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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Novel application of satellite and in-situ measurements to map surface-level NO_2 in the Great Lakes region" *by* C. J. Lee et al.

Anonymous Referee #1

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The manuscript by Lee et al. describes an analysis of measurement results from satellite and in situ-measurements in the Great Lakes region. This comparison study of spatial and temporal variations of NO2 concentrations provides some interesting insights about the measurement techniques and could be published in the ACP special issue BAQS-Met after some major revisions as discussed below.

General comments:

The presented study provides a detailed analysis of the data sets involved and therefore could contribute to the BAQS-Met field study by providing an overview of the spatial and temporal variation of NO2 concentrations. However, I'm missing the novel appli-

cation which is promised in the title. The abstract doesn't mention a novel approach. In the introduction it is mentioned that "The objectives of the work in this paper are thus to develop an approach to use OMI tropospheric column data to estimate spatially resolved surface NO2 concentrations at a regional-to-local scale" and to do this conversion "using publicly available data from a network of permanent surface monitoring stations in place of the CTM". That would be a novel approach, but it is not clear to me how this works. Eq. 1 uses ratios calculated from GEOS-Chem output, and then you write that you "obtain surface concentrations without the use of a CTM" by determining "the average ratio of column to surface concentration (VCD to Surface) for each OMI overpass over the region using the high-time resolution surface monitors." Averaging over the whole region to determine the conversion factor would work if the profile shape is constant over the region, but as you mention later in the manuscript the surface concentrations vary significantly, so most likely profile shapes vary too. Furthermore, I don't see the value in comparing the OMI surface concentrations to ground measurements results (Sec. 3.1.1) if you use other ground measurements for the conversion, you might as well compare the different ground measurements directly. If this conversion is the focus of the paper, it should be described and analyzed in more detail, e.g. by comparing to other conversion methods, like using a CTM, and evaluating advantages and disadvantages. If this conversion is more a tool to filter out certain pattern in the data set and the focus of the paper is more the spatial and temporal analysis of different measurement data sets, then the title and the objectives should be changed accordingly.

Several sections are incoherent in style and information content with a confusing repetition of descriptions and details, e.g. for special time periods or different measurements of the campaign. In some cases, it is hard to track whether certain details are actually relevant and used in the study.

The introduction of this extensive and elaborate campaign is understandably lengthy but would benefit to great extent from either postponing details to subsequent sec-

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tions or introducing overview tables (and maps) for the entire campaign and periods of special interest. This holds also for other parts of the paper.

Detailed comments:

It's not clear to me why the "use of in situ data to determine the surface-to-column NO2 relationship makes this analysis insensitive to potential bias in the OMI NO2 data." You take out the bias by dividing the VCDs by the average VCDs (Eq. 1), but that has nothing to do with using in situ data. Please explain.

Page 17250, line 16: "This approach has the advantage of simplicity since it is not dependent upon CTM runs, which are relatively costly and not available to all OMI data users. -> how about "time consuming" instead of "costly"? CTM runs are not expensive compared to buying and operating measurement instruments, assuming that you don't have to buy a computer cluster to run them. I believe the argument in the following sentence, the elimination of a potential model bias is the more important advantage and could be emphasized more (e.g. take out the word "potential" in this sentence). Why is your method "less sensitive to the OMI data product" and why is that an advantage?

Page 17251, line 9: refer to figure 8.

Page 17252, line 11: the section title is High-time resolution measurement by chemiluminescence, but then you reduce it to 1h averages. Why is that?

Page 17252, line 14: Considering the details provided in the preceding paragraph, the location of the Environment Canada CL monitors should be mentioned.

Page 17253, line 19: "the first 6 periods..."-> Specify periods.

Page 17253, line 28: You mention "additional quality assurance checks", what are those?

Section 2.4: Aura is crossing the equator in south-north direction, not north-south. "OMI was designed to provide daily global coverage", instead of "..intended to

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provide. . .”

Page 17254, line 16 “removing as many as 18 pixels“ instead of “removing as many at 18 pixels”

Page 17254, line 19 “provided by NASA“ instead of “provided NASA“

Page 17254, line 21: You might want to refer to the web pages of the two data products instead of Lamsal et al. 2010.

In the following sentences: The first step of the retrieval algorithm is clearly a DOAS retrieval, but writing that the data product begins by using the DOAS algorithm sounds a little weird. The 365-500 nm you are referring to is the spectral range of the VIS channel, but for the NO2 DOAS retrieval the wavelength range from 405.0 nm to 465.0 nm is used (see Bucsela et al. 2006). Aerosol particles are not considered for the calculation of the AMFs. If this feature had been added (e.g. by calculating your own AMFs) please provide a reference. It sounds a little misleading to write that the AMF “corrects” for the viewing geometry and light-scattering influences. The AMF concept converts the slant column densities (SCDs) which are the direct result of the DOAS retrieval and depend on the viewing geometry and influenced by scattering, into vertical column densities (VCDs) which are (supposedly) independent of those influences. The SCDs are not incorrect, they just represent a quantity difficult to interpret and compare to other data sets. Therefore, I would rather call the AMF concept a conversion than a correction. For the weights of the gridding process you might want to consider adding the measurement error or a term accounting for the cloud influence (see for example Wenig et al 2008, “Validation of OMI tropospheric NO2 column densities using direct-Sun mode Brewer measurements at NASA Goddard Space Flight Center”) in order to further reduce the errors. This reference can also be used to justify using the inverse of the area of the pixel (in contrast to for example 1/areaˆ2 or something like that, which would give near-nadir pixels an even higher weight).

Page 17256, line 15: Please specify "below" for easier reference.

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Sec 3.1.1: Why did you calculate the correlation coefficients between the different stations? It would make reading this section easier, if key values for NO2 concentrations and correlation coefficients would be given in the text.

Page 17257, 1ine 19: “When averaged over a longer period of time, such as a week, NO2 concentrations varied much more spatially than temporally“ -> Isn’t that obvious? Averaging (or smoothing) reduces variability, so please explain why this is worth mentioning. Page 17260 , line17: What are the units of the numbers given in the brackets?

Figure 8: The term “false color” is typically used when showing an image in colors that differs from a color photograph, so I think you can use just “color” instead of “false color”.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17245, 2011.