

In their manuscript AMT-2017-391 titled “Airborne Lidar Measurements of Aerosol and Ozone Above the Canadian Oil Sands Region” Aggarwal et al. describe observations of ozone and aerosol concentrations made in summer 2013 with an airborne lidar over the Canadian oil sands region in northern Alberta. Their main finding is the lack of any ambient ozone enhancement downwind of the oil sands extraction and processing plants even though they represent a significant pollution source. This is an important finding that is contrary to many other studies that have found increased ozone concentrations downwind of urban areas or power plants. The only ozone enhancement the authors observed was in a forest fire plume that had been lofted above the boundary layer. In addition to the lidar observations, the authors use in situ measurements from another aircraft, observations from a ground-based aerosol and depolarization lidar, and HYSPLIT back trajectory analyses to support their conclusions. In the first part of the paper, the authors describe the lidar instrument and the ozone and aerosol retrieval technique, including an innovative approach to compute the differential aerosol backscatter and extinction correction terms making use of in situ aerosol size distribution and speciation measurements. Proper correction of the aerosol effects in the ozone lidar retrieval is critical because of the high aerosol concentrations that were observed above and downwind of the oil sands processing plants. The manuscript is written very clearly and the authors’ reasoning is easy to follow. The conclusions presented in the manuscript are supported by the data and the figures and tables are all necessary, legible, and properly annotated. The topic of the paper fits well within the scope of AMT.

I recommend publication after minor revisions.

Specific comments:

page 5, lines 10 and 17:

UV signals were vertically smoothed over 45 m. The retrieved ozone profiles (Fig. 3b) appear to be reported at a much higher resolution. Error bars are given roughly every 100 m. Is that the effective resolution of the ozone profile observations (i.e. the separation of truly independent data points)? How did the authors compute the derivative of the logarithmic signal ratio? Least-square linear fit over multiple adjacent data points or Savitzky-Golay method? How were the two partial ozone profiles (AN 276/299 and PC 266/299) shown in Fig. 3b combined into one profile? Weighted averaging in the altitude region where both profiles overlap?

page 5, line 18:

What is the source of the air number density profile to convert ozone number density to mixing ratio? Ancillary pressure/temperature profile observations (e.g. nearby radiosondes)? Reanalysis data? Standard air number density profile?

page 6, line 2:

The authors need to describe briefly how their aerosol correction technique differs from other, previously published approaches. Also, include several references, e.g.:

Alvarez et al, 1998 (already in reference list)

Alvarez II, R. J., C. J. Senff, A. O. Langford, A. M. Weickmann, D. C. Law, J. L. Machol, D. A. Merritt, R. D. Marchbanks, S. P. Sandberg, W. A. Brewer, R. M. Hardesty, R. M. Banta, 2011: Development and application of a compact, tunable, solid-state airborne ozone lidar system for boundary layer profiling, *J. Atmos. Oceanic Technol.*, doi: 10.1175/JTECH-D-10-05044.1.

Browell, E. V., S. Ismail, and S. T. Shipley, 1985: Ultraviolet DIAL measurements of O₃ profiles in regions of spatially inhomogeneous aerosols, *Appl. Opt.*, **24**, 2827-2836.

Eisele and Trickl, 2005 (already in reference list)

page 6, line 20:

What reference height for aerosol extinction calibration do the authors typically use? Is it an altitude close to the aircraft (it appears that way from the aerosol extinction time/height plots, e.g. Fig 7a)?

page 7, line 1:

Only the aerosol particle refractive index is needed to compute Q_{ext} and Q_{back} from Mie theory. I don't understand why aerosol size distribution measurements are needed to calculate the efficiencies?

page 9, line 5:

"As reported in the literature, the aerosol correction is small (< 3 ppbv) ..."

page 9, line 26:

"... found in the literature was 5 ppbv ...

page 10, lines 4-15:

I double-checked the SO₂ interference estimates using the Brion et al. (1992-1998) O₃ and Vandaele Hermans, and Fally (2009) SO₂ absorption cross section data. I got interference terms of approximately 1, 5, and 35%*SO₂ concentration for the 266/299, 276/299, and 287/299 wavelength pairs, respectively. The 1% interference that I computed for the 266/299 pair would result in an ozone bias of up to 1.5 ppbv (larger than the 0.3 ppbv bias stated by the authors, but still quite small). Obviously, the magnitude of the interference terms depends on the choice of absorption cross section data. The SO₂ data in particular seem to vary quite a bit between the different published data sets. The authors need to state their sources of O₃ and SO₂ absorption cross section data and provide an error estimate of the interference terms due to absorption cross section data uncertainty.

The authors stated on page 5 that the 276/299 pair was used for O₃ profile retrieval to 1.8 km below the aircraft or about 1.1 km ASL (Fig. 3). On some flights, the boundary layer (with potential SO₂ concentrations of up to 30 – 150 ppbv) reached about 2 km ASL (Figs. 7a and 9a). Therefore, the 276/299 pair with its higher SO₂ sensitivity may have been used to retrieve the O₃ profile in the upper part of the boundary layer, which could have led to biases of up to several ppbv. Please clarify.

page 11, lines 11-12:

“... corrected by **fitting an** exponential decay function to ..., where there **should** be no real optical signal.”

page 12, line 25:

“... (distances from **40** to 60 km ...”

page 13, lines 19-20:

“There is no evidence for increasing O₃ **for up to 10 hours downwind of the oil sands industrial areas.**”

page 15, line 1:

“**depolarization ratio**), ...”

page 15, lines 11-12:

“... reveal a more **spherical shape** of forest fire particles.”

page 15, lines 22-23:

“... and in situ measurements **with both aircraft sampling the same volume of air was not possible.**”

page 17, line 10:

“... the decreased ozone abundance.”

page 18, line 15:

Jacob et al., 2009 not in reference list

page 18, line 26:

“... result **of** meteorological conditions ...”

page 18, lines 25-28:

Perhaps the absence of enhanced ozone downwind was at least in part due to low concentrations of suitable VOCs? Did the Convair aircraft measure VOCs?

References

Omit the following references since they are not mentioned in the text:

Angle et al, Calfapietra et al, Chu et al, Geddes et al, Haman et al., Ismail et al., Langford et al, 2011, Metha et al, Mie scattering source code, Permadi et al, Shephard et al, Wang et al, Yap et al

Page 25, line 21:

“..., 2010**b**.”

Langford et al., 2010a, and Langford et al., 2010b are not in alphabetical order.

Figures

Fig. 4:

Change figure labeling: (b) Uncorrected, (c)-(e) Molecular (in black), Aerosol + Molecular (in red). Was the PC 266/299 pair not used in this case?

Fig. 5: Indicate location of main emission sources.

Fig. 12a: Near 15 km and between 60 and 90 km distance, the extinction coefficient is near 0 within the boundary layer. What caused this? Aerosol fluctuations in the reference region?