Atmos. Chem. Phys. Discuss., 9, C11736–C11741, 2010
www.atmos-chem-phys-discuss.net/9/C11736/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Satellite observations of long range transport of a large BrO cloud in the Arctic" *by* M. Begoin et al.

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Received and published: 25 March 2010

We would like to thank the reviewer for starting the discussion and for his valuable comments. As described in the replies to the referees, we have conducted additional simulations and data analysis.

This review is by Owen Cooper, co-Editor of this manuscript. I am posting my comments now to stimulate the open review process. My review is written without the benefit of first reading the reviews by the anonymous referees and their opinions will have a major influence on my decision regarding publication in ACP. From my first reading of the paper there are 4 topics that in my opinion require additional attention and revision:
1) further description of the PFF methodology and validity;
2) elaboration on the new results of this study;

3) additional comparison and quantification of BrO from GOME-2 and FLEXPART;
4) corrections of writing style and grammar.

1) I don't find the description of the PFF calculation to be adequate and further discussion of the validity of the PFF maps is required. I read Kaleschke et al (GRL 2004) to learn more about the PFF technique and its verification but I still have questions.

What is the resolution of the PFF maps? I assume the resolution is limited by either the meteorological fields and/or the SSM/I sea ice retrievals.

The resolution is limited by the resolution of SSM/I sea ice maps (12.5 km) and JRA-25 meteorological reanalysis data (120 km).

Has the PFF product actually been verified by observations of frost flowers in the field?

The verification of potential frost flowers data with measurements is still an open issue because of the difficulties of measuring the frost flower coverage in the field.

During March sea ice extent is at its maximum and with the exception of polynyas along the coast lines or a few leads that open up in the middle of the pack ice as the sea ice shifts, the Arctic is covered by ice that is at least 4-5 months old. Because frost flowers have a lifetime of just a few days the PFF maps indicate freshly formed frost flowers. This means that the regions with frost flowers must have had open water just a few days before. The PFF map for March 28 indicates that much of the region between Siberia and the North Pole had 10% or more of its area covered by frost flowers, which means that a few days before much of this region must have had extensive lead formation. I find it difficult to believe that such a broad region was so extensively fractured during March. Do the SSM/I retrievals show such heavy lead formation?

The legend of PFF maps wasn't plotted correctly. Shown PFF values were four times too large.

What is the spatial resolution of the SSM/I retrievals?

The spatial resolution of the used SSM/I data is 12.5 km.

How wide do the leads have to be before the instrument can detect them? My impression of the

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PFF product is that it overestimates the regions with frost flowers. I would like to see SSM/I retrievals that show the extensive number of leads that could produce the large regions of PFF.

A main limitation of the potential frost flower (PFF) method is the accuracy of the sea ice concentration data. The sea ice concentration in the central Arctic exhibits errors that are larger than the actual ice concentration variability. Traditional passive microwave ice concentration data over the high concentration Arctic sea ice exhibit errors and biases that are about one magnitude larger than the true variability (Andersen et al., 2007). Although larger coastal polynyas are reasonably represented in 85 GHz SSM/I sea ice products, leads and openings in the central Arctic can not be resolved. Thus, the interpretation of PFF data based on SSM/I ice concentration is very difficult with the present data. One has to know that the coastal values are more accurate than the values at the pole. However, an ice concentration and PFF error field has not yet been derived. This was the reason why the central Arctic region was not discussed in Kaleschke et al. (2004). An improved sea ice concentration dataset may help to calculate more accurate PFF values in the whole Arctic. A step forward in this direction is a new lead detection technique from AMSR-E data which was recently developed (Röhrs and Kaleschke, 2010). However, an improved sea ice concentration and PFF dataset has not yet been produced. Therefore, we decided to omit the PFF topic from the present work.

Andersen, S., R. Tonboe, L. Kaleschke, G. Heygster, and L. T. Pedersen (2007), Intercomparison of passive microwave sea ice concentration retrievals over the high-concentration Arctic sea ice, J. Geophys. Res., 112, C08004, doi:10.1029/2006JC003543

Röhrs, J. and Kaleschke, L.: An algorithm to detect sea ice leads using AMSR-E passive microwave imagery, The Cryosphere Discuss., 4, 183-206, 2010.

2) Please make a better case for how your work has advanced our understanding of BrO explosions. Does your simulation provide better insight into the long range transport of a BrO plume than previous studies? Is this a new method for tracking and simulating a BrO plume?

Observations of BrO explosions in polar regions are not new, and a number of studies have been published using either ground-based observations or satellite data of BrO. There are however

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several aspects in this study which are new and advance our understanding of polar BrO events:

- *To our knowledge, this is the first study directly addressing this type of huge BrO plumes and its displacement over several days. Previous studies interpret enhanced BrO as rather local effects and transport of air masses depleted in ozone, not transport of the BrO itself. In fact, the comments of the reviewers show that this is not a trivial point as it is difficult to understand why the BrO remains in the air masses over several days.*

- *A second interesting aspect of this event is the combination of very high wind speeds and BrO - in most other studies, BrO explosions are linked to shallow stable boundary layers and low wind speeds. In that sense, the event described here is similar to the Antarctic one described in Jones et al., 2009.*
  The type of large-scale BrO plume is in contrast to the more local BrO events which are also apparent in the GOME-2 data shown and can only be fully appreciated in satellite data providing full spatial coverage in spring as is the case with GOME-2 in contrast to the previous data sets from GOME and SCIAMACHY.

- *A third new and important aspect is introduced by the discussion initiated by the reviews which addresses the possible stratospheric impact on the satellite observed BrO enhancement. We do not know of any study investigating tropopause heights, trajectories at different altitudes and the link between ozone and BrO in the literature. Although in the absence of in-situ observations no proof can be given on whether or not the stratospheric BrO has an impact on the observations, in our opinion the discussion included in the revised manuscript does provide an important contribution to this question.*
  In response to the editor's comments, we have tried to make the novel aspects clearer in the revised manuscript.

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3) In the conclusions you state that BrO recycling must have occurred to maintain the near constant BrO levels. But I am not yet convinced this is the case. You need to actually add up the mass of BrO as seen by GOME-2 in the transport corridor to show that the mass remained constant. This can be done as follows: Because the FLEXPART BrO tracer is passive with no removal, the mass of tracer in the plume remains constant even as it disperses. For each daily GOME-2 map identify all grid cells that contain FLEXPART tracer, then sum up the BrO detected by GOME-2 in the same grid cells. If the total quantity of GOME-2 BrO in this transport corridor remains constant (allowing for errors due to clouds, etc.), then you can argue that perhaps the BrO is recycled as it would be unlikely that fresh BrO would be released into the plume at the same rate at which it is removed. On the other hand if the GOME-2 BrO becomes significantly greater than the FLEXPART BrO then you would have to conclude that additional BrO was emitted into the plume. But this of course would only provide a lower estimate of the extra BrO released because it assumes that all of the initial BrO released on March 26 is recycled within the plume. If the original BrO was removed then the additional BrO releases would be much more.

We would like to thank the editor for this suggestion. We have tried to perform the proposed analysis, but this turned out to be difficult. One reason is the significant dilution of BrO in the FLEXPART calculations which is not so apparent from the figures but is significant if quantitative comparisons are made. Also, the spatial agreement between model and data is not perfect and depending on which FLEXPART run is used, different parts of the plume are selected. Finally, the total amount of observed BrO in the event does indeed decrease as can be seen from the figures, and this is more pronounced in the revised data set as the stratospheric correction implemented has a larger impact in the Canadian Arctic. We have taken this point into consideration in the revised paper.

4) Below are my suggestions for improving the paper's style and grammar. If no explanation for a comment is given, please insert/replace the suggested text into the appropriate place in the manuscript.

All suggestions have been incorporated into the revised paper with the exception of sentences

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which have been removed in the new manuscript.

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Interactive Comment