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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Overview of mercury measurements in the Antarctic troposphere" *by* A. Dommergue et al.

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Response to reviewer 4

Taking into account reviewer 3 and 4 comments, we went through the whole manuscript. The manuscript has also been proofread for English and we hope that most of the errors have been corrected. A revised manuscript is proposed.

SpeciïňĄc comments

1) The information on measurement methods (2.1.2) could be relevant and helpful. However, much of this is not from cited information. As a consequence it is either so"commonly known" that perhaps it is not worth having in here or it needs some citations so as to add new content to the Review. If is simply a rehash of what everyone

already does, and has no speciïňĄc signiïňĄcance to the Antarctic then why is it here? I could see this going either way.

We have added a table of the methods (manual silver traps and Tekran automated speciation suites), accuracies and detection limits to the paper.

2) Make sure all acronyms are spelled out. I think “DOAS” is never spelled out or I missed it. “RHS” too.

done.

3) Page 26883 lines 10-17. Here and in other potential locations in the manuscript a comparison to the Arctic may be worthwhile. The authors mention repeatedly that there is far more research results in the Arctic. Why not highlight places where these results are similar to the Antarctic and where (and why) they may be different or not applicable. I realize the authors have an Antarctic focus and that is worthy. However if they want to repeatedly state the missing Antarctic information they should ïňĄll in blanks (where available) to show how the Arctic results could be applied or not. They seem to be in the best position to provide this to the research community. Perhaps a section on the comparisons and contrasts is warranted and would make this both a stronger paper and on that Arctic researchers could learn from. Maybe this information is best for the conclusions? Section 2.3 does this a bit but perhaps instead of focusing only on the Reactivity it could be expanded by a paragraph or so to further discuss the role of sea ice, coastal phenomena, precipitation, inland processes, halogen chemistry?

Though the Hg reactivity in the Arctic is far more documented, our current understanding is limited as shown in Steffen et al 2008. The Hg reactivity in Antarctica shows some similarities but we think that additional or visible processes may occur in Antarctica. The Antarctic continent lacks the long-term Hg monitoring of the Arctic and the polar plateau chemistry may likely be totally different from the published Arctic Hg phenomenon. Many of our datasets are for short time periods. We tried to make it clear in the revised manuscript .

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4) Page 26680, lines 7-11: Is it widely assumed that the frost ïňĆowers are the halogen source or is this a hypothesis? The authors make it sound like a fact but I see it as more of a hypothesis with young ice (not necessarily with frost ïňĆowers present), snow blown over ice, and/or open water potentially providing halogen sources (see Kaleschke etal., 2004 GRL versus Simpson et al., 2005 GRL).

We went through this part

4) Page 26882, lines 5-6. “High” values are mentioned. Please give the range and a comparable range so that the reader can assess what “high” means in this case.

It has been done.

5) Page 26883 lines 25-28: This sentence is confusing. The 10 cm of snow (or water equivalence) per year is somehow associated with 10% of the deposited mercury being buried. Can this be elaborated a little bit beyond simply citing Brooks et al.?

At S. pole the vast majority of the deposited mercury is readily re-emitted back to the atmosphere as GEM. The flux measurements showed that the magnitude of these fluxes far exceeded those required by our estimate of the annual sequestration at depth. The sunlit zone depth was estimated at 10cm for the given surface snow density. The 60 metric tons number (annual sequestration) was obtained from vertical snow samples obtained from the clean-air sector upwind of the main station. All vehicles and personnel are prohibited from the clean-air sector, so the snow profiles are assumed to be undisturbed. The total Hg concentrations at depth were compared to the station recent accumulation estimates, and the 60 metric tons number was calculated by extrapolation to the remainder of the plateau. The ANTCI aircraft based measurements extending from the S. pole indicated that the air chemistry was outwardly similar in all location above the plateau.

6) Page 26685, line 28: What “surfaces?”

It was surfaces covered with snow or ice.

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7) Figure 1 is difïňĄcult to read. Might just be my copy but be aware of this.

We have checked the figure.

8) Can Figures 1 and 2 be set up so that they are both aligned the same way?

It has been done.

9) Does Figure 2 represent a “typical” view of halogen chemistry with respect to geography and ice or is it selected for a reason? Regardless, the reasoning for this speciïňĄc ïňĄgure (as common or an anomaly) should be stated.

It has been clarified in the text. This view is typical from springtime (in terms of BrO concentration)

10) The authors do not mention snow very much. There has been a lot of work on clear sky precipitation, snow, blowing snow in Antarctic and this likely plays a role in the potential for snow and AMDEs. Ie see Domine and Shepson Science 2002 with respect to snow chemistry, ice cores, long term records. I realize the focus here is on tropospheric atmospheric chemistry but obviously ice core results (and the archives they represent) are the only applicable way to address long term deposition for mercury

Ice core results are obviously a way to derive long-term deposition. However this can only be derived if the post-depositional fate of Hg is clearly understood. We know that a part of deposited Hg is reemitted to the atmosphere; consequently the ice-core record may lack some Hg matter. In order to discuss ice-core record, we think that 1) the fate of Hg in the first centimeters and meters of the snow have to be characterized in details;2) a transfer function of hg from the atmosphere to the snow, and the snow to the ice have to be estimated from atmospheric and snow long-term measurements A short discussion on this topic has been added as a perspective.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/9/C11634/2010/acpd-9-C11634-2010-

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supplement.pdf

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Interactive Comment