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Atmospheric Measurement Techniques
Discussions

Interactive comment on "A new differential absorption lidar to measure sub-hourly fluctuation of tropospheric ozone profiles in the Baltimore–Washington DC region" *by* J. T. Sullivan et al.

Anonymous Referee #1

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This paper describes extensively a new lidar system that has been set up for the measurement of tropospheric ozone profile in the Washington DC area. It addresses a new valuable measurement system to monitor tropospheric ozone, although the Differential Absorption lidar technique used here is not particularly new and other similar systems have been implemented around the world since the eighties. I recommend publication in AMT, provided that important comments for improvement are taken into account.

Main comments and suggestions

1. As already mentioned, the DIAL technique used in this paper is not new and similar systems based on the same technique are currently operated in other part of the world. Although some previous literature is cited (e.g. Megie et al. 1985; several Browell et al. publications), the paper does not sufficiently acknowledge previous work on tropospheric ozone lidar measurements, e.g. regarding the optimum choice of wavelength for tropospheric ozone lidars, the previous use of dye lasers or the optimization of the Raman cell gas mixture.

2. Similarly, the authors do not mentions lidar systems in parts of the world other then the US, although such lidars exist in Europe (e.g. in Bavaria and Southern France) or in Japan. The most striking example of this lack of reference to previous work is page 4326 line 12, where it is mentioned that the described system is the only one using SRS cells for wavelength generation aside from another system at Jet Propulsion Laboratory. A system using similar wavelengths has been operated for 20 years at Haute Provence Observatory in France (Ancellet et al., JAOT 1989). In Japan, the AMT paper of Uchino et al. (2014) also describes a system based on the Raman shifting technique. In general, the references strongly focus on the lidar work done in the US, while similar work in other part of the world is neglected.

3. I strongly recommend the use of the International System of Units, especially in the section devoted to the optimization of the Raman cell gas mixture. The use of psi prevents comparison with other published results.

4. The complete development of the lidar equation for ozone measurements in section 2.1 is not necessary. This has been mentioned in a lot of publications dating back in the eighties, which could just be cited here.

5. Only one measurement profile is shown in the paper (Figure 5), a part from the measurement time series in Figure9. For clarity, the figure should display the ozone retrieval from the various channels of the receiving system. An estimate of the bias with respect to ozone sonde measurements should be given.

6. Also for clarity, I recommend to display in a figure the various error sources of the measurements (including aerosols and other absorbers error sources as well as the error due to photon noise) and the vertical resolution as a function of altitude.

Specific comments Page 4322, abstract: It is not necessary to explain the DIAL technique in the abstract (lines 12-18).

Page 4324, l12: Tropospheric ozone profile measurements from the IASI instrument should be mentioned.

Page 4326, l14: Here again reference to important publications from Europe on the performance of ECC ozone sonde uncertainty is missing, e.g. Smit et al., JGR 2007.

Page 4326, l25: 1st DIAL ozone measurements were made in 1977 (Megie et al. Nature, 1977). For stratospheric ozone trend measurements, records exist in other stations then in the US (e.g. Godin et al., GRL 1989; Steinbrecht et al., JGR 2006).

Page 4327, l26: what is the difference between routine monitoring and continuous monitoring?

Page 4328, l16: Cite previous reference on the optimization of tropospheric DIAL ozone measurements.

Page 4329, l18: it sounds strange to cite a reference for the lidar equation prior the invention of the lidar and the laser!

Page 4329, l22: from equation 1, P(r) is the energy and not the power. The lidar signal depends on the wavelength, so it should be added in the equation as an index.

Page 4329, l23: the receiver efficiency and background signal also depend on the wavelength.

Page 4330, l9: the extinction coefficient cannot be considered as absorption coefficient only. What about Rayleigh and Mie scattering?

Page 4332, l19: provide earlier reference for dye laser sources used for DIAL measurements (e.g. Pelon and Megie, JGR, 1982)

Page 4334, l21: There are other references for the optimization of the Raman cells for DIAL ozone measurements, e.g. Papayannis et al., Appl. Opt. 1990; Ancellet and Ravetta, Appl. Opt. 1998; de Schoulepnikoff, App. Opt., 1997; Heese et al., JGR, 2001.

Page 4339, l24: It is not the right reference for dead time correction. A better reference is Donovan et al., Appl. Opt., 1993.

Page 4342, l1: A reference is necessary for the assumed value of the lidar ratio.

Page 4347, equation 16 is incorrect: the error on the ozone number density is the quadratic sum of the error of both lidar signals.

Page 4348, l15: the estimation of the error linked to NO2 and SO2 absorption should be better explained. Why only one value is given and not a range of values? The error depends on the assumption made on NO2 and SO2 profiles.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 4321, 2014.