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Comment on acp-2020-981

Anonymous Referee #2

Referee comment on "Isotopic compositions of atmospheric total gaseous mercury in 10 Chinese cities and implications for land surface emissions" by Xuewu Fu et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-981-RC2, 2021

Review for "Isotopic compositions of atmospheric total gaseous mercury in ten Chinese cities and implications for land surface emissions" by Fu et al. (ACP-2020-981)

The manuscript presents data of TGM concentrations and their isotopic compositions in 10 large cities, many of them considered mega cities, in China. Documentation of such data in open literature is valuable, the data quality is good and the scientific presentation is sound. The primary weakness of the manuscript is a deterministic scientific argument based on the the relatively scattered data, which is difficult for a study of this nature since the sampling was perform at different time (and perhaps by different sets of researchers) at different locations. Given the level of data scattering, it is somewhat uncertain to provide a clear scientific finding, which seemed to be the main criticisms of Reviewer #3 in the previous round of peer review.

Other than a lack of deterministic scientific conclusion, the reasoning and interpretation of data appear to be sound. One interesting feature of the conclusive remarks made by the author group is the attribution of mercury source and the TGM concentration variation primarily to soil evasion, which is somewhat counter-intuitive to the intensive human activities in large cities. The measured TGM concentrations in those cities are highly elevated (Figure 4). Previous work has attributed the elevated concentrations to human activities and the seasonal variation to meteorological factors, which seems reasonable and intuitive. Although this work present additional mercury isotope data, the level of data scattering does not seem justified to rule out the past attribution to human activities and meteorology. Perhaps the authors should at least make an attempt to strengthen the arguments described in their conclusion.

There are also several other areas that can use additional clarity:

- The selection of the ten city sites needs to be justified and the characteristics can be more detailed. Was the selection by design or by incident? If it is by design, discussion should be provided for the intended scientific goals. If it is by incident, discussion should be provided to argue why the data collected from the 10 sites can sufficiently support the conclusion.
- Since the samples were collected at different times and locations where the chemistry of various urban airshed could be substantially different such that the samplers may behave inconsistently. Based on the description in the method section, it seems that the sampling was not duplicated but the analysis was repeated. Some discussion in regard to the consistency of the carbon trap samplers will ensure the confidence on the data quality.
- The 10 city sites have drastically different meteorological patterns other than the generic seasonal patterns described in the manuscript. It is possible that there are local processes forcing the observed isotopic characteristics? This is not clear I the manuscript.
- It will be useful if the authors ca specify what statistical criteria is considered significant for using the relatively scattered data to draw the conclusion.

Overall, the manuscript is considered acceptable after revisions on the scientific arguments and editorial improvements.