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Comment on acp-2021-357

Anonymous Referee #1

Referee comment on "First Observation of Mercury Species on an Important Water Vapor Channel in the Southeast Tibetan Plateau" by Huiming Lin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-357-RC2, 2021

The manuscript by Lin et al. carried out a half-year of continuous measurements of speciated atmospheric Hg as well as a year of measurements of gaseous elemental Hg using a passive sampling technique at a high-altitude station in the eastern Tibetan Plateau. This study combined field observations with backward trajectory analysis, criteria pollutants and a PCA source identification approach, which are used to understand the sources and transport of atmospheric Hg in the eastern Tibetan Plateau. This study is valuable for the atmospheric Hg research topic especially in the pristine Tibetan Plateau where could be potentially impacted by long-range transport of Hg from surrounding anthropogenic Hg source regions. The authors have provided detailed explanations for the variations in the atmospheric Hg, and I broadly agree with the interpretation and hypothesis. The manuscript is overall well organized and written. I therefore suggest a publication of this manuscript in ACP after addressing the following minor to moderate issues.

Line 67-68: the authors mentioned that numerous studies have been conducted in Europe and North America. As I know, atmospheric Hg studies in China have also obtained many advances in recent years, which should be also mentioned here (instead of using a citation of mercury emission study in China).

Line 80-83: I would suggest to cite Hg emission inventories developed in China and worldwide directly. Note that coal combustion is not the exclusive sources of atmospheric Hg in China.

Line 84-111: I saw the authors introduced many studies on air pollutants in the Tibetan Plateau, and I agree this is useful for highlighting the need of the present study. However, I would suggest the authors to make a general description of previous atmospheric Hg studies in the Tibetan Plateau, which would help the authors figure out the knowledge gaps in this study area and strengthen the importance of this study in this research topic.

Line 135: is the rain depth at SET station much higher than the mean in the Tibetan Plateau? Could the author tell something regarding the seasonal patterns in rain depth at SET (noticeable difference between the PISM and ISM)?

Section 2.3: the study by McLagan et al., 2018 (ACP) should be cited. I suggest the author to briefly introduce how to use the passive technique to calculate the atmospheric GEM concentrations. The current information is not very clear to me.

Line 202: why did the authors choose a ending height of backward trajectory of 1000 m agl. A height of 1000 m is almost above the PBL.

Line 206-212: the description of PSCF is not clear to me. A least, the authors should mention the arbitrarily set criterions in GEM concentrations used for different sampling period.

Line 235-237: rain depth is a good proxy for the changes of monsoons. However, I would suggest the authors to show the air mass sources and transport pathways in different monitoring periods. This would help to better show the changes in monsoons. Alternatively, the authors may provide the Indian monsoon index to support the changes in ISM.

Line 395-252: the authors did not show the GEM, GOM and PBM during the ISM3 period. The ISMS is characterized by elevated GEM and decreasing GOM and PBM. Would these observations be explained by wet deposition removal processes?

Line 255: the mean GEM measured by Tekran instrument should be presented.

Figure 4: this figure contains to many information and I can only read the diurnal GEM trend clearly. I would suggest to redraw these figures by separating some of the observations in different figures (some maybe in SI). Also, these figures are lacking of Y axis.

Section 3.2: the authors presented the diurnal patterns in criteria pollutants in Figure 4, but they did not use these data to explain the sources and factors regulating the atmospheric Hg. I would suggest to use the CO (or NO2) to strengthen their hypothesis.

Figure 5: these figures are difficult to read. I would suggest the authors to add tables in these figures, which may include the relative fractions, travelling height, mean GEM, GOM and PBM concentrations for the grouped clusters. Alternatively, they can show these information by text directly in the figures (information using thickness and color of the lines are difficult to obtain)

Line 410-417: would the transport of Hg from southwestern China contribute to the elevated GEM during ISM3?

Section 3.3: the authors mainly use backward trajectories to show the sources and transport pathways. I suggest the authors to add an analysis of wind dependence distribution of GEM, GOM, and PBM. This would help to support the findings using trajectories (trajectory has many uncertainties especially for mountainous monitoring sites.)

Line 420: are these PSCF figures showing the sources of GEM, or GOM and PBM? Overall, the authors did not well explain the sources and transformation of GOM and PBM, neither combined them with GEM to propose the atmospheric processes (or sources) of atmospheric Hg in the high-altitude regions.

Line 465: would GOM be emitted from land surfaces? The elevated GOM accompanied by increasing solar radiation many indicate in situ oxidation of GEM?