

We first thank the referee for his review and for his constructive remarks which were used to improve the manuscript. Our replies to the referee comments are in italic, the changes in the manuscript are in bold.

1. The important issue of VCD calculations is the AMFs. During the flight, the AMFs for SWING also depend on viewing angle, geometric angle, i.e. roll and pitch etc. Are these factors considered in the calculation of AMF?

We thank the reviewer for pointing this out as this was not properly described in the paper. In practice we have built a look up table of AMFs on a grid of the effective viewing angles (taking into account the attitude) and we have interpolated on it to estimate the AMF. This is why we had written 'for each spectrum' p9 L 8. This was made more explicit in section 4.3 of the text.

The viewing angle varies for each spectrum due to the scanning and the variations in UAV attitude. This is accounted for by interpolating the AMFs on a look-up table built on a 10° grid of the viewing angle.

2. The saturated absorption effect of water vapor may bring errors to the spectral retrieve of water vapor. Do the authors consider about this?

The SCD of water vapor does not vary linearly with respect to its column since its fine spectral structures are generally not resolved by UV-vis spectrometers, this is investigated e.g. in:

Wagner, T., Heland, J., Zöger, M., and Platt, U.: A fast H₂O total column density product from GOME – Validation with in-situ aircraft measurements, Atmos. Chem. Phys., 3, 651-663, <https://doi.org/10.5194/acp-3-651-2003>, 2003

We have neglected this effect since in our case, the H₂O SCD is known to be relatively small from the balloon measurements and the fact that we only use the zenith spectra. However, it is important to mention it for future applications where we would use more angles of the instruments

Note that the DOAS retrieval of H₂O is complicated by the non-linearity due to unresolved absorption lines. This leads to a saturation effect which becomes important at high SCDs, as investigated in Wagner (2003). We have neglected this effect considering that we were only using zenith measurements in an area with relatively low H₂O VCDs. The latter can be estimated from the balloon measurements to be around 9×10^{22} molec cm⁻². The associated error could be around 5%. In future SWING H₂O measurements, this assumption should be checked especially if using off-zenith angles and when comparing with ancillary observations.

This was also mentioned as an error source which could possibly bias negatively the measured relative humidity in section 5.2

As discussed in Sect.4.1, these estimated humidifies could be negatively biased by a few percent due to saturation of water vapor. They nevertheless appear realistic.

3. SO₂ is also another main product of exhaust plume from power plant. Since the spectral range of 200-750nm, it would be interesting to have a try to retrieve SO₂ ?

We tried to retrieve SO₂ in our spectra but there was not enough signal in the UV with this spectrometer. This was improved in a follow up campaign (AROMAT-2) when we changed the spectrometer and had a good signal of SO₂ which also enabled us to produce a SO₂ map. This information was added to the new conclusion, and the AROMAT-2 experiments will be presented in the AROMAT overview paper in preparation.

4. The middle panel in Figure 6 presents the potential temperature profiles. However, the data show the temperature increases with height. Is it thermal inversion? Inversion is easy to accumulate pollutants. What's the impact on the distribution of emissions of NO₂?

Considering the main plume, its source is very close to the observations so it is not well mixed in the boundary layer and thus we cannot use it to conclude on the effect of the inversion. But Fig 6 (left panel) also shows a second NO₂ plume at a higher altitude and on both balloon flights, close to the boundary layer. This second plume may originate from another power plant in the south. This is discussed in detail in section 3.3 L. 24-31. We had initially written that the second plume was 'just above the boundary layer'. It could actually be at the top of the BL, trapped in the capping inversion. It is however difficult to state that with certainty with the data. We have reformulated the text to mention this.

Note that the elevated NO₂ layer detected on both soundings appears close to the capping inversion, particularly visible in the morning balloon flight.

5. The Mobile DOAS VCDs are significantly higher (by approximately a factor of 2) than the airborne VCDs. Why? Please give some discussions.

This discrepancy is discussed p13 l.1 to 15, when we present Fig. 13. Actually, the Mobile DOAS VCDs are higher inside the plume but the difference shrinks and even gets opposite outside of the plume. Beside errors on AMF inputs and time differences, we mention the smoothing effect due to the radiative transfer for the airborne instrument. We have added some numbers to be more quantitative with this explanation

At the time of the NO₂ mapping flight, the solar zenith angle was close to 45°. Assuming the NO₂ plume to be 800 m thick, as the balloon flight indicates, leads to a horizontal smoothing of the same length for the airborne observations. Considering the sharp gradients of the NO₂ field close to the power plant, this smoothing probably explains a major part of the difference between the aircraft and the mobile DOAS measurements.

6. Is there any temperature control for the instrument (spectrometer)? For the ascent and descent, how about the spectral stability?

There is no active temperature control, however during operation, the SWING box is heated by the PC-104. The temperature effect on spectra is partly dealt with by fitting a pseudo cross-section corresponding to change in spectral resolution, as described in section 4.1 (p. 7; l.26 -28).

7. The spectral resolution of SWING is 1.2nm or 1.3nm? 1.2nm in table 1, but 1.3nm on Page 3, Line 22.

The spectral resolution is 1.2 nm, the 1.3 nm on p. 3 was corrected.