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Atmospheric
Chemistry
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Discussions

Interactive comment on "Measurement and interpretation of gas phase formaldehyde concentrations obtained during the CHABLIS campaign in coastal Antarctica" *by* R. A. Salmon et al.

Anonymous Referee #1

Received and published: 24 March 2008

The manuscript of Salmon et al. describes gas phase measurements of formaldehyde at Halley, Antarctica. Measurements were obtained over (nearly) a year period using an Aerolaser 4021 analyzer; essentially a scrubber that strips gas phase formaldehyde from ambient samples using sulfuric acid, followed by reaction with Hantzsch reagent, producing a product that can be measured at very low concentrations via fluorescence.

This paper is an important contribution to the current literature for several reasons. First, this set of data represents an extended period (nearly a year) that captures seasonality in the HCHO concentration. Second, this is the first data set obtained from

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Halley, and is important in that it can be compared and contrasted to previous measurements obtained at Neumayer. Although these are both coastal sites in the Weddell Sea, this manuscript shows some intriguing differences between the two sites, indicating we still do not fully understand the sources and sinks of HCHO in Antarctica.

The introduction is sufficiently detailed to give a good background to an audience that may not be familiar with the field of snow/ice photochemistry and polar atmospheric chemistry. My only caution would be in the wording of the final sentence of the second paragraph of the intro. "the snowpack in polar regions is a major source of formaldehyde to the troposphere". It is true that the LOCAL concentration of HCHO can often only be explained with a significant snowpack source, but the wording of this sentence might be interpreted as the snow being a significant source of HCHO to the regional/global troposphere, which is not the case. This is however, a minor point and likely not at all what the authors intended, but it might be interpreted as such if the point is not clarified.

I also applaud the authors for their careful calibration studies. The best way to calibrate is to introduce a gas phase standard at the end of the instrumental inlet line on-site. Unfortunately this is not often feasible, and the authors have done a thorough job of quantifying the calibration, inlet line losses, etc.

In terms of the differences between the Halley and Neumayer data - I am not familiar enough with the meteorology of the two sites; but could a possible additional explanation include varying boundary layer heights? Also, is there a difference in the delivery of aerosol/sea-salt to each site? i.e. could there be greater halogen activity at one sight v. another (and thus differing relative source/sink strength?). Is there a closer vicinity of open/refrozen leads to one site v another? Perhaps the formation of frost flowers could give a large difference in halogen chemistry between sites?

For equation 1, the horizontal line (separating numerator from denominator) should not extend to the left of the =.

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The authors have summarized their data in relatively few figures/tables. I would suggest adding a table that includes some key measurements (as reading exact concentrations from a log scale figure (Fig 2) is often difficult). I would suggest adding a table to include some values such as monthly (or seasonal?) average, min/max, standard deviation to indicate variability, etc. This would help summarize the data without making the reader try to interpret Figure 2 data points quantitatively. Some of these measurements are in the text, but it would valuable to summarize it in one table for the reader.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2337, 2008.