

Review of Total Atmospheric Mercury Deposition in Forested Areas in Korea
By Han et al., 2016 ACP

In general, this paper has been improved significantly; however, I still have some questions and concerns. I suggest to accept this paper after a minor revision.

Line 35, monthly or weekly measurements?

Line 37, monthly data for the correlation? How many data point?

Line 49-51, how this would be useful for global mercury research?

Line 68-70, re-write the sentence, it is difficult to understand.

Line 70, “created” use another word, I suggest to use formed.

Line 106-107, references

Line 131, references, or probably delete it, Hg methylation is a very complicated process, it would be better to explain this in detail if possible.

Line 142, Huang et al., 2015 passive sampler review paper and 2011 Atmos. Env. wind tunnel tests.

The authors need to talk about the field blanks for dry/wet deposition and all other measurements
Line 179 DFCs were placed 2 cm under the soil? Re-write, I don't think this is possible, probably say “The bottom 2 cm of DFCs is covered by soil and soil surface to the chamber top is XX cm” some thing like this. The sentence sounds like the chamber is fully covered by soil.

Line 180, UV light needs some references

Line 192-193, explain what are the uncertainties here

Line 221, what is the RPD range? I expect that might be large, but it should be fine

What are the max capacity of these surface, in case you did not over load them?

Line 257-258, previous studies show no GEM collected on KCl surface, and in Zhang et al 2012, they discussed the potential GEM uptaken by dry deposition measurements is due to the usage of acidified BrCl. Since BrCl was not used in this study, this is not a suitable statement.

Line 262-265, 269-272, if you have figures or tables to present the data, you don't need to repeat the data again in text.

Line 276-278, could the author please do the analysis in detail? In the North American, we are seeing winter time low PBL, I agree GEM concentrations will increase, but I never see GOM concentrations increase in low PBL condition. The authors cited two papers here, Kim et al., 2009 and Seo et al., 2015, I went back to read these two papers, Seo et al., 2015 cited Kim et al., 2009 to make the statement, and Kim et al., 2009 cited Blanchard et al., 2002 to make the statement. None of Seo et al., 2015 and Kim et al., 2009 did a detail analysis on this. I just wonder could the authors do a detail analysis on how PBL decreasing impact atmospheric GOM concentrations?

Line 312-315, could the authors discuss this in detail, is there any information measured at these sites supporting this statement?

Line 320-321, do not understand

Line 379-380, what are the uncertainties?

Line 384-402, re-write this paragraph. There are some things I suggest the authors can look into. Estimated dry deposition should less or equal to measured dry deposition due to no canopy

resistance for KCl surface, no re-emissions for KCl surface. Similar concept for wet + dry deposition and throughfall + litterfall should be considered. There is no (or very small) re-emissions for wet + dry deposition; therefore, the numbers are totally making sense to me. However, the authors did not explain this in detail.

Line 413-419, we know atmospheric GOM concentrations at this site are higher than the numbers measured in Huntington Wildlife forest. However, the net flux in HWF is higher than the number at this site. Does this mean that Hg soil emissions are in Korea way higher the numbers in HWF? If this is true, what could be the reasons?