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 #3.

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

1) Discussion of Fig. 5 on page 15585/15586: Could the authors give an explanation why the signal of the precipitation flux of the ensemble members matches very good in time and amplitude with the measurements, while the other fluxes of sensible and latent heat are shifted in time. I understand that the difference in the sensible and latent heat flux is due to different representation of the ocean compared to the reality. But why does this have no influence on the precipitation signal?

The reviewer is correct that poor comparison of the latent heat fluxes with measurements is a result of the assumption of an ocean surface relative to conditions where the measurements were taken. The reason that precipitation matches closely with measurements is because the timing and amplitude is primarily driven by large-scale forcing that is derived from the measurements. The biases in the heat fluxes have less impact on the static energy and water budgets compared to the forcing (see Table 1), and therefore play a less important role on evolution of precipitation here. This point is clarified in the revised manuscript on p. 16, with the following addition: "Differences between the modeled and observed surface heat fluxes have a limited impact on the timing and amplitude of F_P, given its constraint by the applied large-scale forcing of s and q." This addition, along with the rest of this paragraph in the text, should clarify this issue for the reader.

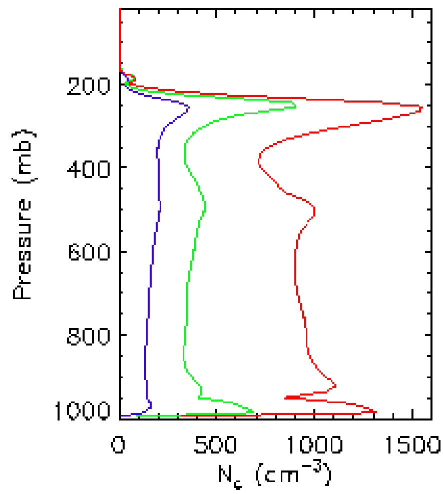
2) P15592, line 18: The authors state that much of the impact of pollution on RSW is due to changes in the liquid microphysics. Therefore it would be nice to get more information about the cloud droplet activation (e.g. activated fraction), especially in the convective updrafts (w> 1 m/s). Fig. 13 only shows the horizontally averaged vertical profiles.

Fig. R2 below shows droplet number concentrations for PRIS, POLL, and SPOLL, but averaged only over grid points with convective updrafts (w > 1 m/s). These profiles of Nc are more constant with height than those that include all grid points containing water in Fig. 13. POLL and SPOLL produce about 2 and 5 times greater N_c in convective updrafts than PRIS, respectively. As expected, activated fraction is generally lower for POLL and SPOLL than PRIS.

We have added the following in the revised manuscript to address this point: "In convective updrafts, the average N_c are roughly 2 and 5 times greater in POLL and SPOLL than PRIS, respectively, but with a lower activated fraction (i.e., ratio of cloud droplet and total background aerosol concentrations)."

We decided not to include Fig. R2 in the manuscript since it is already very long.

Figure R2. Vertical profiles of time-, domain-, and ensemble-mean droplet number concentration, averaged only over grid points with convective updrafts (w > 1 m/s) and with cloud water mixing ratio > 0.01 g/kg.



3) 6.2: Domain configuration tests: The authors show the large differences in the result when the grid spacing is changed from 2 to 4 km, but provide no explanation why this could be. Therefore, it would be nice if the authors could give some ideas.

Differences in results are actually smaller between 4 and 2 km; the biggest difference is between 2 and 1 km grid spacing (see Table 4). The larger change between 2 and 1 km than 1 and 0.5 km that the reviewer point out 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)."

4) It would be nice to have not only the three high- and low-OLR members in the figures, but also the median or mean.

Following the reviewer's suggestion we have added the ensemble-mean OLR and RSW to Figure 2.

5) p15584 equations: Definition of ω is missing.

Clarified in the revised manuscript.