

## ***Interactive comment on “Fate of mercury in tree litter during decomposition” by A. K. Pokharel and D. Obrist***

### **Anonymous Referee #2**

Received and published: 11 May 2011

#### General Comments:

This manuscript provides a detailed analysis of elemental Hg in different soil types associated with decomposition in a controlled laboratory setting. The analysis also includes some estimates of Hg solubilization at different stages of decomposition and comparison to field controls. Results show a significant loss of elemental Hg in the laboratory control samples during decomposition. Quantification of Hg<sub>0</sub> losses from soils is essential for the parameterization of global models since they contain the largest reservoir of Hg and can therefore exert a large influence in atmospheric concentrations. I think the experimental work outlined in this manuscript is very nicely done and I have only a few minor comments on the work as whole. I recommend final publication with minor revisions.

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In general it would be nice to have a few more details about how well the laboratory experiments mimicked the in situ decomposition process (for example the three week wetting period and its implications for evasion). Associated with this first point, what kinds of bounds need to be placed on the flux estimates provided in the experiments when trying to extrapolate to field conditions? Can this even be done with any confidence in the authors' opinions and if not what needs to be done to generate comparable field numbers? Given the simplification of the wetting and drying cycles in soils in the laboratory experiments (and the differing composition of rainwater) – what do the authors feel the experimentally mobilized fractions represent?

A lot of the experimental results seem to be driven by the large temporal change in Hg concentrations Hg:C ratios etc in the aspen litter and contrasted by the pine litter example where little change was observed. Could the authors include in their discussion some explanation about the composition of the aspen litter and the types of carbon pools in these forests compared to the pine forest litter? For example, the lignins in the pine forests are known to be highly resistant to degradation and this helps to explain the observed results. Linking back the results presented in both the field and experimental analysis to fundamental understanding about the composition and types of different carbon pools would be very helpful for increasing the impact of this work.

I find the use of the term “sorbed” Hg throughout the manuscript confusing in that it is used to describe the combined processes of external inputs of Hg that are bound (sorbed) to the soil matrix. For clarity, can the authors be as specific as possible (e.g., inputs of atmospheric mercury to soils etc.).

Specific Comments:

Page 2597, line 16, “un-decomposed” is redundant when referring to “fresh surface litter”.

Page 2598, line 21, I have not heard of these types of glass bottles for this type of experimental design. Is there a reason these jars were chosen?

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Page 2600, lines 1-5: How much field variability was there between C:N:Hg ratios prior to decomposition study for a specific forest-litter type? If no estimates are available here is there some way of bounding this from previous work? Would there also be expected variability in decomposition rates across samples randomly collected at the sampling site? Is there any way to quantify this?

Page 2608, lines 15 onward – There is much discussion in the manuscript about “confirmation of hypotheses” that makes me a little uncomfortable since the questions themselves are not set up as testable hypotheses with a null that is rejected or accepted according to the scientific method. I don’t this is a major problem but it would be nice if the authors could be more careful in how they imply they came to specific conclusions. Generally the experimental results point to another observation or support a premise that was not specifically “tested” here.

Page 2609, line 10-onward – The references cited throughout this manuscript are in general comprehensive. There is one recent paper by Smith-Downey et al, 2010 that I think would add to the discussion and interpretation of these results that is not cited.

Page 2611, line 10: Why not look at the ranges of total deposition (wet+dry) from combined modeling and field observations for each of the different forest sites?

Figures and tables - In general these are difficult to read at this time due to the size of font used. Please adjust for final publication. In addition, Table 2 repeats text and I would recommend avoiding such duplication. There are also examples in the text where the length could be shortened by referring to the Tables/Figures rather than repeating the results.

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Interactive comment on Biogeosciences Discuss., 8, 2593, 2011.

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