

Interactive comment on “Observation and analysis of speciated atmospheric mercury in Shangri-la, Tibetan Plateau, China” by H. Zhang et al.

Anonymous Referee #3

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This manuscript provides speciated atmospheric Hg data collected at a high-elevation site in China. As this site is located in a region that could see air flow from both East Asia and South Asia, I consider these Hg data are valuable. Nevertheless, these data are not well analyzed and presented in this manuscript so I saw little advance in improving our understanding of regional Hg transport and cycling. Some statements or conclusions made by the authors are not back up by data. For example, the author suggested that the high TGM value observed in June–July was caused by the biomass burning activity in the Indochina Peninsula because air masses were coming from this region to the sampling site. However, the major biomass burning season in the Indochina Peninsula is spring. In June–July, biomass burning activity is very low or not existing. In fact, the authors did not provide any data to demonstrate the influence

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of biomass burning activity on air quality at the sampling site. Therefore, I think this manuscript needs a major revision before being considered for publication in Atmospheric Chemistry and Physics. Below is a list of comments and suggestions:

1. Page 11041, line 15: “Backward trajectory analysis of air masses associated with TGM levels . . .” This sentence is not clear. What TGM levels are talking about? High levels or low levels?
2. Page 11043, line 2–5: Wide range of background concentrations of GEM, GOM and PBM are cited for remote sites. However, I won't consider a remote site background if it often sees GEM concentrations greater than 2 ng/m^3 , considering the Northern Hemisphere background GEM level is $1.5\text{--}1.7 \text{ ng/m}^3$.
3. Page 11045, line 3–4: In this sentence, it said that Kunming city is $\sim 650 \text{ km}$ northwest of SAWRS. However, it is clear from Fig. 1 that Kunming city is located to the southeast of SAWRS.
4. Page 11045, line 6: I will say that Southeast Asia (e.g. Indochina Peninsula) is located to the south, instead of southeast, of SAWRS.
5. Fig. 1: Font size of the city names is too small to read.
6. Page 11045, line 9–10: What is the source of the Hg emission data?
7. Page 11045, line 13–14: Is the Tekran 2537A set up on the roof of a building?
8. From Fig. 2 I can tell that TGM was monitored between December 2009 and November 2010. However, it is not mentioned in the section of “Sampling methods and analysis”.
9. Page 11046, line 3: Is this the denuder-based system a manual system separate from the Tekran 2537A or is it connected to the 2537A (but it is not Tekran 1130/1135)? The whole description about this system is very confusing. Please provide a detail and clear description.

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10. Page 11046, line 13: The inlet of the denuder-based system was 1.5 m above ground, but the inlet of the TGM system was 10 m above ground. So the TGM system and the denuder system were not measuring speciated Hg at the same elevation above ground?
11. Page 11047, line 15-16: What is the range of IMI values? Could it be a negative value? How to decide a value is high or low? Higher values indicate stronger westerlies?
12. Page 11048, line 13-18: It seems to me that line 16-18 should be put in front of line 13-15. Please check.
13. Page 11049, line 2: What is n_{ij} ? Is it the same as N_{ij} ?
14. Fig. 2: The highest GEM value looked suspicious. What's the data QA procedure?
15. Page 11049, line: The authors compared their mean GEM value to those of several other mountain sites also in China. I think this comparison should be extended to cover mountain sites in other region of the world, such as the Mt. Bachelor Observatory and Storm Peak Laboratory in the USA (Jaffe et al., 2005; Obrist et al., 2008) and Lulin Atmospheric Background Station in Taiwan (Sheu et al., 2010).
16. Page 11050, line 6: The mean GEM at Cape Hedo is lower, not higher, than that of the SAWRS.
17. Page 11050, line 9-10: Any analysis to support your argument?
18. Page 11050, line 18-19: I don't understand the authors' logic. Why will the air masses have lower TGM concentrations simply because they need to cross high mountains?
19. Page 11050, line 19: Figure "5" should be Figure "4".
20. Page 11050, line 24-28: Any data to support the existence of a diurnal valley breeze system influencing the sampling site? The diurnal pattern of RH or water vapor

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mixing ratio should be included in Fig. 4 because this may be helpful for the identification of the valley breeze system.

21. Page 11050, line 25-26: In fact, air near the valley, not mountaintop, heats up faster in daytime.
22. Page 11051, line 1-7: This argument needs to be supported by data. Otherwise, it's not convincing.
23. Page 11051: I think the authors need to perform statistical tests to see if the seasonal differences in TGM, GOM and PBM concentrations are statistically significant?
24. Can this seasonal difference in TGM be due to seasonal difference in natural emissions? Any evidence to say that natural emissions is not a factor here?
25. Page 11051, line 22-23: "... , a general negative correlation between ...". Please perform a correlation analysis and show the correlation coefficient.
26. Page 11051, line 23-24: Why the higher RH in the summer months might have caused the lower TGM concentrations? Any scientific evidence or reference?
27. Page 11052, line 4-16: "... , likely of biomass origins". Well, the major biomass burning season in the Indochina Peninsula is SPRING, especially in March. Therefore, even though these authors observed high PBM with air masses from the Indochina Peninsula in FALL, it is very unlikely to be due to biomass burning. Any data to indicate the air quality at the sampling site was influenced by biomass burning?
28. Page 11052, line 23-24: I can't understand this sentence "The values IMI represent seasonal rainfall anomalies, ..."
29. Fig. 8-13: What are the altitudes of these trajectories? If an air mass passes an anthropogenic Hg emission source region at a high altitude, its Hg concentration may not be influenced by this anthropogenic emission.
30. Page 11053, line 7-11: Again, biomass burning is unlikely to contribute to the high

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TGM in June-July because it is not the right season.

31. Page 11053, line 11: Figure “1” should be “10”.

32. Page 11053, line 18: “. . . from North Africa and Siberia, . . .”. I don't see this line has passed Siberia.

33. Page 11053, section 3.4: Any statistical test performed to see if these cluster mean TGM values are statistically different?

34. Fig. 12 and 13: Directions of airflow in Fig. 12 and 13 do not look very different. Can the authors explain in detail how they are different?

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