Reply to reviewer 1

We thanks the reviewer for the positive and encouraging review and the comments that helped us to improve the manuscript.

Major comments:

Reviewer: *The authors should include a bit more description of their model setup (e.g. vertical resolution) and parametrisations. How is the condensation of water vapour treated? Is there a sub-grid scale distribution for water vapour and/or vertical velocity at cloud base? With a horizontal resolution of about 3 km, some cloud processes are explicitly resolved and some are not. The authors should provide more details as to which pro- cesses are considered to be resolved and which are not. For instance does the last sentence in Section 2.1 refer to deep convective precipitation only or to deep convection altogether?*

Reply: We will slightly expand the description of the COSMO-DE model configuration and setup in the revised version. For cloud cover and boundary layer clouds there is a treatment of sub-grid condensation in the model, but for the precipitation processes only the grid box mean water mixing ratios are used. Condensation is, for grid-scale clouds, parameterized by saturation adjustment. The model setup is such that deep convection is only happening on the grid-scale, and all precipitation processes are grid-scale. The sub-grid shallow convection, which is restricted to a cloud depth below 300 hPa, can be seen as a pure mixing processes. The consequences and problems of such a model setup are discussed in detail in Baldauf et al. (2011) and the references therein. Although this class of models, which are sometimes called convection-permitting, have some issues, e.g. in the representation and especially the triggering of deep convection, they are, in our opinion, very useful for aerosol-cloud-precpitation studies simply because the grid-scale microphysics is the only parameterization responsible for precipitation formation. In that way they are definitely superior to models with a cumulus parameterization of deep convection.

Reviewer: *In that respect it could be useful to perform a full water budget.... the authors could rule out any buffering effect from surface-atmosphere couplings.*

Reply: We agree that a water budget analysis and an investigation of soil moisture feedbacks would be very interesting to do. But this is something which fits best into the framework of regional climate modeling, i.e. long-term simulations forced with one consistent set of boundary conditions.

For example in the COSMO regional climate community (CLM) such studies are planned or already under way.

Reviewer: *Can the authors specify how they have sampled their experiments? For instance are the precipitation rates in Figure 5-6 and Table 4 an average from different hindcasts with different lead times or sampled at a fixed lead time? (Sorry if I missed where this information is provided in the manuscript). I think that overall the authors could make a better use of their multiple experiments and do more analysis of the impact of spin up / lead time on their results.*

The sampling of the accumulated precipitation is specified in the caption of Figure 5 as 'Monthly mean precipitation amount [...] combined from 6-18 h hindcasts initialized at 00:00 and 12:00 UTC'. In the revised version we will mention this important fact more prominently in the main text.

The different forecasts with different lead times do have different biases and behavior. This is due to the properties and biases of the observations which are used in the data assimilation system. Therefore the different behavior of the different lead times is a very complicated problem which has little to do with aerosol effects. See, e.g., Crewell et al. (2008, Met. Zeit, 17, No. 6, 849-866, esp. their Figs. 8 and 10) for some results on or validation of forecasts with different lead times.

Reviewer: *Referring again to table 4 and figure 5-6-9, there is a question of how big a change in precipitation needs to be of significance...*

We thank the reviewer for this interesting and valuable comment. Yes, in the sense that we cannot rule out slow feedbacks on larger scales our results are probably not fully conclusive. But as it is pointed out frequently in the paper, our focus is on potential aerosol effects on regional scales to test some hypotheses which have been formulated in recent years and especially to provide some quantitative statistical evaluation of buffering effects on the mesoscale, i.e. fast feedbacks in the terminology of the reviewer. We did not aim to investigate slow feedbacks, otherwise we would have chosen a different model or at least a different model setup.

Minor comments:

There is some mix-up of figures 7 and 10.

This occured during the technical editing and slipped thru the proof reading. Sorry for that mix-up. The caption were okay, but plots were wrong.

p 20204, l 22: replace cloud-dynamics with cloud dynamical C8329
Done.

p 20207, l 3: 12004 x 1300 km2 is presumably 1200 x 1300 km2 p 20207, l 23: rations should be ratios
Yes. Done.

p 20207, l 27: I think does simulate is more appropriate than can simulate. After all we are not quite sure whether it should or not simulate a strong response.
Agreed, and done.

p 20208, l 27: if a log-normal size distribution is used, is a measure of the dispersion of the size distribution but it is not strictly speaking a standard deviation. I suspect is the geometric standard deviation.
We agree to the term 'geometric standard deviation' although there are different formulations for the same expression.

p 20209, l 1-3: I found this couple of sentences not very clear. Can the authors clarify?
We have changed this to: 'In contrast to the initial assumptions of the Segal and Khain (2006) scheme, CN are often not completely soluble, this fact is accounted for by a correction factor affecting the number of nucleated droplets. This empirical parameter ϵ is estimated to 0.9 for low and to 0.7 for very high CN number densities. For the vertical profile of CN we assume a constant number density, N_{CN}, up to 2 km and an exponential decrease above that height.'

p 20210, l 15: replace on set of hindcasts with a set of hindcasts
Done.

p 20210, l 8: can the authors specify where the total number of simulations come from? 3 years $\times$ 7 experimental setups $\times$ 92 days $\times$ 2 simulations per day?
Yes, exactly, $3 \times 7 \times 92 \times 2 = 3864$. We did in fact a similar set of simulations for 2007, but this was a slightly older model version and was therefore not included in this study.

p 20213, l 5-10, p 20214, l 7: I think the authors refer to Figure 7 here (the plots, not the caption).
Yes, there was a mix-up with Figs. 7 and 10.

p 20216, l 9: Figure 7 should be figure 10 here.
The plots were wrong (mixed up), the text and the captions are actually correct.

p 20221: Can the authors homogeneize the format of their reference list? Sometimes the article titles are capitalised, sometimes not.
Yes, we can. We will change this after clarification with technical editing of GMD.

p 20228, table 2: if i and s are variances of fall speed, then their units should be m2s2 rather than ms1.
Changed to standard deviations. The underlying assumption is a Gaussian distribution.

p 20230, table 4: what is the unit here? mm/day or mm integrated over the full simulation period?
Those values are monthly mean accumulations, and the unit is simply mm. We could write mm/month, but then it would no longer be an accumulation, but a precipitation rate and we would have to change the caption accordingly.

p 20232, figure 2: the unit is unusual. Could the authors use m3 or cm3? The concentrations are pretty low, is this correct?
This unit is not so unusual. IN concentrations or particle number densities are, in older publications and even some newer papers, often given in 1/liter (e.g. Fig. 9-22, p. 316 of Pruppacher and Klett 1997 or Fig. 10 of PDA09) which corresponds to dm^{-3} in SI units. We have at least made the attempt to use SI-units, dm^{-3}, instead of liter which is not an official SI unit. According to the definition of SI-units the prefix 'd' for 'deci' or 10^{-1} is allowed.
IN particle number densities of 1-10 dm^{-3} are quite typical as it is also shown in the above-mentioned figures.

p 20233, figure 3: why is there no dashed red line on the left plot?
The solid and the dashed red lines fall onto each other, because the rain water path in pretty much the same. Zooming into the PDF acutally shows a bit of a dash pattern on the lower side of the solid red line. This is not a blurry PDF, but the effect of the dashed line.