Atmos. Chem. Phys. Discuss., 7, S8376–S8386, 2008
www.atmos-chem-phys-discuss.net/7/S8376/2008/

Atmospheric Chemistry and Physics Discussions

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.

J. W. Kaminski et al.

Received and published: 15 January 2008

Response to Referee #1

We would like to thank Referee #1 for the review and comments. In general we agree with the presented suggestions and will incorporate these in the final submission. We appreciate Referee's recognition that the model is still being developed and that more model evaluation will be done for future applications.

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Comment:

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.

Reply:

Although we have not conducted specific tests for this case, we have found in the past that the diffusive nature of SL transport is much less at higher resolution. In fact we think of the CLaMs model which is Lagrangian and to which diffusion must be added. Not quite the same but along the same lines. As noted in the text we found that going from 4° horizontal resolution to 1.5° resolution reduced the STE O3 transport. In addition, use of a hybrid coordinate also reduced the STE O3 flux. Now, at present the ozone field is constrained by HALOE observations at every time step above 100 mb so that degree we assume that the lower stratosphere constraints are observations.

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Comment:

Is stratospheric ozone artificially reduced in the model to improve the performance in the troposphere?

Reply:

There must be some misunderstanding here or perhaps we were not sufficiently clear. We have chosen not to do full stratospheric ozone chemistry; partly because the current stratospheric dynamics above about 22-25 km of GEM that we are using is not very good so that we have replaced stratospheric ozone by HALOE observations at every time step to ensure that the flux of ozone at mid- and high-latitudes is more like the "real world";. We note that this is much like has been applied to some (earlier) versions of GEOS-CHEM.

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Comment:

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

Reply:

We had noticed this ourselves and had investigated carefully the placement of the Edgar emissions in the model and everything appeared reasonable at the input level. We also compared the time series of the emissions and compared with other published data and everything seemed OK. We also noted that when we compared our CO with other models (and also MLS CO) that we got good agreement. However, the referee's comments have driven us back to investigate the application of the MOPITT kernel and we have found a problem and these plots have been redone and there is much improvement.

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Comment:

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.

Reply:

We have not applied the averaging kernels for NO2 as the data presented have extracted the NO2 column taking into account the kernel with model data (not GEM-AQ) as a base.

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Comment:

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.

Reply:

These two convection schemes do not work together. We recognise that this a weakness of the model as different convection schemes are used for model physics (water, temperature) and chemical tracers. The convection scheme available in GEM is for coarse grids, Kuo, is not a mass flux scheme and is not readily adaptable for tracers. At this stage in model development we have implemented the Zhang and McFarlane method for tracers. These two methods will differ. The current operational GEM has already gone to a different convection scheme (a modified Kain-Fritsch scheme which is of the mass flux type) and we will use this in the next version of the model. We will mention about this apparent inconsistency in the methods section.

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Comment:

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

Reply:

Thank you. The table has been corrected.

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Comment:

Page 14900, line 12:"All species are solved.." Please change to "The evolution of all

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species with time is solved using .."

Reply:

The sentence will be rephrased.

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Comment:

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.

Reply: We will change the sentence to read: "The J value package used, based on Landgraf and Crutzen method, was developed for MESSy (Jockel et al., 2006) and has been implemented in GEM-AQ";

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Comment:

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

Reply:

The original data was given as NOx flux in Tg of nitrogen per year. Based on the analysis of more detailed regional emission inventories, it has been assumed that NO emission contributes to the total flux in 95% and NO2 in 5% by weight.

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Comment:

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

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Reply:

The LNOx emissions estimate given by Schumann et al., 2007 is 5 +/- 3 Tg/yr. We did a simulation with 12.2 Tg/yr as given by the GEIA website, as well as scaling down to 5 and then 2 Tg/yr and compared with the measured ozone profiles; the process was rather more qualitative than quantitative. Nevertheless, it was felt that there was better agreement with LNOx = 2 Tg/y. We now suspect that this lower end may be due to lack of in-cloud removal of gas species which likely results in too much lower tropospheric HNO3 reaching the UT. Efforts are now underway to improve the washout processes associated with deep convection in the model.

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Comment:

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

Reply:

We agree with this comment and will remove the first part of the sentence.

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Comment:

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

Reply:

The sentence will be rephrased.

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Comment:

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Page 14904, line 25: The measured O3 profiles in the tropics often show a mid-tropospheric 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 mid-troposphere 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.

Reply:

A clearer description of how lightning NOx emissions are incorporated in the model will be added to section 2.2.4 along the lines of "The GEIA inventory for lightning NOx emissions gives a global total of 12.2 Tg/year. Previous model simulations of GEM-AQ indicated that inclusion of these levels of LNOx produced too much ozone in the UT which suggested a reduction would be appropriate. This is consistent with the estimate from Schumann et al. (2007) of 2-8 Tg/year. Based on a qualitative comparison with the Shadoz ozone sondes we determined that an estimate of 2 Tg/year would give "reasonable" results. The monthly mean totals from the GEIA inventory were scaled to give a total of 2 Tg/year. These emissions were placed in the horizontal according to the convective cloud field from the Kuo deep convection parameterization and then distributed in the vertical according to the profiles given in Pickering et al. (1993). These profiles differ for tropical (between 30N and 30S) marine and continental grid points and mid-latitude grid points. The weakness of this method appears to lie primarily in the Kuo convection scheme (which is no longer used operationally) in that it appears to have too much convection, and thus lightning NOx, over the oceans and not enough over the continents."

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Comment:

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).

Reply:

We will add a comment in the text stating what the relative difference in tropospheric ozone column is.

Direct comparison between GOME and ozonsonde plots is not possible, as in the case of the GOME comparison the kernel is applied to the model. However we can see that much of the difference in the ozonesondes profiles lies in the stratosphere, as the tropopause at Churchill is around 280 hPa in SON.

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Comment:

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.

Reply:

As noted above, the Kuo deep convection parameterization does in fact produce convection in the region indicated by the GOME comparison. This issue is now being ad-

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dressed in the model with the implementation of a different convection scheme. Again, as noted above, this is not in the current operational version of GEM.

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Comment:

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 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.

Reply:

As noted above there was as issue with the application of the kernel and this problem is now resolved. Thanks!

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Comment:

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.

Reply:

See above.

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Comment:

Page 14908, line 4: Change EVISAT to Envisat.

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Reply:

Done.

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Comment:

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?

Reply:

We were remiss here in not including adequate information to identify the group in the text (although A. Richter is an author) and the method of retrieval etc. This has been rectified.

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Comment:

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.

Reply:

We used the compilation of EDGAR2.0 and GEIA inventory because the same dataset was evaluated and used in the MOZART-2 model. Also, in this inventory VOC emissions were decoupled into different hydrocarbon groups which reduced significantly

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the uncertainty connected with the decomposition of the VOC flux. We decided not to change the emission dataset during model development and evaluation phase. Although, based on HTAP comparison, the total emissions used for GEM-AQ agrees quite well with those currently used by other global models, the discrepancies between measured and modelled surface concentrations of trace species over Europe indicate that spatial distribution and emission fluxes changed over last decade of the 20th century. At the moment we are considering using RETRO/POET inventories as well as implementing on-line biogenic emissions.

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Comment:

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

Reply:

We will change captions and add labels.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14895, 2007.