

Interactive comment on “The Ultraviolet Visible Hyperspectral Imaging Spectrometer (UVHIS), and high-resolution NO₂ mapping from its first airborne observation” by Liang Xi et al.

Anonymous Referee #2

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Referee comment to: The Ultraviolet Visible Hyperspectral Imaging Spectrometer (UVHIS), and high-resolution NO₂ mapping from its first airborne observation by Xi, L., Si, F., Jiang, Y., Zhou, H., Zhan, K., Chang, Z., Qiu, X., and Yang, D

General comment:

Xi et al. present a new airborne imaging DOAS instrument and results of the first demonstration flight. The results are encouraging and data might be interesting for further analysis such as satellite validation, emission estimates or model comparison. The paper fits very well in the context of AMT. However at some points more details might be required by the reader. Most of them are not critical but require an update of

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the manuscript.

Major Comments:

Through out the manuscript the authors should take care to distinguish between total and tropospheric vertical columns - in most cases the tropospheric vertical column or to be more precise the column below the flight altitude is meant e.g. l 167.

The NO₂ fit shown in figure 4 has some residual structures, which might be noise but might also be caused by a systematic issue. The instrument was carefully calibrated before the measurements. The wavelength calibration is used only as apriori for the QDOAS software - which is certainly necessary. The slit function shape that can be extracted from the measurements using the Mercury-Argon lamp but are not shown or mentioned. Instead a symmetric Gaussian slit function is assumed in the DOAS analysis. The width of the slit function varies significantly within the fitting window (figure 3) - maybe the shape does so as well? I suggest adding at least a figure of the measured slit function for the extreme viewing directions (left, centre and right) including the Gaussian fit.

The observations partly overlap as the distance between the parallel flighttracks was 1.5 km and the swath width is 2.2 km. How good do the observed tropospheric NO₂ columns agree in the overlapping regions, does this depend on the flight direction and time, according to figure 2 some parts of the flight track were covered at least twice.

Minor Comments:

Abstract:

The error given here is not the fitting error but the total error of the VCD - even if the fitting error $\sim 4.8 \times 10^{15}$ molec/cm² is probably the dominant contribution.

Section 2 instrumental Details:

The instrument was build for airborne measurements in the troposphere, however the

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spectral range encompasses the deep UV from 200-276 nm in channel 1, I am afraid the intensity in this channel will be very low. Through out the manuscript the data from the channels 1 and 2 are not shown nor used. Airborne measurements often face the problem of the instruments being too heavy, therefore I am surprised to read that the instruments has a channel 1 that seems not very useful. Maybe the authors can briefly comment about the potential use of the channels 1 and 2, or the former use in a different instrument. For channel 2 I can think of the retrieval of SO₂ or HCHO, both would be interesting for the presented study but may require a more detailed analysis.

The figure of the instrument (figure 1) is a bit confusing it might be clearer if the authors reduce the number of light beams. It seems that part of the "red" light beam originating from the "top" is blocked by the convex grating, I suppose this is not the case, maybe because it is shifted relative to the drawing plane in the third dimension? Does second perspective view help to explain more clearly? A radiometric calibration was performed as well as a spectral calibration. However, it seems the data from the radiometric calibration were not used it might be interesting to see the calibrated intensity in comparison to the LANDSAT 8 albedo (figure 7).

Parts of CCD are blocked to control offset and dark current which is a good idea if this part of the CCD can not be used for real observation. However in section 4 the well established pre-flight dark current and offset measurements are used and the dark measurements at the edges of the CCD are not mentioned.

Section 3 Research flight:

For non Chinese readers some more details about the measurement area might be nice to have. Can you add a map of China indicating where the city of Feiching is and in the context of the paper a satellite observation of NO₂ for the day or season might be included as well.

Section 4 Data processing chain:

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The authors use a wavelength range between 430 and 470 nm for the DOAS analysis including the cross sections of NO₂, O₃ and O₄, as well as the ring cross section. However water vapour also shows some strong absorption lines in the respective wavelength range (Hitran data base, Rothman 2013), this is not included in the DOAS fit. (see general comment)

The tropospheric background of 1×10^{15} molec/cm² as given by Popp et al. (2012) refers to the background around Zürich I am not sure this can be assumed for China as well. Here a satellite observation of the rural background around Feiching might help to estimate a realistic background.

The section about the Landsat 8 data analysis and resolution might be shifted from section 4.3.2 to 4.3.1. Meier et al. 2017 developed a method to retrieve the albedo from the measurements did you consider applying a similar method?

The assumption about the aerosol optical density, SSA and scattering function are partly given but not justified, the details about the optical density are not given directly.

During ascent and decent of the plane you can often estimate the PBL height from visibility or if available dew point measurements. This might be more realistic than the typical summer day assumption of 2000 m. However, the measurements were performed 2 years ago the respective information might be lost. The error is less than 13%, assuming that the PBL reached at least up to 1000 m.

The measurements by Tack et al. (2017) were performed in an SZA range of 40 to 60 degree; in this range the effect of the SZA might be different as in the range between 10 and 40 degree.

Section 6 discussion:

The uncertainty of the cross section is usually dominated by systematic uncertainty and not by the random errors. Only the random part is included in the QDOAS error analysis.

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For the reference region a vertical column of 1×10^{15} molec/cm 2 is assumed, therefore for the error of the slant column it should be multiplied by the respective AMF.

The focus of the paper is on the airborne instrument. A proper reference to the mobile DOAS instrument should be given, if this is not possible it might worth adding some additional information. Is the tropospheric or the total NO₂ column retrieved? How large are the uncertainties of the mobile instrument? Are the same AMF settings (albedo, PBL height, aerosols) used as for the airborne observation?

Technical comments:

Please add an approximate scale in figure 2 or write some comparable information in the caption e.g. the measurement area was $\sim 20 \times 30$ km and the distances between parallel lines were 1.5 km In figure 10 the individual sources are numbered it might help to add the numbers already in figure 2 in addition to the description.

L 130: replace one "across track" by "along track"

L 163: delete the last sentence starting with "the direct output..." this already written in I 152.

L 226: Is [28] a reference? Please use the AMT reference style.

L 320: "are near downwind of several plumes" replace by "inside the plumes" or "downwind of the sources"

Figure 8: "wavelength 20 nm" looks like a copy-paste-error from figure 9 "SZA 20°"?

Figures 10 and 11: please add the approximate position of the figure 11 in figure 10 and use the same numbers in figure 11.

References:

In some references there is a layout problem with subscripts like in NO₂.

Rothman, L. S., Gordon, I. E., Babikov, Y., Barbe, A., Chris Benner, D., Bernath, P. F.,

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Birk, M., Bizzocchi, L., Boudon, V., Brown, L. R., Campargue, A., Chance, K., Cohen, E. A., Coudert, L. H., Devi, V. M., Drouin, B. J., Fayt, A., Flaud, J.-M., Gamache, R. R., Harrison, J. J., Hartmann, J.-M., Hill C., Hodges, J. T., Jacquemart, D., Jolly, A., Lamouroux, J., Le Roy, R. J., Li, G., Long, D. A., Lyulin, O. M., Mackie, C. J., Massie, S. T., Mikhailenko, S., Müller, H. S. P., Naumenko, O. V., Nikitin, A. V., Orphal, J., Perevalov, V., Perrin, A., Polovtseva, E. R., Richard, C., Smith, M. A. H., Starikova, E., Sung, K., Tashkun, S., Tennyson, J., Toon, G. C., Tyuterev, V. G., and Wagn, G.: The HITRAN 2012 molecular spectroscopic database, *J. Quant. Spectrosc. Ra.*, 130, 4–50, <https://doi.org/10.1016/j.jqsrt.2013.07.002>, 2013.

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