

Author comment on “Long-term changes of tropospheric NO₂ over megacities derived from multiple satellite instruments” by A. Hilboll et al., doi:10.5194/acpd-12-31767-2012

Response to anonymous referee #2 (C12271)

Andreas Hilboll et al.

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We thank anonymous referee #2 for her/his valuable comments. Detailed answers to the specific questions are given below:

Page 31771, line 13-15: Since the paper of van der A et al. is several times referred to, it might be useful to add at this time that they degraded the SCIAMACHY measurements to GOME resolution before gridding the data to 1x1 resolution.

We added this information to the revised manuscript.

Page 31776: In the discussion about the discrepancies found in the comparison between NO₂ columns and NO_x emissions, it should be mentioned that meteorological variability plays an important role in the variability of concentrations while emissions will remain constant.

In the revised manuscript, we have addressed this issue citing Voulgarakis et al. (2010) and Hayn et al. (2009).

Page 31779, equation 2: This relation only holds if the instrumental bias between the instruments GOME and SCIAMACHY can be neglected. Otherwise this relation would become a full linear dependency instead of a scaling. I expect this bias to be rather small, but it might be a cause of the differences discussed in section 4.3.

The resolution correction factor Γ is a dimensionless number, derived from the division of two SCIAMACHY measurements. Therefore, there are only GOME measurements on both sides of Eq. 2. Any instrumental bias between GOME and SCIAMACHY would cancel out in the calculation of Γ (see Eq. 1) at least to first order and assuming no large bias between the two data sets, as indicated by validation.

Page 31779, line 12: Values larger than 1 indicate not only that NO₂ is higher than the surrounding area, but more specifically, higher than the area in longitudinal direction for moderate latitudes as a result of the East-West direction of pixels. Maybe this explains the rather peculiar North-South patterns in Figure 7a ?

We agree that only the across-track variability is captured by Γ . This surely explains the mentioned North-South patterns in Fig. 7a. However, it is important to note that the captured variability is not purely longitudinal, because the instrument swath is not oriented in a pure East-West fashion, due to the slantedness of the satellite's orbit.

Page 31779, line 19-21: Although we know the shipping line should be there in the Pacific, I think this line of enhanced values is rather vague, especially in light of the noisy character of the ocean as earlier discussed in the text.

We agree with the referee that the line of shipping emissions in Fig. 7a is somewhat noisy. However, our investigation of the global pattern of Γ has shown that the noisy character of Γ over the ocean is most often a lot more random than here in the Atlantic Ocean (and also in the Black Sea). Therefore, we believe to actually see a signal of shipping-related NO₂.

We have rephrased this passage in the revised manuscript, pointing out the noise of the signal.

Page 31785, line 7: In Figure 12 I do not understand the unit of delta. It seems to me that this should be equal to the unit of Y, thus without yr⁻¹. However, I would like to ask the authors to show delta as a relative quantity (e.g. with ref. year 1996). This would make the comparison with gamma more interesting, since given as a percentage they should be quite similar. In Figure 13 the same unit for delta is shown and should be corrected as well.

Indeed, the unit of δ must be *molec cm⁻²*, without the *yr⁻¹*. We agree that it is more instructive to view δ as a relative quantity. In the revised manuscript, we have modified Figs. 12 and 13 accordingly.

Page 31786: line 7: Here Figure 16 follows on Figure 13 in the text. Confusing.

We corrected the numbering in the revised manuscript.

Page 31787: line 21-23 : Note that especially new sources like powerplants can suddenly appear in the time series in these provinces with relatively low emissions resulting in non-linear and large relative growth rates.

We agree with the referee and have added an according sentence to the revised manuscript.

Page 31790: line 20-21: The authors mention that the error estimates are "challenging". Does this mean that the authors did not calculate these errors. Please explain.

Investigating the uncertainty of the trend estimator $\hat{\omega}$ is challenging because the trend model allows for multiple data values per timestep (i.e. one per instrument). We have employed the bootstrap technique to calculate the standard error of the trend estimator $\hat{\omega}$ and to assess the trend significance via 95% BCa confidence intervals. All this has been included in the revised manuscript.

Figure 14: For direct comparison with Figure 15 it might be useful to show a similar lat-lon range in the top figure.

In the revised manuscript, we have followed the referee's suggestion and added a third subplot to Fig. 15, showing the results from Fig. 14 without significance masking, and in the same clipping as the other two subplots of Fig. 15.

References

- Hayn, M., Beirle, S., Hamprecht, F. A., Platt, U., Menze, B. H., and Wagner, T.: Analysing spatio-temporal patterns of the global NO₂-distribution retrieved from GOME satellite observations using a generalized additive model, *Atmospheric Chemistry and Physics*, 9, 6459–6477, 2009.
- Voulgarakis, A., Savage, N. H., Wild, O., Braesicke, P., Young, P. J., Carver, G. D., and Pyle, J. A.: Inter-annual variability of tropospheric composition: the influence of changes in emissions, meteorology and clouds, *Atmospheric Chemistry and Physics*, 10, 2491–2506, 2010.