



EGUsphere, referee comment RC1
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Comment on egusphere-2022-903

Anonymous Referee #1

Referee comment on "Mercury in the free troposphere and bidirectional atmosphere–vegetation exchanges – insights from Maïdo mountain observatory in the Southern Hemisphere tropics" by Alkuin M. Koenig et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-903-RC1>, 2022

The manuscript by Koenig et al. presents a nearly one-yearly time series of atmospheric GEM and RM at a high-altitude site in the southern Hemisphere tropics, which are difficult to be obtained and would help to fulfill the knowledge gaps on the variations of speciated atmospheric Hg in the southern Hemisphere free troposphere. The authors also aimed to characterize the levels of speciated atmospheric Hg in the boundary layer and lower free troposphere and explored the potential factors controlling the seasonal and diurnal variations of speciated atmospheric Hg. The data as well as the scientific issues addressed are interesting to me. The manuscript is overall well written and contains many thorough discussions. I therefore suggest a publication of this manuscript after an address of the following minor questions.

Based on the diurnal variations in GEM and a mixing model, the authors suggest a binary mixing of boundary air and free tropospheric air and reemission of Hg from vegetated surfaces are the dominant factors controlling the diurnal GEM variation. Exchange of Hg between atmosphere and vegetations are currently a popular topic, which, however, appears unable to explain the observations solely. As predicted in the manuscript, a daytime emission fluxes of from 8-22 ng m⁻² h⁻¹ from vegetated surfaces is needed to explain the difference between the observations and binary mixing diagram. However, many previous studies observed Hg depositions over foliage, and the occasional emission events during daytime generally showed mean reemission fluxes less than 0.3 ng m⁻² h⁻¹ (based on leaf areas and would not exceed 2 ng m⁻² h⁻¹ even through a multiplication of LAI). Therefore, there should be additional daytime emissions around the sampling sites, such as daytime anthropogenic emissions in downslope residential areas, Hg emissions from soils as well as reemissions of Hg from seawater (the authors assume a constant GEM level for marine boundary layer air mass). I wish the authors to discuss the effects of these sources on the diurnal GEM variations.

Section 3.2.1: The authors attribute the seasonal trend to the transformation of GEM to RM in free troposphere. This is not a big issue but seems to neglect other mechanisms

such as seasonal variations in the long range transport of anthropogenic and/or biomass burning emissions. In Figure 3, the seasonal variations in GEM levels resemble those of CO and CH₄ levels. This may indicate the long range transport of emissions should be also important, in addition to atmospheric oxidations. I would suggest to analyze the relationship between air mass transport pathways and seasonal GEM levels to see whether anthropogenic and/or biomass emission would also affect the seasonal changes.

The authors used abbreviations at many places, such as "BB" in line 385 and "H'17" in figure 4, and this makes it difficult to understand the meanings in these discussions. Please consider to use full or standard name.

Line 114: would any Hg emissions from the dormant volcano, and would it have an impact on the observations.

Line 167-168: I am not sure whether the PFA material could isolate the UV radiations. Please consider to delete

Line 175: is the flow rate of 1.3 L min⁻¹ under standard conditions or a volumetric flow rate?

Line 196-198: the digestion method is different from that used in Maruszczak et al., 2017 (20% inverse aqua regia). Have any laboratory test being done to verify such a diluted digestion would digest the RM efficiently?

Line 200: Maruszczak et al., 2016 should be 2017

Line 209-211: it is better to present the purpose for the measurements of VOCs, which should be related to the objectives of GEM or RM studies.

Line 300-304: the authors should make clear that previous studies were mainly conducted at low-altitude sites, and this would help to understand the difference in GEM levels between this and previous observations.

Line 367: the arbitrary specific humidity values should be presented.

Line 587: are the reemissions solely referred as to vegetation foliage or forest ecosystems

(including foliage and soils)?

Line 610-612: these two studies were conducted over meadow in temperate zone, and other observations on the exchange flux over tropical and subtropic forest or soils might be better to support the hypothesis.