Review of the manuscript "Competition between core and periphery-based processes in warm convective clouds – from invigoration to suppression" by G. Dagan, I. Koren and O. Altaratz

The submitted manuscript describes the change of the optimal aerosol concentration Nop with different thermodynamic vertical profiles, where Nop is defined by the trend reversal of the total cloud water mass. This systematic change of Nop with varying initial conditions appears interesting to be investigated in order to explain the divergence of previous studies. Nevertheless, general concerns as well as specific comments are described in the following.

The title of the manuscript refers to warm clouds, but also clouds are simulated which in principle allow for ice production. Although primary ice formation is typically not expected to be very efficient at temperatures higher than -10°C, it should be kept in mind that in this range the ice multiplication due to rime splintering can potentially increase the ice number concentration at these relatively warm temperatures. It would thus be useful to argue why the potential ice in the investigated clouds is negligible nevertheless, or otherwise in which direction the results of this study would be shifted in case that ice effects were not negligible. Furthermore, it should be explained why in addition to pure warm clouds, also profiles were used which result in cloud top temperatures of -10°C. Do these simulations add significant value to the results of this study in spite of the uncertainty that is introduced by neglecting the ice particles?

The authors describe their model to be axissymmetric and I think it should be discussed in more detail what this means for the simulated clouds. Does the symmetry only refer to the model grid, or also to the cloud appearance? Can we imagine a cylindrical domain, or a 2D-plain with axis symmetry? Since the mixing at cloud edges is a basic process that determines the results of this study, what are the limitations of this approach and in which direction could the results shift if a full 3D large eddy simulations was used?

Drag force effects as a result of liquid mass accumulation are mentioned multiple times to explain an impeded cloud evolution and updraft strength. A more extensive discussion on the exact mechanisms (acceleration due to $-gq_{liquid}$?) including references are needed. Also an estimation of the relative contribution compared to the entrainment effect of dry air into the cloud would be useful.

Furthermore, the notation that is used throughout the study should be clearly defined. Some examples are:

- condensation efficiency
- invigoration
- core, periphery
- core and periphery processes

Specific comments:

p. 23556:

lines 5ff: Suggest to mention the kind of model that is used, like single column/2D/3D/idealized or not?

line 9: Which framework is meant, the specific model framework that is used here?

line 23-24: Does the statement refer to cloud-resolving models, i.e. on a scale of single clouds, or also studies on scales larger than some kilometers?

p. 23557:

line 22: Variance should be described more axactly, otherwise I suggest "narrower size distribution".

line 24: Do you mean the change due to a different aerosol size distribution? Also, do you mean the interactions between droplets (not mentioned before in this paragraph), or between the processes (what exacly is meant in this case)?

p. 23558:

line 1: What exactly or which regions of the cloud does the mixing refer to?

line 5: Again, I wonder if the change is that which is caused by different aerosol size distributions.

p. 23559:

line 8: Suggest to explicitly name the key processes which are addressed in the analysis .

lines 19ff: For a better readability and overview of the following text, please summarize the specifications of T*RH* notations in a table. Maybe the description of clean and polluted aerosol conditions could be contained in the caption.

line 20: In my opinion,"idealized" would be a more common notation than "theoretical" profile. More importantly, it is necessary to have references either for the idealized profiles that are used here, or measured data for comparison with typical moist tropical profiles.

p. 23560:

line 5: As described above, a more detailed justification concerning ice particles should be added.

line 17: I missed the technical specifications like spatial and temporal resolution in the first paragraph of chapter 2. Therefore I suggest to shift this description, including the more detailed description and implications of the axissymmetric grid.

line 19: To get a better overview, it would be helpful to have a summary of the clouds' vertical extent.

line 25: Also here it is not clear to me which specific key processes are addressed. Please describe "magnitude" in more detail, for example, I can think of total maxima or in-cloud averages or domain averages.

p. 23561:

line 4: For clarity, I suggest to describe it as a maximum with respect to the temporal evolution within a simulation. Furthermore, I wonder whether the time series of the total cloud mass has a similar shape among the simulations, i.e. only the magnitude varies, or whether the behavior is quite different among the 90 simulations. I see there is an example shown in Figure 4 for profile T1RH1 – are they representative for the rest of the simulations or can more pronounced differences be expected?

p. 23562:

line 11: I wonder how well-established the indicated relationship between condensation efficiency and droplet surface area is. In particular, how much do curvature effects of the smaller droplets on the saturation vapor pressure counteract the increased efficiency due to the larger surface area? Is it negligible, i.e. the surface area effect predominates, or what are the droplet sizes for which curvature becomes non-negligible? Are such sizes reached here?

p. 23563:

line 5: Are there thresholds that define the start of these processes?

p. 23564:

line 23: delete ":"

p. 23565:

lines 4-7: It would be interesting to see the change of vertical velocities, for example in the same manner as in Figure 2, where maximum values are shown as function of aerosol concentrations. Is this possible with the existing model output?

lines 18-20: Is there a way to extract or estimate the relative contributions of the drag force effect and the entrainment effect to the suppresion of the cloud development?

lines 20-27: As I see it, this is a repetition of what is already contained in the text above.

p. 23566:

paragraph 1: I suggest to clarify that the humidity outside of the cloud is addressed, instead of the "RH of the cloudy layer".

line 27: What does a weighting of the mean updraft by the liquid water mean and what is the advantage of weighting compared to a non-weighted mean value? Does a parcel with 0.1g/kg liquid water content have a tenfold weight than a parcel with 0.01g/kg, such that the cloud core mean updraft is highlighted relative to the outer regions? Thus I wonder whether the cloud maximum updraft velocity would yield a very different picture. If not so, I think that this would be a measure that is easier to interpret for the reader. Otherwise, the idea behind the weighting needs to be described.

p. 23567:

lines 3-8: I have difficulties to get the essence of these two sentences, which seem to compare the relative contributions of core and periphery processes. Please rephrase.

Figures:

Figure 2: Since eta is described as a parameter to estimate the relative importances of cloud core and periphery processes, it might be helpful (if not, why?) to have a plot that explicitly compares eta against the resulting cloud mass as shown in Figure 2, for example. However I wonder about the variability of eta with time, so can we gain further insight from the proposed comparison, if not so, why? I could think of three more panels which show the aerosol-dependent eta value corresponding to the maximum (wrt. time) cloud mass situation. It is left to the authors to extend their figure or not, but a short discussion should be added to the analysis.

Figure 4: Here I strongly recommend to show eta as a function of time for the three examples shown in Figure 4. How are minima and maxima of the time dependent eta and cloud mass correlated and is there a systematic shift between the simulations? Maybe a fourth panel could be added, showing the three eta time series that result from the existing panels.