

This paper describes a method to derive average free tropospheric concentrations of NO₂ and Ozone from a passive DOAS instrument and validates the results with independent measurements. This technique can also be applied for other species measurable by DOAS. I suggest publication only after major revisions to the introduction/method section since there are significant shortfalls in the author's rationale. I do not doubt the applicability of the described method. However, I don't think that a single scattering radiative transfer code (p. 8242, ll. 4-20) can be used to confirm that a single scattering geometric approximation is valid. I'm certain that the single scattering assumption is a valid one here because of the fact that those measurements are comparable to airborne MAX-DOAS measurements and studies have been published comparing multiple-scattering radiative transfer calculations with geometric approximations (e.g. Baidar et al. (2013) though for nadir column measurements only). In view of this, the lengthy discussion about ground-based geometrical approaches (p. 8237, l. 27- p. 8238, l.18) seems to be out of place since these measurements are always much more affected by a multiple scattering regime than measurements at a higher altitude in the atmosphere and hence a geometric approach in these cases yields poor quality results. I would suggest to discussing your method in comparison to airborne MAX-DOAS methods. My second major criticism is that this paper does not include any error estimates.

More specific comments/suggestions:

* The pronounced diurnal cycle in the DOAS measurements for NO₂ as well as for ozone (Fig. 7,9) could probably be explained if you reproduce Fig. 5 for a SZA of 85deg. Another guess would be that the single scattering approximation is not valid at such large SZA anymore and I would recommend using stricter selection criteria for the SZA.

* O4-MGA: O4 has been used in several studies to estimate the optical path for MAX-DOAS measurements before. Please cite appropriate literature. Sinreich et al. (2012) were not the first and only ones.

* P. 8249, l. 4-6: What are the detection limits of the NO₂ monitor and for the DOAS instrument?

* P. 8240, ll. 18-19: It would make more sense to call it a path average than the 'concentration at the level of the station' since the aim is to obtain FT concentrations.

* P. 8239, l. 15: Is it clear-sky above the sea of clouds or are the clouds only towards the north, but not the rest of the island? This is confusing.

* Fig. 2 is misleading in helping to understand the calculations in section 1.2 since SZA_1 is not the real SZA in this off-axis geometry. The real SZA for the sketched geometry would be $SZA_1 + \gamma$. However, I assume your method only works if γ is small and $h \ll h_s$. If the sketched geometry would represent the true geometry, then the true SZA of the off-axis direction would depend on the distance d and hence f would be a function of d and with that your whole argument would fall. I suggest redrawing the sketch with a flat Earth's surface.

* If you use f' and d' for the AMF-MGA calculations, then you should also use c_{st}' .

* P. 8241, l. 5-6 and p. 8247, ll. 15-20: what temperatures do you use for the calculation of the O₄ concentration?

* Fixing the rel. azimuth in the simulations to 0 (p. 8246, l.24) while the telescope points to the North, seems to be a poor choice.

* Last, but not least: English syntax problems have to be addressed throughout the manuscript, especially the use of articles. Here is an example, the sentence (p.8237, ll.16-17):

On one hand, unlike the in situ measurements, MAX-DOAS integrate optical paths over few tens of km.

should read:

On the one hand, unlike in situ measurements, MAX-DOAS integrates optical paths over a few tens of km.