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Atmospheric
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Discussions

Interactive comment on “Factors determining the effect of aerosols on cloud mass and the dependence of these factors on liquid-water path” *by* S. S. Lee and J. E. Penner

Anonymous Referee #1

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Review of “Factors determining the effect of aerosols on cloud mass and the dependence of these factors on liquid-water path”, by Lee and Penner

Overview: This paper presents results from a large eddy model showing that, in thin (low LWP) clouds, aerosol-driven differences in condensation/evaporation may have significant impacts upon the cloud radiative properties. In thin clouds these effects may exceed those associated with precipitation suppression (the so-called second aerosol indirect effect). The results are interesting, and complement earlier work (Arnason and Greenfield 1972, Kogan and Martin 1994, Kogan et al. 1995, Wang et al. 2003, Grabowski and Morrison 2006) suggesting important effects of cloud microphysics (and

hence aerosols) on cloud dynamics through the finite supersaturation relaxation time and delayed condensation/evaporation, effects that are independent of aerosol effects on sedimentation/precipitation. Some of this important earlier work is not cited which makes me wonder whether the authors are aware of it. The authors find that in thin clouds, in which precipitation effects are small, increased aerosols may lead to stronger updrafts since condensation and evaporation rates are higher. In some of their model cases this leads to thicker clouds, and in others the opposite is true. This is not entirely surprising since it is likely that the way in which the cloud thickness is changed occurs through changes in entrainment rate, and whether increased entrainment thickens or thins clouds depends upon the environmental conditions and timescale as explored by Randall (1984), Ackerman et al. (2004) and Wood (2007).

I believe that the authors have a result that is very tantalizing since, to my knowledge, no previous study has specifically considered the aerosol indirect effect associated with microphysically-limited condensation. The findings of this work are certainly worthy of publication in Atmospheric Chemistry and Physics. However, the manuscript could be much better: the approach is not particularly well-conceived, the manuscript is sloppy in places, and does not do due justice to previous important work on this subject. For example, statements like “....the role of feedbacks between microphysics and dynamics become more important with the lowering level of LWP” (abstract) give the impression that somehow precipitation-driven aerosol effects do not involve feedbacks between microphysics and dynamics. This is totally untrue as numerous previous studies involving the effects of drizzle have shown. The authors also do not acknowledge the potential and previously-explored problems of supersaturation prediction in cloud resolving models (e.g. Stevens et al. 1996, Grabowski and Morrison, 2006, and see discussion in Reisner and Jeffery 2009) that one might imagine could lead to spurious estimates of microphysics on condensation rate.

I recommend that the authors revise their manuscript to reflect the previous work and to better explore the physical processes driving their liquid water path responses.

Major points:

1) I wouldn't call increasing latent heat flux by a factor of five a "minimized difference in environmental conditions" (P19319, line 5), since LHF increases not only the LWP but also the buoyant driving of the boundary layer. What are the authors trying to show here? If they wish to show that at low LWP, the precipitation is less important, then this could be done by more indirect means such as moistening the free-troposphere and allowing the entrainment of moister air to drive thicker clouds. An alternative would be simply to run with and without precipitation.

2) Condensation rate differences between high and low aerosol runs may be influenced by aerosol-driven precipitation differences because suppressing precipitation drives stronger updrafts (e.g. Stevens et al. 1998, Ackerman et al. 2004, Wood 2007). The paper be far more convincing if these effects were more clearly separated, e.g. by running cases (a) without precipitation; (b) by fixing supersaturation = 0 for condensation purposes; (c) including both effects.

3) Introduction, P19315, line 25, and numerous other places. It is not just surface precipitation that is important for AIEs. Surface precipitation affects the BL moisture budget, but cloud base precipitation affects the cloud dynamics and entrainment (e.g. Ackerman et al. 2004, Wood 2007). Thus, an assessment of whether precipitation is relevant to the AIE problem requires knowledge of the cloud base precipitation.

4) Fig 7: Passive broad-channel radiometers such as MODIS cannot accurately measure temperature and moisture profiles in the boundary layer. No instrument can currently do this with the accuracy required to evaluate the model in question. If this accuracy could be attained with MODIS, we wouldn't need radiosondes, AIRS, COSMIC, and other profiling satellite sensors. The agreement with observations must be almost entirely fortuitous.

5) Sloppiness: What time of day are the MODIS LWP observations made for the comparison with the model? Line 10, P19321 is vague.

Also, in the sentence: "...the small contribution of autoconversion and accretion to LWC implies that the role of sedimentation of cloud particles in the determination of LWC is not as significant as that of condensation." Cloud particles do not require autoconversion and accretion to sediment. I think the authors mean the sedimentation of cloud+drizzle particles. Also, although condensation is the only source of LWC in clouds, it is not the determining factor for LWP since in steady-state non-precipitating clouds the evaporation rate has to balance the condensation rate. Any precipitation is the imbalance between the condensation and evaporation. Since we know that precipitation can result in either more or less liquid water path (Ackerman et al. 2004, Wood 2007, see also satellite study by Coakley and Walsh 2002), this proves that the use of a liquid water budget does not help in determining the key terms affecting the liquid water path. This is the heart of the Albrecht argument, i.e. that a greater sink of LWC should lead to thinner clouds. It doesn't work.

P19326, line 26. Why do increased condensation rates necessarily lead to increased LWP? Stronger condensation rates should lead to greater LWC in the updrafts, but this also drives stronger entrainment which evaporates LWC and reduces the LWC in the downdrafts. Essentially then, in marine stratocumulus, the LWP is not determined only by the rate of condensation, but by the rate at which the MBL is moistened, warmed, and deepened by the entrainment and surface fluxes.

P19327, line 9: Precipitation stabilizes the boundary layer as a whole since it warms the cloud layer and cools the subcloud layer.

6) The authors make a good point that parameterizations should include microphysical limitations to condensation as well as to precipitation. Wang et al. (2003) offers some ideas as to how this might be done in a bulk model.

Minor points:

1) P19316, line 2: What "critical size" are you referring to? The sedimentation of cloud mass is controlled by the mass-times-terminal velocity weighted radius. This will be

similar to the volume radius in clouds with relatively narrow size distributions.

2) Section 3: Why not show CCN-supersaturation spectra used? Why are they changing with time? Are the aerosols interactive?

3) Fig. 10. Why not show condensation rates for all cases? Fig 10 shows the condensation rate in the anomalous case where PI condensation is higher than PD. Why is this case different? I notice that the condensation rate is higher in PD earlier, but then it flips sign. The reason for this, I suspect, is that entrainment changes are a significant sink of buoyant motion that are not isolated in this analysis.

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