

Overall, I think this is a very nice paper, certainly worthy of publication in Biogeo- sciences. I think the authors do mostly a good job of integrating their previous and directly related work to the results of this study, but I can possibly agree with other reviewers that it does at times come across as slightly confusing what things are new findings and what are not. That said, the paper overall hinges on very novel measure- ments of Hg isotopes in both snow interstitial air and soil air. It also presents some nice gradient based measurements of Hg flux and atmospheric stability, which I think do add nicely to the other parts of the paper. I do think the previous work, since it complements these new and novel measurements so well, is in the end largely written in a way that I think is entirely acceptable. If anything, the authors could perhaps go out of their way a little more in the conclusions to more explicitly pinpoint and take credit for the particular novelty of this work in comparison to their previous work. **We thank the reviewer for his positive assessment re-assuring us that the overall structure of the manuscript is justified.**

Specific comments:

Final paragraph of introduction: I find the write-up of these objectives miss the mark a little because they are vague. Is the purpose really just to “better understand” some- thing or is it more pointed in trying to examine whether certain hypotheses hold up when doing some novel measurements? The list of measurements and such comes across as somewhat less focused than is actually presented. I think it is totally fine that this paper is a little descriptive, but I do think this last “purpose” paragraph could be a little more specific. **We agree with the reviewer’s comment and also in response to the comments of reviewer 1 we will provide a more concise and specific description of the objectives in the introduction: “In our previous work we showed that atmospheric Hg(0) deposition to vegetation and soil represents 70% of total atmospheric deposition leading to high Hg levels in Arctic soils (Obrist et al., 2017;Olson et al., 2018). In this study we explore the use of novel mercury stable isotope measurements of Hg(0) in in interstitial snow air and soil pore air to identify the processes driving tundra Hg(0) deposition. We further discuss the effect of terrestrial-atmosphere exchange processes and planetary boundary layer stability on atmospheric concentrations and Hg stable isotope signatures of Hg(0). “**

Line 36 of page 3: Is this large a variation in sample yield problematic for isotope analyses? It seems large to me, especially for mass dependent work, but if it is no issue, this could be stated here. **In general, an incomplete sample yield can lead to mass dependent fractionation during sample pre-concentration. However, based on our data and the extensive QA/QS we have no indication of such a bias. We also would like to stress that at least part of the variation in sample yields is owed to the uncertainty in Hg concentration and cumulative flow measurements.**

First half of first paragraph of section 3.1: This discussion is a little hard to follow because this study measures Hg isotope values in interstitial air, but refers to other studies that measure Hg isotopes in snow itself. Given the discussion, it seems a little unclear whether the snow interstitial air isotope signature is slightly processed (e.g., partially deposited) atmospheric mercury or is re-emitted from the snow itself into the interstitial air. I am sure this is a minor thing and just a point of clarity. **We revised the respective paragraph**

Line 35, page 6: I am unsure about the jump to referring to figure 6 here. I do not believe either of figures 4 or 5 have been introduced yet. **This was an error, we referred to Figure 4 here. The manuscript has been changed accordingly**

Line 18, page 7: Though this says Figure 1I, it looks like “figure eleven”. Perhaps this could be formatted differently to avoid confusion unless this is the required convention? **We added a space between 1 and I to avoid misinterpretation as 11.**

Figure 2: I am unclear on whether the upper values are air above the snowpack or are indeed interstitial snow air? They appear to be above the average snow heights. **Correct, the snowpack height was unusually low in the winter 2016 and the highest inlet of the snow tower (30cm) was sampling in the atmosphere over the course of the campaign as indicated by the average snow height.**