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> Interactive Comment

## Interactive comment on "GEM-AQ, an on-line global multiscale chemical weather system: model description and evaluation of gas phase chemistry processes" by J. W. Kaminski et al.

## Anonymous Referee #1

Received and published: 7 December 2007

## General comments:

This paper presents the GEM-AQ model which is introduced as a global multiscale chemical weather system. The study presents a general overview of the features of the model and an evaluation of its performance on the large scale focusing on tropospheric chemistry and air quality. The paper is well written and structured. Although the model is new it already embraces an impressive set of features and appears to be perfectly suited for the purposes it is designed for. The different elements of the model are presented in a concise way and further details can be found in literature which is properly referenced. The model still has some weaknesses (e.g. simplified treatment



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of wet deposition, no aircraft emissions, lightning emissions from climatology, etc.), which is no surprise for such a recent model. These weaknesses are mentioned and pathways for future improvement are outlined.

The paper will thus serve as reference for future studies using GEM-AQ and as such is worth publishing in ACP.

I agree with referee #3 that the evaluation section is the much weaker part of the paper. The comparisons also reveal some significant limitations of the current model setup. Nevertheless, I believe this is a useful first comparison as it provides an important check for (severe) model deficiencies. Somewhat more quantitative analyses and better motivation of the comparisons along the lines suggested by Ref. #3 would improve the manuscript. Useful examples of global model evaluations are for instance the studies of Hauglustaine et al. (JGR, 1998), Brunner et al. (ACP 2003, 2005), von Kuhlmann et al. (JGR 2003a, 2003b) or Horowitz et al. (JGR, 2003). However, I am less negative concerning the value of the present comparisons as the model is still under development and some results may change significantly in response to the model developments outlined in the discussion section (introduction of a better lightning NOx emissions scheme, addition of aircraft emissions, implementation of the GFEDv2 biomass burning emissions). I am therefore not sure it will make much sense to invest a lot more in the comparisons at this stage. However, a more thorough and quantitative evaluation will be mandatory after implementation of these changes.

The semi-Lagrangian transport scheme, known to cause excessive numerical diffusion, may be a further limitation of the current model setup not discussed in the paper. I suspect that the agreement between observed and modelled O3 in the troposphere and the influx from the stratosphere would become worse if ozone in the lowermost stratosphere was brought into better agreement with the observations. Is stratosphere ozone artificially reduced in the model to improve the performance in the troposphere?

The geographical distribution of biomass burning related CO in GEM-AQ looks weird. It

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is hard to believe that the Edgar inventory does such a bad job in placing BB emissions over Africa. Please check whether the Edgar BB inventory has been implemented correctly in the model.

I generally like the way the simulated fields are compared with satellite observations as the different resolutions, averaging kernels, and the effect of varying tropopause altitudes are carefully taken into account (except for NO2 where no averaging kernels have been used apparently). This is exactly the way such comparisons should be done.

I recommend publication after a number of minor corrections detailed below.

Specific comments and technical corrections:

- Pages 14899 and 14900: The GEM physics package includes a Kuo-type convective parameterization for deep convective processes while for the transport of tracer species the Zhang and McFarlane mass flux scheme is used. It is not clear to me how these two different convection parameterizations can work together.

- Page 14899, line 24: Table A1 lists only 49 species, not 51 (=37 + 14).

- Page 14900, line 12: "All species are solved ..". Please change to "The evolution of all species with time is solved using .."

- Page 14901, line 4: The reader gets the impression here that the Jöckel J value scheme is a different thing than the Landgraf and Crutzen method, which it isn't. Please clarify.

- Page 14902, line 16: The processes mentioned are primarily emitting NO not NO2 (confirmed by your Table A.4)

- Page 14902, line 19: The scaling of the lightning NOx source to 2 Tg/yr should be motivated.

- Page 14903, line 4: At this point it would be better to mention only that hybrid level coordinates were used in the simulation. It is not clear at this point why this should

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improve the STE over polar regions. This discussion should be left to section 3.1.

- Page 14903, line 18. Change "These are obtained" to "These were obtained"

- Page 14904, line 25: The measured O3 profiles in the tropics often show a midtropospheric maximum and lower values below (PBL) and above. The low values in the upper troposphere at the base of the TTL are most likely due to the fact that this is the main level of detrainment of deep convection which establishes a close link between the concentrations in the PBL and the upper troposphere. In situ ozone production in the air masses descending from the detrainment level is then responsible for the midtroposphere maximum. The failure of the model in reproducing these features is thus likely related to the convection scheme, the large overestimation of O3 in the PBL, and, probably, to the way lightning NO is introduced in the model. It should indeed be better explained how lightning NO is added in the model (see comments of Ref. #3) because this may have a large impact on the results, in particular in the tropics.

- Page 14905, line 20: It would be good to know how much 5-10 DU is in relative terms. I am not sure this really agrees with the comparison with the sondes. The comparison with Churchill, for instance, suggests larger differences in October which is not seen in the comparison with GOME. It should also be noted that ozone sondes have not really been designed to measure tropospheric ozone. More reliable measurements are available from the MOZAIC program. The ozone sonde measurements are likely about 10% too high in the upper troposphere (Thouret et al., JGR 1998).

- Page 14905, line 27-29: The explanation is not entirely convincing. The model seems to overestimate O3 in the region of large scale subsidence rather than in the convective regions (cf. evaluation of models over this region by Brunner et al. (ACP, 2005). I doubt that the model has a large source of lightning NOx in this region.

- Page 14907: The CO distribution in GEM-AQ looks very strange, in particular over Africa. In the MOPITT data the CO maximum in October is found over the southern parts of Africa and further north in January as expected from the path of the dry season

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over Africa. In GEM-AQ the CO maximum in January is way too far south and much too small in October. Thus, there seems to be a serious problem with the biomass burning emission inventory. Moving to GFEDv2 emissions will hopefully resolve this.

- Page 14907, line 16: Biomass burning over Africa in January will always be much further north compared to what the GEM-AQ CO distribution suggests, irrespective of the year chosen.

- Page 14908, line 4: Change EVISAT to Envisat.

- Page 14908, line 8: It is important to clearly state which SCIAMACHY NO2 data set is used here as there are different retrievals and versions available from different groups with significant differences. The individual retrievals are depending on the a-priori NO2 profiles used. In order to eliminate this dependency one would need to apply averaging kernel information to the model data. Has this been done here?

- Page 14909: I agree with Ref. #3 that the comparison between SCIAMACHY and GEM-AQ NO2 is hampered by the fact that 1990 emissions are used in the model while there have been significant trends in NOx emissions since then, not only over China. The simulation should be done with more recent emissions. If this is not possible with reasonable effort the limitation of using 1990 emissions should be better stressed.

- Fig. 1: Parts a) and b) of the figure should be labelled. The same holds for other figures (Fig. 2, 4, 7 and 8)

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