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Comment on acp-2022-367

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Community 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-CC1, 2022

Shi et al., reported air Hg measurements in Jan and Jul of 2012-2020, and tried to quantify the potential sources for these measured data annual trends. Generally, these long-term data are beneficial to understand the Hg emissions and air Hg variations in China, since a series of air cleaning actions taken in recent years by the government The subject is of interest; the methodology is robust; the results and discussion are presented well in the most sections. Several issues need the authors further to identify:

Section 2.2

From the authors' description, just using the Tekran 2537 without an annular denuder, the data should be mixed the signals of GOM and some PBM which with the small particle size. I suggest the authors should state clearly about their measurements. If the ratio of GOM and PBM to TGM are <5%, the authors can use the GEM to represent the TGM.

Section 2.5

This section is very important in the whole methodology section, but the authors' description was not very clearly. Several issues need the author further confirm: one is 24h-Latitude and 24h-Longitude? What's the detail representation of these terms, the back trajectory endpoint location during last 24 h? Another one is the air transmission. I would like to say it is the air transportation.

Section 3.1.1

Line 220-230 Given the authors only measured the GEM concentrations in Jan and Jul in each year, the authors should compare their data with the references mentioned the same month data, not the annual average data.

Section 3.2

In this section, the authors mainly attributed their observed Hg seasonal and diurnal cycling to the local anthropogenic emissions and long-range transport. Recently, several studies from the China cities also showed that the regional surface Hg emissions from soils and city bare regions, and Hg chemical transformations in the air of cities, and regional Hg natural surface emissions, such as from the soils and nearby the oceans. I suggest the authors to further incorporate these potential reasons in their discussion.

Section 3.3.2

This section is the key discussion parts of the whole manuscript. From the GAMs modeling results, the authors stated that the meteorological factors are the most important factor to shape the GEM variations. However, the authors mainly stated these factors' contribution which derived from the modeling. From my view, the meteorological factors influencing

GEM variations by several pathways. One is that the meteorological factors drive the Hg chemical transformation, such as UV, RH are highly related to the photo-reduction and GOM formations in the air, specially in the haze, these meteorological factors playing a dominant role in GEM transformation to GOM and PBM in the air. These kinds of studies have been reported in Hefei, shanghai and Beijing. Another important role is that meteorological factors are highly related to the Hg emissions from the natural surfaces. From the current modeling results, the Hg natural emissions from the natural surfaces (e.g., soils, water, etc.) are comparable to the anthropogenic Hg emissions in China mainland. Substantial flux measurements have clearly showed that the elevated temperature and solar radiation can significantly promote the Hg re-emissions from these nature surfaces. Overall, I suggest the authors explain the cause of the contribution of meteorological factors in more detail, specifically related to the Hg emission inventory and Hg transformation mechanisms in the air, by some typical case periods of data (several tens of hour Hg, meteorological factors data) to show their interactions, not just a data presentation.