We thank the reviewer for thoughtful and helpful comments that definitely made the manuscript better.

Anonymous Referee #1
Received and published: 20 November 2012

===============
Summary:
Several simulations of radiative convective equilibrium have been performed with vary- ing concentrations of cloud condensation nuclei (CCN) with both fixed and interactive sea surface temperatures (SSTs). Increased CCN concentrations lead to stronger cloud albedos and a weakening of the cloud greenhouse effect. In runs with an inter- active SST, this leads to cooling of the ocean surface with increasing CCN. Increasing CCN by 10 times offsets most warming experienced with a doubling of carbon dioxide (CO2) concentrations. While cloud changes are broadly similar in the fixed and interac- tive SST runs, the precipitation changes are quite different in both amount (decreasing precip with increasing CCN in interactive runs, little change with fixed SST) and in ver- tical structure (fixed SST runs have vertically coherent changes in precip amount, while interactive runs have opposite sign changes around the melting level from those aloft and near the surface).
=============== Recommendation: Publish subject to minor revisions after taking account of the comments offered here.

The paper is mostly descriptive, but is interesting in that it describes a set of novel experiments in a clean framework. While I have some minor quibbles with the setup (e.g., I would have preferred more realistic tropical insolation with a prescribed sink of energy at the base of the ocean mixed layer), this paper makes a nice contribution to the literature on aerosol indirect effects in deep convection. I especially like the no- tion of performing more cloud-resolving model simulations over a dynamic ocean, so that the surface energy budget is closed. This will gradually force improvements in the representation of cloud radiative effects because of their feedbacks on surface temper- ature in this framework. The paper could perhaps make a greater effort to distinguish between the roles of the first and second indirect effects in the cloud response, if pos- sible. Additional insight into the differing vertical structure of precipitation changes with increasing CCN between the fixed and interactive SST runs would also be useful, if this can be cleanly illustrated.
===============

Major comments (all page numbers are prefixed with 29):
104/5 (p. 22104, line 5): Why does the TOA imbalance need to be zero? The study is motivated by the exploration of tropical deep convection, but the tropics have a net influx of energy at TOA, which is balanced by heat export to the subtropics and extra- tropics through both the atmosphere and ocean. I would have preferred that the sim- ulations be performed with realistic insolation at TOA and that this energy export be modeled by an energy sink in the ocean (probably 50-80 W/m2) that allows the column to equilibrate. I'm not sure that this would change the results of this study, and it's prob- ably not realistic to expect a repeat of the considerable computational effort expended on this study. Still, in future work, it would be desirable to use a more tropics-like in- solation, as this would affect the SW heating due to absorption by clouds and water vapor in the tropics and possibly change the precipitation in the base case. Last, as noted in Hartmann's Global Physical Climatology (chapter one), it is desirable to use the zenith angle whose cosine is the equal to the insolation-weighted coszrs over the diurnal cycle.

Yes, we are well aware that the actual Tropics have a net export to the subtropics done by the atmosphere and ocean. The radiation imbalance at the top-of-atmosphere in regions of deep convection in Tropics changes in rather wide range, from 40 to 120 W/m2. We chose not to introduce some arbitrariness, but just offset the net export by reduction of the insolation. However, the concern by the reviewer is noted and may be applied in our future studies.

106/sec 3.2: I would be curious to see how the PDFs of SWCF and LWCF changed with increasing CCN. This could be computed either over single grid cells or over chunks of the domain. Could you use such an analysis to distinguish between the Twomey effect and cloud fraction/cloud lifetime changes? Similarly, could you add a line to figure 3a that estimates the changes in SWCF due to the Twomey effect alone? This might take work to do it faithfully, but even an estimate would be valuable in thinking about the relative roles of the first and second indirect effects here.

Unfortunately, no 2D snapshots of the SWCF and LWCF were saved to make such an analysis possible. All the statistics have been computed "on the fly" rather than post-processed. One of such statistics is the effective radius from the MODIS-simulator diagnostics package. The results suggest on the change in the optical depth is dominated by the liquid effective radius (factor of 2 change), while the effect of the cloud water path change, about 1.2, is relatively small. Thus, the aerosol indirect effect on solar radiation is dominated by the first (Twomey) effect. We added the plot for the liquid effective radius to Fig. 5. THe corresponding narrative was also added to the text of the manuscript.

109/8-15: Could profiles of cloud fraction and in-cloud liquid (or in-cloud condensate) concentrations help illuminate the issue of what role the second indirect effect is playing here? Do the number concentrations of cloud ice change much with CCN, or do the fixed ice nuclei concentrations constrain this? It might be nice to have figure 6 be a three column figure with the left column showing the profiles of QC, etc. from the FA100, IA100 and 2CO2 runs and the other two columns as before.

See the response to the previous comment. We don;t think that the mean profiles of cloud water can be more useful in understanding the indirect effect than the difference profiles in Fig. 6.

110/1-18: The early part of the paper does a good job talking about how the atmosphere-integrated radiative cooling constrains precipitation. Some of that discus- sion should enter into this analysis as well. Second, the differing vertical structure of the precipitation changes here (fSST vs. iSST) is interesting, I think, and could be ex- plored more. Are these driven by changes in stability, decreasing tropopause height, changes in RH profiles, changes in convective mass flux profiles? If there is some coherent explanation of these different vertical structures, it might add to the paper.

We don't see how the profiles of the relative humidity and convective mass flux could explain the changes in precipitation. We think that the discussion of Fig 7 is sufficient and hope that the reviewer agrees.

Last the changes in the vertical profiles of radiative heating aren't shown in the paper. Is there any information in these profiles that informs the response seen in these sim- ulations? They don't need to be included unless they illuminate some issue, but they could hold an explanation for some of the behavior seen here.

The main effect considered in the paper is the first indirect effect due to change in the liquid effective radius. We added the profile of the Reff to Fig. 5. We don't see how the profiles of radiative heating could help. The critical to our study is the surface radiative flux convergence.

=============== Minor recommendations (all page numbers are prefixed with 29):
100/14 (p. 22100, line 14), suggested rewording: "... agree on the tendency _of in- creased aerosol concentrations_ to make the shortwave ..."

done

101/2: "... _Earth's_ radiative budget."

corrected

102/3: "In RCE, ..." – remove the.

done

102/9: ?? "... using _forcings_ prescribed from observations ..."

paraphrased as " using the see-surface temperature (SST), which was prescribed from observations or simply fixed at some value"

103: It should be noted that no change is made to ice nuclei concentration. Others (e.g., the new ACPD discussion paper of Seigel et al, 2012, doi:10.5194/acpd-12-29607-2012) consider the effects of changes to ice nuclei concentrations.

It is implied. However, we added the explicit statement to the case setup.

104/20: Since SST is itself an acronym, why not use lowercase "f" and "i" to modify it, as in "iSST", "fSST"?

good idea. implemented throughout the manuscript.

105/20: Is susceptibility always defined relative to log10? I would suggest using a subscript "10" to make clear that increases by a factor of 10 are your reference.

Not always. that's why I gave formula as my definition of it. To avoid confusion with natural logarithms, I added explicit subscript 10 in (1) to stress that it is the decimal logarithm.

105/27: "_associated_ with the doubling of CO2."

corrected

107/7-14: In the discussion of the interactive SST runs, a citation to Romps (2011, JAS, http://journals.ametsoc.org/doi/pdf/10.1175/2010JAS3542.1) would be in order. He looked at precipitation changes in small-domain CRM runs with increasing CO2 over an interactive SST. He "accelerated" convergence of the SST to equilibrium by making occaisional abrupt changes to the SST to move it towards an equilibrium value. See the appendix A to his paper. His paper may also have broader relevance to this study, as it looked at precipitation responses to changing CO2.

108/11: I believe that Grabowski and Morrison (2011, JClim, http://dx.doi.org/10.1175/2010JCLI3647.1) talk a bit about the contrast between single-cloud and ensemble thinking about aerosol indirect effects, that is similar to the short timescale-longer timescale contrast here.

110/4-5: "The _conversion_ rates for the moments in _the_ two-moment bulk micro- physics..."

110/17: "iSSP" –> "iSST"

corrected

121/fig 1: I would suggest varying either line thickness or line type to make it easier to decode this plot, e.g., solid for 100,500,2CO2 and dashed for 50,200,1000. Alternately, one group could have double the line thickness of the other.

Following the suggestion by the reviewer, Fig 1 was replotted using different colors and using the alternating solid and dash lines.