

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

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Manuscript no. ACPD-2007-0315 Authors: A. Schönhardt, A. Richter, F. Wittrock, H. Kirk, H. Oetjen, H. Roscoe, J. Burrows Title: Observations of iodine monoxide (IO) columns from satellite

Although the detection of IO from SCIAMACHY has been already reported, this paper represents the first attempt to document the global distribution of this interesting atmospheric molecule and to discuss the spatial and temporal variations observed over a period of 3 years. Halogen oxide chemistry in the troposphere is currently a scientific hot topic, in particular as regards our understanding of boundary layer polar chemistry, and therefore the data set introduced in this manuscript is of high relevance and definitely of great interest for the ACP readership. The paper is clearly written and well

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organized, and the figures are of good quality. I recommend its publication in ACP after attention to the points raised below.

General comments

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 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.

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?

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.

Generally speaking I found the discussions associated to Figure 7 interesting but also to a 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.

Specific comments

Title: I am not sure it is necessary to give the IO acronym here

Abstract, l. 13: change "release or" by "release of";

P. 4, middle of second para.: add reference to Pundt et al.

Pundt, I., J.-P. Pommereau, C. Phillips, and E. Lateltin, Upper limits of iodine oxide in the lower stratosphere, J. Atmos. Chem., 30, 173–185, 1998.

P. 14, last line: Figure 4 shows data over the Sep-Nov period, and not Oct-Nov as indicated in the text

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12959, 2007.

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