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Atmospheric Chemistry and Physics Discussions

Interactive comment on "First-year sea-ice contact predicts bromine monoxide (BrO) levels better than potential frost flower contact" *by* W. R. Simpson et al.

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Review of "First-year sea-ice contact predicts bromine monoxide (BrO) levels better than potential frost flower contact" by Simpson et al.

General

The authors analyse bromine monoxide (BrO) and ozone data measured at Barrow, Alaska. The dataset spans 40 days (DOY 80-120) in the year 2005. The BrO signal is interpreted using meteorological back trajectories in terms of the contact time of

air parcels to different sea ice types and the so-called potential frost flower (PFF) coverage. The latter parameter was invented by the reviewer in order to investigate the hypothesised role of salty frost flowers on polar tropospheric chemistry. The concept of contact time to different sea ice types is innovative and should receive further attention. However, the methods and the interpretation of the results are problematic. Therefore, I suggest major revisions. My main criticism is that the dataset is rather limited and the correlations seem to be not significant. Furthermore, a good theory has to explain also the spatial BrO distribution as it is observable by satellite sensors. This is obviously not the case if one assumes that the bromine source is first-year sea ice (contact) only. Moreover, a good hypothesis should explain the distribution of other tracers such as mercury.

Specific issues

11051 I personally appreciate the term potential frost flowers in the title. However, I doubt that it is widely known. A title such as "A trajectory study .. BrO .. O_3 .. sea ice .. Barrow, Alaska .. April 2005" would be much more descriptive.

11053 5-30 The authors state that there are two hypotheses for the mechanism of bromine liberation from the ocean to the atmosphere. The first one is by the means of salty frost flowers that grow on new ice and the second is by the means of salty snow. In my opinion these "two hypotheses" are intermingled and connected. I would probably divide the hypotheses in, e.g. biological origin of bromine and inorganic sea ice origin if necessary at all.

11054 10 and Fig. 1 I can not see the tracjectory passing over a polynya. A more detailed map including the PFF parameter would be instructive.

11054 15 What forcing data has been used for the HYSPLIT model?

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EGU

11054 20 The authors used the AMSR-E sea ice data available at the Institute of Environmental Physics at the University of Bremen. These data are regarded as "experimental" for certain reasons. The adress www.seaice.de is my personal website. Please use a reference to the Institute of Environmental Physics.

11055 The authors introduce some ad hoc rules to implement dispersion and to define the contact time. This should be done with more care. The stability of the boundary layer and the cloud coverage are certainly also important parameters that should be incorporated in the analysis. This information would be available e.g. from MODIS or AIRS measurements.

11055 According to the proposed concept of a contact time, an air parcel could steadily increase its BrO content as long as its stays above first-year sea ice. This is certainly not true. One has to include a BrO sink in the system.

11056 Voss et al. (2003) examine the sea ice radar backscatter for melt- and refreezing cycles that prevents the discrimination of multi-year ice. Kwok (2004) would be a better reference here.

11057 The authors exclude the most interesting datapoints that exhibit ODEs. More discussion is neccessary as this influences the resulting correlation.

11057pp *The authors have to investigate the significance of the correlations. In doing so they must not neglect the effect of temporal autocorrelation. The number of statistically independent datapoints is drastically reduced for autocorrelated time series. This is my most important point of criticism. All conclusions drawn from the correlation coefficients should be thought over in the light of possibly new results.*

Further issues and suggestions

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- The authors draw a very black and white picture of Arctic sea ice with only two ice types. First-year ice is, defined by the WMO, more than 30 cm thick. Other thinner (and saltier) ice types such as nilas or young ice are probably more important for the processes under consideration. The authors should try to distinguish between the ice types in more detail which is feasible as demonstrated by Jacobi et al. (2006).
- The authors should discuss the influence of temperature, i.e. the role of cold spots as it is mentioned in the study of Bottenheim and Chan (2006).
- In April 2005 it was relatively warm in the Arctic. In my PFF dataset I can only see larger PFF values for December to End of March. At the end of April there were virtually no frost flowers anymore. Therefore, I don't think that the comparison is adequate for DOY$>$100. One could divide the ozone timeseries into DOY 80-100 and 100-120. In the latter part there are no longer lasting ODEs which indicates in fact two different different regimes.
- The authors should compare their BrO measurements to satellite data and fill the gaps of the time series. How can the authors explain the spatial distribution of satellite BrO?
- There are much more station datasets available, e.g. the one mentioned of U. Frieß, that should be investigated with the proposed methodology.
- The authors should discuss the enhanced accumulation of mercury close to a coastal polynya (Bargagli et al., 2005). How could they explain the spatial distribution by assuming a first-year sea ice source of bromine only?

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