Geosci. Model Dev. Discuss., 4, C705–C708, 2011
www.geosci-model-dev-discuss.net/4/C705/2011/

Geoscientific Model Development Discussions

Interactive comment on "Application of CMAQ at a hemispheric scale for atmospheric mercury simulations" *by* P. Pongprueksa et al.

Anonymous Referee #2

Received and published: 22 September 2011

The paper describes a hemispheric application of CMAQ-Hg. The model is run with two different sets of boundary conditions (BCs) from global model results, GRAHM and GLEMOS. The authors investigate the differences in the CMAQ model results in dependence of the BCs used for the run. They also show the differences in the CMAQ results depending on the initial conditions (ICs). The model results for 2005 are compared to observations at ground, and, to a larger extent, to CARBIC flights that took place between 2005 and 2007.

I have several major comments on the contents of the investigation and on the structure of the paper.

1. I would like to raise the question whether this paper fits into the scope of Geoscientific Model Development. It describes an application of an existing model (CMAQ)

on a hemispheric scale. As pointed out by Anonymous Referee No.1, an application of CMAQ-Hg on the hemispheric scale was already reported two years ago (Bullock, 9th International Conference on Mercury as a Global Pollutant, Guiyang, China, 2009 and Bullock et al, 8th Annual CMAS Conference, Chapel Hill, NC, October 19-21, 2009). No new model development is shown here. The paper may be better suited for ACP. The editor may decide on this topic.

2. The authors recommend the use of a hemispheric model to provide boundary conditions for further regional downscaling with the same model system instead of using BCs from a global model for simulations on the regional (continental) scale. This is clearly stated in the abstract, therefore the reader expects to find evidence for that in the paper. However, no comparisons with CMAQ runs on the regional scale are shown in this paper. I am well aware of the fact that the regional CMAQ model results were already presented in an earlier paper, but it is not clear if they are really comparable with this approach (e.g. has the same CMAQ version been used? Were the global model results the same?). If the main purpose of the paper is to show that a hemispheric application of CMAQ is favorable over a regional one, the authors need to show these improvements here.

3. The comparisons to observations presented in this paper to demonstrate the quality of the model results are worthless because they rely on model runs which are not performed in a proper way. As the authors demonstrate in Fig. 9, the CMAQ model results depend strongly on the initial conditions (ICs). Only after about 9 months of simulation time, the influence of the ICs becomes smaller than approx. 10 % (on the idealized cross section through the model domain used here). This means a proper model application needs a spinup time of at least 9 months (maybe better 1 year) to guarantee that the model is in steady state and the results do not depend on the ICs. This is an interesting finding and stated in the paper (page 1736, line 18), but it needs to be separated from the comparison of model results and observations. The authors should follow their own findings when comparing their model results to observations.

4. When investigating the influence of the ICs on the model results, this should not be shown along the flight path alone. The flight path is not representative for the whole model domain. The authors should also distinguish clearly between ICs and BCs and treat them in different sections. They should show more representative transects or maps in different altitudes to show the effects more clearly.

5. The comparison of CMAQ model results to CARBIC aircraft data is presented in a very confusing way. Additional to the problems mentioned in my major comment no. 3 above, Figure 7 and Figure 8 are hard to read and to interpret. It is not clear what the bullets in Fig. 7a really represent and how the underlying color map is constructed (colours in bubbles do not match the underlying colour). The flights comprise a period from of almost two years (2005 to 2007), as stated in line 26 on page 1733, but the figure is only for one year. If you included flights from 2006 and 2007 (what I would presume) it is doubtful to compare them to your simulations for 2005. As far as I understand the simulations rely on meteorological fields for 2005. In Figure 7a it is unclear how the high TGM values at 20 W - 20 E between day 100 and 170 were derived. There are only few data points showing such high concentrations. It is not apparent how an interpolation of the bubbles can produce these patches of high values. Fig 7c and 7d clearly show the influence of the ICs, which is probably the reason why the highest TGM levels are detected at the end of your simulation period. In section 3.3.2 you try to explain these “peaks” with vertical mixing into the upper troposphere (7-10 km altitude). From a meteorological point of view this vertical mixing under stagnant conditions in winter is not plausible.

Figure 7a shows datapoints with rather low TGM values. This may be caused by the fact that the aircraft was flying in the lower stratosphere. I would expect that the model is not able to properly represent the tropopause, the vertical resolution in higher altitudes is too low. Large differences between the model results and the observations can be expected for these cases. This is not discussed at all in the paper.

There are numerous difficulties in the paper connected with the English language and

the way the study is presented. This may lead to several misunderstandings. I will not comment on these individual points until the paper has been restructured. Despite the criticism outlined above, I think the study contains interesting science. I recommend the following:

The study should be restructured along the following lines:

1. You should investigate first what is the impact of the initial and boundary conditions on the hemispheric CMAQ model results? Which spinup is needed to get stable results? How far from boundaries is the influence of the global model results visible? This should be shown for the entire model domain.

2. Then, you can evaluate how this compares to the regional approach (same CMAQ version, same GLEMOS and GRAHM runs). This is widely missing in the current version of the paper.

3. In the end you can show how the properly derived CMAQ results (sufficient spinup!) compare to observations (ground based, CARIBIC). Here, you need to take into account detailed information about the flights (where was the plane, was it in the stratosphere?). Eventually, it would make more sense to show the comparison with line plots instead of the confusing colour map in Figure 7.

If that has been done and the paper has been carefully rewritten, in particular in terms of the English language, it may become acceptable for publication.

Interactive comment on Geosci. Model Dev. Discuss., 4, 1723, 2011.