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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Microphysical processing of aerosol particles in orographic clouds" *by* S. Pousse-Nottelmann et al.

C. Knote (Referee)

christoph.knote@lmu.de

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** Synopsis **

The authors describe a parameterization of the processing of ambient aerosol by clouds which they implemented into the mesoscale numerical weather prediction model COSMO. The authors investigate the effects of cloud passage on the development of subsequent clouds downstream by conducting 2-D idealized simulations of the flow over two hills with orographic clouds developing over both. Simulations were made with warm as well as mixed-phase clouds, and employing an existing parameterization of aerosol-cloud interactions (but without aerosol processing). The authors find strong effects on the aerosol population when including aerosol processing into their simula-

tion, ranging from a vertical redistribution due to evaporating precipitation as well as a change in the size distribution due to cloud processing.

** General remarks **

The manuscript is well written, the methodology used is state-of-the-art and explained carefully, findings and conclusions are presented in a concise manner. The topic of the manuscript fits in the scope of Atmospheric Chemistry and Physics. A number of very interesting findings are presented and conclusions are drawn that will be useful to the aerosol community. I have a few concerns listed below which I would like to see addressed, but otherwise recommend publication in ACP.

** Major comments **

1)The authors use the modal approach to approximate the aerosol size distribution. A process acting on the size distribution can change (at most) three parameters: the overall number or mass, the median diameter and the width or shape of the distribution function (the standard deviation). It is well known that the typically observed aerosol size distribution, that is, the result of all processes acting upon the aerosol population, can be well approximated using a superposition of lognormal distribution functions. I doubt, though, that this translates directly into the validity of this approximation when describing single processes acting upon it. Let me give two simple examples:

* activation to cloud droplets: how can one activate all particles above 35 nm (but none below) into cloud droplets, which implies removing this mass/number from the interstitial size distribution, but end up with a function that is still a lognormal?

* the efficiency of below-cloud scavenging by rain (impaction, interception) is a strong, non-linear function of the size of the particle. How does one fold a lognormal with a size-dependent function of scavenging efficiency and end up with a lognormal again?

I am well aware of the fact that this is a criticism of the modal approach in general and it is not the point made in this manuscript to address this issue. However, the

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processes involved in cloud processing of aerosols are prime examples of processes that, in my point of view, cannot be accurately described using a modal description of the aerosol size distribution. As investigating the effects of those processes is the aim of the authors, I am left wondering what trust we can have in the results of an investigation of these intricate details of multi-phase interactions between clouds and aerosols when the aerosol description itself is approximated in a way that is unable to reflect the processes of interest. This should at least be discussed in the methods section.

2) The authors present a modeling study and do not compare their results against measurements. The underlying parts of the modeling system have been evaluated in a multitude of ways, but the processes added are based on a number of assumptions which bear considerable uncertainty. Hence, I deem it imperative to identify key parameters of uncertainty and conduct sensitivity studies to understand the uncertainty in the final result. As the authors already suggest in the conclusions that the next step will be realistic 3-D studies, this manuscript is somewhat of a "last chance" (and a low hanging fruit as well!) to derive uncertainties on the process level using a model setup that is simple enough so that clear connections between cause and effect can be made. Some examples: how does a wetter/dryer atmosphere affect these processes? What about very clean vs. very polluted conditions? How does overall aerosol solubility affect cloud development? As far as I understand from Zubler et al. (2011) the standard deviations of the modes are fixed, how does this affect the result? The authors stress the importance of precipitation on aerosol processing, but the warm cloud case is non-precipitating. I suggest adding a precipitating warm-cloud simulation.

3) It is unclear from the manuscript how the transport of aerosol mass incorporated in strongly sedimenting hydrometeors (rain, graupel, ...) is treated numerically. During one time step in the model (10 s), a rain drop falling with 5-10 m/s will fall through several layers in the model. Hence at least in the operational cloud microphysics scheme in COSMO (not the 2-moment scheme used here) a special semi-implicit scheme is

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used to describe the sedimentation of precipitation. How is sedimentation of hydrometeors treated in the model, and do the authors apply the same method for the in-rain / in-graupel aerosol mass (e.g. as we did in Knote and Brunner, 2013)? The authors showed that there are intricate connections between the locations where the different processes take place. If the in-rain aerosol mass does not fall in the same way the rain drops do - e.g. because the in-rain sedimentation is diagnosed from the precipitation flux - the location at which it is released upon evaporation, for example, is wrong. As the authors present vertical redistribution of aerosol mass as one of the major findings, the underlying methods need to be described in more detail.

** Minor comments **

2412, 10: is the standard deviation fixed in this implementation of M7 or is it a prognostic variable?

2412, 11ff: the aerosol module does not include the major inorganic ions nitrate (NO3-) and ammonium (NH4+), neither does it consider thermodynamic equilibration of these semi-volatile compounds and organics (OC) with the gas-phase. NO3-, NH4+ and OC together typically constitute the majority of observed aerosol mass. The authors should at least briefly mention these omissions and discuss possible influences on the results.

2415, 4-5: this sentence reads as if the authors would know its bad but keep it for the sake of consistency. If this is the intention, it would be helpful to hint to the reader what the problem is so he/she can understand possible effects on the results. Otherwise please rephrase.

2415, 11-12: see major comment - how can one cut off the tail of a lognormal and still keep a lognormal?

2416, 26: unclear formulation: how is the scavenging by rain happening locally in a grid cell influenced by the amount of rain reaching the ground if precipitation (and hence the incorporated particles) are prognostic quantities? Please explain more thoroughly.

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2416, 28ff: the reasoning that the authors omit graupel "because of the large sedimentation velocity" is not convincing as rain has even higher sedimentation velocities. Please explain and rephrase in the manuscript.

2420, 19-21: that is a good start - regarding the sensitivity studies mentioned in the major comments it might be interesting not only to vary the overall level of "pollution", but also consider more realistic vertical profiles (e.g. how does a dirty boundary layer influence cloud development in cleaner upper levels vs. equally dirty upper levels).

2424, 24-26: why do the authors need to set standard deviations, are they not given by M7?

Fig 17: the isolated peak of high cloud droplet number concentrations in a) (around 750 km distance / 3500m height) seems odd - I guess this is due to the overall low liquid water content in the cold case?

** References used **

Knote,C. and Brunner, D.: An advanced scheme for wet scavenging and liquid-phase chemistry in a regional online-coupled chemistry transport model, Atmos. Chem. Phys., 13, 1177-1192, doi:10.5194/acp-13-1177-2013, 2013.

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