

## ***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.***

**Anonymous Referee #1**

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The authors present 2 year long measurements (October 2008 – October 2010) at a remote site in Mt. Changbai area. Based on these data they describe the diurnal and seasonal variations and try to pinpoint the possible sources of mercury using the potential source contribution function analysis. They also compare their measurements with measurements made between August of 2005 and July 2006 at essentially the same site. They conclude a) that the average concentrations at the site compare well with northern hemispheric mid-latitudinal background, b) that the climatology of the site changed since 2005-2006, and c) they pinpoint several sources which might be responsible for the elevated mercury concentrations.

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The paper is generally well written but has several weaknesses detailed below:

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.
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.
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)?
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.
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.

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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.

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.

8. Table 1 shows that depending on season 10% of the mercury concentrations are below  $0.9 - 1.16 \text{ ng m}^{-3}$  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.

Editorial remarks:

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

Page 4418, line 18: “controlled” instead of “regulated”

Page 4419, line 18: What do the authors mean with “convoluted”?

Page 4421, line 22: Perhaps “the analyzer was calibrated automatically” might be bet-

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ter.

Page 4424, line 15: The authors mean “Starting on . . . and ending on, Wan et al. . .”. The use of “During” is wrong as it restricts the measurements to just the two days.

Page 4430, line 14-17: “We found that the regional surface wind system changed significantly. . .” is a climatological statement for which the data basis is too short (30 year averages are the usual definition of climate). “The observed difference can be explained by different frequencies of winds from. . .” is more appropriate.

Fig. 2: The figure shows a line during the periods without measurements. It should be removed.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/12/C329/2012/acpd-12-C329-2012-supplement.pdf>

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