Response to reviewers. **“Cloud-system resolving model simulations of aerosol indirect effects on tropical deep convection and its thermodynamic environment”**, submitted to *Atmospheric Chemistry and Physics*.

Response to Reviewer #1.

Reviewer comments are in bold, and our response follows in plain text.

1. P. 3, l. 13: Most of the discussion in the introduction reflects the effect of aerosols on warm-phase collision/coalescence only. However, for some cloud types and especially at colder temperatures collision/coalescence is not the dominant microphysical process (in terms of generating precipitation) and compensation through other microphysical pathways may occur. Since the authors find that ice microphysics is crucial in their simulations some discussion on aerosol effects on mixed-phase clouds (e. g., ice nucleation) would be good to precondition the reader.

We acknowledge the potential importance of aerosol effects of deep convective clouds through modification of the ice phase (i.e., ice nucleation). Such effects have been studied previously by several researchers. One reason we didn’t elaborate on this point in the manuscript is because the paper was already quite long. However, we agree with the reviewer’s broader point and have added a short paragraph on this topic in the introduction, along with relevant references. See p. 5 of the revised manuscript.

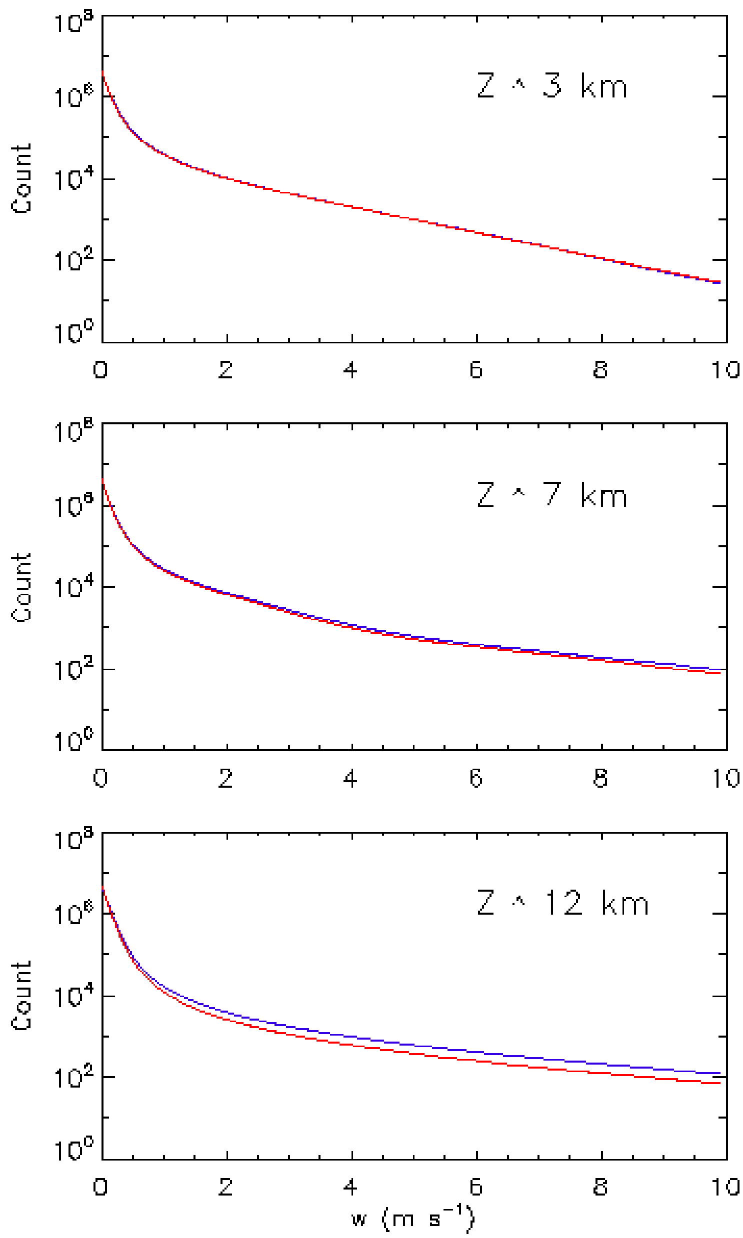
2. P. 4, l. 7: Rephrase sentence: : : : “have suggested that aerosols can either invigorate or weaken convective cloud growth depending on ...”.

We have rephrased this sentence as “….have suggested that aerosols can either invigorate or weaken convective cloud growth depending upon the particular model employed and a number of other factors including environmental conditions.”

3. Fig. 14 shows that the convective mass flux decreases with increasing aerosol number concentration as does the percentage fraction of convective updrafts. However, the decrease in the fraction of convective updrafts seems to be stronger than the decrease in effective mass flux. This would imply that the vertical velocities in individual updrafts could be higher in the polluted case than in the clean case because a similar mass flux must be maintained through a smaller number of updraft cores. Often, in cloud resolving modeling studies comparison of updraft velocities (between clean and polluted runs) are performed by comparing the pdfs of updrafts in terms of vertical velocity rather than mass fluxes. So, how much of the discrepancy found in this study and discussed on p. 22 is due to the use of a different metric? Please clarify.

The convective mass flux in Fig. 14c is an average *only over points containing convective updrafts* ($w > 1$ m/s), and therefore is independent of the area of convective updrafts. Thus, the results in Fig. 14 indicate that when convective updrafts are present, the mass flux associated with them is relatively unaffected by aerosols, but there is a reduced area of convective updrafts in polluted conditions consistent with the small reduction of total updraft mass flux. The reviewer makes a good point about comparison of pdf’s of updraft velocity. Comparison of histograms at three different vertical levels in pristine and super polluted conditions is shown in Fig. R1. These results clearly show that there is a decreased, not increased, incidence (or area) of stronger updrafts in polluted conditions, mainly above 7-8 km, consistent with results shown in Fig. 14. Thus, our findings appear to be robust when using different metrics to compare convective strength; the reduced fraction of occurrence of convective updrafts is not dependent on the particular choice of vertical velocity to define convective updrafts. We've added brief discussion of this point to the revised manuscript (see p. 23), but given the length of the paper we opted not to include Fig. R1.

Figure R1. Ensemble-mean total counts of updraft velocity over the analysis period (1200 UTC 19 Jan to 1200 UTC 25 Jan) for pristine (blue) and super polluted (red) at the three different heights indicated. Counts are shown for equally-spaced updraft bins of width 0.1 m s^{-1} over the range of 0 to 10 m s^{-1}.



4. P. 25: As mentioned by the authors, crucial controls on the outcome of the presented simulations are the microphysical properties of the anvil cirrus and the microphysical mechanisms leading to freezing of supercooled liquid water. It seems that the faith we can put into these simulations will hinge on the confidence we have in understanding ice nucleation (especially the role of heterogeneous ice nucleation versus homogeneous ice nucleation in deep convective clouds). If some of these supercooled cloud droplets would freeze more effectively and earlier through a heterogeneous nucleation mechanisms than the sensitivity of the simulations to the anvil cirrus microphysics would presumably be much smaller. Also, ice crystal number concentrations would be constrained by ice nuclei rather than cloud droplets, which would lead to lower ice number concentrations, larger ice crystals and larger sedimentation velocities. Maybe an additional sensitivity study could clarify the role of heterogeneous vs. homogeneous freezing.

The reviewer brings up a good point. As we describe in the paper, enhanced crystal concentration resulting from freezing of a large number of droplets in polluted conditions is a key to explaining changes in anvil characteristics and hence OLR and upper-tropospheric radiative heating. We actually investigated the role of heterogeneous droplet freezing parameterization in the sensitivity test "HET" (see section 6), in which the formula for heterogeneous droplet freezing from Bigg (1953) was replaced with that of Barklie and Gokhale (1959). This reduces the heterogeneous freezing rate by about two orders of magnitude. This test shows a much smaller difference in ensemble- and time-mean OLR between POLL and PRIS decreases from -10.6 to -2.5 W m^{-2} between BASE and HET (see Table 4). This is opposite to the reviewer's expectations; the effect of aerosols on anvil ice decreases with reduced heterogeneous droplet freezing because crystal concentration is reduced by a factor of ~ 2-4 in polluted conditions relative to BASE, with less difference for pristine. This occurs because droplet concentration is reduced by the time droplets reach the homogeneous freezing level, compared to the concentration at lower levels where heterogeneous freezing occurs, at least under polluted conditions. However, the significance of this result is unclear because ice crystal concentrations and OLR are similar between HET and BASE for super polluted conditions (SPOLL). We have added discussion of this point in the revised manuscript on p. 27 and in the conclusions on p. 32.

5. P. 26, l. 20: The authors argue that the sensitivity tests with respect to domain resolution give little difference when the grid spacing is reduced from 1 km to 0.5 km. I wonder if this is because horizontal advection of cloud variables have been neglected in the model. The significant change in results from 4 km to 2 km may be because the higher resolution simulations give a better representation of the large-scale convection, which is no longer improved at higher resolutions. However, at higher resolutions horizontal transport of (sedimenting and non-sedimenting) hydrometeors clearly becomes more important and should not be neglected.

Perhaps I don't fully understand the reviewer's point, but to clarify, we included horizontal advection of hydrometeors (both sedimenting and "non-sedimenting") at the model grid-scale in all runs. This is critical, even under the relatively weakly-sheared conditions here. What is neglected is large-scale horizontal advection of hydrometeors into and out of the domain (we use periodic lateral boundary conditions), which is the typical approach used by CRM and LES studies. The larger change between 2 and 1 km (not between 4 and 2 km, as stated by the reviewer) compared to between 1 and 0.5 km is interesting. It is well-known that convection is under-resolved at 2 km and certainly 4 km, resulting in broad but relatively weak updrafts due to weaker nonhydrostatic processes associated with larger convective cell sizes (Weisman et al. 1997; Bryan et al. 2003; Bryan and Morrison 2011). My speculation is that this produces relatively small liquid water mass fluxes at colder temperatures (high levels) in the low resolution runs, resulting in a reduced impact of heterogeneous droplet freezing (see response to comment #4 above concerning the important role of heterogeneous droplet freezing). However, further testing is needed to explore this hypothesis (data necessary for such an analysis was not saved during the ensemble runs, which required in-line calculation of statistics due to the huge amount of data). We believe that such work is beyond the scope of this paper but plan to address this in future work.

We have added brief discussion of this issue in the revised manuscript (see p. 28, near top): "Specific reasons for this difference are unclear, but may be related to the marginal ability of models with Δx of a few km or greater to resolve deep convective motion, attributable to reduced intensity of nonhydrostatic processes associated with larger cell size (c.f., Weisman et al. 1997; Bryan et al. 2003)."

1. P. 2, l. 3: Replace "cloud system-resolving" with "cloud-system resolving" here and elsewhere in the manuscript.

Done

2. P. 3., l. 8: Add "stratification" after "atmospheric" or rephrase sentence.

Done

3. P. 4., l. 17: Remove "is" after "response"

Done