

Interactive comment on “Long term NO₂ measurements in Hong Kong using LED based Long Path Differential Optical Absorption Spectroscopy” by K. L. Chan et al.

Anonymous Referee #1

Received and published: 21 December 2011

The manuscript "Long Term NO₂ Measurements in Hong Kong using LED Based Long Path Differential Optical Absorption Spectroscopy" by Chan et al. presents a 2-years time series of NO₂ measurements by a LED DOAS system in Hong Kong and comparisons to in-situ and satellite measurements.

The study is generally suitable for publication in AMT, but needs revisions as listed below. Especially the points 4 and 5 could demand major revisions.

Comments:

1. The title ("long term") is exaggerated; the measurements have been performed for C2403

just 2 years so far.

2. 6619/1-10: Give more details for the measurement procedure. What is a "scan", how long does it take? What does "maximum of 10 scans" mean? How variable in time is the LED spectrum, and what does it depend on?

3. 6620/20: Please specify the "strong seasonal variability".

4. 6622/1ff and Figure 5: In a linear fit, the x values are considered to have no errors. But this is not appropriate here. Note that this is more than just an academic sophistry: it has immediate consequences for the derived slopes. If the authors would choose EPD on the x-axis, slope and intercept would be completely different (and not just the inverse of the current linear fit). For a discussion of this topic and how to deal with it, see for instance Cantrell, ACP 8, 7153ff, 2008. The authors have to use such an orthogonal regression for the comparisons of datasets.

5. Sections 3.2.2 and 3.2.4 As far as I understand, the a-priori profiles used for the calculation of AMFs (3.2.2) are different from the profiles used for the calculation of ground level concentrations. This is an unnecessary wrongness. The authors should apply the profiles which they argue are most realistic for Hong Kong for both, AMFs and concentrations, either by using the averaging kernels, or by a direct calculation of new AMFs using their profiles.

6. Section 3.3 Please set the discussion of weekly cycles of NO₂ from space in relation to literature (Beirle et al., Atmos. Chem. Phys., 3, 2225-2232, 2003).

7. 6628/12-13: I do not agree with this absolute statement:

- natural NO_x emissions do have a diurnal cycle (lightning, biomass burning, soil emissions)

- anthropogenic non-transportation NO_x emissions might have a diurnal cycle (parts of industry will be reduced over night)

- anthropogenic non-transportation NO_x emissions might have a weekly cycle (parts of industry will be reduced over weekend)

8. Figs. 8&9: Please add error bars for the LP measurements as well.

9. For the discussion of weekly and diurnal cycles, it would be very interesting to include the in-situ (EPD) measurements in Figs. 9 and 10, which might also have impacts on the discussion and conclusions.

Minor/technical issues:

a) 6616/18 Please add the overpass time of OMI.

b) Skip 6616/23, it's off-topic here.

c) 6617/20: add a link to the website.

d) 6619/10: add a weblink to DOASIS.

e) 6623/11: Add the reference to Levelt et al. here.

f) 6624/2: Add a reference to the used data product.

g) 6626/6: The OMI ground pixels are ~13km along track and >24km across track (nadir) and considerably larger towards the swath edges, so the average OMI pixel size is larger than 15km.

h) 6626/7: correct "50vkm"

i) 6626/15: insert "monthly means of" after "between the"

j) 6628/25: you might add that this conclusion immediately illustrates the strong need for a geostationary satellite.

k) Figs. 5/6: please use a consistent terminology: mixing ratio in Fig. 6, but concentration in Fig. 5. If a number is given in ppb, it should be a mixing ratio.

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Interactive comment on Atmos. Meas. Tech. Discuss., 4, 6615, 2011.

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