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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Iodine and Bromine speciation in snow and the effect of elevation" *by* B. S. Gilfedder et al.

Anonymous Referee #1

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Gilfedder et al. present an interesting study of iodine and bromine speciation in snow samples that adds to the growing body of evidence pointing to the importance of organic species in atmospheric iodine chemistry. The authors make the valid point that lateral transport away from the oceans is commonly cited as the dominant control on atmospheric iodine concentrations over continents, while their data indicates that orographic lifting has a very strong influence on these concentrations. I wonder whether the wording of these sections of the manuscript shouldn't be altered to emphasise that the dominant control mechanism is removal, rather than transport. Orographic uplift exerts a strong influence on the data reported here because it results in precipitation. (Vertical) air mass movement over mountain ranges without precipitation would probably have no more influence on iodine concentrations than does lateral transport at

constant altitude without precipitation.

There is no discussion of analytical precision in the manuscript. Error bars should be shown for all data points in Figures 2, 4 & 5. This is particularly important for the derived parameters (Org-I and EF) for which analytical uncertainty should be propagated appropriately.

The presence of trace metal data in the manuscript puzzles me. There is no text in the Introduction giving a justification for its inclusion, and the only use made of the data seems to be a demonstration that other snow components behave similarly to iodine, bromine and sodium. The manuscript is perfectly good without this data and I don't think that it would suffer if this data was removed. However, if it is to be retained then the authors need to give further details of the sampling and analytical techniques employed. Many of the trace metals reported here are subject to significant potential contamination problems. Details of the cleaning procedures used for sampling bottles and the thawing and acidification protocols should be given. At what stage were subsamples for I/Br and Na analysis removed, how long were thawed samples left before acidification and how long after acidification before analysis?

A few other minor points:

I am not sure that the statement "bubble bursting is the dominant mechanism for the iodine enrichment" (p 997) is correct. Seto and Duce (J. Geophys. Res., 77, 5339-5349, 1972) examined the role of bubble bursting in aerosol iodine enrichment in controlled laboratory experiments. Although they observed significant iodine enrichment, their results fell well below enrichments observed in marine aerosols. They concluded that mechanisms other than bubble bursting were more important in the marine environment, and more recent work would suggest that these mechanisms were associated with sea-air transfer of iodinated gases.

The text on p 999 should be altered to make it clear that Campos (1997) studied changes in iodine speciation only.

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p 1000. Please give some more details of standard reference material BCR611. I believe it is a groundwater reference material. (This is not a criticism. I am not aware of a more suitable reference material).

p 1003. I think “mountainous” would be a better description of the Alps than “monotonous”.

p 1007. “dust” is a very vague term when applied to the metals studied here. Is this intended to mean soil-derived mineral dust? Many of the metals reported here (e.g. V, Mn, Zn, Pb) have significant atmospheric sources from anthropogenic activity rather than soil uplift.

Figure 2 and discussion. I am unclear whether the results reported here were for the initial transect survey along the Elsa Weg only, or also included the subsequent sample collections.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 995, 2007.