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Atmospheric
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Discussions

Interactive comment on “Observations of iodine monoxide (IO) columns from satellite” *by* A. Schönhardt et al.

A. Schönhardt et al.

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(Referring to the Interactive Comment of the Editor from 28th November 2007)

The Interactive Comment of the Editor is divided into 4 separate General Comments and a few Short Comments, which will be addressed in the following in unchanged order.

Comment 1

The detection of small absorbers like IO by differential absorption spectroscopy is a challenging task, and the authors do a fair job in describing main sources of

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uncertainties in their retrievals. In particular the paper addresses in some details the error sources associated to the AMF calculation, but when coming to the results only IO slant columns are finally given. I can understand that given the number of unknowns associated with the AMF calculation the authors decided to stick to slant column reporting although this is limiting somewhat the discussion (e.g. attempt to explicitly consider the albedo effect could have been undertaken). In any case, I think that this should be (more) clearly stated.

The conversion of slant columns to vertical columns by use of the air mass factor (AMF) is a very important issue in the retrieval of trace gases. It is true that the conversion of slant columns to vertical columns was not performed for our IO results due to a high degree in uncertainties connected to the AMF calculation.
One specifically important aspect in this discussion is the unknown altitude profile of IO. Especially for low albedo scenes, the AMF varies by typically a factor of 2 for different possible IO profiles (cp. Sec. 5). The global and Southern Hemispheric IO maps therefore show the slant columns of IO. Although this is stated in the text and in the figures, it might be not clear enough for the reader. Therefore this has been expressed more clearly in the revised version.

Comment 2

Looking at the global IO map in Figure 4, I am surprised not to see any signature of the Southern Atlantic Anomaly (which usually is a typical source of noise for the low absorbers). Is there a filter applied to remove data affected by the SAA, or is there any particular reason why IO retrieval is less sensitive to SAA?

It is true that there is no signature of the SAA (South Atlantic Anomaly) visible in the

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global map of IO (Figure 4). The SAA impacts on SCIAMACHY data quality because high energy protons reach the detectors and read out electronics. This results in individual detector pixel read outs being saturated and a slight increase in dark signal. Whilst it is possible to remove saturated outliers from individual spectra in a pre screening, this has not been undertaken because the general impact of the SAA on the spectral window for IO is relatively small. So no further action was considered necessary at this stage.

The visible wavelength region used for the retrieval of IO (from channel 3 data of the SCIAMACHY instrument) exhibits the highest signal-to-noise ratio of the measurements from this instrument. Therefore, the influence of the SAA is only small. This small effect is identified more clearly in monthly averages, where the IO amounts scatter somewhat in the SAA region and the fit quality is slightly poorer for some scenes. The effect is not very pronounced and only few datapoints are affected. Therefore, this is hardly visible in the 3-month averaged plot.
A wavelength region typically affected strongly by SAA is the ultraviolet. For absorbers retrieved from SCIAMACHY channel 2 data (e.g. BrO, SO_2, and Formaldehyde) this is usually visible also for longer time averages. Other small absorbers retrieved in the visible wavelength region are also not affected by the SAA. A good example for this is Glyoxal, typically retrieved in the visible spectral region from 436 – 457 nm (channel 3). Comparing global maps of Glyoxal with Formaldehyde [Wittrock et al., 2006], the SAA region is visible for Formaldehyde, while this is not the case for Glyoxal.

Comment 3

The validation part (section 7) should be considered as important here, since we are talking about the observation of a "new molecule"; using SCIAMACHY. I found the discussion on the comparison with CHABLIS data a little bit difficult to follow.

Why not simply showing the validation data in Figure 6, so the reader can easily judge on his own how good it correlates with SCIAMACHY data? Judging from the acknowledgements, the correlative data are effectively available to the authors.

In our manuscript, we compare a time series of retrieved satellite IO amounts around Halley Station, Antarctica, with ground-based measurements published by [Saiz-Lopez et al., 2007a]. Following the suggestions of both, the editor and referee ♯1, this ground-based data from the CHABLIS campaign has been shown as an additional figure in the revised version of the manuscript for better comparison. The exact figure as printed in the cited paper has been displayed.

Comment 4

Generally speaking I found the discussions associated to Figure 7 interesting but also to al large extent highly speculative, as mostly derived from known literature material on the subject, not necessarily strongly supported by the observations. E.g. in my view the MODIS map hardly provides any relevant information. Personally I retain as main result the fact that the BrO and IO spatial distributions are clearly distinct which strongly argues in favor of different activation processes. I am personally not convinced that transport plays a major role in explaining the observed IO distributions, since the gradients between claimed (coastal) source regions and continental regions are in fact rather weak.

- We agree that the MODIS map does not provide enough relevant information for the discussion on the possible sources of iodine species in Antarctica and therefore it has been discarded in the revised version. Due to its general importance and relevance, the discussion related to organic precursors has not been erased completely, but has been shortened.

- Over Antarctica, enhanced values of IO are detected also above the continent. This is a very surprising finding, and explanations of this fact have to be made carefully.
  At present, no established release process of iodine species over the Antarctic inland ice is known. We consider it highly unlikely, that biogenic processes (which lead to iodine release in other regions) play a relevant role over the Antarctic continent.

  In some cases, the gradients from fresh sea ice towards the continent are weak. Nevertheless, transport may be of some relevance. The importance of direct transport of IO is probably low as the IO lifetime is short. Possibly iodine containing precursor substances are transported and release of iodine atoms occurs later on in the transport process. As for IO, the lifetime of these precursors might be too short to explain the observations.

On a daily basis, the scatter and variability in the IO data is too large to easily trace a single transport event. But it is important to note, that transport events for BrO, which also exhibits a short lifetime, are observed to extend extraordinarily far inland. On single days, enhanced values of BrO are detected in a large plume several hundreds of kilometres into the continent. This is shown nicely on a daily map of BrO on "`http://www.iup.physik.uni-bremen.de/doas/scia_data_browser.htm?gas=bro:year=2007:month=10:day=10:view=sh`" for example for the recent days Nov 6th, 2007, and Oct 10th, 2007, as well as for monthly averages such as for example in December 2005 or 2006. In general, also short lived molecules can be transported over far distances if efficient recycling mechanisms exist.

For IO there is another possibility that makes transport processes even more interesting and probable. Organic I is produced, e.g., by the maritime biosphere in the ocean, below and in the ice. Indeed, the exact processes for this still have to be proven. When openings in the ice occur, organic iodine components can be released, and eventually IO forms. At enhanced concentrations, IO produces higher oxides of I, which preferentially being hygroscopic attach to aerosols or themselves are aerosol condensation nuclei. The iodine in the aerosol or later cloud phase is then transported and deposited at the surface. In these regions snow photochemistry (HNO_3 + hν $\rightarrow$ OH + NO_2) results in local high IO amounts via oxidation of HIO_3 by OH (OH + HIO_3 $\rightarrow$ H_2O + IO_3, IO_3 $\rightarrow$ IO + O_2). This hypothesis implies, that IO is recycled and reformed from the aerosol phase also at further distance from the sea ice. These considerations cannot serve as a proof of this process, but give a possible explanation. The suggested process and the corresponding chemistry are subject of further research.

In response to the editor's comments, we have revised the respective paragraph in Sec. 8.2, where the patterns for BrO and IO are compared and the importance of transport for the distribution of IO is discussed. The difference between the patterns is mentioned in that section. In the revised version, we have explained in more detail how IO may be transported towards the continent, following the above considerations. This comprises the discussion of transport of iodine containing aerosols and subsequent recycle and snow photochemistry. We have included the remark, that transport events for the short-lived BrO reaching far into the Antarctic continent are indeed observed.

Minor Comments

- *Title: I am not sure it is necessary to give the IO acronym here.*
  The acronym is not needed in the title. It has been omitted.

- *Abstract, l. 13: change "release or" by "release of"*
  The typing error has been corrected.
- *P. 4, middle of second para.: add reference to Pundt et al.*
  The reference to [Pundt et al., 1998] has been added.
- *P. 14, last line: Figure 4 shows data over the Sep-Nov period, and not Oct-Nov as indicated in the text*
  The mistake in the text describing Fig. 4 as Oct-Nov period has been corrected and now reads "September to November".

References

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