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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Current and future levels of mercury atmospheric pollution on global scale" *by* Jozef M. Pacyna et al.

**Anonymous Referee #1**

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General Comments: This manuscript is the latest in a series of useful emissions inventories and modeling results produced through support of the Global Mercury Observation System (GMOS) project. The first part of the manuscript provides updated present and 2035 emissions estimates. Future scenarios consider a variety of uncertainties about trajectories of growth and implementation of air pollution control technology. The second part of the manuscript contains associated present and future deposition scenarios.

I have a few general comments that the authors can consider and try to incorporate into the manuscript text.

1) I found the information on how the emission inventories were constructed to be vague and lacking sufficient detail to enable reconstruction. This is a particular problem



right now as the authors point out because estimates across groups vary by a factor of 3. I wonder if the factor of three is overstated given convergence in estimates around the energy sector and then much larger uncertainties around artisanal and small scale gold mining. Given these uncertainties, presentation and availability of underlying data used to calculate emissions scenarios are essential and I think should be included in supplemental material accompanying this paper. The authors did not discuss other recent emissions estimates and compare/contrast with their own work and I think this would also be a helpful addition to the manuscript. For example, on page 4, the GDP per capita PPP and industrial goods relationships across countries.

2) I found the language on the GMOS project on page 2, first paragraph and concluding sections to be a distraction from the science presented in this paper. I think the authors should include text about the administration and goals/support of the project since this is not appropriate in this type of manuscript. I am supportive of mentioning GMOS as a successful endeavor but this does not seem like the right forum for the amount of text devoted to promoting it.

3) I don't think evasion of Hg0 from soils and the oceans should be referred to as "natural emissions" – this is propagating misinformation (and underestimating) the extent of human impact on the global Hg cycle. Other modeling studies suggest that these sources are now mainly reemitted legacy anthropogenic Hg. This is critical for understanding future mercury deposition scenarios as well and not mentioned in this paper. The actual present-day estimates for emissions from land and the ocean also do not seem to reflect our best present understanding of these sources and should be enhanced in the context of recent literature.

4) Related to point 3), the modeling approach presented assumes static reemissions from the land and oceans and this is a simplification of global biogeochemical cycle. It should be stated clearly as a limitation of the present analysis. See recent papers (Amos et al., 2013, 2014, 2015) for a thorough discussion of this and the implications of various uncertainties in the global Hg cycle.

5) The models applied here (GLEMOS and ECHMERIT) both use ozone and OH as the dominant global oxidants but this also seems out of date with our best present understanding. Br is mentioned but many additional oxidants are now on the table and a brief review of halogen chemistry and its implications for the global cycle and uncertainties in the context of this work also seem appropriate to include.

6) This manuscript is a bit light on citations – particularly the opening and closing discussions.

Specific editorial comments:

I would prefer to see the information on page 4 under "Database on activities" as a prose discussion rather than these bullets.

Page 4. Figure 1 does not seem very informative to me. Suggest adding to a supplemental section or deleting

Page 5 – since the ASGM emissions are such an important uncertainty, I would particularly like to see detailed information on how emissions estimates were constructed.

Page 9. Can Figures 5-7 be condensed at all?

Page 10. "prompt re-emission (Selin et al., 2008)" is a non-physical process intended to help tune the model. I don't think this should be a part of other models. The authors might instead consider the uncertainty around photoreduction and evasion, particularly over ice and snow covered surfaces.

Page 10. See general comments on oxidants. Shah et al., 2016, ACP is relevant here.

Page 10. The air-sea exchange scheme in DeSimone et al., 2014 is based on prescribed uniform ocean concentrations of elemental Hg so thus does not consider variability in ocean concentrations and changes in ocean concentrations as a function of time. This should be acknowledged.

Page 11. Discussion on terrestrial emissions seems out of date to me as well. Consider



synthesizing and incorporating information from recent diverse terrestrial Hg cycling literature and also global modeling (i.e., Smith-Downey et al., 2010).

Page 13. Future deposition scenarios should acknowledge static global reservoirs and also I assume the same meteorological years for future deposition? I think addressing biogeochemical variability is beyond the scope of this work but it would be good to acknowledge as an uncertainty – particularly given the rapidly changing climate.

Page 14. My impression was that volcanoes are actually pretty well constrained as a source. See Amos et al. (2015).

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