

Interactive comment on “Long-term changes of tropospheric NO₂ over megacities derived from multiple satellite instruments” by A. Hilboll et al.

Anonymous Referee #2

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The authors present two methods which are useful for calculating linear trends in tropospheric NO₂ from multiple instruments. Based on these methods trends are derived for important megacities around the world. The results are compared to earlier work showing that uncertainties in trends were significantly decreased with the newly introduced methods. In general it is a well written paper. A few clarifications might still improve the paper, which I will discuss 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.

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

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variability plays an important role in the variability of concentrations while emissions will remain constant.

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.

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 ?

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.

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.

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

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.

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.

Figure 14: For direct comparison with Figure 15 it might be useful to show a similar

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lat-lon range in the top figure.

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