<u>Reply to the referees of "Competition between core and periphery-based processes in warm convective clouds – from invigoration to suppression"</u>

We would like to express again our appreciation of the reviewers' efforts. We have addressed all of their additional comments and changed the manuscript accordingly. In particular, both reviewers were concern about the cases of the deepest clouds noting that such clouds are likely to be similar to deep convective clouds with strong updrafts and a possible significant contribution of the cold phase. We felt that both reviewers thought that even for such a theoretical study, considering such a deep cloud as warm cloud would "push the envelope" of this study too far. In order to address this problem we rescaled the initialization thermodynamic profiles such that the new most-developed one (new T1 profile) is similar to the old medium profile (old T2) and the inversion of the new medium profile (new T2) is located between the old medium (old T2) and the old shallow (old T3). Since our paper is focused on the interplay between processes such rescaling does not affect any of our results and it allows us to stay away from the cloud's cold regime and therefore from deep convective clouds.

All the figures and numbers in the paper were updated with the new medium profile. The new figure presenting the initialization profiles is shown below (fig. 1):

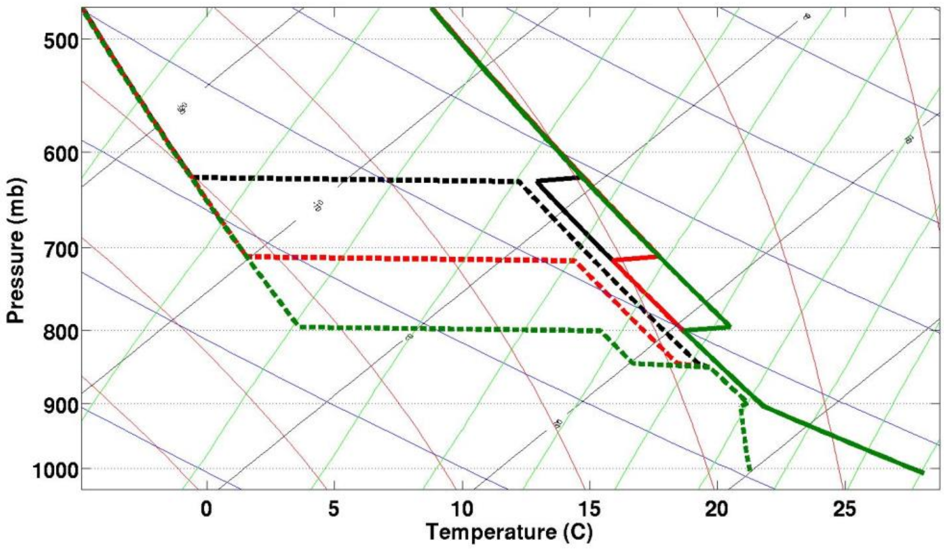

Figure 1. Thermodynamic diagram presenting examples of 3 of the initial atmospheric profilesT1RH1 (black), T2RH2 (red), and T3RH3 (green). Solid lines denote temperature profiles and dashes lines dew-point temperature. In total we ran simulations for 9 different initialization profiles.

Table 1 that summarizes the notation of the different initialization profiles was changed in the revised version:

	T1	T2	T3
RH1	T1RH1: 4km, 95%	T2RH1: 3km, 95%	T3RH1: 2km, 95%
RH2	T1RH2: 4km, 90%	T2RH2: 3km, 90%	T3RH2: 2km, 90%
RH3	T1RH3: 4km, 80%	T2RH3: 3km, 80%	T3RH3: 2km, 80%
Inversion temperature	-0.8°c	6.0°c	12.2°c

Table 1. A summary of the notations, inversion base height and RH levels in the cloudy layer for the 9 different initial atmospheric profiles. The temperature at the inversion is presented in the bottom row. For each profile 10 simulations were run with aerosol concentration of 5,25,125,250,500,1000,2000,3000,4000 and 10000.

<u>A point-by-point replay to Reviewer 1</u>

1) The authors made very significant changes on the manuscript and the current manuscript is much improved. The authors addressed most of my comments very well. The authors have done much better job in recognizing and discussing the previous studies and findings on the topic.

We thank the reviewer for the warm words.

2) **I still have two follow-on comments raised by the new figures that I suggested to added. The first comment is about the aerosol size distribution. First, the figure suggests that the increased aerosols are not likely anthropogenic. From the maritime to the polluted conditions, the significant increase is at 0.1- 1 microns. There is no much increase for particles less than 50 nm. Generally, anthropogenic pollution may enhance**

aerosol nucleation and increase the nanometer particles significantly. This can be addressed by changing the wording. But most importantly, the different shapes of size distribution among the cases creates more complexity to explain the results, for example, increasing supersaturation at some point would not increase cloud droplet nucleation anymore since aerosol changes are not significant for particles < 50 nm. I am wondering why different aerosol size distributions are used in this study, which imposes another factor (effect of different size distribution) that might impact the results but the authors never discuss this.

Authors reply:

We thank the reviewer for this comment. Similarly to the reviewer's idea, our motivation in this study is to reduce the complexity by controlling the aerosol concentration in the simplest possible way, avoiding processes that heavily depend on the distribution's shape. On the other hand we wanted the initial aerosol distribution to be not too far from reality. Therefore for the clean runs (bellow the marine background aerosol concentration of ~295 cm^{-3}) the aerosol size distribution was controlled by dividing it by a specified scalar, such that the shape of the distribution remains the same (a marine shape). For the more polluted simulations, using the marine background distribution and to simply scale it up would result in very unrealistic aerosol properties (more similar to a tropical cyclone) with large contribution of the larger aerosols. Therefore, we used an additional log-normal distribution on top of the background distribution that better represents pollution cases. Again to keep it simple, the aerosol levels were controlled by a scalar. In that way the shape of the addition of aerosol above the background was kept constant and the addition to the larger aerosol bins was smaller and more realistic (at the largest aerosol bin size, used in this study, we kept the concentration constant for all the polluted simulations- figure S1). Finally, to avoid additional complexly regarding the effects of GCCN we truncated the aerosol size distribution at 1 μm.

Many tests were preformed to make sure that the general results do not depend on the shape of the distribution. We used the above aerosol setting mainly to minimize criticism of using nonrealistic aerosols. Throughout the simulations we checked that indeed increase in aerosol loading will derive an increase in cloud droplet nucleation. The figure below (fig. 2) presents the results of an old set of simulations in which a single lognormal shape is used, controlled by one scalar that divides or multiply the distribution (using the old set of initialization profiles). It shows similar trends to the ones presented in the paper.

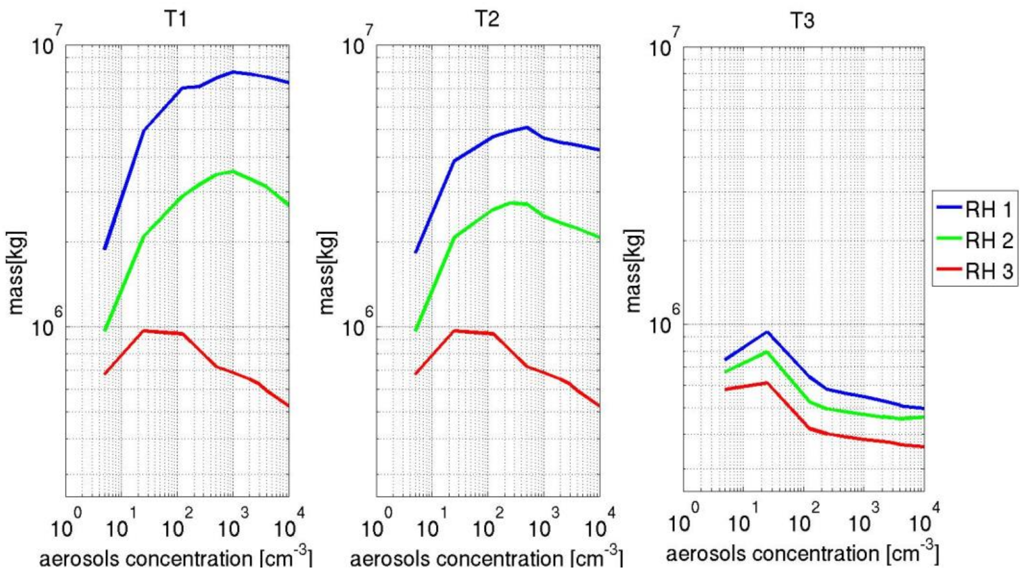

Figure 2. The maximum cloud total mass for each simulated cloud as a function of the aerosol concentration used in the simulation. Each curve represents 10 simulations conducted using the same atmospheric profile (a total of 9 different initialization profiles). T1 represents a profile with an inversion layer located at 6 km, T2 at 4 km, and T3 at 2 km. RH1 represents a profile with 95% RH in the cloudy layer, RH2-90%, and RH3-80%. In this case a single aerosol size distribution shape was used, controlled by a single scalar.

2) The second comment is about cloud updraft vertical velocity. The values are large, especially under conditions of RH1 and RH2. 5- 10 m/s cloud updraft vertical velocity definitely makes deep convective clouds. I understand the study is about idealized tests as the authors argued, but if most of your test cases are not realistic in the nature, then the findings discussed here might not be applicable to the real-world cases.
Judging from the updraft vertical velocity, the 3°C warm bubble initialization is indeed too strong as I pointed out previously. The authors argued that the random perturbation initialization that I suggested produces many shallow clouds. However, isn't that the nature of shallow clouds generally? We do not often see just one piece of shallow warm cloud but a cluster of shallow cumuli. The interactions between the shallow cumuli may change the significance of some microphysical processes especially evaporation and entrainment which could lead to different conclusions. This point at least has to be discussed if the authors insist using warm bubble initialization. But I'd suggest using a weaker warm bubble for some cases to produce more realistic shallow warm clouds (with max updraft velocity less than or around 5 m/s which is more appropriate for shallow cloud cases).

Authors reply:

This comment (and a similar comment by reviewer # 2) was the reason we eliminated the results for the deepest profile (old T1, inversion at 6km) from the paper and instead we added intermediate profile such that the new clouds are bounded by inversions in 2, 3 and 4 km. Please see our detailed answer in the opening. Throughout this study we checked that the warm perturbation bubble does not control the vertical velocities. For example, when following the evolution in time of the maximum vertical velocity for all simulated clouds under the initialization profile T1RH1 (new T1), the clouds start to form after 27 minutes of simulation. Before that the maximum vertical velocities in the domain are less than 1m/s.

Regarding the effect of the cloud field, we agree with the reviewer that it is important. This study is aimed in understanding processes competitions on the cloud scale. How such processes are affected by interactions on the cloud field scale should be, of course tested in a future study. We close the paper with such statement: *"There is a need to further study the synergism between the single-cloud scale processes (as described in this work) to the processes that act on the field scale. The overall aerosol effect on warm cloud fields would be a result of both types of processes."*

Additional minor comments:

1) About "provides an explanation to the reason models suggest suppression and observations suggest invigoration" – This is just an assumption here. There are many other factors that may contribute to the discrepancies between model and observational results. So, suggest rewording the sentence.

Authors reply:

We accept the reviewer suggestion and we have weakened the statement.

The sentence was change in the revised version:

"These results may bridge the ongoing gap between observations and modeling studies of aerosol effects on warm convective clouds. Differences in the studied clouds' dimensions might be the source of some of the discrepancies. Many of the

numerical studies of warm convective clouds focused on trade-like cumulus clouds (Jiang et al., 2006; Xue and Feingold, 2006; Xue et al., 2008; Jiang et al., 2009; Koren et al., 2009; Jiang et al., 2010; Seigel, 2014) where the characteristic cloud size is around 1 km. However, due to limitations in the spatial resolution, earth-observing satellite instruments (such as MODIS) are biased toward much larger clouds (Kaufman et al., 2005; Yuan et al., 2011; Koren et al., 2014). Therefore, our results suggest that warm clouds simulations will more likely capture the descending branch of the trend, whereas satellites data will be biased toward larger clouds that are characterized by higher optimal aerosol levels and therefore will more likely capture the ascending branch".

2) Suggest discussing that the effects (such as lower supersaturation and decreased drag force) resulted from delay of the collision-coalescence process may not be simulated with the bulk parameterization approach.

Authors reply:

We completely agree with the reviewer that bulk schemes are limited and are not likely to capture the delicate interplay between all the relevant processes. We know for sure that the saturation adjustment for example suppresses many of the aerosol effects. But as we are using only the full bin-microphysics scheme in this study we prefer to leave this issue to a future study that will be dedicated only to this and will include runs of both bulk and bin schemes and will show the comparison in a detailed manner.

A point-by-point replay to Reviewer # 2

The authors' efforts to answer the referee comments appropriately are much appreciated. There are several points which contribute to a better understanding of the manuscript, such as additional references and the clarification of notations used throughout the study. I also appreciate the additional effort to oppose drag force and entrainment effects, and an exact quantification would probably be beyond the scope of this study. The added Figure 6 in the revised manuscript as well as some supplementary material add further value. Please find three more minor comments in the following.

We thank the reviewer for the warm words.

General comments

1) The treatment of ice processes

I recognize that the present study involves many idealizations which should not necessarily be seen as a shortcoming. However as a reader I would feel more comfortable to be informed about idealizations as detailed as possible. Regarding the ice, I still would like to know which simulations in particular are affected by supercooled cloud droplets. This could be explained based on Figure 6 (original manuscript) which shows the cloud top height. For example, by using dashed horizontal lines to indicate 0°C and -10°C, or by explaning in the text in which altitudes these temperatures can be expected.

Authors reply:

We thank the reviewer for this comment. As we described above following this comment as well as similar one by reviewer # 1, we decided to eliminate the profile of the deepest clouds (old T1, 6km) from the paper and instead to show another level in between the other two profiles. Therefore the inversion temperature of the largest cloud in the new set of initialization profiles is -0.8°c. The inversion temperatures were added to table 1 (please see above in the general comment part).

2) Table 1- Going in the same direction of the previous comment, I would be interested in the temperatures corresponding to the altitudes listed in the table.

Authors reply:

We thank the reviewer for this comment. We added it to the table.

3) Figure S3- I was just wondering whether the peak at t>100min in the T3RH3 can be interpreted as an artefact in the stage shortly before the cloud disappears.

Authors reply:

The cloud T3RH3_4000 that its surface area to volume ratio is presented in figure S3 disappears completely after 107 minutes of simulation. At the beginning and at the end of a cloud evolution the cloud is small and so its surface area to volume ratio is relatively large as shown in figure S3. This will be the case whenever a small fragment of the cloud remains. We added a clarifying sentence to the supplementary text regarding fig. S3: "*Please note that at the beginning and at the end of a cloud evolution the cloud is small and so its surface area to volume ratio is relatively large as shown in the case of cloud T3RH3 in fig. S3*."