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Interactive comment on "Airborne multi-axis DOAS measurements of tropospheric SO_2 plumes in the Po-valley, Italy" *by* P. Wang et al.

Anonymous Referee #1

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GENERAL COMMENTS

The paper shows that AMAXDOAS can be used for quantifying SO2 emission from known sources. The emission estimates still have large uncertainties, but the authors argue that this can be improved by better knowledge on the local meteorological situation. I agree, and therefore consider it a pity that the authors didn't try or didn't succeed in retrieving the appropriate information from the local meteorological institute on wind direction, wind speed, and boundary layer characteristics (see specific comments 2 and 10).

The paper is generally well written, and with good level of detail, except for some points,

which are specified below.

SPECIFIC COMMENTS

1) The applied cross sections are listed in Section 3.1. Some of them have quite low spectral resolution. The authors should explain how they dealt with the differences in resolution of the AMAXDOAS (please specify) and the laboratory cross sections. Wrong use is a potential source of errors in the SO2 slant column.

2) The uncertainty on the estimate of the power plant SO2 emission is argued to be approximately 50%, dominated by the uncertainty in wind speed (2 m/s) and direction (30°). However, a few other (possibly large) error sources should also be considered:

* The assumption that the SO2 is well mixed from 0 to 1.7 km is not well founded, and the error budget is very sensitive to this assumption. The horizontal distance of the location of the measurement to the power plant might not be large enough for the plume to have reached either the ground or the top of the boundary layer. A more confined plume would change the AMF considerably. For instance, if the SO2 plume at 5 km distance reached up to 1.3 km instead of 1.7 km, the AMF would decrease and the emission estimate would increase with approximately 20%.

* The vertical wind profile in the plume is not discussed. Both the direction and the wind speed can have large variability. Some examples from the ECMWF analysis at 45N, 12E, 12 UT: the wind speed on 27 September 2003 was 1.7 m/s at 250 m and 10.4 m/s at 600 m, the wind direction on 26 September 2003 was 57° (ENE to WSW) at 250 m and 295° (WNW to ESE) at 600 m.

3) Section 4.1, page 2023, line 22: 'roughly well mixed below 1.5-1.8km' is too vague. Use figure or table to quantify. Where does 1.7 km come from?

4) Page 2024, line 5: 'The error of the fit was about 12%.' Is this the error in SO2 slant column? This seems to contradict page 2028, line 22: 'The fit error ... was between 15-50%'

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5) Page 2024, line 16: replace ’measured’ by ’detected’.

6) Page 2024, line 17-19: remove: "which indicates that ... was also similar." This is the subject of the paper and calculated later on. This handwaving argument is out of place.

7) Page 2024, line 28-29: ’should be well correlated ... much higher accuracies’: give values and references.

8) Page 2025, line 1-2: replace ’which is completely ... the SO2 fit.’ by ’which does not overlap the SO2 fitting window.’

9) Page 2025, line 5: repace ’measured’ by ’detected’.

10) Section 4.2: see also comment 2 above. Use a simple plume model and more realistic weather conditions, e.g. obtained from a local meteorological service: vertical profiles of wind speed and direction, and other parameters that should be input to the plume model, like potential temperature. Formula (1) should then include an integral over the vertical.

11) Page 2027, line 24-27: Why not use formula (1), instead of this approximation?

12) Section 4.3: No error estimates are given for the calculation of the SO2 pollution near Mantova. Also the vertical variability of wind direction and speed in not taken into account.

13) Conclusions: "The off-axis data ... proved to be useful to determine plume altitudes". This is formulated too strong; the only thing that is concluded is that the SO2 near the city must be below 500 meter, because otherwise it would have been detected in the upward viewing directions. A lower boundary could not be given. For the power plant one could conclude that SO2 is both above and below 600 m, not that it is "well mixed in the boundary layer". It is likely that the combination of all viewing angles, together with the radiative transfer model can give more information on the vertical distribution. If so, this should be exploited in this paper.

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Interactive Comment

14) Page 2030, line 17-19: "The advantage of ... establish the emissions." This is not completely true. As suggested in comments 2 and 10, the uncertainty can probably be much reduced with a simple plume model and realistic meteorological conditions. The vertical extend of the plume will be determined by the wind speed, the distance to the source, and the turbulence.

15) MODIS teams are acknowledged, but I missed where the MODIS data is used in the text, except for a reference to literature.

16) Figure 3: The dotted line is referred to as 'SO2 fit'. It is however the measured signal after subtraction of the fit to the other absorbers. Therefore I would prefer a term like 'SO2 residual' or something similar. In the caption: " the dotted line is the measured spectrum after subtraction of the fit to the other absorbers.

17) Figure 5: Include latitude and longitude of the mentioned airport.

TECHNICAL CORRECTIONS

* Formula (1) should have 'sin' instead of 'cos'.

* Page 2024, line 5: 90 km instead of 100 km

* Page 2027, line 25: change 'the half with' to 'the half width'.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 2017, 2005.