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Comment

## ***Interactive comment on “Simulation of atmospheric mercury depletion events (AMDEs) during polar springtime using the MECCA box model” by Z.-Q. Xie et al.***

**Z.-Q. Xie et al.**

Received and published: 4 October 2008

We would like to thank the reviewer 2 for his/her comments that have helped us to prepare this final version. The suggestions have been taken into account. Below is a point by point answer to the reviewer 2 comments.

Specific Comments: 1. It is well established that AMDEs are an annually recurring polar spring-time phenomenon that result in a deposition flux. However, whether the result is a net-deposition is still not clear: furthermore, recent research work shows that there is good scientific evidence that this may be doubted. This has been ignored by the authors and should be included in a revised version (see special issue /research front with a number of papers related to this question in Environmental Chemistry, Vol.

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5, 2008, doi:10.1071/ENxxxx)

We agree with the referee that the re-emission of deposited Hg potentially is an important process that needs to be addressed in order to get a correct global Hg budget. However, on the timescale of our model runs (a few days), we do not think that re-emission affects the conclusions that we draw from our model runs.

2. In the abstract and in the text body the authors use the term "destruction of Hg", which is not correct, since Hg is an element. Ozone can be destroyed, Hg(0) can be converted, oxidized or so

We have changed the incorrect term "destruction of Hg" to "depletion of GEM".

3. In the Introduction the authors cite work done in Antarctica. Sprovieri et al. (2002) and Temme et al. (2003) present speciation data from Terra Nova Bay and Neumayer in Antarctic summer, consequently outside the AMDE period. This should be stated more clearly.

Thanks. We have corrected.

4. The acronym MECCA should be spelled out when it occurs for the first time (3rd para of introduction)

The acronym MECCA is now explained in the introduction.

5. Reference to the work by Sander et al. (2006) "to study the role of carbonate precipitation" occurs twice in the intro and in the model description with the same word.

The same word in the model description has been deleted.

6. Since dry deposition was "switched off" in the model, possible implications of this assumption should be discussed more detailed (see Calvert and Lindberg and others)

Under the conditions of our model calculations, there is enough aerosol surface available so that uptake of RGM to aerosols dominates over dry deposition. Nevertheless,

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we agree that dry deposition is a potentially important process under different conditions.

7. Ebinghaus et al. (2002) present a cross-correlation of Ozone and GEM losses with a time lag of 15 min. This should be corrected in section 3.2, 5th para.

In the caption of their Fig. 5, Ebinghaus et al. (2002) state that the highest correlation coefficient is found at a lag-time of zero. The time of 15 min mentioned by the reviewer is only an upper limit because the measurements were made at 15 minutes intervals.

8. Fig. 1: The authors use the term "RGM" and identify a number of Hg-species that could be summarized as such. However, RGM is only operationally defined and one should avoid the impression that we really know what RGM is, in terms of speciation. What the authors say is not wrong, but the implied accuracy of the statement is misleading.

We updated the Fig.1 caption as: RGM refers to all gas-phase species in our model that contain mercury, except for elemental Hg.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13197, 2008.

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