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Interactive comment on "Vertical variations of aerosols and the effects responded to the emission control: application of lidar ceilometer in Beijing during APEC, 2014" *by* G. Tang et al.

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In their paper "Vertical variations of aerosols and the effects responded to the emission control: application of lidar ceilometer in Beijing during APEC, 2014" Tang et al. investigate the effects of emission reduction strategies on air quality during APEC 2014. A key instrument for their assessment is a CL51 ceilometer that is used to determine the vertical distribution of aerosol particles.

I don't want to comment on the paper in total, however, it seems to be necessary to use more precise wording with respect to aerosol remote sensing in the manuscript.

It was recently shown (Wiegner and Gasteiger, 2015) that measurements in the spectral region of 905 - 910 nm are influenced by water vapor absorption, and how signals can be corrected. The strength of the influence depends - among others - on the water vapor concentration, that can be highly variable in time and space. As the CL 51 ceilometer operates at 910 nm it is not possible to determine aerosol properties without taking into account the water vapor effect. For this reason the authors should not use terms as "aerosol attenuated backscattering coefficient profile" (e.g. 13174/9, 13178/24, 13182/19, 13190/27) or "vertical aerosol profile" (13177/23). Moreover, "aerosol concentration" (13177/28) is strongly misleading as only the particle backscatter coefficient (see Wiegner et al., 2014) as an optical property can be derived provided the signal has been calibrated. The derivation of microphysical properties as the concentration (i.e. number density) is not possible.

For the derivation of the mixing layer height it is not necessary to determine the particle backscatter coefficient or the particle concentration; consideration of the (range corrected) signal or the total attenuated backscatter (i.e., consideration of the transmission of air molecules, particles, and water vapor) is sufficient in most cases. The reason is that typically at the top of the mixing layer the aerosol concentration drops significantly leading to a strong decrease of the ceilometer signal, that is not masked by water vapor absorption. Thus, the main conclusions of the paper are expected to remain unchanged, but for the sake of clarity rephrasing is essential.

Another issue is the "validation". As discussed in Wiegner et al. (2014) it is not possible to determine the particle extinction coefficient from data of a simple backscatter lidar or ceilometer. Thus, comparisons with independently determined AOD (here: microtops) suffer from the unknown lidar ratio. A good correlation between AOD and integrated backscatter (but see comments above!) can be plausible but cannot serve as validation. By the way it is not clear what is meant by "AOD concentration" (13183/5).

Finally, I want to mention that the abstract seems to be too long, and that in contrast to the text (13183/13) "variations" cannot be seen in Fig. 5. This figure should be

explained in more details as it is surprising that such a smooth signal is obtained as an average of measurements: it resembles simulated idealized signals (see also Fig. 6a).

References:

Wiegner, M., Madonna, F., Binietoglou, I., Forkel, R., Gasteiger, J., Geiß, A., Pappalardo, G., Schäfer, K., and Thomas, W. (2014), What is the benefit of ceilometers for aerosol remote sensing? An answer from EARLINET, Atmos. Meas. Tech., 7, 1979–1997, doi:10.5194/amt-7-1979-2014, 2014.

Wiegner, M. and Gasteiger, J. (2015): Correction of water vapor absorption for aerosol remote sensing with ceilometers, Atmos. Meas. Tech., 8, 3971–3984, doi:10.5194/amt-8-3971-2015, 2015.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 13173, 2015.