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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.

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General Comments:

This important paper sets out comprehensive sensitivity calculations for the emerging technology for ground-based remote sensing of tropospheric trace gases, namely a UV-visible spectrometer observing the sky close to the horizon as well as the zenith zky. Several proprietary names have been introduced by various workers for this viewing geometry, including bright sky and off-axis, as well as the MAX-DOAS styled here.

This paper should be published because of its thoroughness and because of the success in reconciling calculated and measured O4. Indeed the authors are too modest when discussing the small diurnal and elevation-dependent values of O4 (Figure 14), and their comparison with O4 calculated from meteorological data - these results are

excellent, and the authors should say so. There are also some very novel and useful figure styles in the paper, such as Figures 6, 14 and 15.

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.

Other specific comments:

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.

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 models' 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.

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

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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.

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 earth's 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.

5. Presumably the sensitivity displayed in Figure 5 will also be rather less at the UV-visible 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.

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.

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)

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 con-

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clusions 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.

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).

Technical comments:

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

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 -40°C

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"

Fig15 - is very small for such a busy figure

References:

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