

Interactive comment on “Overview of the 2007 and 2008 campaigns conducted as part of the Greenland Summit Halogen- HO_x Experiment (GSHOX)” by J. L. Thomaset al.

Anonymous Referee #2

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Review of “Overview of the 2007 and 2008 campaigns conducted as part of the Greenland Summit Halogen-Hox Experiment (GSHOX)” by Thomas et al.

This paper presents the major findings of two field campaigns conducted at Summit, Greenland. It is generally well-written and does the job it needs to do. It should be published following minor revisions.

Much of the material has been presented in published papers that have already been through a review process, so I have not commented on these conclusions/interpretations.

My comments are generally minor.

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Specific comments:

P 17137 Line 17 to 20 – this sentence needs some fine tuning, for example, e.g. source of what? Also maybe emphasis directly that the scenario was controlled by chemistry rather than transport.

P17138 Line 1 – The “history” part: The South Pole research involved ambient measurements of NO, and discovered unexpectedly high concentrations. They did not strictly “discover that sunlight shining on snow caused production of NO and NO₂ that were released to the overlying air” but, interpreted their findings as such in the light of a literature survey (as explicitly stated in Davis et al section 3). In their literature survey, Davis et al cite Honrath et al 1999, Honrath et al 2000, Jones et al 2000, and Ridley et al 2000. Yes, the observations reported in Davis et al were done in December 1998 (so fit with your “in 1998...” angle), but Honrath et al’s work was done January 1999 (i.e. 1 month after Davis et al), and the Jones et al was done February 1999 (i.e. 2 months after). Given that Davis used the Honrath and Jones results to interpret their unexpected findings, and that Honrath and Jones actually carried out specific, designed experiments to quantify production and emission of NO and NO₂ from snow, these references need to be included, and the “history” section amended to explain this all more accurately.

P17138 Line 26 – change to “and passive (multi-axis or MAX) DOAS instruments.” The point is “active” and “passive” DOAS

P17139 Line 1 – “fluxing” is not a word; change to “being emitted” or something similar.

P17139 Line 21 – Personally, I think the place for a table summarising the full suite of measurements is in the overview paper. Given that it is already published, then okay, leave it like this. But please check the Liao et al really did include everything. For example, in the Liao et al table, BrO observations by DOAS are not described specifically as either LP or MAX, and a single uncertainty (10%) is given. Is this really for both techniques..? To answer that, I needed to go to the Stutz paper, in which there

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Comment

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is no mention of MAX-DOAS at all.. So, what happened to the MAX-DOAS data..?? If there weren't any good data that could be used, I'd suggest there is no need to mention MAX-DOAS in the overview paper.

P17140 Line 4 – “there is little direct evidence” – maybe not from CIMS or LP-DOAS, but what about the MAX-DOAS observations..?

P17140 Line 13 – what was the meteorological situation during this apparently highest BrO mixing ratios event..? Was there anything unusual that was not seen at other times during the two campaigns..? e.g. was there strong maritime air flow..?

P17141 Line 3 – change to “firn air”

P17141 Line 6 – Liao's table 1, which apparently reports measurements for both 2007 and 2008 does not mention tethered observations – goes back to my earlier point/question above.

P17141 Line 13 – what do you mean by “successfully evaluated”?

P17141 Line 21 – “by” 6 – 20% (could be “to”...)

P17141 Line 23 – “by” 12 and 10%

P17141 Line 26 – “time periods” rather than “intervals”

P17142 Line 1 – was there a consistent bias between LP-DOAS and CIMS? If so, it would be helpful for the reader to know at this point.

P17142 Line 23 – if the pool was Br-, then it would be aerosol; is this what you mean? If you have in mind a gas-phase pool, you should not write Br-

P17142 Line 28 – same point as above re Br-

P17142 Line 29 – doesn't it also seem likely that the source of Br- in snow at Summit is aerosol..?

P17142 Line 29/P17143 Line 1 – I don't think you can say that the bromine loading

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at Summit likely originates from bromine explosion events. The BE would generate gas-phase bromine, and as suggested above, the source of Br- at Summit could easily be via aerosol. The BE could coincide with high aerosol, if the aerosol was the BE bromine source.

P17143 Line 1 – do you mean gas phase or aerosol phase, or are you not distinguishing? Plus, and more substantively for your argument – to what height does convection operate over open water leads? Could this really inject something into the free troposphere? I guess what I'd like to know is, to what height could an open water lead convection plume extend?

P17143 Line 3 – you mean open “water” leads

P17143 Line 16 – no evidence for active halogen chemistry... do you mean “bromine” chemistry..? This is puzzling, given that conclusions from GSHOX are precisely that there is active bromine chemistry at Summit

P17144 Line 28 – Arctic needs capital A

P17144 Line 29 – entrainment over the ocean – I guess this refers back to the discussion about convection over open water leads. If so, keep the two consistent – ocean is not the same as open water leads, for example (even though I wonder about open water leads for such a mechanism, as reflected in my earlier comment)

Fig 1 – “lead” is spelt incorrectly (spelt “leeds”)

Final thought – is it surprising that BrO is not measured by satellites over Summit..? Do you know how close they are to being able to measure these low mixing ratios?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 17135, 2012.

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