditions are not importance leading to small E_N. In contrast, when aerosol loading is large, subgrid variations of updraft and thermodynamical conditions could lead to significant subgrid variations of CDNC, leading to large E_N.

In addition to the analysis E_N, we also added some more in-depth explanation of the importance of LWP and CDNC correlation on enhancement factor simulation at the end of Section 2.2. First, a formula for *combined* enhancement based on the bi-variate lognormal distribution is presented (Eq. 22). Second, we pointed out that the current GCMS, even those with advanced sub-grid parameterization such as CLUBB, only consider the enhancement factor due to LWP E_q, the effect of E_N and the correlation term E_{COV} are ignored. Moreover, an equation is added (Eq. 25) to explain under what circumstances would E_q underestimate or overestimate the combined effect $E_q \cdot E_N \cdot E_{COV}$. In addition, Figure 5 e is added to show the subgrid correlation coefficient of LWP and CDNC and in Figure 11 we discussed the importance of considering E_{COV} in computing the combined enhancement factor.

We feel that these revisions, based on your suggestions, had made the paper more insightful and more revealing.



Dependence of E_N on f_{liq} and N_d. The color map corresponds to the mean value of E_N for a given N_d and f_{liq} bin. The white contour lines correspond to the relative sampling frequency of N_d and f_{liq} bins (i.e., the most frequently observed combination is $N_d \sim 50cm^{-3}$ and f_{liq}~0.1).

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.

Reply: See our reply above.

Line 665: I would argue significantly better.

Reply: agree and revised.

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

Reply: we added the EDMF as another example of "advanced subgrid cloud parameterization scheme". Thanks for pointing it out.

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.

Reply: Thanks a lot for catching these typos and mistakes. They are all corrected.