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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigating sources of gaseous oxidized mercury in dry deposition at three sites across Florida, USA" *by* M. Sexauer Gustin et al.

Anonymous Referee #1

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This study analyzes Hg concentration and dry deposition data as well as supporting data collected at three monitoring sites in Florida. The Hg data includes surrogate-surface collected GOM dry deposition, passive-sample collected Hg concentration, and Tekron collected Hg concentration. Potential sources of Hg were discussed through the analysis of diurnal and seasonal patterns of Hg, emission inventories, correlations with other criteria pollutants, wind directions, and air mass back trajectories. The results are useful in understanding Hg sources and distributions and its potential inputs to various ecosystems in this region. There are places in the paper that needs clarification as detailed below.

1. P18288, lines 1-12: A large portion of the materials was background information and should be in the Introduction, not in the Abstract.

2. P18288, lines 20-23: The dominant sources or the relative contributions from each sources should be stated, if possible.

3. P18290, lines 3-12: I suggest moving the last sentence in this paragraph to the place right after the first sentence and also modify the statement to reflect the more recent results from Zhang et al. (2012) which showed that GEM dry deposition can be much more important than GOM+PBM over vegetated surfaces at rural locations in North America.

4. P18291, lines 11: Was GEM included in the dry deposition budget? Would the percentage contribution be much larger if GEM was included?

5. 18293, line 6: The definition of EGP should be moved to line 2 where it is first appeared.

6. P18295, lines 8-9: Four starting altitudes were used for back trajectories here. The measurements of Hg were made at the surface; why a starting altitude near the surface was not chosen?

7. P18295, lines 7-9: Did the different starting locations and starting heights affect the horizontal location and altitude of the trajectory points? Fig. 5 to 7 show GFDs for all trajectory starting locations and starting heights. Were there any discrepancies in the GFDs for the nine starting locations and four starting heights at the three sampling sites?

8. P18295, lines 13-15: What was the height of the HYSPLIT modeled boundary layer? It was mentioned that a high proportion of trajectory points were above this BL. Was this observed for all trajectory starting heights (500, 1000, 1500, 2000 m)? It would be better if you could show that the free troposphere transport affects lower elevations because previous studies seem to observe this at higher elevation sites only.

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9. P18295 lines 16-17: It states, the location probability represents the fraction of trajectory points in a given cell relative to the number of trajectory points in the most populated cell. Why was this relative to the most populated cell, instead of the total number of trajectory points for each event and class? How many trajectory points were there on average in the most populated cell? This could affect the probability of occurrence in panels a to d of Fig. 5 to 7. E.g., when it is relative to the number of trajectories in the most populated cell, the probabilities would be higher than using the total number of trajectory points for each event and class.

10. P18298, lines 17-21: If the electricity consumption also had the same diurnal pattern, then the direct emission from EGPs might have contributed to this diurnal pattern. Is there information available on the diurnal pattern of EGPs emissions to exclude this possibility?

11. P18298, lines 21-23: Do you have information of vertical profile of GOM concentration to support this statement?

12. P18300, lines 3-8: Is the discussion on Hg + Br reaction relevant to this section because several Hg modeling studies were also able to reproduce Hg measurements using the Hg + Br reaction and suggested that the Hg + O3 reaction kinetics was too slow? Also, since the sampling sites are near the Gulf of Mexico, could this be a major source of Br to the sampling sites and likely more conducive to the Hg + Br reaction?

13. P18302, line 11: In reality, dry deposition happens all the time and over all different surfaces. Frequently, dry deposition is faster during wet conditions than during dry conditions, especially for soluble species like GOM. This sentence needs to be stated differently, for example, the low dry deposition in summer as estimated from the surrogate surface measurements might due to the closure of SS during wet periods, not because the dry deposition was low during wet periods.

14. P18302, line 16: Increased GOM lifetime would not increase GOM dry deposition (long lifetime means slower deposition). The real reason here should be the increased

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concentration of GOM.

15. P18302, 20-29: A brief description would be useful describing how much percentage of the deposition variation was explained by concentration variation (so the reader can guess how much was caused by other factors such as deposition velocity).

16. P18303, the first paragraph: Can the differences in dry deposition between this study and that in Marsik et al. (2007) be mostly explained by concentration differences?

17. P18303, lines 27 to the end of the paragraph: The real reason causing the model underestimation of dry deposition in Peterson et al. (2012) and Lyman et al. (2007) was that too large surface resistance was chosen for GOM in their studies. For example, parameters chosen for HNO3 in dry deposition models were commonly used for GOM to estimate its dry deposition. The two scaling parameters (alpha and beta) were both chosen as 10 for HNO3 in Zhang et al. (2002) and these values were also used for GOM in Zhang et al. (2012). Using these values, the annual dry deposition velocity for GOM was on the order of 1 cm/s at the majority of AMNeT sites. However, a value of 2 was used for alpha and beta in Peterson et al. (2012). Thus the surface resistance for GOM was a few times (up to 5.0 depending on how large aerodynamic resistance was) larger than that in Zhang et al. (2012). This explains why deposition velocity in Peterson et al. (2012) was only a fraction of those in Zhang et al. (2012). If the proper values for alpha and beta were chosen in your study, the measured and modeled value would actually be very close. I suggest adding a brief explanation on this point so future studies can better choose right model parameters.

18. P18305, line 14: Delete the first 'measurement' word

19. P18306, line23: Should it be Fig. 5, instead of Fig. 7?

20. Caption of Figure 1 and Table 1: Is 'EGU' defined in the text?

21. Fig. 5 to 7: Are there any uncertainties with tracking the transport of GOM using back trajectories, since GOM can be removed by precipitation or dry deposition

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along the trajectory path? The modeled precipitation in these figures show there was precipitation upwind of the sampling sites.

22. Caption of Fig. 6: You might use this caption to save space: “Same as Figure 5 except for OLF site”. Same is true for Fig 7.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18287, 2012.