

## ***Interactive comment on “Long-term changes of tropospheric NO<sub>2</sub> over megacities derived from multiple satellite instruments” by A. Hilboll et al.***

**Anonymous Referee #1**

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The paper "Long-term changes of tropospheric NO<sub>2</sub> over megacities derived from multiple satellite instruments" by Hilboll et al. presents different methods to derive trends for tropospheric NO<sub>2</sub> from different satellite instruments consistently, with a particular focus on Megacities. Though the paper is rather technical, the resulting trends are of high impact for atmospheric chemistry, justifying publication in ACP. Before being acceptable for ACP, the following revisions are necessary:

General comments

1. The authors should take care about statements and wording:
  - The abstract promises "several ways", but at the end I found only two.
  - "High" spatial resolution is often mentioned, but not defined.

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- The "physical principles", which sound grandiose, turned out to be just the effect of ground pixel size.
- I don't see how far the fitted trend function is a "statistic" model.
- Why "levelshift"? Why not simply "offset"?

2. After reading the manuscript, it gets clear that the only instrumental difference that was "explicitly accounted for" is the spatial resolution of GOME1. This should be made clear in the abstract, and the ground pixel sizes should be mentioned already in the introduction, as they are crucial for this study.

3. The authors claim that their method leads to lower uncertainties of the derived trends. I do not agree with this conclusion, as the authors just compare their uncertainties (for trends 1996-2011) to those of van der A for 1996-2006. The lower uncertainties may thus just be caused by the longer time period! This has to be revised appropriately (the trends+uncertainties have to be derived for 1996-2006 as well).

Detailed comments:

31768 5: and other instrumental offsets (GOME2A probably differs from GOME2B).

31768 5-7: "All these factors" have to "be taken into account", but in this study, only the spatial resolution is investigated!

31768 9: "Several"? "explicitly account for the instrumental differences"? Please be more specific and honest to what has actually been done.

31768 11: Which instruments are "their", and what is "high"?

31768 12: I understand it now, but it's very difficult to understand this before reading the paper. Please explain in an extra sentence.

31768 17: "All"?

31768 28: "+5-10" I suggest to write "+5 to 10".

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31769 3: show

31770: The key is the spatial resolution, so add it here.

31770 16: New paragraph for DOAS.

31771: Please add Hayn et al.: <http://www.atmos-chem-phys.net/9/6459/2009/acp-9-6459-2009.html>

31773 7-9: The OMI instrument deserves an own paragraph as well. Local time and reference(s) have to be added. OMI ground pixel size is 13x24 km<sup>2</sup> in nadir, but, in contrast to GOME&SCIAMACHY, it is larger for higher viewing angles, and can be even larger than SCIAMACHY at the edges. This should also be indicated in table 1.

31774: The description of the stratospheric correction should be shortened as it is already given in literature.

31776 3-5: I don't get this point - if there is some systematic effect, it should affect all the years from 2003 on, not only 2003!?

31777 20 - 31778 5: A similar approach was introduced in Beirle et al., 2004, which should be referenced here.

31778 10: 5 SCIAMACHY pixels are still considerably smaller than 1 GOME pixel, which is mentioned later in the manuscript, but should be discussed here as well.

31782: Concerning the problems over desert, I would suspect that the clouds may cause some problems. The determination of cloud fractions at the coastline of deserts is very challenging, as the ground pixels cover both bright sand and dark ocean. This can easily cause systematic differences between GOME and SCIAMACHY. The authors should compare the respective cloud statistics (pdf) for GOME, GOME corrected, and SCIAMACHY.

31783 13-14: Why not? I would think that the same method (adding spectra of several ground pixels to fill a GOME pixel) could be applied to OMI and GOME-2. For GOME-2,

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the situation might be even more comfortable, as its backscan could be used directly.

31784 19: "if"

31785 3: "being the..."

31785: In line 13, the authors state that the levelshift also accounts for differences beyond those caused by spatial resolution. In line 18, this statement seems to be forgotten.

31787: The discussion should include other trend studies for SCIAMACHY (Schneider & van der A) and OMI (Russell et al.).

31788 12: See Russell et al.

31791 3: How far is the levelshift really different from the multi-instrument fit? Couldn't it just be regarded as a multi-instrument fit with n=2?

31791 13: "... afternoon, the diurnal cycles of NO<sub>2</sub> and aerosols, as well as the spectral surface reflectance, can lead to ..."

31793: Is this method just proposed or also applied to the time series? Please add the results to tables 4.

31794 10: "instrumental differences": only one, i.e. ground pixel size.

31794 18: "spatial dimension": What should this mean?

31795 6: Has to be demonstrated.

Figure 1: It is helpful to have such a schematic figure, but it should be scaled correctly (320 vs. 60). Please add pixel widths of SCIAMACHY and GOME. I would skip "pixel value" in the legend.

Figure 2 is discussed in section 4.3, so it should be moved after Fig. 4. Fig. 1 is sufficient for the introduction.

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Fig. 6: The authors state that Gamma is noisy over ocean, but it is not pure noise, there are clear systematic patterns. How can they be caused?

Fig. 12: The comparison is not that meaningful, as Gamma is a relative quantity, while levelshift is an absolute column density. Thus, for comparing the patterns, either multiply Gamma with the mean NO<sub>2</sub> column, or divide the levelshift by it.

Fig. 15: Please add the data from Fig. 14 for the same clipping and also without significance masking.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31767, 2012.