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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Temporal trends of anthropogenic SO_2 emitted by non-ferrous metal smelters in Peru and Russia estimated from Satellite observations" *by* M. F. Khokhar et al.

M. F. Khokhar et al.

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Referee 2,

Thanks for raising some important questions about our study. Below you find detailed answers/justifications in accordance to your questions/comments.

1) GOME’s performance and its degradation with passage of time

Reply:

It is true that any satellite instrument’s (GOME’s) performance has probably changed during the seven years period. However, in our case it might have a negligible impact on the retrieved SO2 by DOAS method because of following reasons:

a) DOAS is not depending on the absolute radiances. Thus radiometric degradation does hardly affect the DOAS results. There might be remaining effects of potential degradation of the polarisation sensitivity, but from our analysis we found no indication for such effects.

b) The normalisation procedure (reference sector method – see Khokhar et al., 2005) opted will remove any remaining additive Offsets

c) Also, in our data sets we don’t see indications for artificial trends. In particular we derive positive and negative trends for the individual power plants indicating that any possible remaining artificial trend will be smaller than the observed trends.

2) SO2 profile in Figure 1

Reply:

SO2 profile used in Figure 1 has been correctly attributed to Krotkov et al., 2008 and final draft will be updated accordingly.

3) Comparison between OMI data and this study regarding Peruvian Cu smelters

Reply

A new discussion regarding comparison between OMI and GOME data from Peruvian copper smelters, we added following text as:

Estimated SO2 emissions from la Oroya and Ilo smelters during time period of 1996 – 2002 are 0.95ś0.07 Tg/yr and 1.09ś0.2 Tg/yr, respectively. The extrapolation of the temporal decrease of 25% in the SO2 VCD (over 6 years, see section 4.4) from Ilo smelter will yield 0.7ś0.2 Tg/yr SO2 emissions in 2005. In contrast, Carn et al., [2007] reported 0.3 (+0.2, -0.1) Tg/yr of SO2 emissions from Ilo smelter for September 2004 – June 2005, which describes an overall temporal decrease of 72% in combination with GOME observations since 1996. This dramatic decrease might be related to an upgrading of Ilo smelter (by using advance technology as part of

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the Environmental Compliance and Management Program agreed with the Peruvian government) started in 2004 [SPCC, 2004]. According to SPCC, [2007], the modernization of Ilo smelter completed in January 2007 and resulted in 95% capture of SO2 emissions on average during 2007. It should be also noted that part of the apparent difference between our data and the results of Carn et al., [2007] is probably be caused by different assumptions on the atmospheric radiative transfer.

However, in case of La Oroya smelter, our calculated trend projects an increase of 20%, while OMI data for La Oroya smelter (0.07ś0.03 Tg/yr for September 2004 – June 2005) reported by Carn et al., [2007] combined with GOME data (0.953ś0.07 Tg/yr in 2002) indicates an overall temporal decrease since 2002. Reasons for this strong difference are not clear yet.

According to [Boon et al., 2001] about 10 percent of smelters lacking SO2 emission controls, are mainly from South America, Asia and Africa. Additionally, smelters are located in a less studied region with rare available data and literature. However, this difference might be a result of reduction in smelting capacity [Carn et al., 2007] and/or an undocumented modernization (implementation of sulphuric acid plant?) of La Oroya smelter during 2003 – 2005 period.

In general, we conclude that our assumptions made in section 3 are reasonable and especially well suited to investigate trends over the time of GOME observations. However, care has to be taken if results from different satellite instruments using different retrieval settings are compared. Reliable trends from combined time series can probably only be analysed if spectra from the different instruments are analysed with the same retrieval settings.

And about Thomas et al., [2005], we added following text as ;

From a sensitive study by applying radiative transfer modeling (TRACY-II) for different wavelengths in the chosen wavelength range (see Fig. 1 under link http://www.flickr.com/photos/34386593@N03/3198308213/) it was found that that the

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errors caused by the exact selection of the wavelength are in the order of 10%. Which is comparable to 15% range stated by Thomas et al., [2005] due to spectral variation of AMF between 315 and 327 nm for 5% surface albedo and SZA$<60^{\circ}$. For further details please see response to short comment from W. Thomas.

4) Discussion about South American volcanoes

Reply

Section 4.6: Regional Volcanic eruptions, has been shortened and updated in the final draft. Also, Table 3 containing information about South American volcanoes and their activities during the time period of 1996-2002 has been excluded as well.

5) Norilsk Data and cloud impact

Reply

We have made some further investigation and looked for statistical significance of calculated trends in winter and summer time series of SO2 VCDs over Norilsk region. We used statistical tools (analyse-it and a built in IDL function) and performed regression analyses of both data sets over Norilsk region. Our analysis showed statistically insignificant linear trends; therefore, we replaced the linear fit by fitting a polynomial of 4° (fits better to the temporal variation of data). Although, it is hard to interpret any trend from Norilsk data, however, polynomials (4°) fitted to time series showed almost consistent behaviour for both winter and summer periods. It illustrates multiple behaviour of increase during 1998-2000 and 2002 followed by a decrease in 1997 and 2001 respectively. For further details see Author’s comments to short comment on this paper. Finally the paper draft will be updated according to new analysis and the Figure 3 will be replaced accordingly (For further details please see response to short comments by W. Thomas and also see Fig. 1 under link http://www.flickr.com/photos/34386593@N03/3220435326/).

Additionally;

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a) It is true that the cloud effect is a problem. Especially, in winter time, it is not easily possibly to identify clouds over bright surfaces. But over bright surfaces, the satellite has indeed some sensitivity for SO2 below the clouds.

b) In summertime, clouds could be in principle identified, but in this study no individual cloud correction was performed.

c) In general we assume that the cloud cover has not changed systematically during the years. Thus any remaining trend will come from changes in the SO2.

d) We get confidence in our results from the fact that the results for winter and summer are consistent, although the conditions (SZA, albedo) are rather different.

e) Finally, we don’t retrieve any statistically significant trend for Norilsk. We, however, still think that the presented data might be of some use for comparison with other studies.

6) New instruments (OMI, SCIAMACHY and GOME-2)

Reply

The paragraph about newer instrument (SCIAMACHY, OMI and GOME-2) has been moved from conclusion section to introduction section. Final version will be updated accordingly.

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