

Interactive comment on “Size distribution of atmospheric particulate mercury in marine and coastal atmospheres” by D. M. Feddersen et al.

Anonymous Referee #2

Received and published: 17 August 2012

I like this paper and it provides useful constraints on many parameters. Some editorial comments.

Line 23, pg 14593: GEOS-Chem model does not have the % contribution to deposition in the NE US as high as 40-65%. This seems a little steep. See Atmos. Chem. Phys. Discuss., 12, 2603–2646, 2012. Line 25-30, pg 14593. This sentence is very confusing. Are the authors saying that if one measures the fractions between 0.4 and 10 μm , the dominant sources of Hg will be the ones listed? Fig 8 and Fig 9, would be nice to see r^2 data (Line 30 page 14600). Line 11-12, pg 14601: Authors use the word “correlated” but no statistics are given. Line 16, pg 14601: Don’t understand the line “enhancements in HgP that were associated with those in hydrocarbons. You mean the data in Figure 9? Line 1, pg 14602: change “summer is at much” to “summer was

at much” in general when speaking about data that was collected in the past it should be referred to in the past tense. Line 20, pg 14601: again, when the word “correlate” is used, statistics should be provided. Line 26, pg 14601: was removal of the PM2.5 elutriator on the Tekran inlet also done at Thompson Farm? If not, why only one site. The discussion in general on comparison with Tekran instrument is not very succinct. I think the comparison is useful but the explanation of results is not clear. Line 17: pg 14602: Data “are” not “is”. Conclusions, pg 14603: This section is not complete. Conclusion #1 is too general to be of use. The results of the size distributions to distinguish between marine vs. continental sources of Hg should be summarized.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 14591, 2012.

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Interactive Discussion

Discussion Paper