Review of manuscript “Competition between core and periphery-based processes in warm convective clouds – from invigoration to suppression” by Dagan et al.

This is a basic study for idealized modeling simulations of aerosol effects on shallow cumulus clouds. Most of the findings in the paper (such as increased condensation with aerosol concentration due to larger surface area; delay of collision processes due to smaller droplet sizes, etc) have been well established and I do not think this study provides significant progress in this area. That say, I do not understand the motivation of the study.

Second, the methodology may have problems.

(a) The sharp changes as shown in Figure 1 in the dew point temperature may have a problem. Also, it does not look realistic at all.
(b) The RH of 95% and 90% is too high. This kind of condition is not a usual environment for forming convective clouds. 70%-80% of RH already represents very humid environment.
(c) Strong warm bubble initialization (3 ^{o}C) and without ice processes: for clouds with an inversion layer, this kind of initialization is very unrealistic. Such a strong warm bubble initialization under an extremely high RH environment would lead to very strong convection leading to deep convective clouds. But the authors used an inversion layer to limit the cloud vertical development and also turned off the ice microphysical processes to force a warm cloud. In reality, it would not happen in this way (likely it would be deep convection with mixed-phase and ice phase processes). Radom perturbation would be recommended to do shallow cumulus clouds.

Third, the paper is not clearly written. They created many phrases but they are not well defined and consistently used. See below:

(1) From the title and throughout the paper, the authors use phrases like “core process”, “periphery-based process”, “margins’ effect”, and “margins’ processes” but they were never clearly defined. In fact, they are only about condensation (they mean core process) and evaporation/entrainment processes (they mean margins’ processes but at least three different terms were created for this) in this study. I do not understand why not sticking with physical terminologies in a scientific paper? Creating fancy terminologies may be good for general public which is not the purpose of ACP journal. It only creates confusions for scientists in this area. It is not necessary at all for this study since the involved processes are simple.
(2) Define cloud invigoration: looks like you meant enhanced condensed water here. However, for deep convective clouds, it refers to the enhanced convection or precipitation many times. To avoid confusion with cloud invigoration for deep convective clouds, I’d suggest using cloud mass enhancement/suppression instead of cloud invigoration/suppression.

(3) You have at least three types of Nop: for cloud mass, for surface rain, and for cloud top height. It is currently written in a confusing way. Please be clear about it throughout the paper.
Also, the existence of an optimal aerosol concentration is not new. Although past studies may have not clearly pointed out that the optimal aerosol concentration increases as RH increases and clouds get deeper for shallow clouds, it is something that is easily inferred. Even if this is new, is it enough to make a paper in ACP? I will leave the question to the editor.

Fourth, the study has a narrow literature survey. The papers cited on aerosol impacts on shallow clouds are mainly from one group. Many studies in the same area especially these that have the same findings are not cited. Recommend a thorough literature study and understand what has been done already. As stated above, the findings of this study are not new.

Specific comments

P23559:
1. Methodology: Is it a 2-D or 3-D model used for simulations? What is binary breakup?

P23560:
Provide a figure for size distributions of maritime and anthropogenic pollution.

P23561:
Line 8-10, very confusing sentence.

P23562:
1. "Difference in the total condensed mass are due to increased efficiency of the condensation process and the delay in the collision-coalescence process, in the polluted cloud" – well established already. Need discussion of this.
2. Last paragraph, about larger surface area leading to stronger condensation in the polluted clouds – well established.
3. Line 19-20: I did not see this significantly. Condensation growth strops at 70 min in all three cases. Also, why does condensation have negative value? I think evaporation is included, then the Figure legend needs to be changed.

P23563: nothing new and main points are well established.

P23564:
1) Lines 17—19: please present updraft velocity in clouds. I do not understand the means of the weighted updraft velocity presented in Figure 6

2) Line 26: how do you define cloud margins and core?

Figure 6:

1) Nothing is sensitive to RH when the inversion height is at 2 km, even for surface precipitation, which is hard to believe. This could be related to the problems that I pointed out for methodology. Need explanations how it is happening.
2) About the vertical velocity (middle panels), what do you mean by "the maximum over time of the mean vertical velocity weighted by the mass in each grid point"? Why not plot the maximum vertical velocity directly from simulations?

P23566:

1) Line 26-27: why weighted by the liquid water mass for updraft velocity? Need a figure for the physical vertical velocity (such as maximum vertical velocity) to get an idea about convective intensity of the clouds.

P23567:

1) In the first paragraph, the authors use phrases like "margins' effect" and "margins' processes" but they were never clearly defined. Please refer to my major comment on this.
2) Line 8-10, why should maximum total mass of the cloud is sensitive to cloud top height?
3) Line11-12: what are lighter margins? What is the declining branch? Please describe with physical terminologies.
4) Line 26-27: Collection efficiency should decrease with the droplet number concentration. But the total rain mass converted from the collisions of droplets may not be decreasing with the droplet number concentration.

P23568:

Line 1-2: please verify if you see the same thing in your simulations.

Summary:

Nop in the model is sensitive to condensation and evaporation. Very low Nop for cloud mass (25 cm-3) of shallow clouds with a inversion height of 2 km even at RH of 90% and 95% looks unreasonable. The model could simulate too strong evaporation. Or the methodology was not appropriate. This should be discussed.