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Atmospheric Chemistry and Physics Discussions

Interactive comment on "New evidence for atmospheric mercury transformations in the marine boundary layer" *by* Ben Yu et al.

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This study reported the isotope compositions of total gaseous mercury (TGM) and particulate bound mercury (PBM) in the marine boundary layer (MBL) during summer and winter seasons. The results are novel and very interesting. It has significant contributions to the research field and improve the understanding of Hg transport and transformation in MBL. The manuscript is also written clearly and well organized. Therefore, I suggest a publication of this manuscript in ACP with minor revisions.

Response: Thank you very much for your positive and valuable comments.

Line 202-204, what do the authors mean by "contribution of Hg(II) in wet deposition to both PBM and TGM"? How Hg(II) in wet deposition contribute to PBM and TGM?

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Do you indicate the sources of Hg(II) in wet deposition were not directly from PBM or TGM?

Response: Thank you for pointing this out. Here we try to use D200Hg values to evaluate the contributions of aquatic Hg(II) in atmospheric aquatic phase, e.g., cloud or fog, to both PBM and TGM. Atmospheric aquatic Hg(II) can be re-emitted into gaseous phase after photo-reduction, which has been demonstrated in many publications (e.g., Lyman et al., 2020, https://doi.org/10.1016/j.scitotenv.2019.135575). Since wet precipitation worldwide shares similar higher D200Hg values ($\sim$0.2‰), and Hg(II) is the primary form of Hg in wet precipitation, the observed near-zero D200Hg in this study suggested the contribution from atmospheric aquatic Hg(II) following photo-reduction and re-emission to PBM/TGM should be limited. We have updated a more accurate description on that in revised manuscript.

A general comment that conclusions should be made with caution when comparing statistic slopes derived from different studies. As the air samples are usually mixtures from different sources, and not all sources are well quantified and constrained.

Response: Thank you very much for your kind suggestion. We agree with you that discussion on the mixing of plumes using statistic slopes should be careful. We have added a sentence at the beginning of the discussion on slope 'This fitted curve always indicated a mixing of plumes with different isotopic fingerprints (Demers et al., 2015;Yu et al., 2016;Fu et al., 2018). Especially a $\sim$ -0.1 slope could be shaped when mixing of plumes from anthropogenic emissions characterized by negative δ202Hg and near-zero Δ199Hg values, and plumes from remote areas characterized by positive δ202Hg and negative Δ199Hg values, e.g., three slopes of -0.09, -0.13, and -0.07 observed in TGM from Mt. Damei, Mt. Ailao, and Beijing, China, respectively (Yu et al., 2016), and -0.095 observed in TGM/GEM and source materials worldwide (Fu et al., 2018)'. We also highlighted that isotopic fingerprint in all different sources was not well quantified and constrained in the implication part.

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