

Interactive comment on “Temporal trends of anthropogenic SO₂ emitted by non-ferrous metal smelters in Peru and Russia estimated from Satellite observations” by M. F. Khokhar et al.

Anonymous Referee #2

Received and published: 23 October 2008

This paper uses the seven year GOME data base to estimate trends in output of sulfur dioxide from smelters in Russia and Peru. The large GOME footprint and lack of daily coverage make this a daunting task although the instrument and algorithm sensitivity is adequate to retrieve small (1–2 DU) SO₂ amounts. Nevertheless, the long data record makes this an attractive source of information about sources and trends in air pollution.

As the title of this paper indicates, the main topic is trends of SO₂ emissions measured by the GOME instrument. As with any satellite instrument, GOME’s performance must have changed during the seven years, resulting in bias in the retrieved

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SO₂ values, therefore potentially affecting the trend described in this paper. However, there is no discussion or reference in this paper about GOME's performance and calibration, and no mention that the effect of instrument decay is properly accounted for when deriving trends. This issue must be properly addressed by the authors to give readers some confidence in the trend results.

P 17397 The SO₂ profile in Figure 1 is attributed to Taubman et al. 2006 but does not resemble anything in that paper. Where did the profile come from? In fact, this profile looks remarkably similar to the Chinese profile published in Krotkov, et al., 2008.

Krotkov, N. A., B. McClure, R.R. Dickerson, S.A. Carn, C. Li, P.K. Bhartia, K. Yang, A.J. Krueger, Z. Li, P. Levelt, H. Chen, P. Wang, and D.R. Lu (2008), Validation of SO₂ retrievals from the Ozone Monitoring Instrument (OMI) over NE China, *J. Geophys. Res.*, doi:10.1029/2007JD008818.

P 17402. The paper hardly acknowledges the analysis of Peruvian smelter emissions from OMI published by Carn et al., 2007, although the discussion of the Peruvian smelters is remarkably similar in the two papers, with at least one sentence being virtually identical (possibly both grabbed statements from company literature). Nevertheless, it's important to avoid such repetition for obvious reasons.

That said, this paper could be an opportunity to show how the long GOME record ties into the OMI record with its daily sampling, using SCIAMACHY data to make the transition. For example, does the OMI record confirm the trends found in the GOME record?

P 17405 - 6. The lengthy discussion about plates and South American volcanism adds nothing to this paper.

P 17401. The time series in Fig 3 of SO₂ over Norilsk shows a remarkable variability that is attributed to variations in cloud cover. If clouds are above the plume, masking part of the SO₂, this would mean that only the higher data points are valid measure-

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ments of Norilsk output. In this case the trend line could represent trends in cloud cover rather than changes in production. If the SO₂ plume is above a low cloud layer, then it could be accounted for in the AMF using the reflectivity measured by GOME, although its not clear from the description that the individual measurements are corrected for reflectivity. The large footprint makes this more difficult because the reflectivity under the Norilsk plume could be different from the average in the scene. These measurements are too poorly constrained to draw any conclusions about trends, or possibly even about averages.

P 17409 In the last paragraph the more capable newer instruments (OMI, SCIAMACHY, GOME-2) are mentioned even though they have been flying for years. This would fit better in the introduction where the importance of the long GOME record could be pointed out. It would be appropriate to discuss how GOME SO₂ results compare with the newer data.

Conclusion. This paper should not be published in its current form. It is disturbing to see the inadequate reference to and use of closely related literature, the inaccurate attributions, and lack of objective error analysis for interpretation of trends. Unless a major revision can correct the deficiencies noted above, explain the application of AMF's more completely, eliminate irrelevant sections, and make it more succinct, I would favor rejection.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17393, 2008.

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