Responses to comments of Reviewer 2

We wish to express our great appreciation to Reviewer 2 for the careful review, supportive comments, and detailed editing. Your very careful comments have effectively improved the quality of our draft. In the revised manuscript, we have incorporated all your comments. In the response below, we address each of these comments. The Reviewer's comments are italicized and our responses immediately follow.

Specific comments:

1. Page 2, line 49: The term "reactive mercury", probably, does not reflect the nature of one of the mercury forms mentioned in the paper (HgII). Divalent mercury (HgII) occurs in the atmosphere both in gaseous and particulate forms. So it should be specified as reactive gaseous mercury (RGM) or gaseous oxidized mercury (GOM).

Revised: Correct it. As emissions to air, it should be RGM.

2. Page 3, lines 53-54: "Several atmospheric models such as GEOS-Chem, CMAQ, and HYPLIT have been developed and achieved successful simulations of tropospheric mercury against available observations" The list of contemporary mercury models is much wider. Some information can be found, for example, in (AMAP/UNEP, 2008; Pirrone and Keating, 2010; AMAP/UNEP, 2013)

Revised: We revise the text to include more mercury models and cited the reference that provide more detailed information.

3. Page 3, lines 72-74: "aircraft-based measurements in the upper troposphere and lower stratosphere show that ozone concentrations rapidly respond to the variation in concentrations of total mercury and reactive mercury, indicating the importance of the ozone-OH oxidation mechanism in the atmosphere" Co-variation of ozone and reactive mercury does not necessarily mean interation between them. It can result from similarity of depletion mechanisms of Hg and ozone in the lower stratosphere (e.g. in reaction with halogens) as it is mentioned by Lyman and Jaffe (2011).

Revised: We modify the text to agree with Lyman and Jaffe's conclusion as well as supporting our arguments.

4. Page 4, lines 88: "Anthropogenic sources take up a quarter of the current mercury emissions" There are different estimates of global mercury emissions (e.g.Mason and Sheu, 2002; Selin et al., 2007; Soerensen et al., 2010; Holmes et al.,2010; Mason et al., 2012). Given uncertainty of these estimates, I would say that anthropogenic sources take up from a quarter to one third of the current mercury emissions.

Revised: Revise it following your suggestions.

5. Page 8, lines 185-186: It has been shown by Gardfeldt and Jonsson (2003) that the reaction of aqueous Hg(II) reduction by HO2 should not occure in the atmosphere.

Revised: We noticed this issue and did some comparison experiments to see the effect of turning off HO_2 reduction on mercury wet deposition in model development stage. There exists uncertainty on this mechanism. Although our study shows that turning off HO2 reactions did not significantly change the wet deposition pattern, due to the abundant SO_3^2 in the environment,

there are some studies shows eliminating aqueous Hg(II)–HO_2 reaction may results in unreasonably high deposition and depletion of gaseous mercury in the domain [Lin et al., 2007]. Addressing this uncertainty may beyond the capability of this study. We may further discuss it with other researchers in a new study. However, we revise the text to present this doubt. Thanks for this reminder.

6. Page 9, lines 199-201: It is not clear what is the mechanism of the "uptake by the marine boundary layer" mentioned in the paper. Some more extended description is needed.
Revised: We revise the sentence to better interpret this. It is calculated based on air-sea exchange of mercury compounds.

7. Page 10, lines 220: I wonder why the authors refer the global mercury emissions inventory for 2000 to the Global Emissions Inventory Activity (GEIA). To my knowledge GEIA participated neither in preparation nor in distribution of this dataset. There is no mentioning of GEIA in the paper by Pacyna et al. (2006) cited in the text.
Revised: Yes, we double checked. Our data is from AMAP. We correct this point. Previously we got that GEIA Hg emissions is from Pacyna. So we write it in this paper. Reference to http://gcmd.nasa.gov/records/CANADA-CGDI_Canada_GEIA_MercEmm.html

8. Page 13-14, lines 302, 308, 309: "The most-polluted area" Here and after the authors use term "pollution" when talking about TGM. Mercury is not an air pollutant. Background concentrations of TGM are to low to have any adverse impact on human health or ecosystems. The main exposure pathways are through deposition to the surface and accumulation in biota. So it is more correct to refer pollution levels to atmospheric deposition.
Revised: We understand the mercury health impact process. Here we use the pollution means the adverse effects/modification to original atmospheric environment. It is more from the effect of anthropogenic emissions.

9. Pages 15-16, Figures 2, 3: It is evident that the model significantly overestimates the observed TGM concentrations in the Southern Hemisphere and does not reproduce the inter-hemispheric gradient. Some discussion and explanation of this fact is required.
Revised: As one reviewer commented, the overestimate on TGM level in the southern hemisphere is due to the anthropogenic emission inventory. The inventory overestimates the Hg emissions from South Africa. We have adopted this explanation and revise the text.

10. Page 19, line 424: "The additional bromine reactions will accelerate the transformation of Hg(0) to RGM, the major part of which will stay in TGM" The major part of RGM will not stay in TGM but will quickly (within hours) removed by precipitation or taken up by the surface.
Revised: It is an expression issue. We mean that the RGM is counted into TGM before deposition.

11. Page 19, lines 428-429: "Bromine may also increase the TGM in some places where the reduction reactions become more significant" It is not clear how bromine as a strong oxidant can increase TGM concentration anywhere.

Revised: We just shows the possibility that introducing bromine chemistry together with ozone-oxidation mechanism are possible to increase the mercury reduction. But it is not shown in our experiments. I modify the text to better express our meaning.

12. Page 19, lines 431-432: Including the bromine chemistry should significantly affect wet deposition. Therefore, it would be reasonable to study this effect in comparison with the wet deposition measurements.
Revised: We compare wet deposition observations from NADP with our single ozone oxidation mechanism in the following text. It shows that pattern and concentrations are close. Therefore, the adding Bromine Chemistry result (wet deposition) will definitely be larger than measurements.

13. Page 20, line 443: There are up to 35 MDN sites measuring wet deposition over the period 1999-2001. Selection of 26 sites is not evident.
Revised: We noticed that more sites have data during this period. The selections of 26 sites are based on geography distribution and data continuity. For example, on North Carolina, there are several sites have observations. We use the site with longest measurement during this period.

14. Page 22, Figure 9: As seen from the figure the model predicts strong vertical gradient of TGM at 60N latitude. TGM concentration is below 0.4 ng/m3 at the heights upper than 700 hPa (ca. 3 km). On the other hand, many aircraft measurements showed relatively even distribution of TGM within the lowest 5-7 km (see Swartzendruber et al.,2009 and references herein). It needs some discussion/explanation in the paper.
Revised: As pointed out by reviewer one, the vertical axles may not consider the terrain in plotting. Therefore, I replace this figure with a result from sensitivity experiment to interpret the transpacific transport. In the new figure, terrain correction has been done in plotting the figure.

15. Pages 28-42: A lot of references are missed in the reference list.
Revised: We search and find out more associated reference and add to this version.