Anonymous Referee #1

This paper makes an interesting claim and is suitable for the journal. However, I am unconvinced by the manuscript for several reasons, and would argue that the study must be strengthened to merit publication.

First, studies of aerosol effects in shallow clouds are nowadays normally done at LES resolutions (e.g. 100 m or better). I would not expect realistic results from a 3-km simulation with only 40 or so tropospheric levels, even for deep convection let alone for non-precipitating convection. A large domain does not seem particularly important for this study. The authors' view that "This model was chosen because it can simulate the direct, semi-direct, and indirect effects of aerosols in a realistic manner because aerosols, meteorology, radiation, and atmospheric chemistry are coupled in the model" is naive given the complexity of the processes and reliance on parameterization. I become more skeptical still when the authors tell us that cloud base in the model is too high by over 50% compared to observations, a serious discrepancy. The authors don't even mention whether they are running the boundary-layer scheme or not; it is basically required at this coarse of a grid.

There are a range of model tools available (from molecular models to LES models to regional scale models to global models) because at each scale there are different advantages and disadvantages. We address our choice of model:

" Previous research on these
interactions has been performed with Large Eddy Simulation
models, while we are utilizing the coarser, yet still detailed,
high-resolution WRF-Chem model. Some advantages
of utilizing this regional scale model are that it includes the
feedback of aerosols onto meteorological processes and that
many properties important to our analyses, including cloud
droplet number concentration, aerosol composition, atmospheric
heating rates, and water vapor condensation are calculated
explicitly and not prescribed."

We show that our results for CDNC and LWC are indeed realistic in Figures 2 and 3. The large domain is important because it enables us to compare the same region which was measured during the RICO campaign with the model domain.
The quoted sentence is accurate. These processes are coupled in the model, and rely less on parameterizations than earlier models, hence why this model was chosen. Instead of defining the simulated cloud base and cloud top based on the model levels and a defined threshold of what constitutes the presence of cloud, we have shown the model results and measurement data for CDNC and included reference lines for where the flight observers defined the cloud base and cloud top (Figure 2). We use the MYJ PBL scheme:

"The Mellor-Yamada-Janjic (MYJ) planetary boundary layer scheme was utilized."

The cloud-water and number distributions look good (almost too good!), but this may be a direct result of the retuning that they did for the unresolved w variations.

We used the updraft velocity distribution function equation w=wbar + c × σw which has been used in atmospheric models for over a decade. The value of c implemented was defined by Wang and Penner (2009) and depends on the subgrid-scale variance of the vertical velocity. This improves the simulated CDNC as compared with using the Ghan et al 1997 updraft velocity distribution function. The Wang and Penner function is not more tuned or parameterized than the Ghan function, it is just newer and gives more accurate results in our simulation.

Second, the authors present their results in a confusing way with multiple subscripts on each term and I cannot make out what is being subtracted from what. Since the main point of the paper is to point out water vapor changes, shouldn't those be shown? I'd expect to see some "before and after" profile plots showing exactly how cloud, water vapor, and temperature changed with and without aerosols, so I can make sense out of the radiation changes. A table of numbers is nearly impenetrable.

We have attempted to make the manuscript clearer by replacing the table with Figures 6-8. We have also removed the subscript method of describing the results.

Finally, the authors don't offer any physical explanation for why the water vapor changed in the way it did (whatever way that was, since we weren't shown). Normally budget diagnostics or sensitivity tests would be used to understand why the water vapor changed, in order to present a more assured picture of what happened. As it is, I would count it as likely as not that the result is a model artifact.

"Only the exaggerated polluted case changes the H2O(v) sufficiently relative to the Reference experiment to be statistically significant ($p<0.1$). SO4 2- from the anthropogenic emissions (both the realistic emissions and the polluted case) and from DMS decreases atmospheric H2O(v). Sea salt increases atmospheric H2O(v). The changes in H2O(v) due to aerosols shown in this study are unrelated to aerosol-cloud effects on precipitation, because there is essentially no drizzle (the average precipitation rate over the model domain during the measurement campaign is less than 0.02 mm hr−1). Rather, as described by Xue and Feingold (2006) and Altaratz et al. (2008), aerosol particles are able to enhance both condensation of atmospheric H2O(v) and evaporation of cloud droplets. In the simulated region, SO4 2- decreases H2O(v) and increases CDNC because it enhances the condensation of atmospheric water vapor. Sea salt increases H2O(v) and decreases CDNC because it enhances the evaporation of cloud droplets."