Review of Calderon et al., acp-2022-373

SUMMARY

This study describes observations and simulations of aerosol-cloud interactions via two case studies of low stratiform cloudiness at a continental site in eastern Finland. The cases are characterized by a unique suite of measurements similar to a US DOE Atmospheric Radiation Measurement program site including in situ measurements of basic meteorological variables (pressure, temperature, horizontal winds, water vapor amount) as well as aerosol and cloud properties; and remote sensing of cloud and boundary layer dynamical properties (i.e., vertical velocity) from a W-band Doppler radar-radiometer system, ceilometer and Doppler lidar. The simulations are designed to simulate aerosol-cloud interactions in a highly detailed manner using large eddy simulations coupled to a size-resolved Eulerian aerosol/cloud microphysics framework. The overall goal of the study is to demonstrate the significant degree of microphysical closure that can be obtained with the modeling framework.

A vast (and impressive) amount of observational information was synthesized to provide a comprehensive reference for the simulations and while the purpose of the study was to demonstrate the performance of the model, there was relatively little information given on the model construction and configuration. What model information was given was rather spread out over the manuscript, which made it difficult to understand exactly how the simulations were run. In addition, several major points were made in the conclusions/discussion regarding microphysical mechanisms that were barely mentioned in the results and relevant figures – this felt rather incongruous. As a result, I recommend the manuscript be returned to the authors for major revisions.

MAJOR COMMENTS

- The study builds upon a large body of previous and contemporaneous work to define and characterize the case studies and describe the modeling framework. In general, I found that the authors relied so heavily on references to other works that I was not able to understand much about their approach without close reading of these other manuscripts. I feel strongly that the paper should include further relevant details such that it can stand on its own. This is particularly the case with respect to the microphysics scheme, for which basic information was scattered across several references and sections of the manuscript and supporting information (e.g., number of aerosol sections given in section 3.1.3; the relation of hydrometeor bin sizes to wet or dry particle size in section 3.1.4; an incomplete list of processes given in Table S1). Please define the categories (size, composition, how size is expressed [e.g., wet vs. dry diameter]) and list which moments of the distribution are prognosed (even if only one).

  Regarding the use of references as a stand-in for a model overview in the text/supporting information, a summary of the *current* state of the parameterization is important since I was not able to obtain a preprint of the newest reference (Tonttila et al. 2022, in review, JAMC). Instead, I was forced to go through your code to understand differences between published UCLALES-SALSA configurations and that used in this paper.

- Two processes are discussed in the last few sections of the paper that I did not see mentioned anywhere earlier on:
  (1) Enhancement of collisional growth via turbulence. I could not find any discussion of coupling to turbulence in previous descriptions of SALSA. The code (specifically, mo_salsa_coagulation_kernels.F90) shows that turbulent enhancement of collision-coalescence is tied to a hard-coded eddy dissipation rate rather than responding to local turbulence – and if my assessment of the magnitude is correct, the assumed EDR is rather high and more appropriate for shallow cumulus than stratocumulus (makes sense coming from the Chen et al. 2020 reference). It is also possible that you were discussing some other mechanism for turbulent enhancement of collision-coalescence (i.e., "turbulent fluctuations" at larger scales?), but it was not apparent from the existing discussion. Please clarify.

  (2) Wet scavenging. The last sentence of the paper frames the results of this study as important for the development of wet scavenging schemes in GCMs, but there was no discussion of the impact of wet scavenging on the simulation results. This seems particularly important in the context of Case 2, where sensitivity simulations are performed with aerosol loading artificially reduced by 40% to improve agreement with the in situ drop size distribution and vertical velocity measurements. Did you test the sensitivity of your results to assumed scavenging rates? Could you obtain similar results to the "reduced aerosol loading" experiment by artificially enhancing the rate of scavenging? Given how difficult it is to directly observe scavenging, this seems like an obvious process rate that one might experiment with.

- Presentation of simulation outputs. Let me start by acknowledging: I know your primary focus is on characterizing the aerosol and cloud microphysics output, with a secondary focus on turbulence. So it makes sense that you have many figures comparing size distributions, activation curves, etc., that are difficult to view in a vertically-resolved form. Nevertheless, the lack of profile or curtain-type figures for context on the evolution of the mean thermodynamic and turbulence fields made it difficult to relate to some of the discussion in the text. Specifically, I'd like to see a couple more figures in the spirit of Figures 3 and 9 (time-height curtain plots of cloud LWC and N) for mean thermodynamic fields (temperature and moisture in whatever variables you prefer, perhaps also relative humidity) and turbulence (moments of the w distribution – variance and skewness). You spend some time talking about how the OVL values are validated by obs-model comparisons of variance and skewness, but you never show this. I would imagine that many readers are unfamiliar with the overlapping index metric, so it would be helpful to your argument to make a direct comparison with more "standard" turbulence diagnostics. This would also help with the fact that OVL must be presented on a level-by-level basis; it makes it very difficult to construct a coherent mental image of the vertical development of the turbulence.

MINOR COMMENTS

L37-39: Do your simulations match this heuristic view?

L42-43: This reference to Bougiatoti et al. (2020) is rather over-constrained since it is taken from a single aircraft field campaign over a specific continental region. Are these to be taken as typical values for the whole planet? Shouldn't this be a strong function of boundary layer forcing (surface fluxes, cloud top cooling rate, etc.)? Also, it is not clear that you are discussing respective day vs. night values because of the sentence structure.

L77-78: Other studies have looked at droplet-depletion via turbulence-microphysics interactions; e.g., Remillard et al. (2017) and Witte et al. (2019)

L100: What do you mean by "explicit" calculation? You are not simulating individual particles, so I'm not sure this is appropriate.

L105: Irradiance changes where? The surface? Top of atmosphere? This is an overly vague statement.

L119: here you say the timestepping is leapfrog, but later you say Eulerian-Lagrangian timestepping (L214) – what processes are done with which timestep?

L210: "64 by 64 points" - This is a tiny grid by modern standards. Are you able to spin up realistic turbulence with such a small domain? Also, what is the horizontal grid spacing?

L213: Re: "no significant changes in model outputs" as a function of vertical resolution – Did you compare any measures of DSD spectral width from LES with the observations? There have been several recent studies (e.g., Morrison et al. 2018; Lee et al. 2019, 2021; Witte et al. 2019) that have shown sensitivity of bin microphysics schemes to vertical grid spacing. Given your unique bin arrangement, perhaps it's the case that you arranged your bins similar to the optimal spacing described by Lee et al. (2021), i.e., a combination of linearly-spaced bins for the cloud drop regime and logarithmic bins for the collisionally-dominated regime – but it's not clear from the information you give in the text.

L305: "mass fraction values of…" – The total adds up to more than 100%, so…either go to 3 sig figs or round to reach 100%.

Figure 1 and 2: The markers for Air-Savilahti and Vehmasmäki are indistinguishable without zooming in to 500%. If z=0 is the only level at which Savilahti is displayed, the caption is sufficient to explain data provenance and I suggest you use a single marker for the observations.

Figure 3: This finding re: the increasing trend in N as a function of increased evaporation in cloud is interesting. It seems like such an intuitive consequence of the boundary layer top rising into the dry air above. Very cool.

L391: "significant degree" - Is this in the strict sense of statistical significance, or are you saying it's a large value of OVL?

L420: "moving from fog dynamics to cloud dynamics" – what do you mean? Can you be more specific? I think maybe you're talking about the same phenomenon I was musing on re: my Fig. 3 comment.

L466: what do you mean by "common bin microphysics?" What about the bins is common? The aerosol representation?

L470: "different instruments correlate to each other" -- Correlate, but do not agree. Do you "trust" FM or ICEMET more beyond D>6 µm? They differ by half an order of magnitude in N in Figure 8, although admittedly the modeled DSD has differing agreement as a function of size. Does either instrument have known counting biases in high concentration conditions? Seems like ICEMET should have improved sample volume and optics to detect large particles vs. a forward scattering-based probe.

Figure 8: Whence the secondary peak in the distribution at 20 µm? I think this is where you transition from "cloud" to "drizzle" regimes? Is this just a consequence of the dry vs. wet size difference? A real numerical artifact?

L502: "turbulence was stronger compared to the diurnal case" – isn't this the opposite of what your earlier Bougiatoti et al. (2020) reference said?

L586: compare reduced number concentrations with observed – both are closer to observed case average Nd, worth pointing this out.

L598: "cloud processing is producing larger aerosol particles" – point this out in figures! The info is there in Fig. 13, right?

L622: "but within orders of magnitude below detection limits…" – I'm confused by the word "within" here, does that mean "many" orders of magnitude? Or few? I assume the latter.

L682: Spell out the references to field campaigns, or at least give the appropriate references.

L683: MPACE also used longer-term ARM measurements

SL179-180: "cloud droplets and precipitation droplets" – without knowing these are different categories with different properties, it confusing why you would make this distinction.

TYPOGRAPHICAL COMMENTS

L28: At numerous locations in the manuscript the word “both” is preceded by a comma, i.e. “affecting both, the cloud optical properties and…” – remove these commas.

L33-36: sentence structure does not make sense, in particular the 2nd clause (“droplet formation can be characterized…”) – seems like it should be separated from the 1st clause into its own sentence.

L74: “…but it is…disconnected *from* those aerosol…”

L108: “Arctic” instead of “Artic”

L117: “doubly periodic” instead of “doubly periodical”

L125: “cloud hydrometeor*s*”

L141: Capitalize “In situ”

L221: “which were allowed for the second hour before the actual analysis started” – this wording is very confusing. I think you could drop the phrase “before the actual analysis started” and the meaning would be clearer.

L276: “Figure 1 and Figure 2 [show?] the atmospheric…”

L312: “Large particles … promote drizzle formation” (not promotes)

L392: “Halo” – capitalize

L406: Repeat of altitude “(225 m)” – this can be removed, right?

L436: accidental “mum”

L491-492: remove)’s after Fig. 9 references

L559: “local aerosol sources…r*a*ising the probability”

L578-579: suggested rephrasing: “we decided to investigate the extent to which the modelling results…”

L607: I’m not sure “relevant” is the right word here to describe its importance, but I’ll leave it up to you whether you want to change it

SL184: “non-activated”