

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 #4

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This paper describes advancements in the modeling of global Hg cycling through updated redox chemistry as well as coupling to a more sophisticated ocean model. The approach is thorough and defensible and the paper well-written. I recommend publication after minor revisions.

P5, l. 23-25: Can you add a summary of how accurate these OA concentrations are expected to be? If this isn't known, how does that impact your conclusions?

P8, l. 1-20: This discussion of the mean and SD of TGM concentrations underplays the spatial deviations shown in Fig. 3. For example, the modeled and measured N-S gradients in Europe appear to be in opposite directions.

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P8, l. 28-30: I suggest you cite Soerenson et al., EST 2013, or other observation-based papers to show that this mechanism is consistent with observations.

P9, l. 5-7: It sounds like you are suggesting that a similar analysis for the southern ocean is needed, but both the sea ice and the relatively large riverine flux relative to the Hg pool in the Arctic Ocean are not likely to have any relevance to these southern non-polar sites.

P10, l. 1-5: The figure indicates a -28% mean bias in North America, but in the Gulf of Mexico it appears to still be closer to -50%. Can you quantify the improvement in the wet deposition relative to Holmes et al. (or other models)? Consider adding a figure showing scatterplots of model vs. observations for both TGM and wet deposition.

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

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