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Comment on acp-2021-656

Anonymous Referee #2

Referee comment on "Subgrid-scale Horizontal and Vertical Variations of Cloud Water in Stratocumulus Clouds: A case study based on LES and comparisons with in-situ observations" by Justin A. Covert et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-656-RC2, 2021

Review of Covert, Mechem & Zhang, acp-2021-656

SUMMARY

This study analyzes the effects of small-scale variability of cloud liquid water amount on large-scale microphysical process rates using large eddy simulation (LES) of a single, idealized case study over the ARM Eastern North Atlantic site. Simulation results are compared to observational analysis performed over a comparable spatial scale to the LES domain, with generally close agreement found. The study is timely and, while somewhat oversimplified due to the idealized modeling framework and use of the "case study" paradigm, draws some general conclusions about the vertical structure of horizontal atmospheric variability that are of interest to the ACP audience. I have several major concerns that I would like to see the authors address as well as numerous minor and typographical notes. As such, I recommend the manuscript be returned to the authors for major revisions.

MAJOR COMMENTS

The discussion of results and implications is great insofar as it describes the results of a single simulation. But the comparison across simulations with differing domain sizes as well as the explicit suggestion that the results of the study are generalizable are not justified by the arguments presented.

First, concerning the results across domain sizes: the mean profiles are unlikely to change much as a function of domain size as long as the updrafts and downdrafts dictating cloud organization are well-resolved. This is clearly the case with a nearly 9 km horizontal domain. On the other hand, the variances of microphysical fields have been shown to increase with averaging length scale (some references you cite, I also recommend adding Lebo et al. 2014, Wu et al. 2018 and Witte et al. 2019). This implies that comparing the profiles of variances of thermodynamic and microphysical properties across simulations with widely differing domain size is highly ambiguous. While I realize this is not the main point of your paper, this was one of my main conclusions from looking at Figure 10b. The lack of discussion of the noticeably lower maximum Eq factor near cloud base for the Nc=75 cm-3 case with respect to the smaller-domain simulations leads me to think the authors have not considered the effect of this aspect of model configuration. It is a strong result that LES compares favorably with the observed enhancement factors, both at 30 km. But as a greater variety of grid spacings are used in GCMs and regionally-refined grids become more prevalent, consideration of the scale-dependence of the enhancement factor becomes paramount. This is precisely *because* the variances change but the means don't – the IRV and therefore Eq scale with qc variance. I think the authors can easily address this by running an additional Nc=75 cm-3 simulation on the 8.96 km wide domain. This would both demonstrate the domain-size scaling relationship as well as give appropriate context for analyzing changes in Nc.

Secondly, I think it's a significant overreach to say the results are generalizable. The range of number concentration examined did not strongly affect the cloud fraction (much less the cellular organization), such that it's not clear whether a field of open-cell drizzling stratocumulus would respond similarly. Does the profile change for Nc=25 cm-3, an equally realizable concentration at the ENA site? How does the profile change for differing cloud adiabaticity, which would likely be accompanied by a change in the level at which qc variance begins to increase? For differing EIS (or, more specifically, combinations of surface flux magnitude and inversion strength)? For stronger sub-cloud stratification? For a deeper/shallower, moister/drier or cooler/warmer PBL? In short, you need to make a much more forceful argument that these results are generalizable beyond single-layer not-strongly-decoupled stratocumulus decks of approximately the same Nc and LWP as this case. Otherwise, I think you can only say with confidence that LES adequately reproduces the variability of the observations for this specific case. Larger-scale observational studies of column-integrated microphysical variability have showed a broad range of Eq, even in marine Sc, so saying *this one case* could be the basis for a global parameterization in the age of big data, routine LES and machine learning seems a bit obsolete.

Finally, I suspect the details of the vertical structure of Eq are strongly dependent on the use of a one-moment parameterization. While you didn't get big differences over the range of Nc simulated, a two-moment simulation with varying Nc could yield quite different results, especially at cloud top and base where the variability of number concentration is expected to be greatest. The benefit of re-running these simulations with bin microphysics is not clear to me; I think the computational resources would be better used incorporating interactive aerosol into a two-moment bulk scheme.

One other significant suggestion: I assume you can diagnose autoconversion/accretion rates directly from the output used to calculate enhancement factors. I think the impact of the figures showing the enhancement factor (e.g., Figs. 8, 10) would be greatly increased by including a profile of domain-mean autoconversion rate. This would give a huge amount of context as to the importance of the enhancement factor on precipitation formation. For example, why is it that you focus on the increase of enchancment factor near cloud top, but not the nearly 2x higher maximum at stratus cloud base? An enhancement factor of 4 vs. 2 doesn't really matter if mean autoconversion rate is 3 orders of magnitude lower at cloud base. At some point in the text you mention that autoconversion is expected to peak in the upper part of cloud, but show it and your point will be made!

MINOR COMMENTS

- You use a combination of “sub-grid” and “subgrid” – decide on one hyphenation and use it consistently throughout.
- It appears that Fig. 1a was taken directly from Z21. Is this problematic?
- L268-269: “specifically, that not all the cumulus rise completely into the [Sc] deck” -- is this speculation or did you look into it?

TYPOGRAPHICAL COMMENTS

L6: “variability of the cloud properties *that* determine the process rate”

L66: “To account *for* this bias”

L74: Remove last instance of “to” in phrase: “more homogeneous to (Lebsock et al…”

L141: What are “fair-weather” stratocumulus? You chose a pretty heavily drizzling case…

L145: “Each of *these* selected legs”

L147: “which are used in Z21”

L157: “and peaks at about 1 km for hleg 7”

L161-L165: This is almost a verbatim repeat of what you said before. Is it really necessary to exactly reproduce it?

L189: “in situ measurement*s*”

L217: “*The* cloud boundaries…”
L242: I think you mean: “*Above* this level…”

L262-264: “agrees will with observations and overplotted on…” – the “and” in the middle doesn’t make sense to me. This needs to be rewritten but I don’t have a specific suggestion.

L267: “likely a consequence *of* the increase of…”

L316: either “a large mean value of” or “larger mean values of” – can’t combine “a” and plural

L327: “This allows us to utilize *a* similar analysis *as that* used *to examine* the gradient of…”

Figure 1b: Is the lower x-axis accurate? This doesn’t agree with Z21.

Figure 1 caption: replace “KZAR” with “KAZR”

Figure 7 legend: please note which observed variable you are showing. I assume IRV but it’s not unambiguous.

Figure 7: No indication of what dashed curves mean in legend

REFERENCES

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