

Interactive comment on “Validation of TROPOMI tropospheric NO₂ columns using dual-scan MAX-DOAS measurements in Uccle, Brussels” by Ermioni Dimitropoulou et al.

Anonymous Referee #1

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The manuscript with the title "Validation of TROPOMI tropospheric NO₂ columns using dual-scan MAX-DOAS measurements in Uccle, Brussels" (1) represents a substantial contribution to scientific progress in validating TROPOMI tropospheric NO₂ column observations. (2) Scientific approaches and applied methods are valid and results are discussed in an appropriate way. (3) Scientific results and conclusions are presented in a clear and well structured way.

I strongly recommend the publication of this manuscript, after consideration of a number of specific comments.

Specific comments:

C1

- Could you add MetOp-C as well? (Page 2, Line 14)
- Could you give some numbers? What are the European standards? On how many days the standards are exceeded at this station? (Page 3, Line 20)
- Is this number correct? In Kreher et al. (2019) you mention -50°C. Please check again. (Page 4, Line 5)
- Could you give some details on MMF performance? What are its strengths/weakness compared to other algorithms? (Page 5, Line 13)
- Is there any reasons why you don't use data from radiosondes instead of standard profiles? Is there any nearby station? (Page 5, Line 18)
- How far is the AERONET station away from the MAX-DOAS station? (Page 5, Line 22)
- Is the temperature dependence on NO₂ cross section relevant for UV and Vis, or only for Vis? (Page 6, Line 23)
- Does the direct use of dLeff only lead to underestimation of NO₂ VMR? A factor of three appears quite large. Is a factor of three appropriate for such low elevation angle as used in your study? (Page 7, Line 16)
- Previous studies have highlighted the importance of properly estimating correction factors, but did any of these studies compare direct use of dLeff (dLeffO₄, without fc) with corrected dLeff (dLeffNO₂? I would suggest to also compute dLeffO₄ (e.g. Seyler et al. 2018) and compare with dLeffNO₂. I would be surprised to see a factor of three difference. I suggest to add one plot (dLeffO₄ for UV and Vis) to Fig. 8 (b). This would really help to know how essential are such correction factors for urban settings and low elevation angles. (Page 7, Line 26)
- Did you take AOD, asymmetry parameter, and SSA values from the AERONET station? AOD at which wavelength?

C2

- Why did you not use data from weather station for the calculation of τ_{air} ? (Page 9, Line 7)
- Did you include uncertainties arising from the use of AFGL profiles instead of data from weather stations? (Page 9, Line 25).
- Throughout the whole manuscript you are using the terms MLHNO₂ and MLHMAX-DOAS but actually, as I understand, the two terms refer to the same parameter? I suggest to use only MLHMAXDOAS?
- How far is the ceilometer away from the MAX-DOAS station? (Page 11, Line 10). I suggest to include the position of AERONET and ceilometer stations in Fig. 1, if the position is other than for the MAX-DOAS instrument.
- Can you give some details on cloud screening? Did you use cloud-screened pixels only? (Page 12, Line 16)
- Please also include a few sentences discussion about $dLeffO_4$ and compare with $dLeffNO_2$. (Sect. 4.1, Fig. 8).
- Did you use 11:00 UTC because of TROPOMI overpass? If so, please add this information. (Page 13, Line 5)
- Is it really only up to 200 m? I would suppose values up to 350 m for $dLeff = 10$ km and $EA = 2^\circ$ and also I expect differences for UV and Vis channels, according to Wang et al. 2014 AMT. (Page 15, Line 6).
- Because you mention that one azimuthal MAX-DOAS measurement samples air masses along several kilometers, what about the correlation between MAX-DOAS (geometric approach, e.g. using 30° measurements) and TROPOMI? (Page 16, Line 17)
- Actually I do not see improvement for summer. (Page 17, Line 4)
- Again, not clear which cloud fraction you used for cloud screening. (Page 17, Line 19)

C3

- Is it mean or median? (Page 18, Line 30) Because in the conclusion you state that you are using median MAX-DOAS profiles as a priori. (Page 20, Line 17)

Technical corrections:

- associated with (Page 11, Line 29)
- Two modifications are introduced (Page 16, Line 24)
- Some words (e.g. ... algorithm is based on NO₂ ...) are missing in the first sentence of Sect. 4.4.3 (Page 18, Line 19)

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C4