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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multi Axis Differential Optical Absorption Spectroscopy (MAX-DOAS)" *by* G. Hönninger et al.

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Interactive Comment

Referee response to ACPD 2003-117 manuscript 'First retrieval of global...', by Hönninger *et al.*

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1. General comments

This work presents a rigorous and carefully-worked motivation for the adoption of a fairly new retrieval method, MAX-DOAS, into the arsenal of atmospheric remote-sounding techniques. MAX-DOAS has already proven that it can provide useful measurements of atmospheric trace gases and it remains for this paper to establish the range of applicability of the method. Although the paper is necessarily long and somewhat involved I find it well-argued, sufficiently coherent, and of considerable relevance to atmospheric chemistry in general to recommend its publication in this journal providing that some of my comments are addressed.

In summary, the authors provide a framework for the realization of a multiple-axis, essentially ground-based, sub-column retrieval technique for trace gases. One of the main aims of the paper is a detailed study of the sensitivity of the MAX-DOAS retrieval method to a large number of parameters, including elevation angle, aerosol type, ground albedo, Rayleigh scattering coefficient and so on. They also show earlier

instances of the method in practice for retrieval of column densities for $O-3$, $(O_2)_2$, NO_2 and BrO. Although some of their retrieval results have been published before it is my understanding that this is the first time that they have been presented along with such a sensitivity study and this constitutes the main value of such a publication. Certainly the authors are to be commended for their very thorough analysis of the range and applicability of their method.

Perhaps my only criticism of a general nature is that MAX-DOAS closely resembles profile retrieval techniques used in airborne or space-based remote sensing from limb sounders and the authors have barely made this connection, only in passing. More in specific comments below.

2. Specific comments

No real mention is made in the main text of the spectroscopic details of the instruments used, for example spectral resolution involved in the retrievals or assumed in the simulations and wavelength coverage, although these are hinted at in various places in the text and one can glean this information from other articles published by the authors. I would like to see a table or two of these values somewhere in the text which summarizes which quantities are used for which simulations and/or retrievals, as well as a measured spectrum which carries some of this information with a fit. I would be particularly pleased to see a range of spectra as a function of elevation angle. This also relates to the next point.

The authors have not dwelt on the details of how they do their mathematical inversion, other than by two passing references (section 4.5, end of section 8) and a somewhat obscure schematic (fig.10). The authors describe a technique for determining what they call the "best fit" vertical trace gas profile. This involves

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calculating air mass factors from their Monte Carlo RTM and then solving a linear system of equations, or alternately finding the most likely test gas profile which fits the measurement from a fixed set of profiles. Unfortunately we have no information on the set of equations involved. This begs a number of questions, such as: (1) is the system under or over-determined and how large is the measurement vector per elevation angle ? (2) what is the noise on the measurement and how does this affect the inversion? (3) is the system regularized in some way other than by the limited number of profile shapes. If one is using a pure profile scaling technique, as I suspect is the case here, then it would be useful to know this as well.

3. Technical comments

The authors have already fully implemented a list of typographical and related changes to the original manuscript which I submitted to the editor along with my initial evaluation of the manuscript. Thus I have only two minor comments of a technical nature which should be addressed before publication:

Section 7, second paragraph. The authors refer, confusingly, to both figures 21 and figure 20 when what they mean to do, I suspect, is refer, in all cases, to figure 21.

Figure 18 contains a rapidly-oscillating grey line in the background of the top (ozone) panel which is not explained in the caption. I suggest the authors either remove it or give a legend.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5595, 2003.