Atmos. Chem. Phys. Discuss., 3, S2679–S2684, 2003
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Atmospheric Chemistry and Physics Discussions

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

F. Wittrock et al.

Received and published: 21 June 2004

We much appreciate the very detailed comments by the referee, which helped us to improve the paper.

RC: 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".

AC: We agree and have deleted this part in the revised manuscript.

RC: 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.

AC: We agree and have deleted " and used for campaign measurements or with only

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one horizon viewing direction".

RC: Page 1, right column, second last line: The expression "differential 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.

AC: In the revised version of the paper, we briefly explain both DSC and VC in this section: "This knowledge is essential to convert the differential slant columns (DSC - the difference of the column amount of the absorber integrated along the light path through the atmosphere and the absorber amount in the background spectrum), which can be derived from the absorption measurements, into a vertical column (VC - the trace gas concentration integrated along altitude) with the help of a so-called airmass factor (AMF). This factor is defined as the ratio of the optical path through the atmosphere to the vertical optical path."

RC: 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

AC: We have redrafted the paragraph, taking these comments into account: "As will be shown in this paper, for measurements with the telescope pointed towards the horizon the relative azimuth between observation direction and the sun also plays a role. While simple geometrical considerations or single scattering radiative transfer models as have been used in previous publications (e.g. Hönninger and Platt (2002)) can give a rough estimate of the absorber's concentration, more detailed radiative transfer calculations are needed for quantitative results, in particular for small elevation angles."

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

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AC: We have corrected this.

RC: Page 2, left column, lines 35-38: How and why do the authors distinguish between validation of the RTM and its application? 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.

AC: This is related to comments from Dr. Howard Roscoe in his review. We have redraft the paragraph to make it more clear. "Thus, from the knowledge of the atmospheric pressure and temperature profile the vertical O4 column can be determined. This calculated column of O4 can be used to determine the correct settings for aerosols and surface albedo in the radiative transfer model by varying these values until agreement with the measurements is found, because the diurnal variation of O4 should be very small."

RC: 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.

AC: We have replaced "suggested" with "applied".

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

AC: We have corrected this (not in a circle, but discoidal).

RC: 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.

AC: We do not agree. It's true, that this sentence is not relevant for the results presented in the paper. But it should help the reader to understand, why only data from 2002 and 2003 are shown and what they can expect in following studies.

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

AC: We have added "For large SZA ... ".

RC: 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.

AC: We have rephrased this paragraph to make this more clear to the reader.

RC: 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.

AC: As pointed out by the referee Dr. Howard Roscoe, this equation was first published by Perliski et al.(1993). We have added the reference here.

RC: 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.

AC: We have slightly rephrased the paragraph as suggested.

RC: 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?

AC: We have chosen 370 nm, because this is the center of the wavelength window, where we have analysed O4. The difference to the AMF calculated at the nearby peaks of 360 and 380 nm is very small.

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

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(the measurement and the reference spectrum) can have been recorded at different times, and the atmospheric conditions might have changed in between. 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.

AC: We disagree with the referee in this point and would argue other way round: especially section 4.3 should illustrate that sometimes intuition is not helping. The main application of MAX-DOAS is to derive profile information for a selected trace gas. For this the Fraunhofer reference spectrum (FRS) used in the DOAS retrieval is chosen from the same or a nearby day.

RC: 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.

AC: We have added "at least for the used wavelength region".

RC: 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.

AC: We think, that this statement should be said in the summary of this section. Otherwise the reader might be confused.

RC: Page 6, right column, line 31: Why do the authors express the O4 columns in 1495 x 10Ĺ40 instead of 1.495 x 10Ĺ43 ?

AC: We have corrected this.

RC: 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.

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AC: We have discussed this e.g. in section 4.3 of the paper. In the conclusions further details doesn't seem to be helpful.

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

AC: We have revised this figure as suggested.

RC: 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.

AC: The reason for showing both figures is given in the text: Figure 10 illustrates the fact, that a smaller albedo results in a higher relative sensitivity of the off-axis measurements. Figure 9 is in the tradition of the other figures (5,8,12,13) showing the error introduced by using wrong albedo.

RC: 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?

AC: We have enlarged the figure as suggested. In addition we have revised the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 6109, 2003.

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