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Comment on acp-2022-367

Anonymous Referee #2

Referee comment on "Measurement report: Atmospheric mercury in a coastal city of Southeast China – inter-annual variations and influencing factors" by Jiayan Shi et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-367-RC2, 2022

The manuscript by Shi et al. measured GEM concentrations in January and July in five individual years from 2012 to 2020, and the data were used to explore the potential factors controlling the inter-annual variations. Long-term measurements of GEM concentration are an useful tool for assessing of controls of regional anthropogenic emissions and global changes, and thus the data presented here are valuable. In the present study, the authors combined the multiple approaches including the analysis of GEM concentrations, criteria pollutants, backward trajectory and generalized additive model. I agree that it is practicable and relevant for using these kinds of methods to explore the controls in the change of atmospheric GEM. The manuscript is overall well organized, and can be read easily. I broadly agree with the discussions and findings of this manuscript. I therefore suggest a minor to moderate revision of this manuscript before final publication in ACP.

As mentioned at the beginning of the manuscript, the major objectives of long-term observations are to evaluate the changes in anthropogenic emissions, that is an important part for the implementation of the Minamata Convention on Hg. However, after a comprehensive analysis, the authors mostly highlight that the changes in meteorological conditions were the most important variable in controlling the long-term trend in GEM. This is valuable, but not very striking findings to me because it is well accepted that variations in GEM among different short periods (e.g., monthly) could be impacted by changing atmospheric transport (air transport would change with different periods and subsequently affect the source-receptor relationships). Thus, I would suggest the authors to focus on the impact of changing local and regional anthropogenic emissions and climate on the trends in GEM concentrations, which would better serve for their research objectives.

I am not clear why the meteorology is the major divers of changing GEM concentrations, and it also difficult to differentiate the impacts of meteorology, transmission, and emissions. I suspect that the transmission should be related to meteorology because the changes in local and regional meteorological conditions would further affect the

transmission. Would the meteorology change land surface emissions and or atmospheric reactions that further affect the GEM? In addition, several previous studies reported declines in GEM concentrations in eastern China. Would this be an important cause for the changing contributions from transmission and meteorology? Thus, the authors may better define the three factors, which would help to better understand the real causes for the changes in GEM concentrations.

Line 144-145: why did the authors only conduct a two-month observations at the sampling site? Why not conduct a year of continuous observations for the selected years? A two-month observations in one year are sometime not adequate for assessing the inter-annual variations because of many factors mentioned in the manuscript.

Line 166-167: the definition of local impact relating to air mass within a province might over-estimate the local effect. Why not define the local impact within the city?

Line 198-204: CO is mainly sourced from anthropogenic emissions but has a long atmospheric residence time, it may therefore a best proxy of local anthropogenic emissions. I would suggest the authors to consider using SO2, NO2, or PM10 to define the anthropogenic factor, although these parameters would have relative weaker correlations with GEM. Why use RH and SP to define meteorology? How could these two factors affect GEM concentrations? What are the 24h-latitude and -longitude? Are they referred as the air massed originated outside the city to define long-range transport?

Line223: the range of background GEM concentrations of 1.5-1.7 ng m-3 is somewhat higher to me. Better to use recent global observations.

Figure 2: a statistic of the annual GEM concentrations should be added

Line 274: the GEM lifetime here is not consistent with that in line 76

Line 297-298: elevated O3 and decreasing GEM concentrations should be mainly related to subsidence of free troposphere and daytime production of O3. If the daytime declines in GEM is caused by oxidation, we would expect a much higher oxidation rate than experimental studies.

Line 317: a citation of references should be added here

As shown in Figure 5: a large fraction of air masses originated or passed over oceans,

please add their weighted GEM concentrations in Table 2

Line 424-426: it is difficult to expect low regional anthropogenic emissions because the GEM measured are much higher the background levels in East Asia. I suspect that the other two factors of transmission and meteorology were also impacted by changing local and regional anthropogenic emissions. Actually, I do not know what are these three factors representing. Are the anthropogenic emission and transmissions representing local anthropogenic contributions and regional background? What is the meteorology? Is it representing natural emissions'?