

**Autor response to Reviewer comments 3, the reviewers comments are in normal black font and the autor response are in blue bold font.**

Overall, I enjoyed reading the manuscript by Jiskra et al. on "Insights from mercury stable isotopes on terrestrial – atmosphere exchange of Hg(0) in the Arctic tundra". I agreed with another review that perhaps some information and data have already been presented in previous papers by the team, but I also think this is a very nice "wrap up" paper for all these results, they are complicated and I think the authors did an excellent job to put together the story, despite with some degree of uncertainty. **We would like to thank reviewer 3 for this very positive assessment and his constructive comments.**

I agree with most comments posted by Referee 1 & 2, I only have minor comments here and one suggestion as listed below:

P.2 L3: State percent of Hg to Arctic Ocean derived from Arctic Rivers? I thought Sonke et al. (2018 PNAS) found that values. **It's 44-50 t/year, the number was added to the revised manuscript**

P.2 L4: Suggest "... bioaccumulates and bio- magnifies....", without the latter, we don't have too much Hg problems. **In our understanding of the terminology, the word bioaccumulation includes bioconcentration and biomagnification (see. Alexander, D. E., Bioaccumulation, bioconcentration, biomagnification. In *Environmental Geology*, Springer Netherlands: Dordrecht, 1999; pp 43-44.). no changes made to manuscript**

P.2 L21: Do you want to emphasize abiotically, photochemically and microbially induced re-emission of Hg(0)? How they may be distinguished by Hg isotopes? **In this introduction we want to keep the discussion simple and do not want to emphasize any particular re-emission process. We refer to the publication of Jiskra et al. 2015 with respect to the use of Hg stable isotopes to distinguish different re-emission processes in soil samples. Since in our study we did observe a depletion of Hg(0) in soil gas the re-emission pathways were no subject of discussion. no changes made to manuscript**

P.2 L28: Regarding to "triple isotopic fingerprint", I think we mainly rely on MDF and odd-MIF for that, less so with even-MIF, right? **No, even-MIF (e.g.  $\Delta^{200}\text{Hg}$ ) is an important tracer for atmospheric redox processes and in contrast to odd-MIF the even-MIF signature is not subject to fractionation during post deposition processes. This is why we use a combined triple isotopic fingerprint. We refer to Enrico et al. 2016 and Obrist et al. 2017 SI for in-depth discussion. no changes made to manuscript**

P.2 L29: Regarding to "60-90% of Hg in soils is derived from Hg(0) uptake by vegetation", does this already account for wet vs. dry deposition only? how about geogenic source? **Most studies cited in this context included a potential geogenic source and the percentages provided are relative to the total Hg in soils. Note that for organic soil horizons the geogenic contribution can generally be neglected, for the mineral soils the geogenic contribution varies with bedrock and can make up for example 40 % of the total Hg in the mineral B horizons at Toolik field station (see Obrist et al. 2017 for details on source attribution). no changes made to manuscript**

P.3 L24/25: State the lowest amount of Hg needed for isotopic analysis. **>2.5 ng, this information was added, please note that this is the absolute minimum required, normally we aim to recover at least 10 ng which allows for duplicate Hg isotope analysis.**

P.3 L30/31: Not quite clear to me about "40 vol.% 2HNO<sub>3</sub>:1HCl"? **This refers to a 4.2 N HNO<sub>3</sub>, 1.2 N HCl oxidizing acid, manuscript was changes accordingly.**

P.5 L29: Typo-arctic snow **corrected**

P.5 L39: Wrong unit: ~2000 ng m<sup>-2</sup> **corrected**

P.6 L35: Is it correct to refer the text to Fig. 6 here? **Thanks for spotting this error, we refer to Figure 4 here. Manuscript changed accordingly**

P.7 L1/2: For "...as a promising tracer to distinguish between atmospheric deposition of Hg(II) in precipitation...", do you mean to distinguish deposition of precipitation Hg(II) from gaseous Hg(0)? **No, this sentence refers to the possibility to distinguish between Hg(II) deposition and direct Hg(0) deposition. Gaseous Hg(0) is oxidized e.g. during vegetation uptake and when analyzing e.g. a soil sample Hg originating from vegetation uptake is not present as gaseous Hg(0) anymore but as Hg(II) complexed to soils. However, this Hg inherits the Hg stable isotope fingerprint of atmospheric Hg(0), which can be distinguished from Hg(II) that was deposited through precipitation. No changes made to the manuscript.**

P.7 L31: Such large, estimated enrichment factor is interesting to see, would be interesting to propose how to "test" that experimentally. **We are working on this but do not want to go into detail in present manuscript.**

P.8 L1-10: This is cool explanation! **Thanks**

Last suggestion: Besides summarizing better on the seasonal differences on these processes as suggested by another referee, I wonder if vegetation uptake is the dominant pathway for Hg(0) to deposit onto arctic tundra soils, should the authors consider here (or another paper) to show the global warming effects on Hg(0) deposition in longer summer in the future, and any impacts on Hg isotopic signatures in soils? **Our results do not allow direct conclusions on how climate change will impact Hg and Hg stable isotope systematics in the Arctic tundra. For the revised manuscript we will consider to address some potential implications of climate change on Arctic terrestrial-atmosphere exchange and highlight areas for further research.**