Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-1165-RC2, 2017

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A new mechanism for atmospheric mercury redox chemistry: Implications for the global mercury budget" *by* Hannah M. Horowitz et al.

Anonymous Referee #2

Received and published: 9 February 2017

This is a great paper that represents great progress in the field of atmospheric chemistry. It definitely deserves publication. I have a few specific comments:

1. On page 2 lines 14-15, you talk about how oxidation by O3 and OH is unlikely. It is important, I think, that you bring out and discuss (or debunk, if they merit it) other oxidation/reduction mechanisms that have been proposed, including heterogeneous mechanisms. No one disputes that halogen-involving reactions are important. But there are some strong voices that claim other mechanisms may also be important, and I think it would be useful to give them consideration.

2. On page 2 lines 30-32, you suggest that in-plume reduction isn't important, and give the Deeds and Landis papers as references. Both papers show evidence for in-plume

reduction, and the Landis paper talks about how the process is likely widespread. Maybe I am misinterpreting your text or theirs, but I'm not sure the statement you make here is supportable by the evidence you have given.

3. I am curious about why in Figure 1 the model shows quite high annual mean Hgˆ II at the surface near the south pole and overall shows much higher Hgˆ II in the southern troposphere than in the northern. I think this is a surprising result, and I don't think it was discussed in the manuscript. I would be glad to see some explicit discussion of the reason for the north-south difference in Hgˆ II concentrations. Unfortunately, we have very few (if any) reliable Hgˆ II measurements with which to validate this result.

4. Page 7 lines 8-10: again, some discussion of how inclusion of heterogeneous oxidation chemistry might play a role could be useful here.

5. Page 10 lines 1-5: It is not at all clear to me that these modifications to GEOS-Chem "solve" the problem of underprediction of wet deposition in the Gulf of Mexico. Based on Figure 6, the model is still strongly underestimating Gulf of Mexico wet deposition. I don't think the model even gets the basic spatial trends right, based on my visual inspection of Figure 6. It may be fair to say that it is doing better than before, but to say it has it all nailed down seems to be an overstatement.

6. Page 11 lines 30-31: I don't agree that the "wet deposition fluxes in the model are consistent with observations." This assertion needs to be toned down.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1165, 2017.