

Interactive comment on “MAX-DOAS measurements of atmospheric trace gases in Ny-Ålesund” by F. Wittrock et al.

Anonymous Referee #2

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Review on the manuscript:

MAX-DOAS measurements of atmospheric trace gases in Ny-Alesund

by

Wittrock et al.

General Comments:

The manuscript presents measurements and model results which confirm and describe in detail the suitability of the novel MAXDOAS method for the determination of profile information on atmospheric trace species and aerosols. Special emphasis is spent on the investigation of various effects like surface albedo, multiple scattering, refraction, and especially the azimuth angle (between the sun and the telescopes direction) on

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such measurements using radiative transport modelling. The modelling results are applied to atmospheric observations made under clear sky, and quantitative results are derived on the aerosol extinction and NO₂ profile. The paper is a very interesting contribution towards an operational application of MAXDOAS measurements. It is in general well written and should be published after addressing several minor comments.

Detailed comments:

Page 1, right column, line 24: It is said that "similar but not identical set-up have also been developed by other groups". What do the authors intend to express by this sentence? I suggest to delete the part: "but not identical".

Page 1, right column, line 25: Long term measurements using MAXDOAS instruments are also performed by other groups, e.g. at Paramaribo, Surinam or Neumayer station, Antarctica.

Page 1, right column, second last line: The expression "different slant column" is introduced without definition or appropriate reference.

Page 2, left column, line 1: please also define "VC"

Page 2, left column, line 2: I suggest to replace "expresses the enhancement" by "is defined as the ratio of". At least for satellite and aircraft observations the AMF can have values < 1

Page 2, left column, line 16: I think it was already clear before this study that the surface albedo and the aerosol load have a strong effect on the (MAX-) DOAS observations.

Page 2, left column, line 18: Hoenninger and Platt (2002) already have applied radiative transport modelling for MAXDOAS geometry

Page 2, left column, line 32: The atmospheric O₄ profile not only depends on pressure but also on temperature.

Page 2, left column, lines 35-38: How and why do the authors distinguish between

validation of the RTM and its applicaton? I believe that from an agreement between measurements and model it can be concluded that both the model and observations are validated. However, this includes the variation of the aerosol parameters, ground albedo, etc. (within realistic values) until agreement is reached.

Page 2, left column, 5th line from bottom: Wagner et al. have already applied a similar approach to zenith sky observations and have suggested to apply it to off-axis and multiple axis observations.

Page 2, right column, 7th line from bottom: Are the fibres really positioned in a circle or over a disc?

Page 3, left column, 5 last lines before section 2.2: I suggest to delete this text because it is not relevant to this study.

Page 3, right column, line 23: The photons cover a slant distance before scattering only for large SZA.

Page 4, left column, line 4: Usually the output of the DOAS analysis is a differential slant column (see also comment #3 above). The authors might add some more explanation here.

Page 4, left column, equation (2): To my knowledge this formula was first introduced by Solomon et al., 1987. I suggest to add this reference.

Page 4, right column, second paragraph: Here I suggest that the authors make clear that they investigate the O4 VC which they derived from their observations after applying various AMFs. Otherwise the reader might get the impression that the actual atmospheric O4 VC was changing.

Page 4, right column, line 25: At 370 nm there is actually no O4 absorption line. Would it be better to use an AMF for 360 nm where the strongest O4 absorption takes place?

Page 4, right column, last paragraph before section 4.1: The authors claim that the ap-

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plication of an AMF-difference might be a more realistic choice than the application of an absolute AMF. I think this is not necessarily true, especially since both observations (the measurement and the reference spectrum) can have been recorded at different times, and the atmospheric conditions might have changed inbetween. Often the time difference between measurement and reference spectrum is more than a day. Moreover, I think it would be much more instructive to show the influence of the various parameters on the absolute AMF rather than on the AMF-difference. This becomes in particular obvious in section 4.3.

Page 4, right column, line 4 in section 4.1: The error neglecting multiple scattering strongly depends on wavelength and can be small at large wavelengths.

Page 6, left column, line 18 in section 4.6: It is stated here that 'Some of the parameters are clearly not independent from each other'. This should be said much earlier, e.g. in the sections on ground albedo, aerosols, and multiple scattering.

Page 6, right column, line 31: Why do the authors express the O₄ columns in 1495×10^{40} instead of 1.495×10^{43} ?

Page 7, right column, lines 10-12: This finding is valid only if the reference spectrum was affected by the same atmospheric conditions as the actual measurements. The influence of a changing albedo and aerosol extinction is different for the absolute AMFs. The authors should discuss this here in more detail.

Page 16, Figure 8: Some data are represented by dots, some by dots and lines. I suggest to homogenise this Figure.

Pages 17, 18, Figures 9, 10: What can be learnt from Fig. 10 in addition to Fig. 9? Maybe one of both Figures should be removed.

Page 23, Figure 15: I suggest to enlarge this Figure (why is the most complicated Figure displayed in the smallest size?). Why is the NO₂ profile expressed as mixing ratio and not as concentration?

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