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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigation of processes controlling GEM oxidation at mid-latitudinal marine, coastal, and inland sites" *by* Z. Ye et al.

Anonymous Referee #3

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This paper uses a box model to study the controlling processes of GEM oxidation (or GOM formation) at different types of surface sites, and provides new and important information on the chemistry mechanisms of mercury that might occur in the real atmosphere. It fits well into the scope of ACP. I recommend the paper for publication after addressing the following comments. A major comment is that the box model simulation results should be compared against the measurements of PBM mixing ratios at these sites. This would help the interpretation of some controlling processes such as gas-particle partitioning in the model. Another general comment is that a more detailed description of the box model set up should be given in the paper, for example the exchange of GOM between the free troposphere and the boundary layer. A schematic

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can be very helpful for the readers to understand which processes are discussed in the model. The third general comment is that the paper discusses the importance of different oxidized mercury forms through their oxidation pathways. I suggest the authors also discuss the stability of these oxidized forms in the real atmosphere in the particular environment of each site. Specific comments: 1. throughout the paper: the use of the word “case” in this paper may confuse its readers, as it refers to both different observational days and different model simulations. For example, in page 9, section 2.2 “Case selection”, and in page 13, section 3.3 “Sensitivity analysis”. 2. Title: it would be better if the full expression of GEM (i.e. gaseous elemental mercury) is given in the title. 3. Page 4, line 25. Can the authors describe which parameter is used to account for entrainment from the free troposphere? 4. Page 5, line 12. I do not understand why the “GEM mixing ratios ... are set to be constant mimicking GEM emission flux”. What does this mean in the model? 5. Page 6, line 8. The numbers of reactions are incorrect. 6. Page 6, lines 11-18. The reaction constants for these aqueous Hg reactions should be given either in the main text or in the supplement. Also, I speculate these reactions are also highly uncertain. Do the authors consider the uncertainties associated with them? 7. Page 12, lines 1-9. These several sentences are confusing. At first, it is mentioned that “the patterns of diurnal variation were similar at the three sites”. Then, it is said that “PM showed negligible diurnal variation”. I suggest that a statistical method is used to quantitatively detect the diurnal patterns at all the sites. 8. Table 2: How uncertain are the simulated [Br] at TF and PM? What is the major source of [Br]? How is the concentration of Br2 set in the box model? In addition, are the boundary layer heights at AI and PM set to be constant? Do the authors expect any diurnal variations of the boundary layer height? 9. Figures: The figures throughout the paper should use a consistent way of uncertainty quantification, probably being consistent with the statistical method used for the observations (Figure 1). In the current paper, min-max, standard deviation, and box-whiskers all exist making the readers difficult to compare the uncertainties among these figures. In addition, I suggest the authors merge Figures 2, 3, and 8.

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