Atmos. Chem. Phys. Discuss., 4, S3593–S3597, 2004
www.atmos-chem-phys.org/acpd/4/S3593/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Balloon-borne Limb profiling of UV/vis skylight radiances, O_3, NO_2 and BrO: technical set-up and validation of the method" *by* F. Weidner et al.

Anonymous Referee #1

Received and published: 10 February 2005

1) Does the paper address relevant scientific questions within the scope of ACP?

Yes.

2) Does the paper present novel concepts, ideas, tools, or data?

Yes.

3) Are substantial conclusions reached?

Yes.

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4) Are the scientific methods and assumptions valid and clearly outlined?

No, see below.

5) Are the results sufficient to support the interpretations and conclusions?

see below

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Not really – no description of profiling technique and hard to get information on RT code.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

As far as I can tell, yes.

8) Does the title clearly reflect the contents of the paper?

Somewhat – see comments regarding the word “validation”.

9) Does the abstract provide a concise and complete summary?

Yes.

10) Is the overall presentation well structured and clear?

Yes, but additional information in the figures would make life easier. See below.

11) Is the language fluent and precise?

Yes

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes

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13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

No

14) Are the number and quality of references appropriate?

Yes

15) Is the amount and quality of supplementary material appropriate?

N/A

Overall

My overall impression is that this is potentially an interesting and valuable paper but that it needs considerable work.

I see three major issues that need to be addressed:

1. "Validation of the method", from the title. I do not think a method can be validated with just a description and a couple of favourable comparisons, particularly since the limb scatter technique is so new. Furthermore, there is no description of the method! There appears to be a total of three sentences describing the method – completely unacceptable. That is, the method of retrieving profiles involves spectral fitting to obtain SCDs which is fairly standard and there is some discussion of this. However, the inversion of these to obtain profiles is new and difficult and to requires a thorough discussion of the technique, the assumptions, uncertainties, etc... Getting back to the title, I suggest changing the title to "...set-up and description of the method" or "...set-up, description of the method, and sample applications"

2. Essential to retrievals using limb scattered light is the RT code. There were a couple of unnerving characteristics about the Tracey model – see points below. The only reference to it was a PhD thesis and it was not readily found on the internet; I suggest making the salient parts of it accessible (and give a link). I would strongly encourage

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the authors to make detailed comparisons with a proven limb RT model to diagnose some of these discrepancies.

3. Uncertainties and Error bars. Where do these come from? Figures 2, 3, 7, 9, 10, 11, 12 all show error bars on the observations and/or model calculations yet there is no mention of them anywhere.

Other points

- For NO_2 and BrO how are diurnal effects handled; that is, while they are minimized along the LOS due to the viewing geometry, the fact that the observations are made for SZA$\cong$90° means the incoming light has passed through air at smaller SZAs and hence probably less NO_2 and more BrO. Is this accounted for and if not what are the potential errors?

- BoxAMF: AMFs usually imply a RT calculation at a single wavelength, is this the case here and if so it is dangerous to do so over a 50 nm fitting window (as for NO_2) due to non-linearities in the growth of the optical depths as a function of tangent height/altitude. I would suggest you forward simulate the entire fitting window at some reduced resolution.

- Nadir: it is mentioned several times but no results are shown

- Page 7642, line 7–8: I have not heard before of a substantial amount of stratospheric aerosol in the 0.01 mm size range. At 360 nm, the size parameter is less then 0.2 which makes these Rayleigh-like and you would need a huge number of then to substantially alter the radiance. (Checking out the Hirsekorn reference it seems that the impact of these small scatterers Overall I would argue that this is an extra-ordinary claim and you need to base your ascertain on refereed literature.) In any event, with the observation point at 12 km adding extra aerosol should lead to a decrease in limb radiance so I don't think this argument is even consistent. Also, should the larger difference not be at 490 nm then due to the larger relative role of aerosol scattering? Finally, why not try

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adding a small aerosol to the simulation just to see what happens.

- Page 7642, line 19–20: If you are using a fully spherical, ray-tracing model the 1/cos(SZA) problem should not be an issue, and if they are you need to further explain, and would this not propagate into the retrieved profiles? Furthermore, the largest differences occur around 89.5° and agreement at 90° seems good. This does not seem to make sense.

- Figure captions: heights should be in metres, not km.

- Figure 2: it would be useful to attach some SZAs to the altitudes

- Page 7645, line 21: The fraction of multiple-scattered photons that is simulated, 5%, is too small. See, e.g. Oikarinen et al., JGR, 104, 31 261, 1999. Depending on SZA, wavelength, etc..., I would say a minimum of 15% and more likely 20–25%. The fact that you are floating at 32 km and the Oikarinen calculations were for a space-based instrument is not will impact this ratio only marginally.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7631, 2004.