Atmos. Chem. Phys. Discuss., 11, C8973–C8977, 2011
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Atmospheric Chemistry and Physics Discussions

ACPD

11, C8973–C8977, 2011

Interactive Comment

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 #2

Received and published: 14 September 2011

I read this paper with considerable interest as I had thought that such a study would have been a valuable contribution to efforts assessing indirect aerosol effects. Overall the study confirms my expectation that aerosols have a significant impact on cloud properties, but the impact on the surface precipitation is rather small (if not insignificant). Because of that I think this is an important study and it should be eventually published. However, my suggestion is that the authors address specific issues that I list below. In particular, I think the most recent papers that the authors miss to cite provide an important support for the authors' statements.

1. I suggest the authors have a look at the following papers:

Grabowski, W. W., 2006: Indirect impact of atmospheric aerosols in idealized simulations of convective-radiative quasi-equilibrium. J. Climate, 19, 4664-4682.

Grabowski, W. W., and H. Morrison, 2011: Indirect impact of atmospheric aerosols in idealized simulations of convective-radiative quasi-equilibrium. Part II: Double-moment microphysics. J. Climate, 24, 1897-1912.

Morrison, H., and W. W. Grabowski, 2011: Cloud-system resolving model simulations of aerosol indirect effects on tropical deep convection and its thermodynamic environment. Atmos. Chem. Phys. Discuss., 11, 15573-15629.

Koren, I., Feingold, G., and Remer, L. A.: The invigoration of deep convective clouds over the Atlantic: aerosol effect, meteorology or retrieval artifact?, Atmos. Chem. Phys., 10, 8855-8872, doi:10.5194/acp-10-8855-2010, 2010.

The first two papers discuss results from idealized model simulations that show a small impact of CCN on the surface precipitation. The discussion in the two papers is relevant to conclusions drawn from the current study. Part two provides the context for the NWP-type simulations, the differences between regional and global effects in particular. Both papers argue that the cloud ensemble modeling, in contrast to the single-cloud modeling, is the only appropriate context for the aerosol effect as far as climate is concerned. The authors make similar statements in the paper under review.

The ACPD paper is relevant because the authors show similar results (i.e., significant impact on cloud properties and small impact on the surface precipitation) and offer a different explanation for the "observed" invigoration hypothesis. I will discuss this more below.

Koren et al. provides an updated discussion than the Koren GRL et al. paper. However, I am also aware of a paper by dr. Steve Massie that questions Koren et al.'s findings applying similar methodology, but different satellite data. I saw a submitted paper long time ago, but I was not able to find it in my records, neither I saw it on JGR webpages.

ACPD

11, C8973–C8977, 2011

Interactive Comment

My suggestion is that the lead author contacts dr. Massie at NCAR (massie@ucar.edu) and inquires about its status.

2. Although not relevant for the current paper, I would caution the authors in applying implicit scheme and long time steps to represent cloud dynamics. Implicit schemes usually remain stable because they “slow down” the advection in the physical space. When cloud dynamics is concerned, this suggests that clouds simulated by applying the implicit scheme may have their vertical velocities reduced. I urge the authors to investigate this issue thoroughly applying, for instance, classical deep convection tests such as those reported in old Klemp and Wilmhelmson papers. This is particularly relevant for the microphysics that is sensitive to the simulated updraft velocities. Again, this is not for this paper, but for the COSMO model numerics team to explore.

3. The discussion of the homogeneous freezing (bottom/top of 20209/20210) is unclear to me. What is meant by the sentence starting with “The competition of...” and “fictitious downdraft” in particular?

4. The discussion at the bottom of p. 20214. As shown in Morrison and Grabowski ACPD paper, changes in the cloud microphysics can explain observed changes of the cloud field without invoking changes in the updraft strength (in fact, convection became slightly weaker, not invigorated in the polluted case in that paper). To support the authors’ statement, one should look at the maximum velocity within the cores, not the core depth.

5. Similarly to 4, I find the statement at the opening discussion of section 3.2 unsupported by the analysis. I think the authors have the data to distinguish effects discussed in Morrison and Grabowski, that is, to distinguish convective invigoration (stronger updrafts) from more condensate being lofted higher due to its smaller fall velocity. The latter is different from the invigoration concept. The same comment applies to the discussion on page 20219 in the conclusion section, the “dynamical feedbacks” in 3. there in particular. I think the question about effects on dynamics is still open and the analy-

ACPD

11, C8973–C8977, 2011

Interactive Comment

sis presented does not convince me about the presence of such effects. For instance, is there a clear signal if one looks at pdfs of the vertical velocity at various heights? If invigoration comes about more latent heating and higher buoyancy above the melting level in the polluted case, such a signal should be visible in the statistics of the vertical velocity.

6. The impacts of the surface temperature are interesting, but they can be put in the context of previous studies. Foe instance, there is strong evidence that organized convection is affected mostly by dynamical processes, and that such processes concern cold pools. The first statement comes from the kinematic study by Slawinska et al (QJ 2009, p.1906) that show no effects of aerosols on surface precipitation from squall-line-type convection when the flow is prescribed. The second statement comes from dynamic simulations of squall lines by Morrison et al (MWR 2009, p. 991). Their comparison between simulated squall lines with various microphysics schemes suggests that microphysics affects squall line dynamics through the impact on cold-pool strength (this also echoes results from Grabowski and Morrison CRE paper listed above). Do the results presented in the paper under review are consistent with the analysis of simulations discussed in these papers (i.e., do colder cold pools exist in high-CCN environments)? More analysis of cold-pool characteristics (e.g., along the lines of the discussion in Grabowski and Morrison 20011) would add to the discussion of the air near-surface temperature.

7. In a few places in the text the authors take a defensive position saying that the "large-scale forcing" is prescribed through the imposed lateral boundary conditions and this has a significant impact on the results. I do not think this is fully true. First, in the summertime, deep continental convection is driven mostly by the surface forcing. Since the area under investigation is much larger than a single individual convective system, I think the large-scale advective effects are much smaller than the surface forcing. Second, even significant surface precipitation differences lead to small differences in the outflow humidity and the latter is not imposed, that is, the model can "vent out"

ACPD
11, C8973–C8977, 2011

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as little moisture as it wants through the lateral outflow boundaries. It follows that the results are quite robust in my view, especially considering that the model is applied across 3 seasons of JJA (the latter should be stressed in the abstract).

8. Finally, a few minor comments. Line 16, p. 20210: "on" should be "one". Figure 7 is supposed to show temperature, but the vertical axis label says "precipitation". Are Figs. 7 and 10 simply switched? Are color labels correct in Fig. 8? If so, the core depth seems to be larger for some but not all high-CCN runs compared to similar low-CCN runs. Overall, the figures were plotted too small to legitimately evaluate them.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 20203, 2011.

ACPD

11, C8973–C8977, 2011

Interactive Comment