

Interactive comment on “Novel application of satellite and in-situ measurements to map surface-level NO₂ in the Great Lakes region” by C. J. Lee et al.

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The paper introduces an interesting approach to link surface measurements and satellite observations of NO₂, which allows to derive maps of surface-level NO₂. It is well written and clearly structured. The methods (and their limitations) are explained comprehensibly. It should be published on ACP after minor revisions.

We thank the reviewer for the general comment. All specific comments have been considered carefully and responses and provided below.

Comments: 1. Page 17248: It is mentioned later, but it should be pointed out already
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here that there are two different OMI NO₂ products, with significant differences. Thus, it is not straightforward to compare the quantitative results from the various comparisons of OMI and in-situ data.

Thank you for pointing this out. The first full paragraph from page 17248 now ends: “Recent validation studies indicate that biases in the satellite retrievals remain and must be addressed when interpreting the data (Hains et al., 2010; Herron-Thorpe et al., 2010; O’Byrne et al., 2010; Lamsal et al., 2010). It is also important to note that a number of different data products with significant differences exist, adding to the complexity of comparisons between OMI and in situ data.”

2. Page 17254 Line 21: The reference to Lamsal is misleading here; for both SP and DOMINO, one separate reference should be given.

Agreed. This sentence now reads: “Two commonly used, publicly available data products generated from the raw data collected by OMI are: the Standard Product (SP) provided by NASA (http://disc.sci.gsfc.nasa.gov/Aura/data-holdings/OMI/omno2_v003.shtml) and the DOMINO Product provided by the Tropospheric Emissions Monitoring Internet Service (TEMIS) (Boersma et al., 2007, <http://temis.nl/airpollution/no2.html>).”

3. Page 17264 Line 1: This is rather surprising; is there at least a relationship of H to e.g. temperature for a relaxed significance criterion?

We also found this result surprising. However, no relationship was found even for large p-values. It is unclear what is driving this seasonality, however, it was seen in characteristic height values for Toronto, Canada, as well. In future, it would be worth examining the seasonality of the lifetime of NO_x and NO/NO₂ partitioning. Additionally, seasonal biases in the OMI retrieval cannot be ruled out. In particular, the DOMINO product which was used for this study relies on TM4 which has been found to have boundary layer mixing problems. This characteristic height should be strongly

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related to boundary layer mixing, however, why this effect would be strongest in fall is still not clear.

4. Page 17265 1st paragraph: Is the decrease also observed in the in-situ data? 5. Page 17267 Line 1: Please specify "sufficient".

We have added "(average monitor spacing < 100 km) " to the sentence.

6. Caption Fig. 2: Please add "OMI" after "average" and give the period of the campaign.

Done.

7. Fig. 4: Std might be added as error bar.

Added.

8. Fig. 8: I was bemused by the yellow lines and first interpreted them as a sharp gradient in NO₂; please choose a color which is not contained in the colorscale (like grey) and explain them in the legend (roads, borders, something else?)

Fixed. The borders are now marked in grey.

9. Fig. 11: As in Fig. 8, the respective in-situ measurements should be added as color coded circles.

Added.

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

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