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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.

Anonymous Referee #4

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This paper describes an intercomparison of simulation results of the same mid-latitude convective case during the STERAO campaign by 8 mesoscale/cloud scale models with chemistry or chemical tracers. The transport of the following species is studied: CO, O3, NO (or NOx in the models) and soluble species like HNO3, H2O2 and CH2O. The modelling results are also compared with measurements of the above mentioned species (except for the soluble species) during the STERAO field campaign This provides an indication of how models are behaving with the transport of chemical species during a convective event and moreover, which processes are important to properly model this transport.

General comment: The paper is well structured and provides inputs which are worth being published in ACP. However my feeling is that the authors do not go far enough in the comments of their modelling results (see my comments on the single/double moment scheme, on ozone in the upper troposphere and on lighting produced NOx) and do not comment so much which kinds of scheme are doing the best job. Another important issue is the larger scale effect that can be deduced from the flux calculations. Some models are doing a pretty good job for one of the species. They do a less good job for the flux calculations of the same species. On the contrary, some models are doing a better job at a larger scale (flux in the anvil). What to deduce from this? For example what would be the lead to follow for a better representation of each chemical compound: double or single moment scheme for tracers? Explicit electric charge for lightning produced NOx? Etc. Those inputs could help the mesoscale/cloud scale modeller in his choice of new parameterization. I recommend this paper for publication in ACP after the general and specific comments are addressed.

Specific comments:

Figures. It would be helpful to add a figure showing the temperature profile measured during the event (the altitude of the initial tropopause level could be seen, as it seems to impact the modelling results of ozone in the upper troposphere) and the initial temperature profile used in the models to trigger convection (warm bubble). This might be done adding a panel in Figure 1. p. 8039 line 12: please give a typical lifetime for CO and O3 to show that it is significantly higher than the lifetime of a convective storm.

p. 8039, line 19: please give the acronym for STERAO. Is there a specific reason for choosing this case for the intercomparison?

p. 8040: concentration unit. Volume mixing ratio (which is equivalent to the molar ratio) is more frequently used than molar ratio. It would be better to replace nmol.mol-1 by ppbv, pmol.mol-1 by pptv, etc.

p. 8040, line 15: “obtained from the literature”. Please give the reference.

Section 3. Description of the models. Each subsection describes one of the mod-

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els used for this intercomparison and is written by the concerned researchers. The information given in each paragraph is not always consistent from each model. For example, the radiative scheme is not always specified. The top boundary condition does not appear for some models (e.g. Wang's model). Please make sure that the same number of information appears for each model (and in the same order). I also noticed that the grid domain is not always the same and the vertical resolution is not the same. I suppose that the domain was chosen in order to get the best modelling result for one specific model. Whatever it is, the reason should appear in the text. Time step. When the time step is specified, please explain if it is the meteorological or the chemical time step. Lightning NOx scheme: it is not always clear to me what is produced by lightning in the models. Is it really NO, NO2, or a partitioning of these species. Table 2: it should appear more clearly whether the tracking in ice is included or not.

p. 8041, line 15: "daytime chemistry". What is meant by that? A chemical scheme that includes photolysis reactions?

p. 8053, lines 10-15. Double moment scheme versus single moment: Why RAMS and DHARMA have anvils similar in width to the model with single-moment scheme? More generaly, can a general conclusion be drawn in this study about the importance of the type of microphysics on the modelling results? It could help a cloud scale modeller or a future user to go toward one of these schemes.

Section 4 Why 3 warm bubbles were used in the simulation while the observations clearly show a double cell structure? Is it because the chemistry results fit better with the observations? It is noticed later in the study (p. 8058 line 13) that all of the models overpredict the air mass flux in the anvil. What would have been this flux if two warm bubbles had been used instead?

Section 4.2 p. 8054, lines 11-12: "to a derived cross section obtained from several transects". Please add a few words about how this cross section was obtained and above all what is the uncertainty associated with this method.

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p. 8055, lines 2-8. In Figure 6, the DHARMA and Spiridonov models depict an increase of NOx during convection, although the production of NOx by lightning is not included in the models. Is this due to vertical transport? This should appear in the text. Lines 6-8: I do not fully agree with this sentence because of the results from RAMS. This model includes a parameterization which is used by a significant number of mesoscale/large scale models. It is surprising to see that for the 10 km downwind panels, the RAMS results are only slightly higher than the DHARMA and Spiridonov models. Could the RAMS user authors comment on this? Would another choice for the transect (slightly shifted in time, in location, or in altitude) lead to the same results? I think a more detailed discussion should be written here about the lightning NOx parameterization. From the information provided in the manuscript, it seems that the conclusion is that the Explicit Electical Scheme (in Meso-NH) is needed for a correct modelling of NOx at a local scale. Thus I would be less optimistic in the sentence lines 17-19. I would replace "that model parameterizations are capturing..." by "that some of the model parameterizations are capturing..." Please note that the conclusion is not the same at a wider scale since the RAMS model does a rather good job in the vertical distribution of NOx (Fig. 10) and in the fluxes (table 3).

p. 8056, 1st paragraph. Again, is there a further conclusion to be drawn here from the results with single-moment scheme models versus double moment scheme models?

p. 8056. O3 cross section in the upper troposphere. The results obtained by C. Wang and RAMS model are interesting and need further comments. It is stated that the high O3 concentration at the top of the anvil may be due to the strength of the updraft in connection with turbulent mixing at the tropopause. Please remind here the initial altitude of the tropopause level (a dotted line could be added in Figure 9). Would turbulent mixing be efficient at the time scale of the simulation to be seen in Figure 9? It is well known that wave activity (especially when the wave breaks) generated by convection may favour the transport of species across the tropopause. Is a gravity wave activity computed by the models? Which ones? Does it depend on the top boundary condi-

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tions? Could the results shown here only be due to a reversible vertical displacement of the tropopause during convection? To check this, the evolution of the isentropic level of the initial tropopause height could be investigated. Another point to be discussed is the potential role of lightning NOx on ozone formation in the upper troposphere. I expect this process to have a small effect at the time scale of the simulation, but this may be not negligible, especially before sunset. Please check.

p. 8057. NOx and NO cross section: You compare NO observations with NOx model results. In order to make the comparison easier in Figure 10, I propose to plot NO(observed)*1.3. This time, the RAMS model does a better job than for the results shown in Figure 6. The NOx flux computed by the RAMS model (table 3) is very close to the flux deduced from observations (as for the C. Wang model). On the contrary, Meso-NH computes a lower flux than observed while it is doing a very good job along the transects in Figure 6. Could it be concluded from this that the parameterization of Pickering et al., (1998) is better tailored to regional scale/large scale studies than to very local studies? Is this the contrary for the parameterization of Barthe et al., (2005) within Meso-NH? I think such a discussion would improve the manuscript since one of the aims of this study is to improve the parameterizations related to the transport of chemical species by convection or related processes (Cf. Introduction p. 8038 lines 28-29)

Section 4.4 p 8059 line 7. “Other field campaigns”. Please give some of them.

p. 8060, lines 5-6: “Meso-NH model does not include gas or aqueous chemistry”. I understand from section 3.5 that only the soluble species do not react. Are all the chemical species passive tracers in the model? If yes it should be written more explicitly in section 3.5

p. 8061, line 2: For HNO3, all the models except the RAMS model has anvil mixing ratios that are depleted...” Replace “has” by “have” It is an unexpected behaviour for a model which includes scavenging of soluble species. Is there a reason for this?

Conclusion: p. 8062 lines 14-22: again I think that the statement written here is too simple. A more detailed comment needs to be written here accounting for the questions mentioned above in this review. A comment on the potential impact of the microphysical scheme is also welcome. A sentence about what to do to properly simulate the possible intrusion of stratospheric ozone or other species would also improve the conclusion. About the need of field campaigns including measurements of soluble species: I fully agree with this. Can other recommendations for future campaigns be made here? For lightning NOx parameterization?

Technical comment:

In Figure 6 panel d) please, change the Y axis so that all the model outputs can fit within the frame.

p. 8062, line 1: the year of the reference Cohan et al. is missing.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 8035, 2007.

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