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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Aerosol-cloud-precipitation effects over Germany as simulated by a convective-scale numerical weather prediction model" *by* A. Seifert et al.

**Anonymous Referee #1**

Received and published: 30 August 2011

The manuscript by Seifert et al. presents some results from a set of short atmospheric simulations over Germany using an NWP convective-scale model. While the simulated cloud properties are sensitive to assumptions made on the concentrations of CCN and IN, there is less sensitivity for the (domain-averaged) surface precipitation. There is some sensitivity however for orographic precipitation with some compensating effects downwind. These are important new results which strengthen our understanding of aerosol-cloud-precipitation interactions both from an NWP and a climate perspective. Overall this is a well written and good paper and as a result and I certainly recommend publication in Atmospheric Chemistry and Physics of a revised version. There are however a number of small issues that need to be addressed. I also make suggestions



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Interactive Discussion

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for some more analysis.

**Major comments**

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 processes 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?

It is interesting to see that there is even less sensitivity of the precipitation rate to changes in CCN and IN concentrations on the second day of simulation as compared to the first day of simulation (figure 7). This supports the idea of a buffering system which is less sensitive to the microphysics after some spin up. Of course, there will always be some degree of spin up because the boundary conditions come from the operational forecasts throughout the 48 hour simulation and the model keeps adjusting to this. It could be that there would be even less sensitivity of the domain-averaged precipitation to the cloud microphysics if the convective-scale model was forced by a consistent larger-scale model. In that respect it could be useful to perform a full water budget (flux of water coming in, precipitation at the surface, evaporation, flux of water coming out, rate of change of water vapour in addition to the condensed species shown in Figure 3). This would also show the spin up effects more clearly. I think it is pretty clear that the buffering effect is coming from cloud microphysical and dynamical effects but the authors could rule out any buffering effect from surface-atmosphere couplings (e.g. less precipitation, more evaporation, and more recycling of water into precipitation).



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With two 48-hour hindcasts per day, there are four hindcasts for each particular time. 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.

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 (in a practical rather than statistical sense). It is clear that given current uncertainties in forecasting precipitation, a change of a few % for a two-day forecast is negligible (except maybe for systematic effects due to some coupling with the orography). However this may still be of some relevance from a climate perspective, especially in the context of recent developments in our understanding of fast and slow feedbacks (or adjustments). This said I am not sure the study can be very conclusive in that respect. I would expect the % difference in surface precipitation to go down if the size of the domain was further increased (i.e. there would be less impact from spin up due to boundary conditions), but a bigger domain could also increase the role of slower feedbacks (e.g. changes in surface temperature and evaporation with some effects on water recycling in the domain, water saturation in the atmosphere, etc... ). Given that surface temperature hardly changes in the different experiments (figure 10), it is probably safe to say that the (small) changes in precipitation are due to a combination of the effects of spin-up and fast atmospheric feedbacks, but it is not really possible to disentangle the two.

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

**Detailed comments**

p 20204, l 22: replace “cloud-dynamics” with “cloud dynamical”



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p 20207, l 3: “12004 x 1300 km$^2$” is presumably “1200 x 1300 km$^2$”

p 20207, l 23: “rations” should be “ratios”

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.

p 20208, l 27: if a log-normal size distribution is used, $\sigma$ is a measure of the dispersion of the size distribution but it is not strictly speaking a standard deviation. I suspect $\sigma$ is the geometric standard deviation.

p 20209, l 1-3: I found this couple of sentences not very clear. Can the authors clarify?

p 20210, l 15: replace “on set of hindcasts” with “a set of hindcasts”

p 20210, l 8: can the authors specify where the total number of simulations come from? 3 years $\times$ 7 experimental setup $\times$ 92 days $\times$ 2 simulations per day?

p 20213, l 5-10, p 20214, l 7: I think the authors refer to Figure 7 here (the plots, not the caption).

p 20216, l 9: Figure 7 should be figure 10 here.

p 20221: Can the authors homogeneize the format of their reference list? Sometimes the article titles are capitalised, sometimes not.

p 20228, table 2: if $\sigma_i$ and $\sigma_s$ are variances of fall speed, then their units should be m$^2$s$^{-2}$ rather than ms$^{-1}$.

p 20230, table 4: what is the unit here? mm/day or mm integrated over the full simulation period?

p 20232, figure 2: the unit is unusual. Could the authors use m$^{-3}$ or cm$^{-3}$? The concentrations are pretty low, is this correct?

p 20233, figure 3: why is there no dashed red line on the left plot?



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p 20233, figure 3: I would expect the solid and dashed lines to start from the same point (and then spin up differently), but that does not seem to be the case if one extrapolates the lines to t=0. Can the authors draw all the lines from t=0 and explain why some of the solid and dashed lines do not start from the same value if this is indeed the case.

p 20234, figure 4: the grey areas look like white. If there is no mixed phase clouds, then shouldn't the authors indicate this as 0

p 20237, figure 7: I wonder whether there is any benefit of showing cumulative precipitation rather than precipitation. The caption is incorrect and seem to have been swapped with that of figure 10.

p 20239, figure 9: the caption says that the data has been averaged over one of the three subdomains but only one bar is shown for each experiment. Can the authors clarify?

p 20240, figure 10: the plot does not correspond to the caption. Please insert the correct plot(s). Delete "the" before (a).

p 20241, figure 11: again not clear what unit is used for the precipitation.

p 20243, figure 13: replace "in 5 km" with "at 5 km"

supplementary figure 1: replace "experiment" with "experiments"

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