Atmos. Chem. Phys. Discuss., 6, S5700–S5703, 2006
www.atmos-chem-phys-discuss.net/6/S5700/2006/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Improved ozone profile retrievals from GOME data with degradation correction in reflectance" *by* X. Liu et al.

Anonymous Referee #4

Received and published: 23 December 2006

This manuscript presents an approach to correct for the effect of instrument degradation on the GOME reflectance measurements and its impact on the retrieval of ozone profiles for one particular retrieval algorithms. The approach rely on the assumption that the mean reflectance does not change over a given time period and region during the lifetime of the instrument. The fact that this assumption does not depend on a priori knowledge on the atmospheric composition presents a major advantage of the method. I find the subject interesting and suited for publication in ACP after some revisions.

General comments:

1. The underlying assumption that the mean reflectance does not change in time is mention in the paper. I think this assumption has to be motivated. Why is this valid

and under which condition is this valid? Why did the authors choose a given latitude region? 2. The authors assume that only the radiance response of the instrument is changed due to the degradation. However, it is very likely that also the polarization sensitivity is changed. For example, this can be studied looking at a dependence of the correction on solar zenith angle (or better scattering angle for the single scattering geometry). 3. I miss a more extended validation of their approach. The presented validation is only shown for Hohenpeissenberg. This limits the validation to a small set of solar geometries. It would be interesting to see here a more extended validation. For example how does the retrieval perform at higher latitude? Also a timeseries of a validation at particular altitude may help to see if a seasonal dependence is present in the validation.

Specific comments:

Page 8287 line 16-17: I think the surface fit does not present the main drawback of the 'forward model' approach. The surface albedo is used, to the best of my knowledge, in all algorithms as a kind of nuisance parameter. If de degradation could be described by a modified surface reflection, the problem of instrument degradation would not be a point at all for ozone profile retrieval. To my opinion, the disadvantage of the 'forward model' approach is that one assumes that the atmosphere can be characterized with independent measurements. Next to the uncertainties of ozone sonde measurements one has to assume an ozone distribution above the sonde. A combination of different types of measurements, e.g. radiosonde and lidar measurements is not as trivial as it may seam from a first glance. Furthermore this approach does not allow one to distinguish between forward model errors and instrument errors. And last but not least, using ground based measurement to recalibrate our instrument we get a problem when validating the ozone retrieval product with independent measurements. Its advantage is that the 'forward model' approach may allow one to check the calibration of the instrument and not only its change in time. The last point is not possible using the approach presented in the paper. Overall, I miss here a more detailed discussion of

ACPD

6, S5700–S5703, 2006

Interactive Comment

the pro and contras of the different approaches.

Fig 1a: caption: “coadd refers to the average degradation of the for scan positions” Do you average the four individual curves for East, nadir, West, and backscan pixels or do you average the corresponding spectra. I think you have to average the spectra. This is not necessarily the same and may be the reason for the jump at 307 nm in Fig1 c.

Fig1 c: For me those are too many lines for this small figure.

Page 8289 line 12-13: Does the radiance jump at the subchannel boundaries of channel 1 not cause a problem in the retrieval? Or is this spectral range not used in the retrieval.

Page 8289 line 21-23 This point was not clear to me? It has probably something to do that you removed the solar zenith angle dependence from your data due to a least squares fit (line 21-23 p8288). However you also mention at line 21-23 p8288 a season variation which is removed from the data as well. At one geo-location both are correlated, but this is not necessarily true for the whole data set. I think you should be more precise here.

Page 8290 line 21-24: Do you mean that due to a stronger constrain less information is extracted from the measurement and due to that the algorithm is less sensitive to spectra biases? If so, the smaller retrieval bias in the tropics can not be explained by the fact that ‘the on-line degradation correction works better’. This point was not entirely clear to me?

Fig. 4: Why this difference at -60 and + 60 degree latitudes in the TOC but not in TC?

Fig. 5 There is a clear degradation present at the shortwave UV. These wavelengths are particular sensitive to ozone in the upper stratosphere. However, the improvements of the degradation correction mainly occur in the troposphere. I would mention that errors at the longwave UV are of crucial importance for the retrieval due to the low sensitivity of the measurement whereas errors at the shortwave UV are less critical.

ACPD

6, S5700–S5703, 2006

Here the ozone sensitivity of the measurement is high, which means that the same error has a minor effect on the retrieval.

Fig. 7: I would use here two panels next to each other, one without degradation correction on with degradation correction.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8285, 2006.