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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The response of precipitation to aerosol through riming and melting in deep convective clouds" *by* Z. Cui et al.

Anonymous Referee #1

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The paper uses a 2D axisymmetric model to simulate the effects of varying concentrations on the development of precipitation in three continental clouds from three locations, having three different temperature and humidity profiles and one maritime cloud with background meteorological conditions from the central pacific.

Their simulations show that the precipitation responses differently to input aerosols in continental and maritime clouds. They show that precipitation from continental clouds reach maximum values when the aerosol input is between 180 and 430 cm-3 depending on the atmospheric conditions. In contrast, precipitation from maritime clouds increases with increasing aerosol (CCN) concentrations.

They found that in continental clouds the cloud drops are rapidly depleted by riming shortly after the start of precipitation and that precipitation is dominated by the melting

of ice particles. The peak in precipitation is explained by the changes in riming efficiency. When the concentration of cloud drops is small, the size of the drops is larger and the riming efficiency is high. When the drop concentration is high and the available water vapor is lower (as in continental clouds) the drop size is small and the riming efficiency is low, leading to slower growth of the ice and graupel particles and to reduced precipitation. In maritime clouds the response was different with precipitation increasing with increase in aerosol loading. The reason is that the lower cloud bases in these clouds make the warm process responsible for the rain development. The transition from warm to mixed phase does not reduce the riming because the drops are larger.

I found their comment on the top of Page 29019 in which they state: ". . . our simulations suggest that the peak precipitation intensity is more sensitive to thermodynamics than aerosols" to be a very important one. This is especially true in light of the uncertainty of the effects of aerosols on precipitation. I think that their results show that the aerosols affect the timing of rain initiation and maybe the division between the contribution of the cold and warm precipitation processes, but that the total precipitation is more a function of the atmospheric temperature and humidity profiles. I would like to see this important point expanded in the paper.

I found a few major problems that the authors should address in their paper:

1) The model is axisymmetric and thus cannot account for the effects of wind shear on the development of precipitation. 2D models with shear show that many of the small ice crystals at the upper parts of the clouds are blown away to produce anvils, thus reducing precipitation. In continental clouds this could be a major factor since riming is slower and the ice crystals are smaller. In maritime clouds, where the number of cloud drops is small, the ice crystals that are formed have only very few drops to rime with, thus the ones that do not grow by riming will also remain small and be blown away by the horizontal winds at the upper parts of the clouds.

2) The major conclusions of this paper depend on the rate of growth of ice. Namely

the number and size of the ice crystals make a big difference in the development of precipitation. However, the paper uses the parameterization of Meyers et al to simulate the development of the ice. This parameterization has been shown to overestimate the ice formation. Although good parameterization of ice formation in clouds is one of the major deficiencies of our understanding clouds and precipitation, I expected the authors to address this point at the beginning and not leave it to the end of the concluding remarks.

3) I think that it will be valuable to estimate the effects of modifying the parameterization of ice formation even by artificially enhancing and decreasing ice formation by a certain factor. Another option is to use another published parameterization of ice nucleation (e.g. recent publication by DeMott which suggests a dependence on total aerosol concentration > 0.5 microns) which leads to lower concentrations of ice crystals. Such evaluation could reveal the sensitivity of the conclusions to the rate of formation of ice.

4) Page 29009 – the reference to Levin and Cotton should be: Springer press, 382 pp, 2009 and not the WMO report.

5) Page 29013 – second paragraph – although sulfate formation has been shown to be small, it is important to note that particles coated with sulfate have been measured. It is certainly conceivable that some ice nuclei could be coated thus modifying ice nucleation. Furthermore, some particles that are insoluble and are inefficient CCN could become effective as GCCN. These points should at least be discussed.

6) Section 4.1 – why is the range of CCN in the maritime clouds larger than in the continental ones?

7) Although ice multiplication and condensation-freezing is included in the model, why is there no discussion of the contribution of these processes to the ice formation and the precipitation development? This could certainly be important in maritime clouds.

8) Page 29018 – The contribution of graupel particles in the development of precipita-

tion is an important point that has been discussed in other publications (e.g. Teller and Levin, ACP, 2006; Yin et al, Atmos. Res., 2000) and should be referred to here.

Minor point:

Caption of Table 3: Should be maxima

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