

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

Anonymous Referee #2

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In general, this is a thorough presentation of a new inventory for mercury and a test of its application in global modeling. It is a rather straightforward analysis, appropriately conducted. However, the paper doesn't really do enough analysis to identify new information of relevance to global mercury science. I would suggest that the authors revise to more strongly identify either 1) the differences between this and previous inventories, or 2) the ability of modeling to constrain what we know about anthropogenic mercury. I would also suggest that the authors be more specific about the scientific questions they are trying to answer.

Specific comments follow:

abstract: one can't 'assess' future concentrations. Rephrase here and elsewhere throughout the draft.

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page 1 line 26: re-emissions aren't "natural processes" – suggest using another word

page 2 line 1-5: this information seems like PR for the GMOS project and I think it is out of place in a scientific publication.

Page 2 line 10: "This contaminant does not degrade easily in the environment": Mercury, as an element, does not degrade *at all* in the environment. Rephrase.

Page 2, line 16: Ambio, 2007 is an incorrect citation

Page 2, lines 20-25: In a paper focusing on global mercury pollution, this introductory focus on activities in the EU seems misplaced.

Page 3, lines 9-13: These questions are all important, but none of them is convincingly answered in the paper (and in fact are too ambitious for any one paper). It would be helpful if the authors narrowed this scope a bit.

page 3, lines 17-20: I get that this study is EU funded, but this information really should only be in an acknowledgment.

page 4, line 4-6: Is the goal for this inventory to be comparable with AMAP/UNEP 2013 or is the method just consistent? This should be clarified, as previous AMAP/UNEP emissions inventories are not directly comparable.

page 8, line 18: IPCC acronym is incorrect.

page 8 line 22: this is the first reference to these acronyms (CP, NP, MFR) and they need to be explained

Section 2.5, 2.6: much more detail here is needed on the assumptions of these scenarios. In particular, the description of the 2035 scenario is not well described – it is unclear even which energy scenario was used for it, for example.

p 9 line 31: how is biomass burning treated in the new inventory?

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p 10: I would argue that the current evidence for the Br mechanism is stronger than the authors give credit to. However, from a global budget perspective, the choice of mechanism doesn't affect the question and results. I would suggest the authors focus on this point rather than try to defend an outdated mechanism.

page 10-11: If ECHMERIT has an online parameterization of natural and secondary emissions, shouldn't the results include at least the (shorter timescale) response of surface reservoirs? Clarify.

page 15: line 17-28: This text on GMOS is again over-the-top and not appropriate in a scientific paper.

Annex a: It would be useful to list quantitative mean values for measured and simulated Hg at these sites. Not all of these results have been published in the peer-reviewed literature, as far as I can tell

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