

## ***Interactive comment on “Total Atmospheric Mercury Deposition in Forest Areas in Korea” by Jin-Su Han et al.***

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[Comment 1] Abstract: It's just a list of numbers. What's missing is why the authors did the study and why the results they found matter. The Abstract needs a punch line. -> we revised the Abstract as follows on Line 34 to Line 51

[Comment 2] Introduction: Could be greatly improved by including a clearer statement of the problem or scientific question they're trying to answer with this dataset. The logical progression of the Introduction is a little hard to follow and it doesn't build a clear storyline for the rest of the paper. ->The introduction has been modified as follows Line 58 to Line 114.

[Comment 3] Methods, Site Description: The authors need a clear statement of why this particular location in Korea was selected. Lines 117-120 provided somewhat of an

C1

explanation, but it feels too vague. what does this site tell us that other sites don't? -> We added the following information about the sampling site as follows on Line 127 to Line 133.

[Comment 4] Section 2.4. QA/QC: Too many acronyms are introduced. Makes the text difficult to follow. ->We revised section 2.4. QA/QC as follows on Line 209 to Line 221.

[Comment 5] The authors use a knife-edge surrogate surface for PBM and GOM dry deposition measurements (Section 2.2.1). It would be useful to provide some discussion on how this method compares to other surrogate surface methods (e.g., the work done by Mae Gustin's). -> We provided some additional discussion and refer to Gustin et al. (2016) as follows on Line 141 to 154. Additional changes were made to this section based on Reviewer 2 comments.

[Comment 6] Page 8, lines 220-228: If all of these numbers are important, I suggest condensing into a table. It's difficult to parse text right now. -> We revised seasonal dry deposition data as follows on Line 241 to Line 247.

[Comment 7] Page 8, lines 240-243: The importance of this paragraph is unclear. Could it be deleted? -> We deleted Page 8, lines 240-243

[Comment 8] Page 9, lines 260-270: Which explanation do the authors think is most plausible? The text currently gives the impression the authors are just guessing. A more thoughtful scrutiny of the proposed explanations would be welcome. ->This section has been revised as follows on Line 276 to Line 285.

[Comment 9] Page 10, line 282: "Therefore, all of the Hg deposited..." What fraction is lost? What fraction is retained? This could be really interesting. -> Unfortunately we did not make any direct measurements of what was collected on the leaves and how much remained after a precipitation event so we can not address this question. However we did add a bit more discussion on Line 296 to Line 300.

[Comment 10] Page 10, lines 294-296: "... the rest of the variation is likely due to

C2

variations in local. . ." The "rest" here being >80%, correct? A more rigorous explanation of the majority of the variability seen in the data would be helpful. Being able to explain less than 20% does not give confidence in the interpretation. ->We added further explanations as follows on Line 312 to Line 321.

[Comment 11] Page 10, lines 307-309: Why is an  $r^2=0.59$  ( $p<0.05$ ) "significantly correlated" and  $r^2=0.56$  ( $p<0.05$ ) "moderately correlated"? -> Those should be the same - we corrected "moderately correlated" to "significantly correlated" as follows on Line 330.

[Comment 12] Page 12: If you are missing data in January and February, the stated assumption that "fluxes were assumed to be equal to the average of the flux of the month before" doesn't make sense. How are you handling consecutive months of missing data? -> We corrected this mistake about missing data as follows on Line 377 to Line 380.

[Comment 13] Page 13, lines 382-383: It would be useful here to be specific and describe what "both input approaches" are. It's not that clear what approaches you mean. -> We added further explanations as follows on Line 406 to Line 408 : "The yearly estimated budget of Hg was calculated using both input approaches (Total input = wet deposition + dry deposition or Total input = throughfall + litterfall) as follows..."

[Comment 14] Section 3.8: This section is disjointed and lacks cohesion. Revision strongly encouraged, with a focus on building a logical progression. -> This section has been revised as follows on Line 406 to Line 419.

[Comment 15] Conclusions: The manuscript needs a Conclusions section. Without Conclusions, the manuscript incomplete and doesn't seem mature enough for publication. A couple of strong synthesis statements from the authors about why their results add to our knowledge in the Hg field would really help the paper. -> We added a conclusions section as follows on Line 423 to Line 451.

C3

[Comment 16] Supporting Information: I encourage the authors to make their data available in the SI. This will make it easier for other interested scientists, especially modelers, to compare against the data in Korea. The mercury community will be excited about this dataset and want to weave it into their comparisons – make it easy for them! -> We revised SI as follows on Line 31 to Line 50.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/acp-2016-7/acp-2016-7-AC1-supplement.zip>

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-7, 2016.

C4