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Atmospheric Chemistry and Physics Discussions

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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.

J. W. Kaminski et al.

Received and published: 15 January 2008

Response to Referee #3

We would like to thank Referee #3 for the review and comments. In general we agree with the presented suggestions and will incorporate these in the final submission.

In order to address the main concern about the "evaluation aspect of the paper" we will make an effort to synthesise model evaluation sections so an overall model performance can be presented to the reader as per suggestions.

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(1) While examples of comparisons at individual locations and seasons are welcome, they need to be drawn together with some assessment of performance over global scales. The simplest way to do this is to use a statistical approach over a larger number of measurement locations to show that mean or median mixing ratios and variability are in accord with observations. This has been done in part for SCIAMACHY NO2, but could usefully be done with surface, sonde and aircraft measurements too. An advantage of this approach is that it is necessarily climatological in nature, and thus biases due to geographical and meteorological sampling are minimized. In general, the paper would clearly benefit from a more numerical assessment of model performance. How much do the mean O3, CO or NO2 columns or burdens differ based on Figs 3, 5-7? The pattern-matching exercise presented here is useful but not sufficient.

Reply:

It is true that most of statements of agreement or otherwise have tended to be more qualitative in nature than quantitative. However, that is partly due to the nature of the comparison; we are not comparing a specific incident. Also, using the profile data for ozone on a station by station basis for the Logan data and SHADOZ data allows us to begin to get a grasp on the limitations of the model with respect to emissions and convective transport. And the error/variability-bars allow one to see the regions of disagreement. We will improve the discussion to reflect these points. For a comparison such as NO2 columns, a scatter plot (even with a correlation coefficient) is useful but still limited so that 2D plots carry a lot more information - but of nature less quantitative. We will add a correlation plot for CO data (with the corrected implementation of the kernel, see below), but the 2D plots still carry much information.

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Comment:

(2) Model performance can partly be assessed with non-observables, for example ozone budgets, trace gas burdens and lifetimes, etc. At present, one or two of these

global diagnostics are covered very briefly at the end of the conclusions. I believe these diagnostics deserve their own section in the main body of the paper, and this would also provide an opportunity to compare results with previous studies, as summarized in, e.g., IPCC 2001, or Stevenson et al. [JGR, 2006].

Reply:

We will expand the presentation of "non-observable" variables that provide a global characterization of model performance.

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Comment:

(3) Clear justification is required for the choice of observations used in the comparisons. This will make the comparisons more meaningful and will avoid any accusation of "cherry picking".

Reply:

See above reply to recommendation (1).

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Comment:

4) Clear justification is required for the choice of emissions used. In particular, the scaling of lightning emissions and omission of aircraft emissions appear somewhat arbitrary, and the methods used for distributing lightning and biomass burning emissions need a clearer introduction. While it is appropriate to use 1990 emissions for comparison with ozonesonde climatologies, they are not ideal for comparison against recent satellite measurements of short-lived tracers. If the focus is 2001-2005, why not use 2000 emissions?

Reply:

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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.

Reply:

Our original research focus was on lower troposphere chemistry and air quality. Currently we are supplementing the model with additional stratospheric chemistry and aviation emissions.

We used the compilation of EDGAR2.0 and GEIA inventory because the same dataset was evaluated and used in MOZART-2 model. Also, in this inventory VOC emissions were decoupled into different hydrocarbon groups which reduced significantly the uncertainty connected with decomposition of VOC flux. We decided not to change emission dataset during model development and evaluation phase.

Although, based on HTAP intercomparison, total emission values for GEM-AQ agrees reasonably with those used 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 that moment we consider using RETRO/POET inventories as well as implementing on-line biogenic emissions

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Comment:

The abstract is insufficient: it outlines the approach taken, but does not report any results. This should be addressed by adding a couple of sentences that summarize

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model performance, ideally in a quantitative manner.

Reply:

We will expand and modify the abstract as suggested.

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Comment:

p.14898 l.18: It would be helpful to enumerate meso-gamma scale for readers outside the meteorological community by adding, e.g., "(2-20 km)".

Reply:

Yes, it will be added in brackets.

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Comment:

p.14902 l.14: The origin of the biomass burning data needs to be described here - move the explanation from the conclusions (p.14911 l.1-5) to this introductory section.

Reply:

These lines refer to a future study. The information does not belong to Section 2.2.4.

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Comment:

p.14902 l.6: the phrase "archived in 2000" is irrelevant and potentially misleading, and should be removed.

Reply:

The phrase will be removed.

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Comment:

p.14902 l.20: Are monthly-mean lightning emissions applied uniformly throughout the month (based on the mean convective cloud distribution), or are they only applied when convective clouds are present (i.e., they are event-specific)? This is an important distinction, and further clarification is required here. In addition, it would be interesting to know why emissions were scaled down to 2 Tg/yr, and why aircraft emissions were omitted.

Reply:

See the responses to Referee #1. Yes, the lightning emissions are only applied when convective clouds are present (event-specific) as predicted by the deep convection parameterization. See above reply to recommendation (4).

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Comment:

p.14903 l.2: The sentence starting "To better account for stratosphere/ troposphere exchange in polar regions..." is puzzling and should be rephrased clearly. Does it mean "To reduce excess stratospheric influx..."?

Reply:

See responses to Referee #1. We will remove the first part of the sentence and address the influx in section 3.1

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Comment:

p.14903 l.22: How often are these climatological variables updated? Surface roughness, in particular, would benefit from frequent update (monthly at the very least). Are

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inter-annual variations considered? What is the source of this data?

Reply:

The fields are updated monthly and inter-annual variability is not considered. The data is from the Canadian Meteorological Centre operational weather prediction databases.

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Comment:

p.14904 l.25: The over-prediction of tropical upper troposphere ozone may also be influenced by the stratospheric boundary condition imposed above 100 hPa. Has the sensitivity of the results to the location or magnitude of this upper boundary been assessed?

Reply:

We consider it unlikely that the tropical UT ozone is influenced by the LS boundary condition as the flow is upwards at this location. In some sense we are constrained as we don't want the boundary too low as this would be too far from the stratosphere in places. But by the same token we if we go too high (say 50 mb) then the transport will be excessively influenced by the stratospheric transport in a region where the vertical resolution is compromised.

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Comment:

p.14906 l.15: CO is a good tracer of transport, but note that primary emissions contribute less than half the total source [see, e.g., Shindell et al., JGR, 2006], so chemical processes also contribute to the comparisons here.

Reply:

We note that CO is produced from the oxidation of hydrocarbons in the present chem-

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ical mechanism. We would add that an important part of the total CO is produced by oxidation of hydrocarbons.

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Comment:

p.14908 l.21: Is GEM-AQ sampled at the same time of day as the SCIAMACHY measurement?

Reply:

Yes, the model is sampled within 30 min of satellite overpass.

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Comment:

p.14909 l.18: "may be too low": underestimation of emissions over China is to be expected if the EDGAR emissions for 1990 are used. One of the coauthors has previously pointed this out clearly [Richter et al., 2005], so it should come as no surprise here. Is the underestimate consistent with the trends estimated in these earlier studies?

Reply:

Perhaps we should have said instead of "may be" use "will be".

This will be re-worded with Richter et al. (2005) referenced. They show that there has been a significant increase in NO2 concentrations of about 50% over the industrial areas of China with more pronounced increase in the winter months. This is consistent with the larger under-prediction in the January plots.

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Comment:

p.14909 l.20: The significance of section 3.4 is greatly weakened by the sentence starting on line 28, and by the poorly-justified choice of August 2001 results to compare with October 1992 data. It is not clear that we can learn anything from this. It would be better to choose a more recent measurement campaign, or one less heavily influenced by biomass burning (or anthropogenic) sources.

Reply:

There is an error in the text "the same period August 2001". The corrected sentence will read "However, we do compare observations taken during TRACE-A from 21 September to 26 October 1992 with model results for the same period in 2001 so that the same general weather features might be present."

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Comment:

p.14910 l.16: HNO3 is high and remarkably uniform in the upper troposphere here. Might this be responsible for elevated NOx and hence overestimated ozone production in the upper tropical troposphere rather than lightning, which at only 2 Tg/yr is probably underestimated?

Reply:

We now suspect that any "excess" NOx may be due to lack of in-cloud removal of HNO3 which likely results in too much lower tropospheric HNO3 reaching the UT which then photolyses to NOx. Efforts are now underway to improve the washout processes associated with deep convection in the model.

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Comment:

p.14911 l.8: The meteorological biases need to be assessed and at least partly understood before the chemical biases can be explained. Has any previous study examined these?

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Reply:

The GEM model is an operational weather prediction model and it being evaluated every day by the Canadian Meteorological Centre according to the WMO procedures (Cote et al., 1998a and b).

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Comment:

Table A2: The product yields denoted by betas aren't explained.

Reply:

They will be added as footnotes.

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Comment:

The vertical coordinate in Fig 1 is pressure, but in Figs 2 and 10 is altitude. For consistency it would be helpful to use altitude in Fig 1.

Reply:

The ozonesonde observations are provided in pressure as a vertical coordinate. We have selected to present model data in terms of vertical coordinate of the observations.

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Comment:

Fig 10: Identify model and observations in caption, or add a legend.

Reply:

We will add in the caption.

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Comment:

p.14897 l.23: rephrase, or replace "for" with "of the importance of an"

Reply:

This sentence will be rephrased.

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Comment:

p.14898 l.7: replace "will allow for introducing" with "allows"

Reply:

This sentence will be rephrased.

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Comment:

p.14906 l.12: incidents -> episodes

Reply:

This sentence will be rephrased.

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Comment:

p.14906 l.17: insert "is" between "but" and "also impacted"

Reply:

This sentence will be rephrased.

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Comment:

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p.14912 l.17: Zang -> Zhang

Reply:

Spelling will be corrected.

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

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