Review of Dziekan et al, GMDD, 2019

Summary

This manuscript introduces a LES model with a dynamical core model and a fully coupled Lagrangian microphysical model for warm clouds. Both model parts have been described previously in two separate GMD publications. The manuscript repeats the description of the main components of the dynamical core and the microphysics part in a condensed form. A case study of a drizzling marine stratocumulus, which has been used in a model benchmark study (11 participating LES models in a 2009 publication), has been performed and the simulation results have been compared to the 11 LES models.

The content of the present manuscript is well suited to be published in GMD and I recommend publication after major revisions. I believe that the manuscript would benefit from inclusion of a second test case.

Major comments

1. As the main components have been described elsewhere, and the coupling of the two parts seems to be rather straightforward, a stronger emphasis could be put on the model verification. You compare it to an ensemble of 11 reference models with quite some spread. But no one knows what the truth is. So far, I am not really sure what conclusions are to be drawn from your comparison and how I should interpret your results? Can you conclude anything e.g. from the fact that your model lies above or below the ensemble mean for some physical quantity? Better describe what you expect from your comparison exercise. Focusing on one specific test case gives only a snapshot of the model's overall behaviour and it is not clear how robust and general your findings are. It would be interesting to see how your model behaves in another well-chosen test case.
2. To be frank, resorting to the iLES approach comes in handy as you don't have to implement a SGS scheme. I could live with it if your model is purely Eulerian. As the Lagrangian model has no implicit numerical diffusivity (neither in spectral nor spatial space) and the iLES approach is not applicable in the microphysics part, SGS random perturbation velocities could be included in the transport equation of the superdroplets in order to mimic subgrid scale motions. However, without a proper SGS scheme that estimates TKE it is not straightforward to prescribe such perturbations. This shortcoming should be clearer mentioned.

Minor comments

- P1. Last row: Isn't libmpdata++ the dynamical core? What does it mean "it is built on top of" it?
- p.4, l.22: Without defining what a collision between two SDs is, it makes no sense to say the probability needs to be increased. Please rephrase.
- p.5, l.14: I do not understand the inclusion of $w_{LS.}$ This would mean that the SDs move relative to the surrounding (Eulerian) air!?

- Sec 3.2.: The implementation of the various condensation algorithms is not clear to me. Given that Ψ_{new} and Ψ_{old} are known, you do a linear time interpolation between the two values. And the difference between the two approaches is the choice of the grid box from which you pick the Ψ values. What I stumble upon is the quantity Ψ_{new}. Is it known beforehand? In my understanding, sub-stepping would simply mean that condensation (growth of droplets, depletion of water vapour and latent heat release) is treated with a smaller time step and clearly involves a dynamic update of the variables θ and q_v in each sub-time step.
- Sec 3.3: I do not fully understand why you solve a prognostic equation for q_l in the Eulerian model part. Wouldn't it suffice to diagnose q_l from the SDs? I understand that q_l is used for the computation of the buoyancy term (Eq. 6). Do you need it elsewhere? Can you estimate the error of using two different definitions of q_l? You write that you want to avoid an additional synchronization? Would this issue still matter in a parallelised implementation?
- Sec 4.1: Can you comment why you use a split definition (Hall +Davies) for the collision efficiencies?
- Sec 4.2: In particular, the differences between the *per-cell* and *per-particle* approach are so small that I am not fully convinced that the one is superior over the other one. It would also help to see the spread of the 10-member ensemble of a specific 2D simulation. Is it really significant that in the one case the N_c-profiles slightly decrease with altitude, whereas in the other case they slightly increase? Can you be sure that in other test case, your finding (superiority of the *per-particle*) would be the same?
  This is one example why I recommend a second test case.

Typos, language issues and other formal things:

- In general, the usage of articles "a" and "the" is not correct on several occasions. Sometimes you miss the article, sometimes it is misplaced. Please try your best, the rest will be handled by Copernicus services.
- The Exner function pi should be defined close to Eq. 3
- p.4, l.16: collisionS
- There is a difference between which and that: https://www.wisegeek.com/what-is-the-difference-between-that-and-which.htm
  Accordingly, "which" in p.4, l.19 and l.24 must be replaced by "that". There might be more such mistakes.
- p.4, l. 23: dropletS
- p.12, l.10. not sure if "VAR" is self-explaining?
- p.12, l.31: visible IN
- p.13, l.11: impact IN 3D simulations than IN 2D simulations
- Caption of Fig. 4: Please correct "On the vertical axis is height …"
- P.17, l.17: concentration