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Atmospheric Chemistry and Physics Discussions

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.

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We thank the anonymous reviewer very much for dedicating his time to read the manuscript and present important issues of the manuscript. We carefully studied these comments and revised the manuscript. Replies to these comments are listed separately as below.

Comment 1. An Ecosystem Research Station can be expected to measure also other parameters in addition to atmospheric mercury but only mercury measurements are presented. In this the paper is accompanied by many other papers on mercury measurements and thus the authors should not take this criticism personally. I would like

to emphasize only that mercury is just one of the many atmospheric trace species and its relations with the other ones can help us to improve our knowledge of atmospheric mercury cycle.

Reply: It is really a limitation of this manuscript without any comparison between Hg and criteria pollutant data. Indeed, there are continuous measurements of CO2and O3 at the sampling site S2. We have tried to apply for the use of these data sets since 2007, but unfortunately we are unable to get the authority of using these data sets.

Comment 2. The 2 year-long measurements might have been termed "long-term" perhaps 15 years ago. However, since about 1995 there is an increasing amount of sites with continuous mercury record over 5 years and more, albeit not many in China. By this measure I think that the designation of presented measurement as "long-term" is a little bit overdone.

reply: We changed the item of 'long-term' to 'two years' in the revised manuscript

Comment 3. For comparison with other data sets it is not unimportant what standard conditions are used: standard pressure of usually 1013 hPa, 273.14 or 293.14 K or what? This is being frequently forgotten but should be clearly stated in the experimental section of each paper on mercury measurements. Are these the same standard conditions as used by Wan et al. (2009)?

Reply: We provide this information in line 25-26 on page 5.

Comment 4. The comparison of measurements with measurements starting in August 2005 and ending in July 2006 needs more elaboration because the data are far from being normally distributed. An additional comparison in terms of medians and their standard error or percentiles which are less dependent on extreme values might be more revealing. As it is now, the comparison is blurred by the pollution events and their frequency.

Reply: We presented the mean, median, and ranges of TGM concentrations at the

sampling site CBS. The mean concentration at S2 cited in this manuscript was a geometric mean value during the calendar year of 2005-2006. This value should be less dependent on some extreme values. Both the median and geomean values at S2 was two-fold higher than the present study.

Comment 5. The authors explain the difference between the 2008-2010 and 2005-2006 data sets, among others, by local sources, partly from the town of Baihe, 4 km from the sampling site of the 2008-2010 data. The distance of Baihe to sampling site of the 2005-2006 data of 5km (Wan et al. 2009) should be stated. Measured by distance, the influence of Baihe on 2008-2010 data should be larger than on 2005-2006 data.

Reply:The distance reported in the previous study by Wan et al. (2009) might show the distance between the S2 and the centre of Baihe town, and it is not very correct. Actually, the distance from S2 to the edge of Baihe town was about 500 m; whereas the distance between CBS and the edge of Baihe town in the presented study was about 1.4 km. We showed this information in line 1-2 on page 5. For the discussion of impact of Hg emissions from Baihe town, we made a detailed clarification in line 23-27 on page 10.

Comment 6. The discussion of the difference in terms of change of local climatology is not correct because three years of measurements are too short to establish a climatology of any site. The difference in frequency of pollution events between the two periods would probably be the more accurate description.

Reply: The declaration of the change of wind system here is really overestimated. We changed the item of 'wind system' to 'the change of relatively frequencies of surface winds' throughout the manuscript.

Comment 7. The monthly variations shown in Fig. 8 display monthly averages and standard deviations and thus suffer from the same problem of highly skewed distributions which are discussed above. An additional display of e.g. 10th percentile or other parameters less dependent on extreme values might be preferable.

Reply: We drew a new diagram for showing the monthly means of TGM at CBS. This new figure shows the 10th, 25th, 50th, 75th, and 90th values of TGM at CBS.

Comment 8. Table 1 shows that depending on season 10% of the mercury concentrations are below 0.9 – 1.16 ng m-3 and some measurements even below detection limit in summer and autumn. Wan et al. (2009) also observed concentrations approaching the detection limit of the instrument but their 10th percentiles were never substantially below the hemispherical background. The much lower 10th percentiles reported here are far below the hemispherical background and thus deserve a detailed analysis because they can potentially provide information about some hitherto unknown mechanism for mercury removal from the atmosphere. Are these low concentrations single points or events covering certain periods? If events, what is their meteorological characteristics, backward trajectories? Are they real or artifacts? Could they be the reason for the difference between the 2005-2006 and 2008-2010 data? An answer to these questions might be difficult without supporting measurements stressing the discussion in point 1.

Reply: We indeed observed this kind of atmospheric TGM depletion event at the study site of CBS. This depletion event was observed every year but only during summer time from June to late September since the year of 2009, and this kind of depletion was most pronounced in the year of 2010 with many TGM concentration lower than the detection limit of Tekran 2537A (0.15 ng m-3). It is also certain that this depletion only occurred during the night under the easterly flows. However, we are not very certain what potential mechanism it is. As suggested in line 12-17 on page 12 in the revised manuscript, intrusions of air masses from free troposphere might be probably the major reason, but foliar adsorption might be also a possible explanation for this kind of depletion. Poissant et al. (2008, JGR) also reported some very low TGM concentration (<0.5 ng m-3) in a maple forest and attribute this to vegetation leaves uptake. We have tried to use trajectory analysis to study the origins of air masses related to these depletion events. But maybe due to the limited meteorological data

in this region, we were unable to realize strong evidence of downslope air massed from troposphere. Hence, it may be difficulty for us to make some further explanations in this manuscript. We planed to do some further works with regard to this specific TGM depletion in these years, and we expect to have more clear explanations for this mechanism.

Editorial remarks:

Comment: Although generally well written, the paper still requires editing by a native English speaker. The suggestions below are only a few examples.

Reply: We carefully check the writing and grammar throughout the manuscript and made a lot of corrections. The following errors aroused by the reviewer were corrected with marks of red letters.

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