Baya & Van Heyst - Authors' Comments to Anonymous Referee #2

The authors greatly appreciate the time and effort that Anonymous Referee #2 has spent in reviewing our paper. Below are the Authors' Comments in response to the comments and issues raised by Anonymous Reviewer #2. Original reviewer comments are in italics while our Authors' Comments are in normal text.

Anonymous Referee #2

Received and published: 22 April 2010

This paper has some really nice data that will be of interest to the Hg research community and others interested in the biogeochemical cycle of Hg. However the presentation still requires additional effort. This manuscript is improved over the original draft. A huge issue is that my copy of the paper has the same text given on pages 8 and 15 and later describing the Hg flux data. I think this may be the authors' error however it could be just the electronic version I received. No matter what this needs to be corrected. It also would be helpful to have line numbers. Because of the formatting errors and lack of line numbers my critique below is generalized.

I. *Page 2. There have been many papers describing factors controlling air soil exchange. References need to be provided. The abstract describes terrestrial ecosystems as being an important sink and then this is not described further or sufficiently. This sentence would best be removed from the abstract.*
   - The sentence has been removed from the abstract to prevent reader confusion.

II. *In the introduction they suggest a significant proportion of the atmospheric Hg accumulates on the soil surface. They then only provide one outdated reference (1995). If they are going to make such a strong statement it should be supported by other papers. I think that this also is not necessarily the current thinking.*
   - References to Lindberg et al (2007), Mason and Sheu (2002) and Mason et al (1994) have been added along with land and ocean Hg flux estimates to indicate that land surfaces are currently believed to be a sink of Hg.

III. *In the intro they state they are measuring elemental Hg flux then in the text it is described as TGM which is it?*
   - As pointed out by AR #1, a better descriptor of what the Tekran 2537A measures is GEM rather than TGM and all references to TGM have been switched to GEM in the manuscript.

IV. *It would be nice to have a location map.*
   - A location map has been added to the manuscript.

V. *A better description of the method to measure Hg in soil is needed.*
   - This has been included in the revised manuscript.

VI. *Their sampling height description is not clear. Was the lower intake always at 0.35m? Their averaging for flux calculations was not clear. I think overall they calculated a flux every 30 minutes?*
   - For the majority of the study, the lower intake height was 0.35 m. As the corn crop grew the lower and upper intake heights were adjusted periodically to keep them above the canopy. This has been clarified in the manuscript.

VII. *Pg 8 is where there is text out of place. Then this text is again on 15 and 23. It would be best to put all the flux data in one section. Also the authors should consider their significant figures on the air*

concentration data as well as flux data throughout the paper, there are too many.

- This has been addressed in the revised manuscript.

VIII. *It would be useful to tell the reader clearly which seasons correspond with which soil conditions- bare, snow crop etc in the text. This is information that is useful. The fact they are measuring flux over a vegetated surfaces and snow covered surface should also be more carefully considered in the discussion.*

- Details of ground cover for each month and season is given in Table 2. The influence of ground cover is addressed in section 3.3.3 of the revised manuscript.

IX. *In discussion of statistical analyses I would recommend that parameters be described as correlated not influencing. On page 15 multiple parameters are described as having a significant influence. Yet later, only solar radiation is described as having a significant influence. More detailed statistical analyses of this data would be useful as well as presentation of this. They also describe wind direction as having an influence this could be used in the statistical analyses by applying integers for specific quadrants. I think a little more analyses of the data would be useful.*

- Details about the statistical method used and p values are given in the revised manuscript.

X. *Correlation analyses for RGM versus Hg (0) and Hgp would also be useful. On my pg 21 they describe wind roses for only a four day pollution event. What about non pollution events? More information is needed here before the reader can determine whether their data interpretation is reasonable.*

- Results of statistical analysis performed to investigate the correlation between RGM versus Hg^0 and HgP are mentioned in the revised manuscript.. The wind roses are given as an example of events when high Hg species concentrations were recorded and the wind was mainly blowing from the west.

XI. *Section 3.5.1 this section and the following are an attempt to sort out factors controlling flux.*

XII. *Statistical analyses on different time steps are needed. A paper that uses previous work that has stat analyses to put fluxes in context of controlling factors that may be useful to these authors is Environ Sci Techn. Hartman et al. 2009 and references therein.*

- Addressed in the revised manuscript.

XIII. *The effect of individual conditions still needs more consideration and the data more clearly presented.*

XIV. *3.5.1 describes solar radiation as being most significant yet there is no statistical data to support this.*

- The p value from the multiple regression analysis is given in the revised manuscript; $p<0.05$ indicating statistically significant relationship between net radiation and GEM Flux.

XV. *The figures with the data showing trends associated with specific environmental conditions are interesting however as stated earlier a detailed statistical analysis on different time steps as well as seasons would be useful. For example when the ground was covered with snow the flux is described as low. Was it also cloudy and extremely cold? I agree that snow would cap the soil but emissions from snow have also been reported in the literature. A more detailed assessment of the factors correlated with observed values than the qualitative discussion presented is needed.*

XVI. Pg 26 I have some issues with this discussion that focuses on soil moisture despite the fact that the surface was covered by snow. There is no discussion of flux from snow.

- Addressed in the revised manuscript.

In the conclusions it sounds like statistical assessment of trends was done. If so the type of analyses needs to be clearly presented. A conclusion is that multiple parameters are influencing flux and qualitatively the effect of different parameters is considered, however the analyses are not presented in the paper.