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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Atmospheric energy budget response to idealized aerosol perturbation in tropical cloud systems" *by* Guy Dagan et al.

Anonymous Referee #2

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Review of "Atmospheric energy budget response to idealized aerosol perturbation in tropical cloud systems" by Dagan et al.

This paper studies the differences in the radiative and energy budgets in a tropical environment when the cloud droplet number concentration (CDNC) is changed (as a proxy for changes to the environmental aerosol concentration). The simulated periods are two separate 2-day within the same week where the convection is either predominantly shallow or predominantly deep - each case is simulated with CDNC values. The authors find substantial changes in amount of energy absorbed by the atmosphere by changing the CDNC and find substantial differences in the response between the two sets of simulations.

The main component of the paper is a breakdown of the energy budget into radiative,

sensible heating and heating through precipitation formation. The radiative component is later broken down into shortwave and longwave fluxes at both the surface and top of the atmosphere. The differences are also quantified in the time evolution of near-surface temperature, precipitation, cloud fraction and in-cloud water contents.

Overall, I find the study to be well formulated with a clear motivation and simple but successful strategy for breaking apart the components contributing to the changes in the atmospheric energy budget. There are no substantial shortcomings that should prevent the publication of this study; however, I have a few suggestions that could improve this contribution which are explained below.

Primarily my suggestions are aimed to help the authors achieve their stated aim of better understanding the physical processes behind aerosol effects on the atmospheric energy budget. I see that their study does indeed achieve this, at least partly, but that these results are not clearly expressed in the abstract nor the conclusions. Throughout the paper, the authors do a good job of describing the differences between their simulations and quantifying these differences (although in parts the quantification could be improved) - however, it is mostly left to the reader to put these pieces of information together to get an understanding of the physical processes involved. As a result, my overall impression of the authors conclusions and abstract are: "we found another case where aerosol-cloud interactions behave differently under different environmental conditions," which could be relatively simply converted to "these processes (*see below) contribute to the different energy budget changes for shallow and deep convection when CDNC is changed"

(1) From my understanding of the presented results, it seems that the large difference between the shallow case and the deep case is the potential for a large upper-level cloud fraction change in the deep case. I understand this as an increase of the anvil area, resulting in reduced LW emission from the surface/lower atmosphere and therefore a warming contribution of the larger anvil. In the shallow case, the upper level cloud fraction also has a systematic change, but because it occupies a smaller part of

the model domain - the overall change in the energy budget is controlled by the change in low cloud fraction and the Twomey effect. If the authors agree with this, I suggest adding a paragraph into the conclusions and a sentence in the abstract clarifying these physical changes in the model and their impact on the energy budget.

(2) Breakdown of the vertical mass flux changes with CDNC into component parts The vertical mass flux of water is shown to change between the simulations with different CDCN. What is the cause for this change? Either the vertical velocity should be increasing (which seems not to be the case from the vertical velocity distributions in Figures 11 and 19), so either the updraft area is increasing [implying wider updrafts?] or the in-cloud water mass in increasing [because of a less efficient precipitation-forming processes?]. To what extent are these two factors important? Furthermore, what happens to the vertical mass flux at (e.g.) 800 hPa - where the total water content is quite similar between all CDNC concentrations - is there still an increased vertical mass flux?

(3) Large contribution from residuals A large contribution to the overall energy budget is within the residual term, which the authors state should reduce to zero given a long enough averaging time. How can the authors be sure that this is true and that the large component in the residual term is not a "buffering" effect - e.g. changing stability of the atmosphere to compensate for the changing energy budget? Can the 3D distribution of the residual values be used to quantify this at all?

(4) There appears to be a mismatch between TWP in Figure 8 (lower right plot) and qt in Figure 9 (lower central plot). Similarly in Figures 16 & 17. The vertical profile of qt is quite similar for 3 simulations in the shallow case (Figure 9; excluding the 500 cm-3 line). Similarly, the qt values from 3 simulations in the deep case are also similar (Figure 17; excluding the 20 cm-3 line). However, in figure 8 & 16 these is clear separation between all the TWP lines throughout the simulation. By quick calculation the spread in the TWP timeseries seems too large to be explained by the differences in qt (which are mostly between 650-400 hPa). How can this difference be explained? Is the TWP only including cloud and ice, but ignoring rain water? Similarly, is the LWP

only cloud, ignoring rain?

(5) Following from the above point: is rain water radiatively interactive in the model? If not, to what extent does this removal of mass from the radiatively interactive cloud species have on the Twomey effect calculations performed, given that the rain water mass is almost equal to the cloud water mass at some heights?

(6) Impact of simplifications The approach of simply modifying the CDNC instead of the aerosol concentration of the atmosphere ignores several potentially important processes/feedbacks (e.g. activation of CCN/IN, size distribution of aerosol, direct radiative effects) - the authors should comment on these shortcomings in the conclusions.

(7) Robustness of results The authors should comment on the robustness of these results, in light of the fact that single simulations (rather than ensembles) of two individual case studies are performed. The results in figures 8 & 16 suggest a clear separation between all 4 CDNC concentrations from early on in the simulation - however, the vertical profiles of qi, qt and CF in figures 9 & 17 suggest that the 20 CDNC cm-3 simulation is the only one of the four that is substantially different (particularly at upper levels, which seem to be most important in this story).

Minor points: please show the mass flux from all four simulations in figures 11 and 19 to be consistent with the other plots in the paper.

Lines 335-338: please be more quantitative about the results of the test with the offline radiation calculations as to the relative contributions of the cloud fraction and TWP changes. Line 367: please quantify the "vast majority" of LW flux changes due to cloudy rather than clear skies.

The plots in figures 10 and 18, currently described in the caption as Hovmöller plots, would be better described as time-height plots.

Is there an explanation for the relative minimum of cloud water content at 650 hPa in all simulations? I struggle to find a physical explanation for this.

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