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Atmospheric Chemistry and Physics Discussions

Interactive comment on "SO_2 and BrO observation in the plume of the Eyjafjallajökull volcano 2010: CARIBIC and GOME-2 retrievals" *by* K.-P. Heue et al.

Anonymous Referee #1

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This paper reports on satellite and aircraft measurements of SO2 and BrO in a volcanic plume following the 2010 eruption of the Eyjafjallajökull volcano. It appears to be a carefully done piece of work. The measurements are of high quality and the methodology is rigorous. I believe this paper should be published, but after addressing a number of points. Specifically, I find the paper rather long and technical. In its current state, it would probably be more appropriate for Atmos. Meas. Tech. Although I understand this paper has been written to be part of the special issue dedicated to the atmospheric implications of the Eyjafjallajökull, I found the paper rather weak in the conclusions for a journal like Atmos. Chem. Phys.

General comments

1) I find disappointing that the authors limited the discussion to general statements, in substance 'SO2 and BrO could be measured in the plume using CARIBIC' and 'Both SO2 and BrO observations agree well with simultaneous satellite observations'. As there is currently substantial interest in measurements of volcanic BrO, I expected (before reading) that the authors would try to interpret their results a bit more. Personally, I would have found interesting to relate these observations to other published BrO results (e.g., Bobrowski et al., 2003, 2007) and say more on the chemistry in the plume at the time of the measurements (e.g., via the BrO: SO2 ratio). Moreover, it is currently not fully clear in the scientific community why BrO is detected for some volcanoes and why not for others. As BrO could be detected using GOME-2 only for several days among more than a month of eruption of Eyjafjoll, it could be very interesting to relate the BrO observations to the different phases of the eruption, in order to better understand the origin of bromine in volcanic plumes.

2) From my knowledge, it is the first time that volcanic BrO has been detected from an aircraft DOAS instrument. In my opinion, the authors should briefly explain what is exactly/can be the added-value of such aircraft measurements with respect to other observations (using ground-based or satellite DOAS instruments) for (future) studies of volcanic bromine emissions.

3) In general, an effort must be done to improve the readability of the paper (see below for some specific comments). Although I understand that the authors would like to show their other (nice) CARIBIC data (mercury, hydrocarbons..), I am not convinced it really brings something to the discussion on SO2 and BrO.

Specific comments

-As far as satellite detection of volcanic BrO is concerned, a reference to the work of Theys et al., 2009 (GRL) should be added.

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Interactive Comment

-In section 2.2 no details on RTM settings is given for the calculation of AMFs. It would be good to mention somewhere that the latter will be provided in sect 4.4.

-An error of 7% is associated to the AMF but if I understand well it is a statistical error coming out of the Monte Carlo RTM. It doesn't include any error acting systematically due to parameters (albedo, clouds,..) of the RT simulations. This error of 7% is then used directly to estimate the VCD error just like it would include all sources of errors. Please clarify.

-Fig. 1: it is clear from the SO2 residuals that remaining O3 absorption structures are present. Although it probably doesn't affect too much the quality of the SO2 SCD data (as the SO2 signal is strong), it is not mentioned at all. For BrO, I am not convinced it is a good fit. I am concerned by what happens below 335 nm where O3 signatures can clearly affect the quality of the fit. This must be addressed. The retrieval of O4 looks particularly bad in Fig 1!

-Is there a specific reason why not using the same fitting window/settings for CARIBIC and GOME-2 (especially for SO2)?

-The reader has no possibility to evaluate the quality of the fitting results for GOME-2 (neither SO2 nor BrO). A similar figure as Fig.1 should be added.

-L 211: please further explain why an O3 cross-section scaled with a polynomial is used? How this procedure can account for the dependence of the atmospheric light paths with wavelength?

-L 244-248: it is unclear what AMF has been used for the satellite data (geometric AMF or RT sim).

- L288-end of sect 3.2: I don't understand why Fig. 7 shows only a comparison between O4 SCDs* (calculated) and SO2 SCDs (measured)? Although both data sets shows a clear anticorrelation which is a good point, there is no way for the reader to judge if the O4 SCDs * agrees well with the measured O4 SCDs!

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-L 311: I am bit surprised by the value of 0.95 for the single scattering albedo. I would have expect a lower value as the ash emitted by Eyjafjoll are quite absorbing aerosols. Please comment.

-In my opinion, Figure 11 is useless. Simply state that many other satellite instruments could similarly observe the SO2 plume during this period (and maybe add a reference to the SACS website or another site).

-Figure 12 could be displayed for the whole Western Europe (just as Figure 11). This would strengthen the GOME2 BrO results if one could identify BrO close to the volcano as well..

-I found that the GOME-2 BrO results were not very well used. I would find very interesting to study the BrO:SO2 ratio based on GOME-2 elsewhere than in vicinity of the CARIBIC observations area (e.g. close to the volcano) and at other dates than the 16th November.

-Add a short discussion on the BrO/SO2 ratio estimate you obtain. How does it compare with other estimates in the literature.

Minor comments

-section 2.2: I found perturbing to first give a background on DOAS, then introduce the instrument and then present the DOAS settings. It would help the reader to first introduce the instrument and then talk about DOAS (background and SO2 and BrO settings).

-page 6: footnote – this sentence is a little bit ambiguous. What is “normal” O4 SCD? Star means the true O4 SCD? While ‘no star’ means measured O4?

-L194-201: at this stage of the paper, it is hard to understand the role of Figure2.

-Fig 9 has to be adapted. It is difficult to read. Please increase the width of lines.

-Harmonize the color bars of Figs 6 and 13 to facilitate direct comparisons.

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Typos

L30: “distriubtion” → “distribution”

L121: “Togetehr”→ “Together”

L122: “programm”→ “program”

L133: “colud” → “could”

Caption Fig. 4: “The flight pressure altitude”→ “The flight altitude” (?)

L411: (Bobrowski et al., 2007, e.g.) → (e.g., Bobrowski et al., 2007)

Caption Table 2 : BrO error of 1.8 ppb→ 1.8 pptv

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 29631, 2010.

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