

***Interactive comment on* “Column ozone and aerosol optical properties retrieved from direct solar irradiance measurements during SOLVE II” by W. H. Swartz et al.**

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Major comment:

This work shows great success in retrieving ozone from direct solar measurements, without having to use Differential Optical Absorption Spectroscopy (DOAS). In DOAS (despite its name) spectra and cross sections are high-pass filtered in wavelength (not differentiated), which eliminates calibration constants and spectrally smooth scatterers such as air molecules and aerosol. It is very easy to retrieve ozone from solar measurements by DOAS, much more difficult by the absolute calibrated spectroscopy

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described here.

But nowhere do the authors say why they embarked on this much more difficult task. Absolute spectroscopy is obviously almost essential for aerosol retrieval, but why try it for ozone?

Rather than the current failure to mention DOAS, a complete work would justify this otherwise strange choice. If the justification is “to see if it was possible with useful accuracy” then that is an entirely acceptable rationale, to which could be added “then the reader might have faith in the accuracy of our aerosol retrieval, for which we have no independent validation unlike ozone”.

Some discussion on rationale is essential.

Minor comments:

1. There is a lot of DOAS history that is ignored, particularly in the Introduction. This is essential background to show the pedigree of the work.
2. By using O<sub>2</sub>O<sub>2</sub>, instead of the O<sub>4</sub> used by everyone else in the subject, the less well-informed reader may think you are referring to something other than O<sub>4</sub>. This is dangerous, despite the strict accuracy of O<sub>2</sub>O<sub>2</sub>. If you were making a specific point about it being a very short-lived molecule so that its lines were inherently smooth and wide by the Uncertainty Principle, so a specific DOAS analysis could be used, then it would be understandable. But you are not.
3. Section 3.2.2 partially ascribes deviations between measurements and calculations at short wavelengths to “instrument red leak”. If by this you mean scattered light at longer wavelengths then it cannot be, as you have just said this is less than 1e-9. If not, please explain the phrase.
4. Section 3.2.2 partially ascribes deviations between measurements and calculations at short wavelengths to “tropospheric clouds”. If by this you mean that the scattering efficiency of clouds fitted at visible wavelengths differs to that in the UV, that is

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almost certainly wrong - clouds are grey scatterers. If you mean that the path length of Rayleigh scattering is increased by the multiple scattering within a cloud, this exposes a difficulty with specifying density in your scheme, perhaps a free parameter would be better. If you mean that the path length aerosol scattering or ozone absorption is increased, these would be equal at all wavelengths so would simply look like more aerosol or ozone.

5. Section 3.2.2 states that the UV part of the spectrum is largely ignored because of its large noise. But this misses a large part of the information content about ozone - the noise may be 10 times larger in the UV, but so are the ozone cross sections. And this is not consistent with the statement in paragraph 1 of Section 4, about O<sub>3</sub>(320 nm) and O<sub>3</sub>(600nm) - please clarify.

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Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7403, 2004.

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