

Interactive comment on “Evaluating the effects of China’s pollution control on inter-annual trends and uncertainties of atmospheric mercury emissions” by Y. Zhao et al.

Anonymous Referee #3

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This is an extremely detailed study that provides important information about mercury emissions in China and recent time trends. It goes beyond previously-published studies by explicitly treating uncertainty in emissions with a Monte Carlo approach. In addition, the study extrapolates trends to 2030, putting together future emissions scenarios. However, it does not add to the understanding of the Hg problem in China, and gives no indication of whether this estimate is objectively better given our understanding of the environment and atmospheric chemistry. Thus, this paper may be more suitable as an emissions inventory development paper for a journal such as GMD?

The paper is lengthy, detailed, and extremely dense. In general, lack of English lan-

C9643

guage editing makes the text confusing, introduces error, and limits the ability of the reader to evaluate the science.

A major limitation to the paper which should be addressed in any revision would be the comparison to previous studies. The paper states (p. 26825): “the estimated China’s total Hg emissions in this work can hardly be compared directly with other studies” and gives reasons for this, but regardless of the methodological differences, this is critical for the reader to put this work into context. There are numerous global inventories which contain data for China for different years (2000, 2005, 2010). There are also several top-down estimates based on atmospheric observations. A more thorough discussion of how these results differ would be useful to the reader.

One substantial oversight is that the authors do not compare to or cite the recently-published 1970-2008 inventory of Muntean et al (Science of the Total Environment), of which three years overlap with the present work, and which takes a novel approach to mercury inventory development. In addition, Muntean et al. also introduce a methodology for projecting emissions from ASGM which the authors might find useful.

While there is substantial uncertainty analysis conducted in the paper, the authors do not well-integrate this with their main findings of trends and projections of future emissions. Given the large uncertainties, why should the reader believe these (relatively small in comparison) changes and projected trends? I think the authors have a method that can account for these effects (with the correlation analysis in the Monte Carlo analysis) but it is not stated clearly.

Also, the authors do not state whether they plan to make this emissions inventory available for modeling, which would enable quantitative evaluation of it vs. measurements.

A few more minor comments follow.

The “ASGM” acronym is not as commonly used (M=mining, usually)

p 26826 use of term “actual facts” is confusing

C9644

There are a lot of figures/tables (6 tables, 10 figures) – the authors should consider moving some of them to the supplementary information. In addition, the authors might consider shifting some of the technical details to SI and incorporating more discussion/literature comparisons in the main text.

Given the amount of primary data used, it is unclear in places what is new in this work vs. what has been previously published or collected together. A revision to the writing could help make the use of information from citations more clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 26803, 2014.

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