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Atmospheric Measurement Techniques Discussions

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.

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Responses to comments of Referee #1

Many thanks to the reviewer for the helpful comments. Our responses to each comment are presented below.

1) Comment(s): The title ("long term") is exaggerated; the measurements have been performed for just 2 years so far.

Response: The expression "long term" in the title is used as there are only few con-

tinuous LP-DOAS measurements of this duration. Typically, similar measurements are carried out in the form of intensive short term measurement campaigns. However, we followed your advice and removed the words "long term" from the title. 2) Comment(s): 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?

Response: A more detailed description of the measurement procedure has been added on page 3, lines 72-75 of the revised manuscript.

3) Comment(s): 6620/20: Please specify the "strong seasonal variability".

Response: A sentence describing the seasonal variability of NO2 has been added on page 5, lines 118-119 of the revised manuscript.

4) Comment(s): 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.

Response: A total error weighted least square regression is now used in figure 5. However, it had almost no influence on the results, since the error of the LP-DOAS measurements are small compared to the EPD data. The error of the hourly averaged LP-DOAS measurements is about 0.05ppb which is about 10 times smaller than the EPD data. When the errors of one data set are much smaller than the others, the result of a total least square regression is close to the result of an ordinary least square regression.

5) Comment(s): 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 fo Hong Kong for both, AMFs and concentrations, either by using the averaging kernels, or by a direct calculation of new AMFs using their profiles.

Response: The OMI a-priori profile is now used for the VCDs to ground concentrations conversion instead of the GEOS-Chem annual mean profile from July 2009 to June 2010. The focus this paper is about validating the OMI NO2 data product, so we don't want to change it by calculating our own AMFs. We did some AMFs calculation for some typical cases in both summer and winter. The results show that a more up to date profile for the OMI retrieval would result in higher VCDs by a factor of 1.15. Comparison and discussion of the OMI a-priori profile and the GEOS-Chem profiles are added on pages 14-15, lines 263-275 of the revised manuscript.

6) Comment(s): 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).

Response: A reference for the discussion of weekly cycles of NO2 is added on page 17, line 308 of the revised manuscript.

7) Comment(s): 6628/12-13: I do not agree with this absolute statement: - natural NOx emissions do have a diurnal cycle (lightning, biomass burning, soil emissions) - anthropogenic non-transportation NOx emissions might have a diurnal cycle (parts of industry will be reduced over night)

Response: Page 18, lines 325-327 of the revised manuscript have been revised, now stating that power plant and industrial NO2 emissions from the Pearl River Delta should show a distinct dependency on wind direction which could not be observed with our LP-DOAS.

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

Response: Error bars for the LP-DOAS measurement are not plotted in figure 8 and 9 as they are very small (<1% for individual measurements and typically less than 0.05ppb for hourly averaged data) and thus would not be visible in the plots. However, the variability (1σ standard deviation) of the LP-DOAS measurements is now added to figure 8 according to Referee #2's comments.

9) Comment(s): 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.

Response: The EPD data are now shown in figure 9 and 10. Weekly cycle of NO2 obtained by the EPD monitoring stations in general agree with the LP-DOAS measurements, showing a reduction on Sunday. However, the reduction of the EPD data is slightly less pronounced than the LP-DOAS measurements. Daily cycles of NO2 measured by LP-DOAS and both EPD stations show similar characteristics with peaks in the morning and evening rush hours. Discussion of the EPD measured daily and weekly cycles are added on page 17, lines 312-315 and lines 320-322, respectively.

a) Comment(s): 6616/18 Please add the overpass time of OMI.

Response: The OMI overpass time is added in the abstract of the revised manuscript on page 1, line 15.

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

Response: Off topic statements have been removed from the introduction of the discussion paper on page 6616, line 23.

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

Response: A link to the website hosting the NO2 measurement data is added to the introduction of the revised manuscript on page 2, line 38.

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

Response: A web link of the software DOASIS is added on page 4, lines 82-83 of the revised manuscript.

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

Response: A reference for the OMI instrument (Levelt et al., 2006) is added on page 10, line 185 of the revised manuscript.

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

Response: A reference of the OMI data product is added on page 11, line 200 of the revised manuscript.

g) Comment(s): 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.

Response: The gridding technique we used assigns larger weights to smaller pixels, so the average pixel size would be close to the nadir pixels. The average OMI pixel size is about 800km2 which would correspond to a circle with a radius of slightly more than 15km (15.96km).

h) Comment(s): 6626/7: correct "50vkm"

Response: Corrected.

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

Response: Done.

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

Response: A statement emphasizing the need for a geostationary satellite observation of trace gases with strong daily and weekly cycle was added on page 18, lines 335-337 of the revised manuscript.

k) Comment(s): 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.

Response: Consistent terminology “mixing ratio” is now used throughout the manuscript.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/4/C2804/2012/amtd-4-C2804-2012-supplement.pdf

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