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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 #4

We would like to thank Referee #4 for the review and comments. In general we agree with the presented suggestions and will incorporate these in the final submission.

Comment: ====================

The paper provides a basic description of the representation of physical and chemical processes included in the model, although these descriptions should be expanded to include more detail and some of the reasoning for the (sometimes odd) choices that were made. The paper also includes some comparison of simulated concentrations with those observed from aircraft, sondes, and satellites. [..]

Reply:

In our paper we were faced with a decision to focus on events or a general comparison, otherwise it would be much too long in detail in order to be useful in science. We choose to follow the more general route and show that the model does a "good" (or better) job by comparing with the measurements that have become available and of course bearing in mind that even at 1.5° horizontal resolution it is not a air quality (AQ) regional model. (We should mention that the current version of GEM, mesoglobal GEM is running at 0.33°, which would, with AQ modules represent a regional AQ model with a global domain.) For example, for our comparison of the ozone sondes we, of course, compared many more that we showed in the paper and at the end showed both good and poor cases. For the "Logan"-sondes there was no case that was poor for all seasons, but there was not much tropical data. A comparison with SHADOZ was more revealing in that pointed to potential limitations with the means by which the current version of the model handles convection and that of course is related to lightning but we felt no need to show lots of plots. For NO2 we did a general comparison focusing on general regions knowing that China's emissions were far from correct and that biomass burning (and Boreal forest burning) has a large annual variability. But it was important to see if we could capture the general behaviour. We consider that our analysis showed that. Our comparison with the aircraft data was mean only to address the issue of other species not generally covered by satellite data such as ethane. However, the meteorology was not that for the various expeditions. However, we feel that the analysis was sufficient to evaluate the model. Ideally, we could have focussed on a single experiment but this has already been done in all (or most) cases. And we do plan to analyse the data from INTEX expeditions.

Comment: ====================

Abstract - The abstract is pretty devoid of content. It should at least in-

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clude some *quantitative* results, as well as qualitative descriptions of the model strengths/weaknesses (e.g., from evaluation vs. observations).

Reply:

We will re-write the abstract to include quantitative findings and the overall synthesis of model evaluation.

Comment: ====================

p.14896, l.20-25 - Rather than "scenarios", you should refer to these as different configurations" (or "applications") of the model. Also, provide a brief description of the strengths/weaknesses of GEM-AQ that have been identified from these previous studies. I assume that some of these properties will carry over into simulations with the present configuration.

Reply:

We will make a clear distinction between model scenarios and configurations. As well, we will add some description of findings from previous studies.

Comment: ====================

p.14897, l.22-23 - What do you mean by "a growing recognition for on-line implementation of tightly coupled environmental processes"? Do you mean that there is a tendency for more models to follow this approach, or that there have been studies demonstrating a (scientific) need to follow this approach?

Reply:

We agree that the statement could be seen as too strong. With the advances in computer technology it is not really important if coupling of different modules is done "on-line" within the same computer code (i.e. GEM-AQ) or with the use of other communication methods. This paragraph will be re-written accordingly.

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Comment: =====================

p.14900, l.3-4 -Explain why tracers are transported by a different convective scheme than used in the host models. What are the implications of this inconsistency? This is discussed to some extent in the final paragraph of the paper (Section 4). But, this inconsistency, the reasons for it, and possible implications should be discussed first in the methods section.

Reply:

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.

Comment: =====================

p.14901, l.7-15 - Are aerosols included in the simulations being described here? If so, why are they not included in Tables A1, A2, and A4 and in the total number of species listed at the beginning of Section 2.2?

Reply:

We will modify Section 2.2.2 to explicitly state that the aerosol package is activated in the GEM-AQ model simulation. However, since the focus of the paper is "gas phase" we feel that enough information is provided. We are continuing with model scenarios and evaluations. The impact of the aerosol package will be evaluated in the next stage. Aerosols are included to allow for the heterogeneous oxidation of N_2O_5 to HNO_3, which can be an important source for the conversion of NO_x to HNO_3, as well as in-cloud oxidation of SO_2 to H_2SO_4.

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Comment: =====================

p.14901, l.24-25 - How important is in-cloud removal likely to be vs. below-cloud scavenging? It seems that neglecting in-cloud removal is likely to introduce a large bias for soluble tracers (unless below-cloud removal is enhanced to make up for this omission). Are similar removal processes included for aerosols?

Reply:

These processes are included for aerosols and in-cloud removal will be added in the next version of the model. The impact is not large in a column removal sense but probably non-trivial on UT NOx and HNO3, and thus important.

Comment: =====================

p.14902, l.5-17 - Are the same emissions used for each year of the simulation?

Reply: Yes, and we will emphasise this in the text.

Comment: =====================

p.14902, l.19-22 - Explain why such a low value was chosen for lightning NOx? Typical values are more like 3-6 TgN/yr. Describe in more detail how the distribution of lightning NOx was done. Do you scale by convective cloud-top height (e.g., Price et al., 1997)? Do you distinguish between land and ocean convection? You refer to lightning NOx as an important explanation for model biases later in the paper. Thus, you need to provide an adequate explanation for how it was prescribed in your model.

Reply:

Yes, the lightning emissions are only applied when convective clouds are present (event-specific) as predicted by the deep convection parameterization.

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

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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.

Comment: ====================

p.14904, l.2-6 - Comment on trends in ozone concentrations and the validity of comparing simulated results using 2001-2005 meteorology and 1990 emissions with these observations (from 1980s and 1990s).

Reply:

Note that Logan 1999 says: "Trends in tropospheric ozone are small, less than +/- 1%/year since 1980 for Northern Hemisphere stations; tropical data for trends are generally lacking [Logan, 1994; WMO 1998]." We do not anticipate any ozone trends in the model given the nature of the emissions (i.e., constant on a yearly basis and no feedback of meteorology on biogenic and biomass burning. The 5-year run can only give a measure of variability driven by meteorology.

Comment: ====================

p.14904, l.7-10 - Qualitative statements such as "good agreement", "under-predicted", "most variability"; should be quantified. Within what percentage did model and observations agree, how large was the underprediction, how large was the variability in model and observations. The lack of quantification is a general problem throughout the paper.

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

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comparison; we are 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.

Comment: ====================

p.14904, l.12-13 - Can you quantify the cross-tropopause flux of ozone in this version of the model (and in the sigma coordinate version)?

Reply:

This is discussed in section 4 (475 Tg/year). As stated in section 4, a comparison of hybrid vs. sigma coordinate system was done for 4 x 4 degree resolution, with a reduction of about 40%. The comparison was not done at 1.5 x 1.5 degree resolution. We will add a reference to section 4 in the text.

Comment: ====================

p.14904, l.18 - Change "larger" to "large".

Reply: We will modify the sentence.

Comment: ====================

p.14904, l.24-27 - Quantify the over-prediction here. Show a plot of the lat-lon distribution of the lightning NOx source in the model. The total source is low, so the distribution can cause a regional overestimate of NOx and O3 (if the distribution is wrong), but cannot cause a global-mean (or probably even zonal-mean) overestimate.

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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."

Comment: ====================

p.14904, 1-2 - How did you attribute these biases to excessive biomass burning emissions (vs. too efficient transport from the surface to 500 mb)? At what altitude are the biomass burning emissions injected? Integrate the discussion of Figures 5 and 6 to provide a more coherent picture of the biases in emissions and vertical transport in the model.

Reply:

Emissions are injected at surface (as discussed in section 2.2.4). We have found a problem with the application of the MOPITT kernel. Fixing this has led to an improvement of the comparison shown in Figs. 5 and 6.

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Comment: =====================

p.14907, l.23-24 - Has this bias in MOPITT CO been noted previously?

Reply: See above.

Comment: =====================

p.14907, l.27 - Clarify what lifetime you are referring to here. The lifetime of NO2 itself is typically only minutes during the daytime.

Reply: This is a typo as it should really refer to NOx not NO2.

Comment: =====================

p.14908, l.9-10 - SCIAMACHY does not measure the total vertical column directly. It measures the "slant column" of NO2, which must then be converted to a vertical column, using an "air mass factor" (e.g., Palmer et al., 2001) to account for the vertical distribution of NO2 and the vertical sensitivity of the detection method.

Reply:

We have corrected the text to reflect the process of extracting the NO2 column from the observations.

Comment: =====================

p.14908, l.10-14 - Explain. Are you subtracting a zonally uniform value for stratospheric NO2? Are you talking about longitudinal variability of *stratospheric* NO2 from CMAM?

Reply: Yes, "stratospheric" should be added to the sentence.

Comment: =====================

p.14908, l.21-25 - What was the magnitude of the tropospheric NO2 column in the reference sector (calculated using the tropopause method)? How do you know that the thermal tropopause method shouldn't produce values that are 25% higher than [..]

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Reply:

We chose the reference-sector method to be consistent with the method used with the SCIAMACHY data.

Comment: =====================

p.14908, l.27-28 - It is not obvious from the figure that NO2 is underestimated by an order of magnitude over China in September.

Reply: This (September) was a typo. The comment should refer to January.

Comment: =====================

p.14908, l.29 - Compare the NO2 biases shown here with those found for CO in the previous section to distinguish between lightning NOx biases and biomass burning biases. In general, throughout the paper, a synthesis of the results found for different species would add significantly to the scientific content of the paper.

Reply:

With the discovery of the kernel issue we will present a more coherent picture of NOx and CO emissions.

Comment: =====================

p.14909, l.5-18 - This comparison needs to be made much more quantitative. For instance, give the percent bias and correlation globally and in each region.

Reply:

We will present the correlation coefficients for the scatter plots. Given the heterogeneity of the NO2 distribution, it seems to us that presenting biases would be misleading.

Comment: =====================

p.14909, l.12-13 - This statement seems to apply only to China and Africa. I don't see any points in the range (1-2)x10ˆ15 in South America.

Reply:

The text as stated is perhaps to vague and we will modify it to be more specific. For example, it can be noted that the SCIAMACHY values for South America have a smaller variability than the GEM-AQ values.

Comment: ====================

p.14909, l.17 - Point out that this discrepancy is likely due to the use of 1990 emissions vs. 2004-2005 observations. By what percentage are emissions from China estimated to have increased over this period?

Reply:

We will add a sentence along these lines "GOME and SCIAMACHY see a 50% increase in NO2 columns over Eastern China in the period 1996-2004 (Richter et al., 2005)" to the text.

Comment: ====================

p.14910, l.3 - Why do you choose August 2001 model results, rather than Sept.-Oct. 2001 (or 2001-2005) for comparison here?

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."

Comment: ====================

p.14910, l.2-13 - In order to fully interpret the differences between the model and observations, it would be useful for you to describe the amount (and seasonality and

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location) of biomass burning in 1992 versus climatological values.

Reply:

We will mention that 1992 was an extreme year for biomass burning and we will refer to a recent paper by Schultz et al. (submitted to GBC, 2007).

Comment: =====================

p.14910, l.16-18 - Unclean. Do you mean NOx from convective transport (from surface sources) or NOx produced by lightning? Are you suggesting that this indicates insufficient convection or insufficient lightning NOx production?

Reply:

It was rather unclear, but it was meant to indicate that both a lack of convective transport and lightning may be responsible. We will reword these statements to ensure clarity.

Comment: =====================

p.14910, l.22-25 - Be more specific here. What biases are indicated by the compareisons with GOME and SHADOZ? Where is the model too high or too low, and by what amount? How do the treatments of deep convection and lightning NOx contribute to these biases?

Reply: We will present a more extensive summary/discussion.

Comment: =====================

p.14910, l.26-27 - Are the biomass burning emissions constant for an entire season, or do they vary monthly? (Presumably, there is no interannual variability in the model emissions.)

Reply:

Biomass burning emissions for all species are available as monthly averages and there is no interannual variability.

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Comment: ====================

p.14911, l.5-7 - Where in the paper was this sensitivity demonstrated? Quantify the sensitivity of model results to the height of emissions. You should mention the height of biomass burning emissions in the methods section, not here for the first time.

Reply:

The sensitivity issue is with respect to the paper in preparation. We show in this paper that mid-tropospheric injection of emissions results in better agreement with high altitude observations (aircraft campaigns and satellite data).

Comment: ====================

p.14911, l.8-10 - Can you quantify the stratosphere-troposphere flux of O3 in this model?

Reply:

This is discussed in section 4 on page14912, l.5-6, but we will move the discussion on metrics to a separate section.

Comment: ====================

p.14911, l.18-20 - Describe more specifically the biases found in these comparisons. I didn't think that the lack of year-specific emissions was the biggest problem identified.

Reply:

We did not mean to imply that this was the biggest problem. We will add some discussion of the weakness of deep convection in the present model configuration.

Comment: ====================

pp.14911-14912 - The discussion of CH4 and CH3CCl3 lifetimes and of the ozone budget should be moved to a main section of the paper (possibly a new section), rather than just in the Conclusions. These metrics are an opportunity to [..].

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Reply:

The lifetimes originally were in a separate section. As noted above, we will move the discussion on metrics to a new section and compare with other work (e.g. IPCC and Lawrence, M.G., Jockel, P., von Kuhlmann, R., What does the global mean OH concentration tell us?, Atmos. Chem. Phys., 1, 37-49 2001.

Comment: ====================

p.14912, l.1-3 - Are these CH_4 and CH_3CCl_3 lifetimes vs. tropospheric OH or total model OH? Using tropospheric mass or total atmospheric tracer mass? The most use full metric is total atmospheric mass divided by loss by tropospheric OH. IPCC TAR value gives CH_4 lifetime of 9.6 yrs vs. tropospheric OH, and 8.4 yrs including stratospheric and soil losses. The TAR used a CH_3CCl_3 lifetime vs. tropospheric OH of 5.7 yrs.

Reply:

We will re-calculate the lifetimes using tropospheric OH, along the lines of Lawrence, M.G., Jockel, P., von Kuhlmann, R., What does the global mean OH concentration tell us?, Atmos. Chem. Phys., 1, 37-49 2001. and will expand the discussion.

Comment: ====================

p.14912, l.3-7 - A recent paper [Wild, ACP, 2007] includes a discussion of the sensitivity of the calculated cross-tropopause O_3 flux to the tropopause definition used. You may wish to refer to that discussion in interpreting your diagnosed flux across the 200 hPa pressure level.

Reply: OK, will do.

Comment: ====================

Figure 2 - You don't need to show plots of the comparisons of temperature. You can just describe the level of agreement, and then show plots for O_3 only. Do these plots

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show the mean values for the 4 stations? If so, indicate this more clearly in the caption.

Reply: We will change the caption.

Comment: =====================

Figure 4 - Plot instead as a single (annual) timeseries for each station.

Reply: We have tried this and the plot was not legible even across two columns.

Comment: =====================

Figures 5-6 - Indicate units in the caption. The labels above each plot ("ppb") do not agree with the values shown in the colorbars (VMR).

Reply: We will modify colour-bars in these figures.

Comment: =====================

Figure 7 - Is the title for the upper right panel (b) supposed to say "SCIAMACHY NO2 tropospheric column - Sep 2004"?

Reply: We will correct the caption to read "January 2005".

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

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