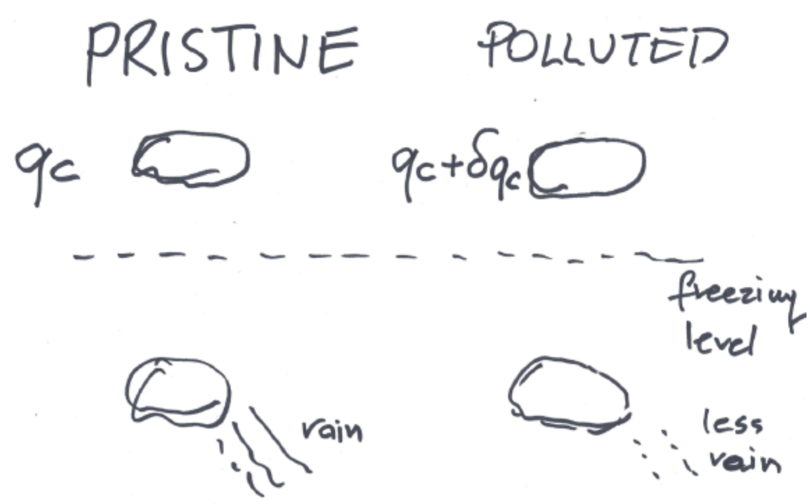
Rebuttal of the authors' responses to the Referee 2 comments:

I appreciate the authors' responses to my initial comments. However, the response has fundamental flaws and thus I cannot consider the issues settled. I think the authors need to reconsider my comments about the invigoration and correct obvious flaws in their arguments. Below, I copy in blue their original responses and provide my rebuttal.

First, for the "cold-phase invigoration", the latent heat released by freezing is not compensated by the mass of cloud water. The buoyancy is mainly contributed from the thermal term corresponding with latent heating and hydrometeor loading. The freezing process does not lead to any change in the hydrometeor loading, so drop freezing increases the buoyancy through latent heat release of fusion. Also, the ice particles after freezing may participate in other ice microphysical processes (i.e., deposition and riming) to further release latent heat.

The authors clearly misunderstood my argument. The figure below illustrates the starting point for my argument:



The left part of the panel shows a cloudy parcel that rises through the melting (freezing) level in pristine conditions. The total liquid condensate above the freezing level is q_c. The right part of the figure shows situation when a similar parcel rises in polluted conditions. Because of the less efficient warm rain processes, the parcel above the freezing level carries more liquid condensate, $q_c + \delta q_c$. Freezing of δq_c in the authors' opinion (and in many other papers) is the reason for the invigoration. However, to carry δq_c across the freezing level requires extra buoyancy when compared to the left panel. As shown in section 2a of Grabowski and Morrison (2020), the two effects approximately balance each other. It follows the original sentences in the manuscript under review that say: "… a well-known theory is that increasing aerosol concentrations can suppress warm rain as a result of increased droplet number but reduced droplet size. This allows more cloud water to be lifted to a higher altitude wherein the freezing of this larger amount of cloud water induces larger latent heating associated with stronger ice microphysical processes, thereby invigorating convective updrafts…" are simply incorrect and require additional explanations. For instance, the invigoration would be possible if the frozen condensate was removed through precipitation processes.

That said, I really do not think referring to invigoration is needed for this manuscript. If the authors insist, then the introduction and references to the invigoration in the text should provide a less biased discussion, for instance, as in Grabowski and Morrison JAS papers and as in Varble (JAS 2018).

Second, for the "warm-phase invigoration", there is limitation to use quasi-equilibrium supersaturation as an estimation of in-cloud supersaturation. Previous studies (e.g., Korolev and Mazin 2003, Pinsky et al. 2013, Politovich and Cooper 1988) showed that the quasi-steady assumption is invalidated in conditions of (a) low droplet concentrations (pristine condition) and (b) intense condensation and evaporation (e.g., cloud base and strong updrafts) due to long relaxation time (larger than a few seconds). Figure r2 shows modeled supersaturation and quasi-equilibrium supersaturation, updraft velocity, phase relaxation time and droplet number concentration from the simulations SBM_noanth and SBM_anth. The calculation of S_{eq} and phase relaxation time τ follows Eq. 3 and Eq. 4 in Pinsky et al. (2013), respectively. Seq is much higher than the true supersaturation especially above cloud base (Fig. r2a) where low droplet number concentration (Fig. r2c) and strong updrafts (Fig. r2b) are seen. The averaged τ are around 10-15 s. The exact closed supersaturation equation (Eq. 6 in Pinsky et al. 2013) shows that condensation depends on droplet number and size, and more droplets in the polluted clouds increase condensation and decrease supersaturation. Although S_{eq} in SBM_anth is indeed much smaller than SBM_noanth, assuming $S \approx S_{eq}$ would lead to some invigoration effect but the effect would be much reduced because the condensation rate in the clean condition would be significantly overestimated by S_{eq}.

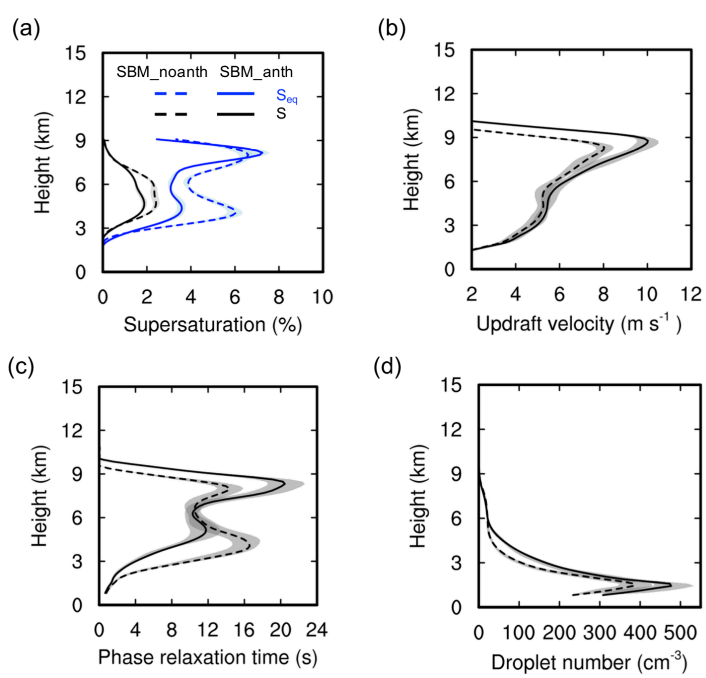


Figure r2 Vertical profiles of (a) supersaturation and quasi-equilibrium supersaturation, (b) updraft velocity, (c) phase relaxation time and (d) Droplet number concentration averaged over the updrafts with value greater than 2 m s−1 from the simulations SBM_anth and SBM_noanth, MOR_SS_anth and MOR_SS_noanth over the analysis domain as shown in the red box in Figure 7 during the strong convection period (2000 – 2300 UTC, 19 Jun 2013). The shaded area marks the spread of ensemble runs.

The phase relaxation time scale and the quasi equilibrium supersaturation estimates in the authors' response above *are simply wrong*. Below I include a table from Politovich and Cooper (JAS 1988) that shows phase relaxation time scale for different combinations of droplet concentrations and radii. These values are much smaller than those shown in the authors' response.

TABLE 1. Time constant characterizing supersaturation. (Values of $\tau = 1/(a_2 I)$ s for p = 771 mb, T = 4.3°C)

Radius (μm)	Droplet concentration (cm^{-3}) 100	300	500	1000
2	14.1	4.7	2.8	1.4
3	8.7	2.9	1.7	0.87
5	4.9	1.6	0.98	0.49
10	2.3	0.77	0.46	0.23

The key question is why?

The explanation is relatively simple. The authors say that they use the formulas from Pinsky et al., eq. (4) therein. However, Pinsky et al. apply a simplified (and in my view incorrect) droplet growth equation that is different from the comprehensive formula used in Politovich and Cooper (JAS 1988). The key point is that one has to apply exactly the same droplet growth equation in the phase relaxation time scale calculation (and thus in the quasi-equilibrium supersaturation) as used in the numerical model. I expect Khain's bin microphysics applies a correct droplet growth formulation that is close to the one used in Politovich and Cooper (JAS 1988), and not the simplified droplet growth equation applied in Pinsky et al. The supersaturation simulated by the model can be compared to the diagnosed quasi-equilibrium supersaturation only *if exactly the same droplet growth equations are used in both*. This was the case for the relatively good agreement shown in Grabowski and Morrison (JAS 2017), at least below the freezing level, see Fig. 15 therein. In summary, the values shown in the authors' response above have to be corrected.

The above discussion requires the authors to modify their responses and revise their paper accordingly. Note that the second part of my rebuttal impacts some of my other original comments. I strongly object publication of the manuscript unless those comments are appropriately addressed.