

## ***Interactive comment on “Gaseous elemental mercury (GEM) fluxes over canopy of two typical subtropical forests in south China” by Qian Yu et al.***

**Xinbin Feng**

fengxinbin@vip.skleg.cn

Received and published: 1 September 2017

Forest is an important ecosystem on the earth. Characterizing the role of forest in the global biogeochemistry cycling of Hg is an important research topic in global Hg cycling studies. This study investigated the GEM gradient at two typical forests in subtropical zone of China. GEM concentrations and GEM fluxes measured are valuable for the atmospheric Hg studies in regional and global scale. I don't see major errors in the sampling techniques and method, and the discussions, for most cases, are sound throughout the whole manuscript. I would suggest a publication after addressing the following questions.

C1

Major points: The result from this study found the forest in subtropical zone of China is a net source of atmospheric GEM, and I have no doubt for this result. My question is, why the net emissions were observed in the study areas, soil emissions or foliar emissions? The authors did discuss this scientific issue in section 3.4, but we still don't know the exact causes. I think this is critical for atmospheric science. As we know, previous field observations on foliage/atmospheric Hg fluxes mostly revealed a net sink of GEM. If forest canopy is a net source in the study area, this would be an important finding. If the net emissions of GEM from forest were caused by elevated soil Hg emissions (soil Hg concentrations were elevated), then future studies regarding the mass balance of GEM in forest using litterfall approach approaches should also consider the soil emissions or reemissions. The net emissions observed in this study might be also due to many other factors including the specific tree species (evergreen tree species generally have higher uptake capabilities of GEM relative to deciduous tree species), lower leaf area index, reemission of dew water and transpiration stream, which should be also assessed in the manuscript.

Minor points: Line 79: why did the authors select the two sampling heights at QYZ and HT? Are you any previous studies to support your setting; Will the chose of different sampling height affect the flux result? Line 81: 'half canopy height'? The lower sampling heights were 25 and 22.5 m agl, which were much higher than the canopy height. Section 3.1: I think the annual variations of GEM gradient at QYZ and HT are also important and should be presented in Figures. The authors should also show the annual, seasonal and diurnal trend of GEM gradient. Line 155: the authors should note the sampling height of the annual mean GEM concentration. Line 156: the global and northern hemisphere background should be referred to GMOS studies. Line 159: References are needed here Line 161: Do you have any evidence for this hypothesis? I strongly suggest the authors analyze the source-receptor relationships at the sampling sites? Measurement of GEM in the atmosphere is also an negligible part of this study. Line 190: should be 'have positive values at QYZ'. Line 196: References are needed here. Line 227: the source from WS mercury mining area? Any evidence? Line 297:

C2

the study only reveal the whole forest is a net source, but not vegetation, you did not measure foliage/atmospheric Hg flux. Table-1: the relative abundance of major tree species should be listed.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-349>, 2017.