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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spectral actinic flux in the lower troposphere: measurement and 1-D simulations for cloudless, broken cloud and overcast situations" *by* A. Kylling et al.

Anonymous Referee #2

Received and published: 2 May 2005

General comments:

The authors present a nice and thoroughly, well-written paper on spectral actinic flux measurements and their interpretation for (complex) cloudy atmospheres. The paper documents a very useful study and it adequately summarises and builds on the experimental expertise on actinic flux measurements and theoretical insights in the cloud effects on actinic fluxes that have been developed since the late 1980s. The presentation of the measurements in the figures can be much improved. Further I have a few specific comments and technical corrections.

Specific comments:

Introduction page 1: ...increases the NO concentration in the upper troposphere which leads to enhanced ozone formation rates.

These statements need more explanation. The production of NO (or NOx) by (extra) UV radiation is not what is meant here, and the exact relation between photolysis rates and the chemistry (ozone formation) deserves some more attention. The main effect the authors presumably would like to refer to is that underlying reflective clouds shift in the upper troposphere the photostationary state between NO2, O3 and NO towards NO+O3 by enhanced NO2 photolysis, which in turn enlarges the changes for NO molecules to react with, e.g., peroxy radicals(HO2) instead of ozone, thus leading to ozone formation (Thompson, 1994). Although it is likely the most relevant effect, this is only one of many more chemical mechanisms because photolysis rates of many other compounds are changed simultaneously with the NO2 photolysis. Suggestion for change:

...shifts the photostationary state relationship (NO-O3-NO2) towards NO, thereby favoring NO-to-NO2 conversions by other compounds than O3 (such as HO2), which effectively increases the ozone formation rate in the upper troposphere. Other photolysis rates are affected as well and these changes may either add or counteract to the chemical effect of enhanced NO2 photolysis.

Introduction page 2: I would suggest to add one more reference to relevant experimental work in this section: Matthijsen, J. et al., Photodissociation and UV radiative transfer in a cloudy atmosphere: Modeling and measurements, J. Geophys. Res., 103, 16,665-16,676, 1998. These authors showed, I think for the first time, how UV variability translates into chemistry variability (OH) in a complex cloudy atmosphere.

Section 2.2 Why did the authors for the determination of the total ozone column not also compare with the satellite observations of total ozone column from e.g. GOME or EP-TOMS, as publicly available on the Internet? Please include the total ozone

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columns measured, satellite-derived and actually used with discussion.

Figure 1: Are the results for wavelengths smaller than ˜295 nm considered of sufficient quality to include in these figures? I assume that measurement uncertainties grow for these wavelengths. Please indicate in the text a threshold that is used for the uncertainty and remove data for small wavelengths if these do not meet the specified uncertainty.

Section 5.1. Fore-last paragraph. 'decrease' should be 'increase' (in the downwelling actinic flux)

Figure 10: could you add lines representing cloud base and cloud top in these figures? This also helps to better locate where the theoretical maximum is to be expected in the observations.

Figure 10: Can you add to the paper the information on why the descent is not shown for day 263?

Section 5.3.2. Possibly cloud layers were present at other altitudes on day 256 and missed by the PVM? This may help to explain the large difference in optical depth as derived from the cloud parameters and the radiation measurements, respectively, and so better use the non-representativeness of the PVM measurements as the explanation for the differences and not the horizontal inhomogeneity by itself? Maybe you can argue in this line that on days with sufficient wind speeds ground-based radiation time series are likely to be (more) representative for larger areas.

Reference list: Webb et al. 2004, in preparation. This paper is already published, I thought?

Technical corrections

Introduction page 2 Spelling error: ..-Guerau de Arellano (not Gureau) The same spelling error is made in 5.2.2 and the reference list.

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Interactive Comment

Table 1. caption. Spelling error: acronyms

Table 2. header Spelling error: acronym missing a space: Timeresolution

Table 3. header missing information: Time given in UTC, sza in degrees.

Section 2.2: 2nd paragraph. Grammar: Albedo measurements ... were....

Section 3: Grammar: ...values in their table 1 are....

Section 4: Grammar: In Fig. 1 is shown examples => In Fig. 1 examples are shown

Figures 1,7,8: I can hardly see the green measurement line which is the most important. I suggest to either remove the model (as the red lines includes the information) or just to exchange the blue and green colors if this helps as well.

Figure 3: The font size of the symbols should be increased. Also, my preference would be not to use colors if it is not functional (also when a journal is fully interactive) => better choose another symbol instead.

Figure 5: Could you decrease the figure size?

Figure 6-15: Could you increase the figure size and symbol sizes if this is in fact needed to present important information? If the editor would object w.r.t. the length of the paper it would be better to reduce in the amount of figures than to present all figures of whicj a significant fraction is not adequately readible.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1421, 2005.