

Interactive comment on “Long-term monitoring of atmospheric total gaseous mercury (TGM) at a remote site in Mt. Changbai area, northeastern China” by X. W. Fu et al.

X. W. Fu et al.

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Received and published: 23 April 2012

Anonymous Referee #2 Received and published: 1 March 2012 Comment: Overall the paper is relevant to the atmospheric community but requires some significant modification before being published. My comments on this paper are as follows:

Reply: The authors thank the anonymous reviewer for dedicating the time to read our paper and provide comments. We studied these comments carefully and revised the manuscript following of the comments. All of the revisions and corrections were marked with red in the revised manuscript.

Comment: 1. Two years is not a long term study. This needs to be readdressed in the
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text as such.

Reply: We changed the title to ‘two years of measurements of...in Mt. Changbai area, Northeastern China’

Comment: 2. One overall issue that needs to be addressed when comparing the 2 sites and indicating a change over time is the site characteristics. From my reading, Mt Changbai is in a forested area – what is the differences between these 2 sites (in terms of foliage etc) if any. Figure 1 is unclear and doesn't provide the reader a good sense of the differences between the locations. It is challenging to read and needs to be of better quality. Was there ever simultaneous measurements at both these sites in order to ensure comparability of the 2 data sets?

Reply: We added better quality figures for showing the locations of the sampling site CBS and S2, and also describe the difference of the two sampling sites in line 20-24 on page 4.

Comment: 3. In section 2.2 you indicate that the surrounding areas were characterized by flat terrain and thus the meteorological data is comparable. This needs to be defended and also is confusing as I thought both sites were forested and measurements were above the canopy.

Reply: It should be noted that there was no meteorological parameters were observed at the site CBS. But there is a standard meteorological station at S2. Since the two sampling sites were not very far from each other and the relatively flat terrain the study area, we used the meteorological parameters at S2 to do wind rose and wind dependence discussion at CNS.

Comment: 4. TGM is considered RGM + GEM. If you have a filter on the inlet on the outside of the sample line, are you sure you can measure TGM? The sample line is very long, is it heated all the way to make sure that all the RGM reaches the instrument? This needs to be addressed in the experimental section.

Reply: We made a detailed explanation why we refer atmospheric Hg measured here to TGM in line 20-27 on page 5.

Comment: 5. What was the data treatments? Was the data quality assured? If so, by what process? How did you determine the detection limit of 0.15ngm-3 as stated in section 2.2?

Reply: We showed the QA and QC information of this study in line 1-6 on page 6. The detection limit was calculated from the Tekran manual. It was suggested the detection limit of Tekran 2537A is about 0.1 ng m-3 at the flow rate of 1.5 l min-1. Based on the flow rate of 1.0 l min-1, we estimated the detection limit of the present study to be 0.15 ng m-3.

Comment: 6. Section 2.3 discusses the PSCF analysis – I am not an expert in this area and thus am assuming that this is a reasonable approach. Please refer to comments, if any, from the other reviewer on this subject.

Reply: Yes, we made revisions following comments regarding PSCF calculation and discussion

Comment: 7. Figure 2 looks like you plotted the raw data. Need more details on what this data is: :5 min, 30 hourly averaged?

Reply: Figure 2 shows the 5-min averaged TGM concentrations during the whole study period.

Comment:8. Section 3.1. The TGM data look more than intermittently going above the 1-2 ng m-3 northern hemispheric range (which needs to be referenced).

Reply: We added references to the revised manuscript in line 13 on page 7.

Comment:9. The mean TGM is lower over this time period when this data was collected. Were the other measurements that you compare this data to collect around the same time period, same seasons etc?

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Reply: We explained this issue as shown in reply to the previous reviewer's comment

Comment: 10. Please reference the remote areas in Europe and North America that have average annual concentration from 1-2 ng m-3? Slemr reported 1.7 in 2003 and has updated it to be lower more recently. The range is important to discuss, the median and maxima as well. It's a bit misleading to compare this data to background sites in other areas without considering all the data and not just the mean for a certain time period.

Reply: We added some discussions on the median, maximum and range of the TGM concentrations in sect. 3.1 of the revised manuscript, which were marked with red. Some of the important references in Europe and North America were also cited in the revised manuscript.

Comment:11. I am surprised that there is no discussion of TGM uptake by the forest. This could be a wonderful discussion and rationale for differences between this and other sites in the country. This should really be added into the discussion of this data.

Reply: We made a discussion on the adsorption of TGM to forest foliage in line 16-22 on page 8.

Comment:12. Have you done back trajectory analysis to look at source areas and specific plumes from Korea? (section 3.2)

Reply: The PSCF results were obtained from a synthesis evaluation of trajectory and TGM events. Many of the trajectories with elevated TGM levels were mainly found in summer during the study period.

Comment:13. page 4426 – I don't understand the relevance of opening season (Figure 5)? Where is Baihe? Please indicate what sources there are and what the context of this local town is to the data.

Reply: We explain the opening season in line 18-20 on page 10. The location and sources of Baihe two were shown in line 6-8 on page 5.

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Comment:14. Figure 5 should be cleaned up in terms of the x-axis; properly label the times and indicate data averaging in the title

Reply: We revised this figure on page 24.

Comment: 15. Page 4427 – line 5. Before you had said there was no seasonal variation. Please clarify.

Reply: Revision was made as shown in line 22-23 on page 12. We changed it to “there is no consistent seasonal pattern during the calendar year of 2008-2009 and 2009-2010.

Comment: 16. Page 4427 line 19 – what are the PSCF factors? Have they been specified in the text? Figure 4 is not clear – needs to be of better quality.

Reply: We defined the PSCF factors in line 21-22 on page 6. The detailed information of how to calculate these factors were given in sect. 2.3 on page 6 and 7.

Comment: 17. page 4429 line 5. I'd like to see this looked at in more depth as this is likely an important issue with regards to this site

Reply: We added some discussions on this issue in line 16-22 on page 8

Comment: 18. Page 4428 - line 9-in reference to Figure 7, why is the summer so different than the other seasons for the diel cycle (diurnal is reflective of solar radiation, diel is reflective of time of day)

Reply: We speculate some reasons for this different diurnal pattern observed in summer in line 16-21 on page 12.

Comment: 19. Figure 8 needs to have the x axis cleaned up and more descriptive in the figure caption.

Reply: Figure 8 was revised as shown on page 27.

Comment: 20. page 4430 – line 12 – I don't really see how this paper shows that this

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is indicative of remote background concentrations. You haven't convinced me of that.

Reply: We delete this sentence in the revised manuscript.

Editorial comments:

Reply: We thank the anonymous reviewer for dedicating time to check the English writing in the manuscript. We read the manuscript many times and make many revisions to the English grammar and writing. Some grammar errors aroused by reviewers were all corrected in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 4417, 2012.

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