Geosci. Model Dev. Discuss., 7, C616–C621, 2014
www.geosci-model-dev-discuss.net/7/C616/2014/

Geoscientific Model Development Discussions
Open Access

GMDD
7, C616–C621, 2014

Interactive comment on "A new WRF-Chem treatment for studying regional scale impacts of cloud-aerosol interactions in parameterized cumuli" *by* L. K. Berg et al.

Anonymous Referee #1

Received and published: 23 May 2014

Berg et al. have implemented a new treatment of cloud-aerosol interactions within parameterized shallow and deep convection in WRF-Chem. Unfortunately, they make a number of in my opinion rather questionable and poorly motivated choices in their model formulation, and the model description is unclear on several important issues. The results section is a rather lengthy description of rather unsurprising results, while there is little actual model evaluation with observations (the last parts of the results section being the relatively more interesting parts). Furthermore, in my opinion the authors need to demonstrate that the deep convection parameterization they use is actually appropriate for their grid size of 10×10 km^2. Given the major uncertainties inherent in the new parameterizations, the advantages of using them at all did not become sufficiently clear to me. It seems, that the control simulation agrees with the resolved simulation as well as the simulation in which the new parameterizations are used, although on the whole I find this particular comparison rather superficial. I recommend not to publish the manuscript in GMD in its present form. It might, however, be suitable for publication after major revisions. In particular, I recommend to perform one or more additional case studies for which observations are available that can be used to better evaluate the parameterizations.

Major comments:

Model formulation:

p. 2661, line 20ff: "For all attachment states, the aerosol species associated with cloud droplets in the sub-grid convective clouds are treated explicitly, but only within the convective cloud routines" - A considerable fraction of precipitation in deep convection originates from the stratiform anvil region. Does the above sentence mean that once the trace gases and aerosols are detrained from the updraft, they automatically become "unattached"? Also, is uptake by cloud droplets treated in the resolved clouds? (I can not find this information in Chapman et al.) or does the detrained trace gas have to be taken up by falling hydrometeors? If yes, is this uptake kinetically limited?

p. 2663, Sect. 2.2.1: "but for shallow convective clouds, the average (over different clouds) vertical velocity is used": Activation depends on the local vertical velocity, which is not the same as the multi-cloud average. The authors should at least discuss the potential impact of their assumption. They should mention whether their cumulus parameterization provides either an ensemble of updraft velocities or else assumes

GMDD
7, C616–C621, 2014

a PDF. In case it does neither, I would suggest to perform a sensitivity study with an assumed PDF of in-cloud updraft velocities that is consistent with the in-cloud average updraft velocity. One can expect that using the average leads to an underestimate of cloud droplet concentration, so it would be good if the authors could perform a sensitivity study on this issue.

p. 2663, line 26: “Cloud water can also be converted to cloud ice, but currently this is not included as part of the aerosol wet removal” - Does this mean that all dissolved aerosol and trace gas is released from the hydrometeors upon freezing? If yes, this would be inappropriate for most species. In particular, several model studies have shown that releasing trace gases from hydrometeors upon freezing in deep convection strongly enhances upper tropospheric mixing ratios and reduces wet deposition.

p. 2663, line 6ff: “The environment mixing ratios for interstitial aerosol are assumed equal to the grid-cell mean values, and are zero for convective-cloud-borne aerosol.” and p. 2664, line 12ff: “The environment gas mixing ratios are assumed equal to the grid-cell mean values” - How can this be justified at a 10×10 km^2 horizontal resolution? Is the maximum updraft area fraction really small compared to the grid box size? As far as I can see, this assumption must lead to an overestimate of the uptake of trace gases. This overestimate is more severe for smaller time steps.

p. 2664, line 17ff: “The wet removal rate for gases only considers the removal of gases dissolved in cloud droplets; uptake of gases by rain is currently neglected.” - Why?

Model setup:

At 10 km resolution a part of the deep convection will be resolved. Yet, the authors use

GMDD
7, C616–C621, 2014

Interactive Comment

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

a deep convection parameterization that was tailored for a lower resolution. They need to either demonstrate that this works or else find literature that demonstrates that this parameterization is appropriate for this resolution. Also: what is the maximum updraft area fraction in the 10×10 km^2 grid boxes? As the size of the updrafts approaches the grid resolution, the assumption in the cumulus parameterization that the updraft mass flux is balanced by compensating subsidence within the same grid column breaks down. Given the potential problems with a cumulus parameterization at this resolution, one must be demonstrate that the benefits outweigh the potential problems.

Analysis:

- Sects. 5, 5.1, and 5.2: I am not sure what can be learned from this analysis. In my opinion, it neither serves the model evaluation, nor does it contribute to an improved understanding or a better quantification of the effects of clouds on aerosols. This section should be shortened drastically. In my opinion, it does not add significantly to the existing published literature.

The comparison with the high resolution simulations seems somewhat superficial, and I do not quite see what has been actually learned from it. Maybe one could try to isolate some systematic differences which could then be attributed to a cause.

Summary and conclusions:

p. 2683: "It should be noted that the current modifications do not include the treatment of other indirect effects, which will be included at a later date." - How? Since the appropriate parameterizations do not exist, the authors should be more specific on what their plans are.

GMDD

7, C616–C621, 2014

Interactive Comment

Minor points:

Sect. 5.3: “They used perturbation of CO (CO0; defined as the difference between the instantaneous measured CO and the average CO observed during a flight leg) as an indicator of increased aerosol.” - The authors take into account only shallow convection. But did they also filter for drizzle? Since sulfate aerosols are subject to scavenging, while CO is not, this method breaks down in case of precipitating clouds.

p. 2665, line 9f: “The integration is explicit in time and uses simple upstream finite differencing for the vertical transport terms.” - Updraft cores in deep convection tend to be rather well defined, yet here an extremely diffusive transport scheme is used.

p. 2665, line 21ff: “Initial mixing ratios of interstitial aerosol and trace gases are set equal to the grid-cell mean mixing ratios at the end of the active cumulus effects calculation” - This results in an overestimate of the uptake which will be larger for shorter time steps.

p. 2666, line 9ff: “The sub-grid cumulus lifetimes, as defined within the cumulus parameterization, ranged between 30 and 60 min, and the cumulus effects on aerosols/gases are calculated once only when a cumulus is triggered in a grid column. On subsequent chemistry time steps, no more cumulus effect calculations are performed until a new cumulus is triggered in a column.” - I do not understand what is done here. Can deep convection be triggered in two subsequent time steps? If not, why should the storm stay in a 10$\times$10 km^2 grid box for 30 minutes?

GMDD
7, C616–C621, 2014

Interactive Comment

p. 2666, line 19ff: "The net changes to the aerosol would be the same in either case, but the changes are applied somewhat sooner in the once-only approach (when a cloud triggers rather than over its lifetime" - This would only apply if the aerosol processes and chemistry were linear.

If the model is formulated in a mass conserving manner, this should be mentioned. If not, this would be a major point.

Interactive comment on Geosci. Model Dev. Discuss., 7, 2651, 2014.