Atmos. Chem. Phys. Discuss., 7, S7570–S7576, 2007
www.atmos-chem-phys-discuss.net/7/S7570/2007/

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.

Anonymous Referee #4

Received and published: 10 December 2007

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

This manuscript describes the simulation of global gas-phase tropospheric trace species in the new GEM-AQ model of meteorology and chemistry. 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. These comparisons with observations are necessarily

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

not comprehensive in a single paper of this length, but some effort should be made to describe whether these comparisons are representative. Also, some additional interpretation and/or generalization of the model biases/strengths should be included to turn these comparisons into a true model evaluation.

As currently written, this article would be a better fit as a Technical Note rather than a Research Article in this journal. Some additional explanation of the choices made for parameterizations in the model, and some additional interpretation and evaluation of the results would help to make this manuscript a meaningful Research Article.

Specific Comments ——————

Abstract – The abstract is pretty devoid of content. It should at least include some *quantitative* results, as well as qualitative descriptions of the model strengths/weaknesses (e.g., from evaluation vs. observations).

1. Introduction

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.

2. Modelling approach

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?

2.2 Air quality modules

p.14900, l.3-4 – Explain why tracers are transported by a different convective scheme

ACPD

7, S7570–S7576, 2007

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.

2.2.2 Aerosol package

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?

2.2.3 Gas-phase removal processes

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?

2.2.4 Emissions

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

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.

3.1 Ozone

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).

ACPD

7, S7570–S7576, 2007

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 under-prediction, how large was the variability in model and observations. The lack of quantification is a general problem throughout the paper.

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)?

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

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.

3.2 Carbon monoxide

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.

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

3.3 Nitrogen dioxide

p.14907, l.2 – Clarify what lifetime you are referring to here. The lifetime of NO2 itself is typically only minutes during the daytime.

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.

ACPD
7, S7570–S7576, 2007

EGU

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?

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 the thermal tropopause method shouldn't produce values that are 25% higher than the clean sector method? That is, do you have independent observations to validate the clean sector method, or could this discrepancy indicate problems with that method? Which version of the model columns agree better with SCIAMACHY? If the discrepancy between reference sector method and thermal tropopause method were due to a model bias in the reference sector, that would be a strong argument for using the thermal tropopause method instead (otherwise you would be subtracting off the wrong reference-sector value). So, why do you still use the reference sector method as your primary method?

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.

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.

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.

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.

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?

ACPD

7, S7570–S7576, 2007

3.4 Other species

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

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 location) of biomass burning in 1992 versus climatological values.

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?

4. Discussion and conclusions

p.14910, l.22-25 – Be more specific here. What biases are indicated by the comparisons 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?

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.)

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.

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

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.

pp.14911-14912 – The discussion of CH4 and CH3CCl3 lifetimes and of the ozone

ACPD

7, S7570–S7576, 2007

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 introduce some science into this paper (for instance, values can be compared with previous studies, and possible causes of discrepancies can be discussed). It would be useful to present a more complete O3 budget for the model, if possible.

p.14912, l.1-3 – Are these CH4 and CH3CCl3 lifetimes vs. tropospheric OH or total model OH? Using tropospheric mass or total atmospheric tracer mass? The most useful metric is total atmospheric mass divided by loss by tropospheric OH. IPCC TAR value gives CH4 lifetime of 9.6 yrs vs. tropospheric OH, and 8.4 yrs including stratospheric and soil losses. The TAR used a CH3CCl3 lifetime vs. tropospheric OH of 5.7 yrs.

p.14912, l.3-7 – A recent paper [Wild, ACP, 2007] includes a discussion of the sensitivity of the calculated cross-tropopause O3 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.

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 O3 only. Do these plots show the mean values for the 4 stations? If so, indicate this more clearly in the caption.

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

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).

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

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