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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Cloud-scale model intercomparison of chemical constituent transport in deep convection” *by* M. C. Barth et al.

M. C. Barth et al.

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We appreciate the comments given by the reviewer and thank him/her for his/her interest in our paper. Responses to the specific comments of the referee are given here.

Specific comments:

1. Page 8040, lines 3-4:

a. How sensitive are the model results to the chosen initial perturbation of 3C and the number of warm bubbles?

The choice of 3 warm bubbles by Skamarock et al. (2000) was based on obtaining a good representation of storm structure and evolution, particularly of the transition from a multicell to a super cell. This statement is now included in the case description. We have performed a sensitivity simulation with the WRF-AqChem model using a 2 bubble initiation. We find that the storm still transitions from a multicell storm to a quasi-supercell, but is smaller (both horizontally and vertically) with weaker updrafts, and a smaller anvil cross-section. This results in only minor effects on the tracer transport: the magnitude of the anvil enhancement or depletion is similar to the 3 bubble results for CO, O_3, CH_2O, H_2O_2, and HNO_3, the magnitude of NO_x in the anvil is 20-30% greater because of the smaller volume that the NO is placed in, the fluxes of air mass and CO are the same because of normalizing the flux by the anvil area (which is 3 times smaller) , and the flux of NO_x is greater.

b. In figure 3, doesn't the observations point more towards using two warm bubbles?

No, 2-4 convective cells were observed during the multicell stage of the storm. This statement has been added to the text (Section 4.1).

c. In relation to this, does the choice of the location of the transect T1 in Figure 3 for the models have any impact on the results shown in Figures 5, 6 and 11? How large is the variability of the chemical compounds (see also comment on Table 3)?

Yes, the choice of the location matters. Barth et al. (2007) show horizontal cross-sections of various species at the 11.5 km m.s.l. altitude. Their plots indicate the variability. The transects plotted in this paper are chosen by the participants to be the most appropriate at ~10 and ~50 km downwind to compare with the observations.

2. Page 8041-8051, model descriptions:

a. The contents of the various model descriptions are not always consistent. Tables 1 and 2 give a good overview, but the specific items are not always discussed in the text for each model. For example, for the models that are listed in table 1 as "no radiation" models, I assume there is some radiation module in the models, it's just that it isn't interactive? It would be interesting to know how the radiation is described in these models and what is assumed for closing the radiation calculations.

b. I'm also wondering for the "aerosol models", what is assumed for the initial aerosol distribu-

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tion?

The descriptions of the models have been revised so that they are now more consistent. The issue of the radiation modules is as the referee suspects. That is, all the models have a radiation module, but only the C.Wang model activated the radiation for the simulation. Because the table is misleading, the radiation characteristics have been removed from Table 1 and are only discussed where needed in the text. Also included in the revised model descriptions are the initial aerosol distributions used in the C.Wang and DHARMA models.

3. Page 8054, line 28: I'm not so sure I agree on the conclusion that "all models do a good job transporting these passive tracers to the anvil". The UMd/GCE model simulates too low CO concentrations (and too high O3) 10 km downwind and too high concentrations 50 km downwind. Is this just a result of the choice of the transects? Or time variability?

The model results all are within 10-15% of the observations, with most models within a few percent. This is a good job. The model results can be sensitive to the location and time of the transects. To give an idea of the heterogeneity, Figure 4 of Barth et al. (2007) shows appreciable horizontal variation of CO in the anvil. The sensitivity of the CO, O_3 transect results is now included in the paragraph, and the last sentence has been revised to be more quantitative.

4. Page 8055, NOx comparison: Why does the RAMS model show such low NOX values despite the included L(NOx) mechanism?

The results from the RAMS model are generally lower than the other models with a lightning-NO_x production scheme. The details of the RAMS lightning-NO_x parameterization (Pickering et al., 1998) indicate that small amounts of NO (111 moles NO/IC flash) are produced (remembering that intracloud flashes dominate) and placed in a large volume (above -15°C isotherm (which is 6.5 km m.s.l.) for cloud regions $>$ 20 dBZ) leading to a reduced NO_x mixing ratio. At t=3600 s, the UMd/GCE model has lower NO_x mixing ratios as well. Their scheme produces more NO per IC flash (195

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moles/flash) but mixing ratios are reduced because of the large volume of cloud $>$ 20 dBZ. This is now discussed in the paragraph on the NO_x transect comparison.

5. Page 8056, lines 9-14: For the models with a prescribed diameter for ice, how much would this diameter have to be changed in order to give a reasonable agreement with observations? Is it within reasonable numbers?

The diameter chosen does give a reasonable agreement for WRF-AqChem and UMd/GCE. Meso-NH, SDSMT, and Spiridonov models result in much higher ice concentrations. As stated in the paper, the number concentration is very dependent on the assumed ice diameter. As an example, when the diameter for ice in WRF-AqChem was changed from 45 μm to 35 μm, the maximum number of ice particles changed from 593/liter to 1258/liter.

6. Page 8058, lines 17-20: This sentence is not completely clear to me, do all models show an underestimate of the CO flux if a correction of the mass flux is made or only all models within the 33% error?

All models would underestimate the CO flux if the mass flux were corrected. The sentence has been revised to be clearer.

7. Figure 7: For clarification, include in the figure text that the location of the cross section is similar to the location of T2.

This text has been added.

8. Table 3: The star is missing in the figure text to explain the NOx flux for the observations

The star is included. It is the last line of the table footnote.

9. Table 3: What is included in the numbers for mean and std deviations on the last row of the table? Is this for all models? I think it would be more interesting to see the mean and std dev for all individual models and for the observations in order to get some estimate of the variability.

The mean and standard deviations listed are for all the models. Following the referee's

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suggestion, we now include standard deviations for each of the models. These kind of statistics are not available for the observations, but their uncertainties are discussed.

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