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Atmospheric Measurement Techniques Discussions

Interactive comment on "Comparison of TROPOMI/Sentinel 5 Precursor NO_2 observations with ground-based measurements in Helsinki" *by* Iolanda Ialongo et al.

Anonymous Referee #1

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The manuscript by Iolango et al. compares satellite-borne TROPOMI NO2 measurements to ground-based PANDORA NO2 measurements in Helsinki. This paper is very well written (I don't have many minor comments) and contributes to the TROPOMI validation effort. The topic of the manuscript is important as currently not many validation papers have been published and it is important to validate the measurements taken by the new satellite instrument TROPOMI with ground-based observations. In terms of methods, there is not much new added in this paper and it is actually quite similar to Iolango et al. (2016) except that it uses TROPOMI measurements instead of OMI measurements. I have a few suggestions how the manuscript could be strengthen. My greatest concern is their method of how the high-resolution model CAMS was used to

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re-estimate the TROPOMI tropospheric columns.

I would advise some major revisions mostly concerning the re-calculated tropospheric VCDs. The manuscript can be published in Atmospheric Measurement Techniques after these issues have been addressed.

Major concerns

1) Iolango et al. claim that a-priori profiles have been replaced with high-resolution CAMS profiles (e.g. in the abstract p.1 l. 7; p.4 l. 25-27; p.14 l. 3-5). However, this is not true when reading the method section (p.7 l. 1-7); in fact, the tropospheric columns are simply scaled with the tropospheric CAMS columns (not profiles). Replacing the a priori profile shape with the profile shape of a high resolution model is a common technique to improve satellite tropospheric NO2 columns. However, to do this new AMF have to be estimated, e.g. Goldberg et al. (2019), McLinden et al. (2014); Russell et al. (2011), Palmer et al. (2001); Martin et al. (2002) and lots more. The a priori vertical column densities do not have a linear relation to the TROPOMI tropospheric columns. To replace the standard low resolution profile shape with that from a high resolution regional model, an new AMF has to be estimated; the relationship is not simple due to the radiative transfer in the atmosphere.

In the comparison, it can be seen that this is not a good method as the columns are simply scaled, leading to a worse product than the standard tropospheric columns. As the CAMS model is a high resolution model near a city or hot spot, these columns will be larger than for the lower resolution TM5-MP model, leading to R>1 (in eq. 3), and thus all TROPOMI columns are scaled up. Thus, it is intuitive that the scaled columns are better for high concentrations, but overall worse.

I would suggest to either use CAMS to estimate new AMFs (similar to the references provided above), or to cut this part out of the manuscript.

If CAMS is used to estimate the AMF, more description of the model is needed, from

the description on p.4 l.25-30 it is not clear what time stamp was used. Is an hourly output used? Are these interpolated to the time of the overpass?

I am also confused, why CAMS above 3km was used (3-5km). The largest impact on the tropospheric AMF comes from the high concentrations near the surface (in the boundary layer) around cities or other NOx sources. High-resolution models are used to improve the satellite tropospheric columns, because of the improved profile shape primarily in the boundary layer close to the emission sources, not to correct for the profile shape of the free-troposphere.

2) The Kumpula AQ in situ measurements are converted from surface concentrations to total columns, based on the correlation between the PANDORA and in situ measurements. One concern is that these two instruments are not co-located and are quite likely measuring two different airmasses. Especially, since the in situ measurements are taken near an airport, and thus have likely high concentrations near the surface that may or may not be captured by PANDORA, depending on the winds etc. Further, the good correlation is primarily driven by three measurements that measured high amounts of NO2 for the PANDORA and in situ measurements. I would suggest cutting this figure (Fig. 5), since it is not used for any qualitative comparison, a similar figure is provided in Fig. 2.

3) A little more can be done in this paper in terms of validation. Here are some suggestions:

- On p.4, l.1-3 Iolongo et al. claim that the differences should be small between the OFFL and NRTI version. I think this paper would provide a good opportunity to quantitatively identify the differences between the NO2 NRTI and OFFL version (e.g. similar as Garane et al., 2019 who quantified the differences between the OFFL and NRTI TROPOMI O3 columns to ground-based observations).

- There may be limited measurements available but perhaps looking at the differences between TROPOMI and PANDORA NO2 columns in terms of TROPOMI's SZA, cloud

fraction etc. similar as in Beak et al. (2017) Fig. 5 or Fig. 7

- Further, adding a boxplot showing the differences between the TROPOMI and PANDORA columns binned in low, medium, high columns (e.g. 0-0.6 , 0.6-1 , >1 10ˆ16 molec/cm2) would also improve the paper and provide more contents to the discussion. This is already discussed on p.10 l.1-5, but a figure would help.

- The paper would improve if the time period of the comparison could be increased maybe use 1 year of data (April 2018 to April 2019). Maybe one concern would be data in the winter time with snow cover, but the difference between summer and winter observations could also be investigated.

Minor comments

Figure 2: The lines are confusing and misleading, the columns are completely unknown when no measurements are taken. I would suggest replacing the line plot with a scatter plot, at the very least for the TROPOMI, and PANDORA 10min avg. measurements.

Figure 3: It's hard to tell the difference between weekdays and weekends. I would suggest replacing the "weekend marker" with a triangle marker (or something similar). It is also sufficient to reduce the size to a 1-column plot.

P. 2 l. 5: "Netherlands" -> "Netherlands Space Office"

p. 3 l. 10: According to the AMT author guidelines dates should be written as dd month year: "on the 13th October" -> "on 13 October"

p.3 l. 14: "UV-Visible (UVVIS)" -> "UV-VIS" (as defined on p.2 l. 24)

p.3 l. 20 DOAS already defined on p.2 l. 25

p.3 l. 29: "15.04-30.09.2018" -> "15 April to 30 September 2018"

p. 3 l. 32, p.4. l. 1 : NRT -> NRTI

p.4 l. 12 : 15.04.2018-30.09.2018 -> 15 April to 30 September 2019

p.4. l. 18 -21: maybe move Fig. S1 from the supplement into the main paper. It is discussed here in a few sentences and seems important.

p.6 l. 3: FMI not defined, please define. Also, are these ground-based measurements publically available? If, so please provide the link where it can be downloaded.

p. 10 l. 11: Figure S2 -> Fig. S2 (from AMT author guidelines)

p.10 l. 25-30: as suggested in the previous section, this can be cut together with Fig.5

p. 13 l. 22: “We find this partially. . .” -> this has not been concluded or found from the analysis in this paper; maybe change it to : “This is partly due to the profile shapes of the low resolution TM5-MP model used to compute the standard TROPOMI tropospheric NO2 columns and thus. . .”

p. 15 mention that this study is using summer observations only (unless the time period has been changed, see previous suggestions), with no snow cover (?)

p.15 l. 4: the comparison to the results from Griffin et al. could be a bit more quantitatively: were the results similar, how similar? Include some numbers.

References

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