We would like to thank the anonymous reviewer for very elaborate, useful and thoughtful comments that have been very helpful in improving the manuscript.

Title: Cloud-resolving modeling of aerosol indirect effects in idealized radiative-convective equilibrium with interactive and fixed sea surface temperature

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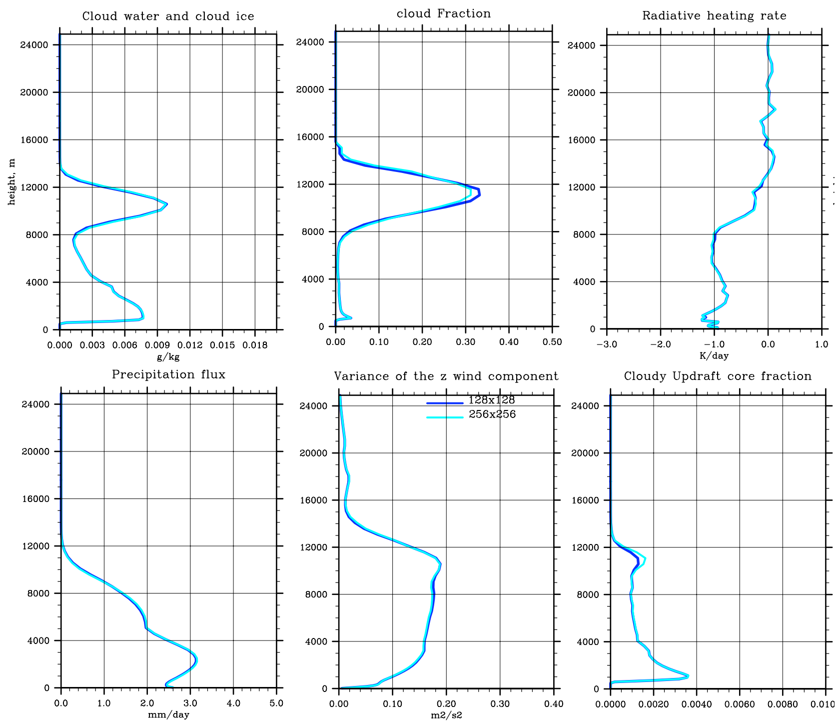
General

The goal of this study is to investigate aerosol indirect effects on deep convection through the use cloud resolving model simulations run to radiative convective equilibrium. While such studies have been performed before, this study is not only unique but necessary in that is makes use of an interactive lower SST boundary. All of the previous studies on this topic have utilized fixed SSTs, which, while making useful initial contributions to this topic are missing the important oceanic feedbacks that occur on longer timescales. Such an approach is crucial if we are to fully understand AIEs on tropical convection on longer temporal scales. As such, this study represents an exciting first step in this regard. However, there are some major flaws with the experiment setup that lead this reviewer to seriously question the validity of the results. These include the grid domain size and the state of equilibrium. Other lesser concerns, although still not trivial, are the lack of mean wind, which will also impact the results, especially the anvil properties and hence the radiative response, as well as the assumptions made in the aerosol scheme. As such, it is felt that this work is not suitable for publication in ACP until these issues are addressed. These concerns, together with more minor issues are discussed in detail below.

Major Comments

- A major concern with this study is the size of the model grid domain. The small grid domain has a number of serious potential effects associated with it including inflated cloud fractions and enhanced subsidence. Studies by Tompkins (2000), Stephens et al (2008) and others have shown the sensitivity of RCE simulations to grid setup, in particular 2D versus 3D. The organization of convection when run using a 2D domain, is significantly different than that in 3D. This is due to the fact that the corresponding subsidence (necessary for RCE) that develops in association with the convective cores in 2D requires a larger area than that in 3D (in 2D it occurs along a line between, whereas in 3D it occurs as a circle around the convective core). Under RCE, the small domain used by the authors will result in the associated subsidence being forced into artificially small regions, which will produce warmer, drier clear regions than normal, which will have a range of feedbacks on the aspects such as the surface fluxes, column temperatures, OLR etc. Other impacts such as entrainment will also be impacted. The small domain used here is also likely to artificially inflate cloud fractions, which appears to be the case here, with cloud fractions of 57% mentioned on line 20 on page 29108. This will have an impact on the AIE-induced radiative responses discussed here, potentially overinflating their importance. The authors do acknowledge in their conclusion that this is one potential caveat of their study, and that it will be investigated in the future, however, this reviewer feels that this is a significant caveat, and one which may lead to a significant misinterpretation of the AIEs on deep convection. The reviewer is certainly sensitive to the computational intensity of such simulations, however it is recommended that the authors perform several sensitivity tests to grid domain sizes for ONE of the control simulations. If such sensitivity tests demonstrate that the trends are relatively robust, then the results presented here would be more acceptable. However, without such tests (including the fact that RCE has not been demonstrated in the manuscript as described below), it is difficult to know whether the observed responses are simply a function of factors such as artificial cloud fractions, inflated subsidence and the simulations not being in a state of equilibrium.

We don't quite understand the reference to the differences between 2D and 3D simulations. All our simulations are 3D.
We disagree that the domain used in this study (128x128 km2) is too small. It could be the case if we had strong shear, and hence we would also need a big domain to represent a squall-line, for example. in our case, the sizes of the deep clouds and corresponding anvils are generally smaller than the domain size. To test the effect of the domain size, we conducted 50 day long RCE simulations over constant SST using the 128x128 km domain of this study and twice as large 256x256 km2 domain with the same 1-km grid spacing. Several vertical profiles averaged over the last 20 days of each simulation are shown. One can see that profiles of fields like the cloud fraction, cloud water/ice, updraft core fraction, precipitation flux are virtually identical. Certainly, a choice of some different microphysics package would generate much bigger differences between the simulations than the domain size. The low and middle level cloud fractions are also virtually identical, less than 0.1%. The biggest difference is in the high-cloud fraction, in the order of 1%. The difference in bulk radiation fluxes are about 1% as well. Therefore, we don't agree that the choice of the 128x128 km2 domain somehow "inflates" the cloud fraction, subsidence, etc.
We chose the 128x128 domain instead of 256x256 km2 domain because of the expense of running the RCE for 700 days. Each simulation took us about 1 month to complete, which includes the run time itself as well as waiting in the queue of the supercomputer. As the domain is relatively small, the parallel performance is greatly diminished. Running one additional 700-day simulation with the domain twice as large would take in our estimate about 3 months. Considering how little in terms of the RCE statistics the doubling of the domain size actually does, we do not feel that such a huge cost of performing a suggested sensitivity run is justified.



Pg 29104: " ..... it takes at least 700 days for the SST .... to get sufficiently close to the equilibrium as illustrated in Fig 1. ... The last 100 days of each run are used for sampling of the statistics." What does "sufficiently" close to equilibrium mean? Fig 1 does not convince this reviewer that the simulations are in equilibrium. Some admittedly very rough calculations using approximate values from the plot show that the temporal evolution of SST in the last 100 days to be quite similar to that of several of the 100 day periods before this one for most of the runs. Based on this plot, it would therefore seem that the simulations are not in equilibrium and hence radiative convective equilibrium is under question. Th authors need to demonstrate more convincingly that the simulations are in RCE, otherwise it is difficult to isolate AIEs from changes induced by the lack of RCE. The authors need to include plot(s) of fields similar to those shown previously by others such as Tompkins and Craig (1998), Grabowski (2006), Stephens et al (2008) and van den Heever et al (2011). Can the authors also please provide more accurate assessments of the change in SST for the last 100 days compared to the change in SST for the full 700 day period?

The RCE with interactive SST has two time scales, one is related to the adjustment of the atmosphere to the changes of SST (usually less than 30 days), and the other, much longer, to the adjustment of the whole system. Note that the above sited authors realized that it takes relatively short time to achieve the RCE with the fixed SST. For example, van den Heever at all (2011) ran their model for 60 days. Grabowski (2006) ran the model for 120 days, but reported that the equilibrium has been achieved after 40 days. Therefore, the atmosphere in our simulations, despite the slow drift of the SST, at any moment is in fact in RCE with the underlying SST as the change of SST over 30-day period is very small (less than 0.05K).

For the least equilibrated SST in the 50/cc run (we don't care so much about the 2xCO2 run as it is included only as the rough estimate of the doubling-CO2 effect on SST), using the exponential relaxation model with constant relaxation timescale, we found by fitting that model to the SST evolution curve shown in Fig 1 that it would take another 600-700 days to approach the estimated true-equilibrium temperature of 300.8 within 0.01K, that is another 0.25K. For the 1000/cc case, the estimate value is 298.3 instead of 298.5K, and it would also take another 400-500 days to reach. The other cases are closer to the initial 300K state and hence are better equilibrated. Note that should we run both limiting cases to the true equilibrium, the linear dependence of SST on the log(CCN) depicted in Fig. 2 would be even more apparent. The other statistics would also change somewhat, but the the overall conclusions of this idealized study would not change.

Answering the last question, change over the last 100 days of the least equilibrated 50/cc case is 0.05K, the 100 days before, 0.08K, the whole 700 day change is 0.55 K.

Thus, the system does approach the true equilibrium.

The model setup has no mean wind (pg 29104). The lack of vertical wind shear has significant implications for more organized convective systems in the tropics, which are highly dependent on such flow regimes. Organized convection plays a significant role in precipitation production in the tropics (e.g. Nesbitt et al 2006). Also, the lack of background vertical wind shear will influence the organization of convective anvils and hence high cloud fraction, which may have important implications for the radiative responses examined here. Can the authors please comment on the impacts of the exclusion of mean wind on the validity of their results given that their goal is to examine AIEs on deep tropical convection in the tropics?

In this study, we chose the simplest system for the RCE, that is when no ambient wind shear. We don't think that introduction of all the complications that exist in the actual tropics (equatorially trapped waves and associated super-clusters of convection, for example) would make the study more clear. For example, which shear does the reviewer mean? What magnitude? Which wind profiles to choose? Is it low-level shear or top-heavy shear? How about directional shear in addition to vertical shear. Etc., etc., etc.

We don't want to speculate on the effect of shear on our results without actually running the model. This is a very interesting topic indeed (the effect of aerosol on organization on mesoscale) and could be addressed in some future study. We can't cover all the possible topics in one study.

Pg 29103 lines 8-10: Does this imply that supersaturation is not allowed to exist within the model? Also, the aerosol treatment in the model appears to be relatively simple. Given the importance of this treatment to the topic, further details on the treatment of aerosols should be included or highlighted. For example, no aerosol sources of sinks exist, and thus processes such as wet deposition, which can be very important in deep convective regions, are not represented. Also, very clean conditions may be aerosol limited, and hence supersaturation will increase as limited aerosol amounts cannot efficiently deplete it. This has a number of feedbacks on cloud water, radiation etc that will not be captured here. Such points, potentially important to this study, should be discussed here.

All the details of the microphysics scheme can be in the referenced Morrison et al (2005) paper. We have not designed that microphysics, we simply adopted it. We just note that that double-moment microphysics has been used in the recent published studies by Grabowski and Morrison of the indirect aerosol effects.

Minor Comments

- **There are a number of minor grammatical errors that will presumably be addressed during the editorial phase.**
- **Pg 29102 line 9: "using prescribed from observations or". It is assumed that SST is missing in this sentence?**

Rephrased.

The introduction would be more effective if the stated goal included the fact that the authors are including the impacts of interactive SSTs on the system. This is not explicitly stated in the introduction.

Thanks for noticing that shortcoming.Tthe following phrase has been added to the last paragraph of the introduction: "In this study, we examine such an effect of changing aerosol on the SST predicted by a simple slab-ocean model."

Grabowski (2006) on AIEs in RCE should be referenced in the introduction.

Actually, it has been referenced.

Pg 29103 lines 19-23: Further details are required on the ocean model implemented here. Is it correct to state that the SST stays the same, but that surface fluxes vary based on the atmospheric conditions, and hence the oceanic heat content can vary? The description of this portion of the model could be better. Further more, the approach to keeping the equilibrium SST close to 300K described on page 29104 needs to be better described.

Slab-ocean model technique is rather standard and extensively been used in GCMs. We added some additional details: "The SST Ts can be specified or calculated using a simple slab-ocean model. In this model, the ocean mixed layer with prescribed depth h and heat capacity of water cw can change its heat content per unit area cw hTs through the surface radiation fluxes, enthalpy fluxes, and prescribed ocean-transport flux (so called q-flux)." Please note that if SST is prescribed, the heat content is not relevant; it simply implies that all the surface fluxes and the q-flux are in perfect balance. This also means that the surface imbalance of surface enthalpy and radiative fluxes equals the q-flux. We keep the equilibrium SST at 300K for the control run by choosing the insolation (by trial and error) such that the implied q-flux equals to zero. In equilibrium, the net surface flux and the top-of-atmosphere fluxes are equal.

Pg 29104: It is not clear why FA100 and IA100 are the control simulations representing typical clean maritime conditions when the cleanest CCN number concentrations being tested are 50/cc?

We assume that 100/cc is typical maritime; while 50/cc represents some "pristine" conditions, which is even cleaner than "typical".

Page 29104: The abbreviations "IA" and "FA" are introduced and then immediately the text switches to "ISST" and "FSST." While it is understood that A refers to the variable of interest, this could be made clearer.

IA and FA used in case names; while iSST and fSST (new names now) refer to the group of runs with interactive or fixed SSTs regardless of aerosol concentration.

Pg 29105 line 17: "as in some other studies of .... AIEs it is natural to ...." The appropriate references need to be supplied.

We added a reference to Platnick and Oreopoulos (2008)

Pg 29105: The result that double CCN has the same effect regardless of initial and final CCN concentration is interesting, and more discussion on why this is the case would be most useful. This finding is one of the more compelling results of the manuscript and deserves a more thorough discussion.

Frankly, we don't know for sure. It is the result of both microphysics and radiation nonlinear interactions. Unless a similar result is shown by using some other microphysics scheme, we cannot say how robust this result actually is.

Pg 29106-29108: Many of the changes in a number of the microphysical fields are very small, on the order of 1 or 2 %. Are these changes robust? This should be highlighted in the text.

we not sure which field the reviewer means. The changes in cloud water path and liquid effective radius (see the updated Fig. 5) that determine the optical thickness of clouds are quite robust.

Pg 29106: How are clear and cloudy columns defined?

The clear column radiation is diagnosed by the radiation code by forcing the cloud water/ice in the given column to be zero and computing the radiative transfer. It is done only for diagnostic purposes, specifically to be able to estimate the cloud radiative forcing.

Pg 29106 line 20: "... so that the effect of decreasing cloud fraction of anvils and corresponding IWP" The high cloud fraction differs by less than 2% across all of the simulations performed. Can the changes in the OLR really be attributed to such small changes in the cloud fraction of the anvil as stated here? Can the authors please comment?

In fSST case, the increase of OLR with increase of CCN by about 1.5% is consistent by reduction of the high-cloud cover by about the same amount. In iSST case, the overall decrease of OLR with increase of CCN is mostly due to cooling of the whole column and corresponding cooling of the cloud tops following the reduction of the SST.

Figure 3, panel d does not seem to have a value for 50 FSST.

Thanks for noticing that. Corrected.

Pg 29107 line 12: ".... Which would probably not change the results ...." Until such simulations are performed such statements are highly speculative and should probably be removed.

Removed.

Pg 29108 line 9-11: In the paper by Van den Heever et al (2011) cited by the authors, they appear to observe the invigoration effect of deep convection, and precipitation increases on the order of 6 or 7%. However, the overall shallow cloud precipitation decreases due to the greater subsidence produced by deep convection. This would seem to be a different mechanism from what is described here, or do the authors observe this process?

As we did not partition into the shallow and deep precipitation, for us it would be difficult to definitely say if the above sited mechanism also at work in our simulations. This is definitely something to look after in the future studies.

Pg 29108 line 24-25: As stated above the cloud fractions are very small. It would be useful of the authors could assess the statistical significance of this result.

The changes in cloud fraction for the fSST case (about 1.5-2%) is consistent with other changes. We added a plot of the liquid effective radius which clearly demonstrates the dominance of the first indirect effect in the overall indirect effect (decrease of the effective radius by about the factor of 2 while increasing the liquid water path only by 20%), therefore, the small change in cloud fraction, which constitutes the second indirect effect is not crucial for our argument.

Pg 29108 line 28: ".... are robust and qualitatively similar between ..." Have the authors assessed whether these differences are statistically significant?

Well, the changes in various water paths exceed 10-40%, and hence robust.

Pg 29109 line 21: Perhaps the figures are too small, but why aren't there any apparent vertical shifts in microphysical trends as SSTs (and consequently the whole atmosphere) warm? This has been briefly touched on here, but requires further discussion.

We agree that the increase of the frozen precipitation is not supported by the lower freezing level. We rephrased the passage as

"This is probably because the cooler troposphere temperature in iSST cases cause the local increase of cloud ice as the result of heterogeneous freezing and further increase of cloud ice due to the Bergeron-Findeisen process, which also contributes to the increased production of the frozen precipitation. "

Pg 29110 line 7: The statement appears to be initially confusing. Is the lower cloud ice path due to the fact that other species like snow are increasing? This should be made clearer.

One of the main sources of frozen precipitation in the model is the accretion of cloud ice; therefore, substantial increase of frozen precipitation should lead to decrease of cloud ice amount. We agree that the way the original text is a bit confusing. We edited it to be "This explains the monotonic increase of snow (Fig. 5d) and graupel (Fig. 5e) water paths and the corresponding decrease of the column cloud ice (Fig. 5b) as the result of accretion by the frozen precipitation. " Hopefully, it is clearer now.

Pg 29109 line 13-14: "In ISST cases, though, there is a considerable decrease of cloud water below 2 km, which could be explained by the effect of entrainment of dryer environment on the liquid water content at cooler SST." When the authors are discussing the response below 2km, are they referring to the cloud water below 2 km in those columns that are experiencing deep convection, are or they referring to shallow clouds only or both? This needs to be made clear. Given the grid spacing used here it is difficult to confidently speculate on such effects of entrainment. Can the authors please comment?

We refer to both. The reviewer is right in the notion that it is hard to use the word entrainment when 1-km grid spacing is used. In the model, at such a grid scales, the numerical and SGS diffusion is what actually happens. Therefore, it is more appropriate to use the term "mixing with the environment" rather than entrainment. We updated the text to reflect that notion: "...considerable decrease of cloud water below 2 km, which could be explained by mixing with the dryer environment, which tends to reduce the liquid water content at cooler SSTs. "

Pg 29109 line 29: A reduction in snow and graupel is noted in the ISST cases, and yet very little change in the high cloud fraction, thus the anvils are becoming thinner in these cases? Given the radiative implications, this point should probably be made.

Cloud fraction is computed for relatively small amounts of cloud ice as being the threshold. Those small cloud ice mixing ratios don't affect much the precipitation, but determine the cloud fraction. This is probably why the high cloud fraction is not that sensitive to the amount of precipitating ice.