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Interactive Comment

Interactive comment on "Limb scatter ozone retrieval from 10 to 60 km using a Multiplicative Algebraic Reconstruction Technique" *by* D. A. Degenstein et al.

Anonymous Referee #1

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Review of

Limb scatter ozone retrieval from 10 to 60 km using a multiplicative algebraic reconstruction technique

by D. Degenstein, A. E. Bourassa, C. Z. Roth, and E. J. Llewellyn

General comments:

This is an interesting manuscript describing a technique to retrieve ozone profiles from limb scatter observations. Several similar papers were published in recent years, but the novelty of this approach is that a simultaneous retrieval in the Hartley-Huggins and

Chappuis bands is performed. In general I find the paper very well written and fairly easy to follow, and the agreement with SAGE II ozone measurements between 20 and 50 km is impressive.

In my opinion the paper should be published after the authors have revised the manuscript following the general and specific comments described below. In general I think that several important aspects and diagnostics are missing:

1) Vertical range: It is simply stated that ozone can be retrieved between 10 and 60 km using this approach, but this statement is not backed up in any way. The differences with SAGE II become really large below 20 km and above 50 km, which is of course not necessarily a problem with the OSIRIS retrievals, but one wonders how accurate the retrievals are below 20 and above 50 km. With optimal estimation one can use the averaging kernels to determine the degrees of freedom and the information content at different altitudes. If this cannot be done with MART, I suggest including a retrieval based on synthetic data with an initial guess that differs significantly from the true profile. The relative differences between retrieval and true profile will then allow drawing draw conclusions on the accessible altitude range.

2) Vertical resolution: nothing is said about the vertical resolution of this data product. Does MART allow you to calculate averaging kernels that may be used to establish the vertical resolution of the retrievals? The vertical resolution should be mentioned.

3) Validation: The agreement with SAGE II is indeed impressive. However, I know from my own experience, that very good agreement in a global mean does not necessarily imply, that the agreement is as good for all latitudes or solar zenith angles. I suggest extending the validation section by showing comparisons for different latitude and/or solar zenith angle ranges.

Specific comments:

1) Line 4, Abstract: ' .. the altitude range from 10 to 60 km ..'

ACPD

8, S5273–S5277, 2008

Interactive Comment

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Interactive Discussion

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It is certainly not true that the entire range is accessible for all conditions, e.g. in the presence of clouds. This statement should be corrected, and adjusted to the outcome of the sensitivity study described in general comment 1).

2) Line 59: ' .. and that at the Chappuis wavelengths, IT IS still optically thin down to below 10 km.'

It's not clear what 'IT' is. You certainly mean that the atmosphere (TP to instrument) is optically thin, but the sentence doesn't say this. Please clarify.

Furthermore, I don't think the intended statement is really true. Even for the longest triplet wavelength the LOS optical thickness will be close to 1 near 10 km, and for the center triplet wavelength it is 1 typically at 20 km. If we use tau < 1 (or rather tau $<< 1$?) as the definition of optically thin, then the atmosphere is not optically thin any more.

3) Lines 65 - 70: Perhaps I missed it, but I think the normalization TH for the 351 nm radiance profile is not mentioned in the paper. This should be done. I assume it is be the same as for the absorbing wavelength of each pair? One aspect that concerns me is the possibility that baffle straylight contamination (which will likely be non-negligible at 351 nm and upper stratospheric and mesospheric THs) will bias the UV pairs, and lead to systematic errors. Given your excellent validation results, this does not appear to be a problem below 50 km or so. But the strong deviations from the SAGE II profiles above 50 km may be related to baffle scattering contamination of the UV non-absorbing wavelength (see also point 8).

4) Line 87: 'An important feature of the SaskMART algorithm is that more than one measurement vector element can be used to retrieve the state parameter at any altitude.'

I'm not sure if you intent suggesting that this is not a feature of other retrieval approaches, e.g., optimal estimation. I think most of the other techniques also have this feature.

5) Line 221: ’Typically the spacing ranges from 1.0 km at low tangent altitude to 2.5 km at upper tangent altitudes.’

This also implies that the vertical resolution depends on altitude. Can you describe what the actual vertical resolution is (as a function of altitude)? I don’t think this is mentioned in the paper.

6) Lines 233 - 236: Did you use the stratospheric aerosol and NO_2 profiles retrieved from every individual limb measurement for the ozone profile retrieval from this measurement? Or perhaps latitude-dependent monthly means?

7) Line 261: ’Coincidences in the tropics do not occur with these tight criteria’

This does not appear to be true given the coincidence at 15S shown in Fig. 5

8) Section 5: Results: SAGE II Comparison

The really large differences above 50 km are not discussed at all. They probably approach -35 percent or even more at 60 km. This must be addressed in some way, particularly considering that you state that ozone can be retrieved up to 60 km with this technique. A possible explanation of the discrepancy may be the diurnal variation of ozone, which becomes more important above 50 km.

I think this discrepancy between OSIRIS and SAGE II may also be due to baffle scattering affecting the 351 nm limb radiance profile (see also the previous point 3). If the 351 nm radiance profile is also normalized at the reference TH of the paired absorbing wavelength (i.e., 65 km for 292 nm and 302 nm), which will likely be contaminated by baffle straylight, then all values of the normalized radiance profile at 351 nm will be affected by a low bias. If you then divide by this, you will get a high bias in the paired profile. More radiance means less ozone, roughly speaking, i.e. this effect will lead to a low bias, which is qualitatively consistent with the SAGE II comparisons. I urge the authors to check whether this is, what’s happening here.

9) Lines 296 - 299: ’The application of the technique to the measurements made by

ACPD

8, S5273–S5277, 2008

Interactive Comment

the OSIRIS instrument demonstrates agreement with coincident SAGE II occultation measurements to within 2% from 18 to 53 km altitude over a large range of geolocations and solar zenith angles.'

I don't think this statement is really backed up by the paper. You only show a global average over all collocations. These indeed cover a large range of geolocations and solar zenith angles, but your statement suggests that the agreement is as good for all different geolocations and solar zenith angles. As mentioned above, I suggest including more detailed validation results that include different latitude bins, SZA bins etc..

10) Lines 301 - 302: Can you include the link to this OSIRIS data website?

Typos:

Line 173: 'altitude' should read 'altitudeS'

Line 286: 'instrument' should read 'instrumentS'

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8, S5273–S5277, 2008

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