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Atmospheric
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Discussions

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

F. Wittrock et al.

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We would like to thank Dr. Howard Roscoe for his very detailed and helpful comments.

RC: However, some revisions are necessary. In particular, it is difficult to follow some earlier arguments in the paper because the error calculations are displayed in contrasting ways in the figures. This is despite the assertion on p6118 line22 that "All" are presented in the same way. Figures 5, 8, 10, 12 and 13 show the change in vertical column due to various changes or scenarios, as advertised on p6118, whereas Figures 6, 7 and 9 show air-mass factor or its change, or slant columns, which change in the opposite sense to vertical columns. Furthermore, Figures 5, 8, 10, 12 and 13 show relative changes, whereas Figures 6, 7 and 9 show absolute changes. As well as disagreeing with the text, this alternation is very confusing. The authors should at least make the text agree with what is presented; better would be to recast Figures 6,7 and 9, so that all show a similar product.

AC: First of all: In the web version of the paper the figures 9 and 10 but not their captions are swapped. Nevertheless some more explanations: We agree with the referee, that the text in the beginning of section 4 could be misunderstood. However, rather than recasting the figures we prefer to revise the text. The main reason is, that the paper shows for each parameter one figure illustrating the error introduced to the vertical column when setting the respective parameter to an inappropriate value (Figures 5,8,9,12,13). Additional figures are shown, when more information is necessary to illustrate the influence of one parameter. In the redrafted version of the paper we have added some more details concerning the interpretation of each figure.

Other specific comments: RC 1: This is primarily a technical paper, with a careful sensitivity analysis and a description of the apparatus. There is only one sample scientific result, NO2 on one day. The title should reflect this technical nature. The existing title leads us to expect a set of measurements of more than one scientifically important trace gas. Although it is technically important, so far O4 is not scientifically important.

AC: We agree with Dr. Howard Roscoe and have revised the title: "MAX-DOAS measurements of atmospheric trace gases in Ny-Aalesund - Radiative transfer studies and their application"

RC 2: The argument on p6112 lines15-20 (O4 from meteorological measurements can be used to validate radiative transfer models, which can then be used to determine aerosol and albedo by comparing the modelsŠ O4 with that from measurements) is circular and contradictory as written. If the authors mean that the diurnal or azimuthal change in O4 calculated by the model should be zero if the aerosol and albedo are correct, they should say so.

AC: Reviewer 2 has made a similar statement concerning this paragraph. We have rephrased it. "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 radia-

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tive transfer model by varying these values until agreement with the measurements is found, because the diurnal variation of O4 should be very small."

RC 3: Figure 1 and the text in Section 2.1 are vague about how light gets from the lamps to the fibres. If the illumination of the fibres by the lamps is non- uniform, have the authors tested to ensure that the fibres are long enough that the lamps uniformly illuminate the grating? If they do not, the spectral resolution and wavelength calibrations will differ between lamp and atmospheric measurements.

AC: We have tested the illumination of the fibres. No indication of varying spectral resolution has been found.

RC 4: The sharp weighting function at small elevation angle illustrated in Figure 4 should allow a major advance in vertical profiling by this technique, but the authors make little of the idea. They also do not explain why it is so sharp - it cannot be the sphericity of the earthŠs atmosphere as this would give a function of half-width several km. Do we assume that it is sharp because of large multiple scattering by air molecules at small elevation angles? If so, the sharpness will be very dependant on wavelength, a point not mention in the text or caption, and will be less at the UV-visible wavelengths normally used for measuring NO2, and much less at the visible wavelengths often used for measuring O3.

AC: The sharp peak of the AMF (weighting function) in figure 4 is mainly due to the simple geometric elongation of the light path through the layer nearest to the DOAS system and neither multiple scattering nor sphericity play a dominant role at least for 60° SZA. Consequently the dependence of the AMF on the wavelength is not very pronounced. We have added some more explanation to the text to make this point clearer to the reader. The capability of vertical profiling with the MAX-DOAS technique has been illustrated in section 5.2. More details to this is shown in the paper by Heckel, et al. and was not the main focus of this paper.

RC 5: Presumably the sensitivity displayed in Figure 5 will also be rather less at the

UVvisible wavelengths normally used for NO2 (though this is a useful result unlike the less-useful result in point 4 above). If so, the text or caption should say so.

AC: There is not a big difference in sensitivity to multiple scattering between 370 nm and 450 nm.

RC 6: One of the important results of this paper is the very strong azimuth dependency of low-elevation air-mass factors. The physical reason for this is alluded to in Section 4.2 line12, but a full explanation and/or a sketch would greatly add to the paper.

AC: We have added a reference to the properties of Mie scattering to the paper (Van de Hulst, 1981).

RC 7: The paper demonstrates that it is quite feasible to deduce NO2 from the UV spectral region in this work (325 to 413 nm). However, dedicated measurements of NO2 that avoid artefacts due to H2O and O4 demand a spectral range that extends to at least 450 nm (e.g. Roscoe et al. 1999)

AC: We agree with Howard Roscoe, that the UV is not the usual spectral region to deduce NO2. However, in 2002 and the beginning of 2003 there was no second spectrometer for the wavelength region larger than 413 nm available. In a forthcoming study we will present time series of tropospheric NO2 derived in the 425 to 450 nm spectral region.

RC 8: The conclusions make assertions regarding SO2 and HCHO that are not demonstrated in the paper and are probably not demonstrable from other work; indeed the bands of SO2 are so far into the UV that it is doubtful that MAX-DOAS could measure it except in the worst pollution, and whilst HCHO has bands extending to 350 nm they are becoming weak here (see for example Roscoe & Clemitshaw Fig 2A). The conclusions also make assertions regarding BrO and O3 that are not demonstrated in the paper, nor mentioned earlier except in the introduction, although other work could be cited to show that BrO and O3 could be measured by this technique. The authors have a point to prove if they wish to retain the assertions regarding each of these four trace gases.

AC: This paragraph should also be seen as an outlook to future applications. For clarity we add some references to further studies (e.g. Heckel et al. have shown, that HCHO can in fact nicely be retrieved with MAX-DOAS).

RC 9: Whilst many of the comments by Anonymous Referee 2 are well-founded, and we even share some such as about the smallness of Figure 15 (see below), not all should be accepted. For example, contrary to the comment about "Page 4, left column, equation (2)", Solomon et al (1987) used the intensity-weighted approximation for calculating AMFs, rather than using Equation (2) on p6118 of the paper which calculates AMFs from the ratio of intensities with and without absorber. Equation (2) was first suggested by Perliski & Solomon (1993) but was not used there. Its first use was probably by Sarkissian et al (1995).

AC: We have enlarged the figure as suggested and have accounted for the second comment by including the reference Perliski (1995).

RC Technical comments:

p6113 line3 - as the paper is in English, then "O" should be replaced by "E"

p6113 line5 - "setup" is by now acceptable as a noun, but the verb should be "set up"

p6114 line14 - presumably "nm" is a typo, the focal length is 257 mm

p6114 line20 - presumably "-40 K" is a typo and should be -40C

Fig12 legends - for consistency, "no aerosol" should be "background aerosol", and "enhanced extinction" should be "Arctic haze"

Fig14 caption - to emphasise that these are not calculations from a model as in the previous figures, replace "on" by "from slant columns measured on"

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Fig15 - is very small for such a busy figure

AC: We have corrected all the above mistakes.

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

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