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Interactive comment on "BrO/SO_2 molar ratios from scanning DOAS measurements in the NOVAC network" *by* P. Lübcke et al.

Anonymous Referee #1

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Review of BrO/SO2 molar ratios from scanning DOAS measurements in the NOVAC network

By P. Lübcke, N. Bobrowski, S. Arellano, B. Galle, G. Garzón, L. Vogel, and U. Platt
Submitted to Solid Earth Discussions

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In this work the authors describe a procedure they implemented to automatically process UV spectrometer data obtained by automated NOVAC type I ground-based mini DOAS stations for BrO and SO2 retrievals. A remarkable ∼3-year-long record of observations collected by two NOVAC stations located at Nevado del Ruiz volcano, Colombia, is included as a demonstration dataset. The obtained timeseries covers a very

interesting period where Ruiz experienced significant unrest and based on their observations of changes in the BrO/SO2 ratio before and after a small eruption in June 2012, the authors suggest that plume BrO/SO2 ratios may eventually be useful in tracking volcanic activity. While the authors choose not to discuss possible causes for changes in BrO/SO2 at Ruiz, their stated intent is to apply similar techniques to other NOVAC datasets in the future and to introduce automated BrO retrievals as an operational volcano monitoring parameter. There is a lot to like in this manuscript: the authors (and field personnel) are congratulated on collecting an outstanding dataset that is of high value and clearly should be published. Also, the manuscript is in general well-written and clearly presented, despite some language and organizational issues and a few minor editorial mistakes that deserve attention.

My main concerns with the manuscript in its present state are twofold: i) the data-processing procedure and the discussion of possible pitfalls in automating the procedure are not as robust as they could be (discussed below), and ii) the example dataset is – at present – the most novel aspect of this work and its greatest strength but alas, it is not discussed in detail since it is simply used as a tantalizing example of an extremely beautiful DOAS dataset. If the data processing procedure were stronger (the purpose of the paper) I wouldn't feel so let down by the lack of discussion of the Ruiz dataset. Since developing a processing procedure is the main goal of the study, I wonder if a more specialized journal might be better suited for publication (e.g. in the spirit of Bobrowski et al., 2010) that would allow for the approach described here to be more fully developed. If the emphasis is placed more on describing and interpreting the dataset from Ruiz, I believe the choice in publication is appropriate but then the authors must substantially re-think the purpose of the present work. As is stands, I feel the manuscript shows considerable promise and touches on two important subjects – each important and worthy of attention – but that neither is considered as fully as they deserve. My main recommendation is to re-work the data processing procedure to be more fully developed (see comments below). Also, the authors might consider submitting the manuscript to another, more specialized journal. My final recommendation

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would be to start writing up a detailed analysis of the Ruiz dataset since I'll be very interested to read it! Please see below for further comments.

Specific comments

My main concerns regarding the algorithm developed here is that a few important issues are not discussed and that the procedure should, in my opinion, be more generalized and contain better assessment elements to assure quality control. For example: is any consideration given to meteorologic clouds and how they may affect retrievals or be identified in the data? My concern stems from the fact that the summit of Ruiz (and ∼6000 m, the likely transport altitude of the plume) is oftentimes very cloudy. What impact would clouds have on the retrievals and how are cloudy data identified and dealt with? Is it possible to compare cloudy vs. non-cloudy data to see if there are impacts? I am also wondering if any consideration is given to ash in the plume and its effects on the retrievals? Ash must be an issue since it covered the solar panels and temporarily knocked the stations offline.

With regard to quality assurance, I appreciate the discussion on the effects of temperature. However, might some other additional methods prove advantageous for assuring measurement quality? For example, the present procedure fits BrO in the region 330.6 – 352.75 nm (Section 3, pg 1850, line 15). Could additional BrO fits in other regions (e.g. 327-347, 327-357, etc.) be used as in internal check on the fitting procedure and to assure nothing is being missed? The same goes for the SO2 retrieval; if an additional window is available (e.g. that described in Bobrowski et al., 2010) could that be used to determine if the data are impacted by ash, the distance to the plume, etc.? What tools are included to assure the procedure is working?

All of these thoughts are motivated by my concerns that the procedure outlined in the manuscript seem somewhat "tuned" to the Ruiz case and do not seem sufficiently generalized for application to other sites. For example, the threshold values chosen (e.g. Section 4, pg 1853, line 6) seem somewhat arbitrary and dictated by the present data

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set. The present work would be much stronger if it outlined a more robust, generalized approach to processing long DOAS timeseries. Such an approach should include quality assurance steps and methods to try to identify data compromised by technical or environmental conditions. Along the same lines, I am surprised that a toolkit of sorts or some kind of collection of scripts is not included as an online supplement to the work. Such a toolkit would be of considerable value and would provide a basis for further code development, especially since the authors plan to implement an automated retrieval routine at observatories that host NOVAC instruments (Section 5, pg 1855, lines 14-15). More fully developing the procedure to assure measurement quality and providing a toolkit would substantially increase the value of the present work.

Minor comments

Abstract, pg 1846, line 2: I disagree; the ratio of BrO to SO2 is not like other commonly-measured halogen sulfur ratios (e.g. HCl/SO2) since BrO is not a primary product emitted from volcanoes. I realize that the operational use of BrO/SO2 for monitoring is somewhat out the scope of the present work, but I would advise extreme caution in interpreting this parameter in terms of volcanic activity. Of course, this is the question we all hope to address. . .

A flow chart illustrating the data processing procedure would be helpful.

Section 2: please specify the wavelength range of the instruments.

Section 3, lines 29-32: please specify how much shift and squeeze are allowed.

Pg 1847, line 20: I would suggest replacing “reach” with “approach.” Other geophysical methods operate at second to sub-second sampling frequencies; only SO2 camera can truly be said “reach” these levels.

Pg 1851, line 11: How long does it take the system to make 4 consecutive scans? I.e. what is the time resolution?

Pg 1852, line 3: “is thought to not be influenced by these temperature issues” – is there

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a reference or other means to substantiate this?

Pg 1852, line 23: “ratioing” is misspelled and should probably be replaced by “Taking the ratio of two values. . .”

I would suggest integrating much of the “pitfalls” discussion in Section 5 into Section 2 or 3, especially the issue concerning BrO line shape (Section 5, pg 1856, lines 24-30). In particular, since this issue seems fundamental could it be addressed in the present work? Also, introducing new data or ideas not previously mentioned in the article should be avoided in the conclusions.

Figure 8: The daily average SO2 emission rate values appear to be very different than daily maximum values that have been available elsewhere (presumably from the same dataset, e.g. Fig. 4 in Herrick). Can you comment on the discrepancy? For example, the highest SO2 emission rate shown in Figure 8 is ∼90 kg/s or ∼8000 tonnes/day. Herrick Fig 4 shows maxima up to 33,000 tonnes per day (around 400 kg/s). Is there really that much daily variation to drag down the emission rates? Also, is there a way to assess the model wind speeds and how well they are performing? Can you provide the url from where were the data accessed? Perhaps it’s just a scaling issue, but it appears as though the flux data are cut off earlier than the column densities displayed in Fig 6.

REFERENCES

Bobrowski, N., Kern, C., Platt, U., Hörmann, C., and Wagner, T.: Novel SO2 spectral evaluation scheme using the 360–390 nm wavelength range, Atmos. Meas. Tech. Discuss., 3, 863-889, doi:10.5194/amtd-3-863-2010, 2010.

Herrick, J., Bulletin of the Global volcanism Network (August 2012), vol. 37, 8. Accessed on 27 Nov 2013 from http://www.volcano.si.edu/reports_bgvn.cfm.

Interactive comment on Solid Earth Discuss., 5, 1845, 2013.