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

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Journal: ACP

Year: 2013

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.

- 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. The 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 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?
- 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.

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?
- 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.
- Grabowski (2006) on AIEs in RCE should be referenced in the introduction.
- 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.
- 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?
- 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.
- Pg 29105 line 17: "as in some other studies of …. AIEs it is natural to …." The appropriate references need to be supplied.
- 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.
- 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.
- Pg 29106: How are clear and cloudy columns defined?
- 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?

- Figure 3, panel d does not seem to have a value for 50 FSST.
- 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.
- 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?
- 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.
- Pg 29108 line 28: "…. are robust and qualitatively similar between …" Have the authors assessed whether these differences are statistically significant?
- 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.
- 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.
- 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?
- 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.