

***Interactive comment on “Sulfur dioxide
(SO₂) from MIPAS in the upper
troposphere and lower stratosphere 2002–2012”
by M. Höpfner et al.***

Anonymous Referee #2

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This paper presents new SO₂ data from MIPAS. It is a very good data set (in particular the limit of detection is impressive) and it could be very useful e.g. for comparing with nadir data. The paper is well written and structured, the methodology is scientifically sound and the authors are also honest as for the limitations of their dataset. Therefore, I think this study should be published in ACP after some (mostly minor) changes.

Comments

- P2, l 76: Please add a reference to the early study of Yang et al Yang, K., X. Liu, P. K. Bhartia, N. A. Krotkov, S. A. Carn, E. J. Hughes, A. J. Krueger, R. J. D. Spurr, and S. G.

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Trahan (2010), Direct retrieval of sulfur dioxide amount and altitude from spaceborne hyperspectral UV measurements: Theory and application, J. Geophys. Res., 115, D00L09, doi:10.1029/2010JD013982

- Table 2 should be extended for higher SO₂ columns (a maximum of 10 DU is not enough). How this error can influence the lifetime estimates?

- Section 2.4.1 a short paragraph introducing ACE-FTS would be useful, including references. Please define ACE-FTS and its satellite platform. Please provide general information on instrument characteristics (e.g., spectral resolution).

- Section 2.5.1: please rename 2.6 (there is no section 2.5.2) The significance of this section is limited compared to the other sections. It has the merit to exist but no conclusions can really be drawn notably for P2 because of the remaining bias (Fig 5).

- Page 5, l 419: Please recall what is the typical value for the half-width of the averaging kernel.

- Page 5, l 460-473: it would be good to assess the importance (in %) of the 3 effects. At least the second effect (saturation) could be evaluated (see previous comment on Table 2). Is the use of alternative spectral windows (less affected by saturation) feasible?

- Section 3.2 : My main point of criticism on this study is related to the lifetime estimates.

a) P6, l491: it is unclear what is done to appreciate the “linear behavior”. A fitting line could be added on Fig 13 for illustration purpose.

b) The authors retrieve 13.3d, 23.6d and 32.3d which are really high values and are only supported by limb measurements from MLS but not by nadir sensors, except the estimation by Karagulian et al. (2010), which is also questionable (read below). Therefore my question is whether it could be that the limb lifetime estimates are biased high due to a different sampling/coverage of the volcanic plumes than the nadir sensors.

c) Clarisse et al. (2012) presented new results for the Kasatochi case using IASI that

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are improved compared to Karagulian et al., 2010. Therefore the 18d estimation lifetime is probably outdated (read end of section 4.1 in Clarisse et al., 2012). Hence, lines 557-562 (P6) are probably not applicable anymore. Note also that Theys et al. inferred a lifetime of 7 days for the eruption of Puyehue, i.e. far from the estimate of 32d given in Table 3.

d) It would be interesting to redo the lifetime calculation for the integrated total SO₂ mass (not resolved for the 3 atmospheric layers). As it is now, the estimation of τ_i assumes that there is no transport/exchange of SO₂ between the different layers, which is not guaranteed.

e) Since the mass estimates are not independent of the lifetime which is assumed (when not fitted from the time series), it would be good to use a lower lifetime value (say 10 d, ie in better agreement with the nadir estimates) and check if it improves the agreement with the nadir data for the total mass.

- P 7, l595-599: I think this is a quite unlikely explanation. The typical time scale for the atmospheric circulation is much larger than the SO₂ lifetime.

Figures

- Fig. 10: it might be better to use a non-linear color scale to better represent the range of values

Typos

- P1, l 52 : SO₂ should be in parenthesis (as a definition)

- P 7, l650: 'depolarisation' -> 'depolarisation'

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 5801, 2015.