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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Soil-atmosphere exchange flux of total gaseous mercury (TGM) in subtropical and temperate forest catchments" *by* Jun Zhou et al.

Anonymous Referee #2

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The authors report flux measurements of total gaseous mercury (TGM) on 5 plots in subtropical forest and 5 plots in temperate forest in four seasons. They use the dynamic flow chamber (DFC) method and describe the flux dependence on ambient TGM concentrations, solar radiation, and temperature. The diurnal variations in different seasons are described.

The measurements are valuable but the authors stretch their interpretation by taking the measured fluxes as being representative for the whole investigated ecosystems. DFC measurements are well suited to study the flux mechanism, i.e. flux dependence on temperature, soil moisture, ambient TGM concentration, solar radiation, soil tem-

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perature, substrate concentrations, etc. But they are unsuitable for determination of the representative fluxes for a given ecosystem because a) only a small area is being measured (20 x 30 cm here) and b) covering of the soil by DFC changes its status (e.g. by heating the soil or vegetation by glasshouse effect). In other words: really representative fluxes have to be measured by micrometeorological methods, DFC methods can provide only empirical relationships for extrapolating them to the whole ecosystems. The problem with this paper is that the authors try to estimate ecosystem fluxes as if their measurements were representative for them, despite being aware of the problems in DFC measurements (mentioned in meagre 3-4 lines).

I recommend the publication of the paper provided that the authors stick with the mechanistical interpretation of their results and avoid the temptation of extrapolations to the whole ecosystems (made e.g. in “Conclusions and study implications)”. This would need some changes in the text. The authors also discuss the observed correlations and relations predominantly in physicochemical terms. By this they neglect the soil microbiology – this also needs to be rectified.

Specific comments:

Line 50: "..long longevity. . . is able to undergo over long distances..“?

Line 59: Not all fires are “natural”.

Lines 147-149: “semi-cylindical” and “20 x 30 cm” – how does it fit together? “Six inlet holes” where?

Lines 147-153: How was the chamber installed on the soil: was it partly buried into the soil to seal the chamber-soil gap, if so to which depth? Are you sure that you do not suck ambient air through the soil or through the gap between the chamber and the soil, at least partly, instead of sucking air through the inlet holes? The resistance of the soil with respect to air flow can be surprisingly small, it may be smaller than the resistance of the inlet holes, resulting in sucking of air through the soil. If that happens,

the measured fluxes are not what was intended to be measured. Eckley et al. (2010) do not mention this problem.

If I understand the text properly then the chambers were permanently (during the measurement period) on the soil. If so, then the plot under measurement would e.g. not receive any precipitation? In other words: the measurements would not be representative for uncovered soil. Please specify.

Gold cartridges: what type? Those of Tekran or other? Please specify.

Lines 176-178: In these few lines the authors mention the problems with fluxes measured by DFC and, essentially, salvage themselves using Eckley et al. (2010) reference. The chapter "Conclusions and study implications" is written as if there were no problems.

Line 185: soil organic matter (SOM)

Line 194: Sampling TGM in pore air is mentioned – how was it made? What were the results?

Line 267: were

Lines 273-279: The influence of soil humidity is discussed here only in terms of physicochemical terms. It is well known that microbiological processes in dry soils are greatly enhanced by occasional precipitation.

Line 296: "physicochemical properties" – what about microbiological ones?

Line 368: Photo-reduction of Hg2+ may be a major driver in waters but hardly in soils which are impenetrable to solar radiation. More plausible is the explanation by higher soil temperature and the related higher microbiological activity.

Paragraph, lines 417-433, Figure 5: Are these correlations made with data from all seasons? I would expect different compensation points for different seasons.

Figures 3 and 4: What does the x axis mean?

Figures 5 and 6: Are these plots seasonally resolved? If not please state that data from all seasons were used.

SI, “Environmental measurements”: The measurements of soil temperature (depth) is not mentioned here and neither in “Experimental”. This parameter is the crucial one for physicochemical and microbiological processes in the soil. According to Figure S 6 it seems to have been measured. I would prefer to discuss all relationships in relation to soil temperature instead of solar radiation. Solar radiation is essentially only a sort of proxy parameter for soil temperature. It is also not applicable for the night.

SI, description of MDL: Any information about the Hg content of litterfall and soil?

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