Atmos. Chem. Phys. Discuss., 4, S624–S629, 2004
www.atmos-chem-phys.org/acpd/4/S624/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Circumpolar transport and air-surface exchange of atmospheric mercury at Ny-Ålesund (79° N), Svalbard, spring 2002" *by* J. Sommar et al.

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Received and published: 30 April 2004

Sommar and co-authors present a new data set which contributes to our understanding of mercury depletion events not only at Ny-Alesund, but at all polar regions. They have measured several different mercury species and BrO concentrations using high-level measurement techniques to obtain good temporal resolution and come close to a mass balance for atmospheric Hg. Based on the field campaign and some modeling approaches, they conclude that the mercury depletion events observed during their study result primarily from transport of already depleted air masses and that in-situ chemical reactions and local deposition were not as significant at this location. Below I detail some suggestions for improving the manuscript. The primary area that I think



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could be strengthened is that of the bi-directional transfer of Hg between the air and snow pack. Some additional calculations and discussion would clarify their findings and possibly elucidate the importance of deposition to or emission from snow during depletion events.

SPECIFIC COMMENTS

1. Anyone familiar with the atmospheric mercury literature knows that there is no consensus on acronyms for the various mercury species. In the atmospheric science community, "PM" usually refers to Particulate Matter. I think the authors should consider changing the acronym for particulate mercury to avoid confusion. Other acronyms that may be less confusing and have been used by other authors (including some on this paper) are Hg-P, Hg(p) and Hgp.

2. Methods for RGM and PM. Were RGM and fine particulate mercury measured using the commercially available Tekran 1130 and 1135 speciation units? If so, include this information. If not, note that the units were designed and built by the authors and give some more information so readers know how this method would compare to other methods. Most importantly, is the filter for fine PM downstream of the denuder or do they have separate inlets? It has been found that with no denuder in front of the filter, RGM adsorption can cause a positive artifact for particulate mercury. Also, how were the RGM and PM from the manual samples analyzed? Was RGM also analyzed by thermal desorption with a Tekran 2537 as the detector? The reference for the TPM method, Berg et al., (2001), contains two methods. Which method was used for this study? For the TPM, there was no KCl denuder upstream of the filter. I think it is worth a brief discussion of possible particulate artifacts in the methods section based on findings from Berg et al. (2001) and Lynam and Keeler (2002).

3. Surface snow measurements. How were the surface snow samples collected? Was a sampler used to only collect freshly fallen snow samples (precipitation collection) or was snow removed from the snow pack covering the ground? If the later, how much



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snow was removed? Was it removed to a certain depth? How did you normalize your sampling so as to obtain a consistent amount of snow deposition?

4. Mercury deposition in snow. If I am matching up ZS and DI bars correctly on the graph, there appear to be several times in addition to May 5 when concentrations at the two locations were very different. Can this also be explained by different sampling times or differences in additions of fresh snow? Or is there some site difference?

Also, I would be very interested in an estimation of the deposition of mercury associated with the snow, especially during the GEMDEs. The deposition could be estimated by knowing the surface area of snow sampled. Does this term bring you closer to a mass balance for GEMDEs? One of the main conclusions of the paper, that air masses are depleted in Hg before being transported to Ny-Alesund, would be better supported if the amount of Hg deposited by snow fall did not account for the atmospheric losses. You note that during the major GEMDE total atmospheric mercury was at less than 20% of typical values. At other sites, the total is closer to 70-90%. This implies that most of the mercury has been lost by deposition either before reaching your site, or at your site. The discrepancy between sites clearly suggests that deposition is occurring somewhere, can you estimate how much at your location?

How does the deposition in snow compare to evasion from snow and do they balance each other? You suggest this in your conclusions, "a significant part of the oxidized mercury deposited onto snow and water surfaces is quickly re-emitted". I would like to see some estimate of the difference between deposition and evasion. The finding leaves the reader wondering how much of the mercury deposited is not then emitted and may actually enter arctic ecosystems? If all of the Hg is re-emitted, are the GEMDEs important to the local ecosystems?

5. Evasion of Hg from snow, section 3.2.3. What time interval was the modeled emission rate of 20-40 ng m-2 h-1 calculated for? Was it for the time period shown in Figure 7 or for the whole study? If it is for the whole study, it is surprisingly high and warrants



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further discussion in the text.

TECHNICAL CORRECTIONS

1. Abstract; some sentences could be shortened for clarity and ease of reading. Suggested shortening: The concentrations of RGM (reactive gaseous mercury), PM (particulate mercury), and BrO column densities (detected by DOAS) were very low except for a few individual samples during the major Hg0 depletion event. BrO vertical column densities obtained by the remote satellite ESR-2 and trajectory analysis indicate that the air masses exhibiting low Hg0 concentrations originated from areas with high BrO densities.

2. Abstract, line 13. You write that "The strongest modulation was observed for total mercury concentration in snow". This sentence could be clarified. Are you comparing increases in Hg before and after GEMDEs in RGM, PM and snow? I'm not sure it is a straightforward comparison because RGM and PM are measured per volume of air, while snow is measured per liquid volume, and the snow naturally concentrates Hg from the air. Perhaps it would be better to note that concentrations of Hg in snow showed a large (15x) increase in response to GEMDEs.

3. p. 1729, line 25; you state that temperature-driven processes are less important, but what are they less important than? Add a phrase at the end of the sentence to clarify what they are less important than.

4. p. 1732, line 20; insert "a" to read "hence to a certain extent"

5. p. 1733, line 24; This sentence is not clear, what ranges up to a few ten ppt? Also, "a few tens of ppt" is clearer. I would rewrite sentence as: Ozone destroying halogen species (XO) have been found in concentrations up to a few tens of ppt during ODEŠs at this location since 1995 (X=Cl; ...Wittrock et al., 2000).

6. I suggest using the word "automated" instead of "automised" or "autonomous" when referring to measurement instruments.



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7. p. 1740, line 7, change "trough" to "through"

8. p. 1741, line 13, make it clear that the reason the first GEMDE was only measured at ZS was because you were not sampling at DI yet.

9. Airborne transient mercury fractions, section 3.2.1, second paragraph. I suggest adding "ZS" to the sentence to read "The RGM measured at the higher altitude (at ZS) showed..." so as to be clear that you are referring to differences between ZS and DI. Also, I think you should include the range of concentrations measured at the two sites during the inter-comparison, especially since it is difficult to read the fine resolution on the graph, and possible that a high red bar is hidden by a high concentration blue bar on figure 2e.

10. p. 1745, line 22, move parenthesis to read "reported by Steffen et al. (2002)."

11. p. 1745, line 25, make it clear that the Hg-tot was measured in snow by writing "... a large increase in Hg-tot in snow was observed in consecutive samples..." Also should figure reference be for 2f and not 2d?

12. p. 1750, line 16, change "get" to "become"

13. I suggest making the ZS station red and the DI station blue in figure 3 to match colors in figure 2.

14. I initially though that the tick marks on figure 2e were bars denoting RGM measurements, and only on closer inspection realized that they were not blue bars, but black tick marks. The tick marks should be removed from this plot.

15. I had difficulty matching up figure 2f with the text. Are tick marks for a date centered between the ZS and DI bars for that date? I would expect both bars to be to the right of the tick mark. I think figure 2 should be increased in size (if permitted by the journal) so that more date labels can be added and it can be read more easily.

16. The shading is misaligned in figure 4b.



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REFERENCES

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Interactive comment on Atmos. Chem. Phys. Discuss., 4, 1727, 2004.