

The study by Qin et al. investigates the atmospheric TGM concentrations at two islands and over the coastal oceans in east Asia, and explores the effect and contributions of anthropogenic emissions based on atmospheric tracer ratios and a receptor model. The finding from this study is valuable for understanding the cycling of atmospheric Hg in coastal ocean regions. This study provides many observational data, and does an insightful analysis of the datasets. The manuscript is well written and I broadly agree with the interpretations of the new data. I think the manuscript is currently in good quality. I have provided a number of minor comments that hope to be considered by the authors.

1. the abstract: the levels and distribution patterns of atmospheric TGM are one of the major contributions of this study, which are better to be briefly summarized in the abstract.

2. line 39: these numbers should be linked to the seas investigated in this study.

3. line 228: better to specify the equations for the calculation of Schmidt number of elemental mercury. In addition, provide the methods for detection of wind speed and water temperature.

4. line 235-239 and 247-248: this study describes well the spatial and temporal distribution patterns. As far as I have concerned, recent long-term continuous observations of TGM in mainland China have already showed significant declines in the past decades (e.g., Feng et al., 2024, NSR). My question is whether long-term changes in TGM in Chinese coastal oceans is similar to the mainland. I therefor suggest the author compare with previous observations and show the trends of TGM in coastal oceans in east Asia.

5. line 261-263: this statement is very speculative and in contrast with the discussions in sections below. The good relationship observed could be affected by many factors. Generally, ocean Hg emissions are largely controlled by wind speed, while the effect of temperature and solar radiations is relatively small.

6. line 280-281: have any of previous studies provided solid evidence that increasing temperature would facilitate strong ocean Hg emissions?

7. line 284-285: note that the natural emissions in this study is mainly associated with seawater emissions. while the discussions here is mainly reasonable for soil Hg emissions.

8. line 327-330: I thought the authors should determine the TGM/BC ratios based on the slope of the correlations between TGM and BC. It seems the ratios are calculated by observed levels of TGM and BC. Note that BC is not long-lived atmospheric pollutants and ready to deposit more quickly than TGM, and this is why high TGM/BC ratios was observed at locations far from sources.

9. line 370: it is interesting to provide quantitative analysis of the contributions of anthropogenic sources. My question is whether the results from this study agrees or is different from previous studies. Recently, several studies quantify the contributions of anthropogenic emissions to atmospheric TGM in rural areas in China (including HNI) based on Hg isotope approaches. I would therefor suggest the authors compare their results with previous isotope and modelling studies.

10. line 381-384: see my previous comments on TGM/BC ratios. Are there any strong relationship between TGM and BC over the seas? If yes, better to use relationship slopes to estimate the contributions of anthropogenic emission over the seas, but not using the island results.

11. Line 422-423: better to show the values or ranges of previously observed DGM in the

seas.

12. Line 440-441: I would suggest the authors to provide detailed information regarding the DGM and air TGM concentrations, wind speed, water temperature , and calculated exchange flux at each of the sampling sites in the supporting information. This would be valuable for future studies in air-sea Hg exchanges.