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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimates of mercury flux into the United States from non-local and global sources: results from a 3-D CTM simulation" *by* B. A. Drewniak et al.

Anonymous Referee #1

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General Comments:

This paper discusses the use of the MOZART global atmospheric chemical transport model to assess the transport of Chinese mercury emissions to the U.S. This appears to be the first time this particular model has been applied to simulate mercury. As such, the authors need to focus more on demonstrating the realism of this new model. The only evidence of model accuracy given is comparisons of observed and simulated elemental mercury air concentration over one site in Ohio. The modeled data are relevant to the 950 mb pressure level while the observed data were taken at the surface. Little is known about vertical gradients of elemental mercury concentration in general, and

no such information is provided for this one site in Ohio. Thus, there is practically no demonstration of realism for this first attempt at MOZART-based mercury modeling. Unfortunately, there are some technical issues discussed in the paper that lead me to suspect the modeling is not realistic. The emission inventory used only includes elemental mercury emissions and there are problems with the gridding of emissions data for input to MOZART in other locations. This suggests to me that the model simulations were faulty from the start. The model does not treat aqueous-phase mercury chemistry. Given the general scientific uncertainties that all models suffer from in regard to atmospheric mercury chemistry, I can understand why the authors would want to keep things simple for their first try with MOZART. However, I am not aware of any published atmospheric mercury model that completely neglects aqueous chemistry. I am left with the overall impression that this work does not provide much new information that can be taken with confidence. It does provide something of a progress report on the addition of mercury simulation within the MOZART model, but I am troubled by the use of the current model for assessment purposes. I do not believe the assessment provided here should be published without some significant evaluation relative to observed data.

Specific Comments:

In the Introduction, use of the nomenclature (here and in following sections) of HGE for elemental mercury and HGO for oxidized mercury is confusing. The typical nomenclature is Hg(0) or Hg<super>0 for elemental mercury and Hg(II) or Hg<super>II for divalent oxidized mercury. Monovalent oxidized mercury compounds are generally unstable at atmospheric conditions. Plus, HGO could be misinterpreted as HgO (mercuric oxide).

In Section 2.2, it is stated that reactions of HGE with HCl and Cl<sub>2 are included in the model, but no reactions involving these species are listed.

In Section 2.3, in the description of the three simulation cases, case 2 (No China) is said to be <quote>Same as Pacyna but with anthropogenic emissions from China

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set to zero.<unquote> Then in the following paragraph it is stated <quote>The No China and the Streets cases had identical emissions except for China.<unquote> This implies that the Streets and Pacyna emissions are the same, but they obviously are not. A much more clear explanation is needed.

In Section 3.1, it is stated <quote>The variability of Hg concentrations in South Africa, Australia, and Europe (Fig. 5) is the result of differences in gridding the emission distribution input to MOZART.<unquote> This technical error needs to be corrected. It detracts greatly from the confidence the reader is going to place in the model simulations and suggests a haphazard approach to the entire modeling effort.

In section 3.2.1, there is no attempt to compare the model simulations of mercury wet deposition with the many observations taken as part of the Mercury Deposition Network (MDN). The MDN data provide what I believe is the best opportunity to show realism in the MOZART simulations. The authors need to show some evidence that their model matches these observations to some degree.

In section 3.2.2, it is stated <quote>Dry deposition patterns are very similar to the wet deposition patterns.<unquote> However, there are no model results shown to support this statement. I would assume that what is meant here is that the differences in dry deposition between the modeling cases are similar to the differences in wet deposition between the cases. If any atmospheric mercury model showed similar patterns for wet and dry deposition, I would be very suspect of that model.

In the Conclusions section, Shetty et al. (personal communication) is cited. It appears that the Shetty et al. work published at http://dx.doi.org/10.1016/j.atmosenv.2008.08.026 could be cited instead.

In Figure 5, for comparison purposes, it would help to use the same color scale and to show all seasons as is done in Figure 4.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 19861, 2008.