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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Worldwide trend of atmospheric mercury since 1995" *by* F. Slemr et al.

Anonymous Referee #1

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This paper presents surprising but well-documented insights about the time trends of mercury from long-term trends in the atmosphere, mostly based on two sites (Mace Head and Cape Point) but with additional data from ship cruises. The authors document well their conclusion that Hg is decreasing in the atmosphere, and suggest convincingly that neither primary anthropogenic emissions nor changes in oxidation can explain it. They suggest that re-emission of anthropogenic mercury is responsible, a hypothesis which could use more explanation and support in the paper. But this is not the main focus of the paper, which is mainly about diagnosing a substantial downward trend, and I suspect that nailing down the cause will be a long-term challenge for many Hg researchers!

The paper is clearly publishable in ACP, and I suspect it will make a strong impact on the global Hg community. One suggestion I have is that the authors revise the language to make the structure and content of the results section flow better and to highlight their

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major points. I would also suggest the authors pay attention to a few comments and suggested revisions as indicated below:

p 2357 line 17: what does "without any data selection" mean?

p 2357, line 22-23: "taking into account that anthropogenic emissions represent only about 1/3 of all emissions" – that's a great point. I initially thought when I read the abstract, why can't the emissions inventory be wrong? It might be helpful, to drive home the point (this statement is a bit oblique) to spell out the back-of-the-envelope calculation – that this means that the emissions inventories have to be wrong by 40-90%, and that the stated uncertainties are on the order of +/- 25% or so for most sources. This is indicated later, but could be usefully included up here in a concise way.

p 2360: section 2.2: can you indicate some quantitative information on the errors in these previous comparison studies, to put them into perspective with the signals you are deriving from the trends?

p 2363: "the rather narrow spread. . .and similar changes with time shown by Slemr et al. 2003" – it would be nice to expand on the time trends derived by the earlier paper. Am I right to conclude that this sentence means that the authors now think that the Slemr et al 2003 conclusions were incorrect? If so, a brief review of the general conclusions in the earlier paper would be helpful in a few sentences, and a judgment on which of the earlier conclusions holds up in the latest analysis. Perhaps also this paper could be referenced in the introduction to better signal what's new in the most recent analysis.

p 2364: the NH/SH comparisons are very interesting. I'm wondering if a simple box model approach would help illuminate and clarify the relative trends?

p 2365: unprecedentedly large for almost any trace gas: can you give an example from other trace gases to back up this statement and put it in perspective? How much are

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other gases changing at these sites?

p 2366-7: I think the authors are a bit too quick to dismiss the ocean. The changes are larger in the SH, and there's a difference in the fraction of land in the NH and SH, of course. How does this fit with the trends? What about the subsurface ocean (the authors might look at Soerensen et al 2010, ES&T)? This is where I think a box model analysis/back-of-the-envelope calculation might be helpful, perhaps with a figure.

Tables/Figures: I would encourage the authors to make tabular data available (perhaps as supplementary material) to help inform the development of the improved models they suggest.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 2355, 2011.