Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tracking and quantifying volcanic SO$_2$ with IASI, the September 2007 eruption at Jebel at Tair" *by* L. Clarisse et al.

L. Clarisse et al.

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This is our response to the referees. Changes refer to the revised manuscript that we will submit.

* General Comments (both referees)

The authors would like to thank the referees for their positive feedback.

* Specific Comments (report of N. Krotkov only)

1) The introduction now covers the historical complementary of IR/UV as suggested.

2) Comment on: "Application of optimal Estimation method [Rogers 2000] to volcanic SO2 requires better justification, specifically 1) applicability of Gaussian a-priori covariance matrix to volcanic SO2; and 2) selecting diagonal values of the a-priori SO2 ma-

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Interactive Discussion

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trix based on IASI BT difference measurements (p. 16927) is not consistent with basic assumptions of the optimal estimation technique". And from the technical comments section: "Specify explicitly the retrieved state vector, X. Provide more details on the choice of a-priori parameters. What a-priori values were selected for SO2 molecule? The background SO2 a-priori is not appropriate for volcanic plume. Justify the choice of a-priori covariance matrix, especially for SO2. Provide explicit Sa matrix at least for one sample retrieval."

For clarity: background values for SO2 were used as a-priori; while the covariance matrix was chosen Gaussian, with diagonal values proportional to the BT difference. We have made this more clear in the text too.

The referee rightly points out that these choices are not appropriate in the general philosophy of the optimal estimation method. In fact using the Optimal Estimation along with a covariance matrix with very large diagonal and small off-diagonal elements, as assumed here, comes close to using a more standard least-squares minimization. An a-priori and covariance matrix could be constructed using the vertical profiles of many different SO2 plumes. Such information is not available and would furthermore likely remain inadequate viewing the dramatic and unpredictable character of such events. Still we have chosen for the more general optimal estimation fit in this study, as this is also the preferred method for the other interfering molecules and as it gives detailed information on the results (eg vertical sensitivity). We believe that an important point was precisely made in this paper in showing that the optimal estimation method provides useful information on the retrieved SO2 profiles, including the peak altitude, despite having good a-priori information on SO2. We have added a few clarifying lines in the text. We have also explicitly added that the retrieved state vector consists of the surface temperature and the partial or total columns of the different retrieved molecules. For the explicit form of the Sa matrix we have referred to Barret et al (2005), which specifies the Gaussian covariance matrix in formula form.

3) We have changed the abbreviation for "Degrees of Freedom for Signal" to the com-

monly used DFS as suggested.

4) We express our gratitude to the referee for having recalculated the SO2 masses from OMI; which fit now much better to the values we found. We have replaced the first paragraph on p.16930 with the suggestion of the referee.

* Technical Comments (both referees)

The technical corrections, which were mostly corrections of typos, spelling and grammar mistakes of both referee have been followed as suggested. The only exception is the comment on the unbalanced parenthesis in equation (4). We have double checked this formula and it does correspond with Rogers(2000), Page 85 formula (5.9). The final three comments of the report of N. Krotkov have been discussed above as part of the specific comments.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16917, 2008.