

Interactive comment on “A subsiding regional forest fire aerosol layer at Whistler, BC: implications for interpretation of mountaintop chemistry observations” by I. G. Mc Kendry et al.

Anonymous Referee #2

Received and published: 20 October 2010

General comments

This manuscript describes the use of in-situ observations together with passive and active remote-sensing measurements in investigating a regional-scale forest-fire smoke layer. This study details the importance of placing individual measurements into context to understand the evolution of such a smoke layer over time and highlights the impact of including lidar data in the analysis.

The comprehensive in-situ measurements are described succinctly, however it may be that an attempt is being made to infer too much from the lidar data. The ambiguity arising from having only a 2D (time-height) slice through a 4-D (volume-time) evolving

C8892

situation (especially in mountainous regions) has already been noted by a previous comment on this manuscript.

What the lidar data certainly does provide unambiguously is the times when the smoke layer interacts with the boundary layer, which can then explain the difference in the timeseries for the surface measurements. I feel that this is in fact the major advantage of combining the lidar remote sensing with the surface-based in-situ measurements; rather than trying to extrapolate further, possibly incorrectly, to attempt to explain what is causing the intersection of the smoke layer and the boundary layer without additional evidence.

I believe this manuscript is suitable for publication, but could be improved by considering the following points detailed below.

Specific comments

No units for the lidar or ceilometer time-height plots (Figs. 5A and 5B) make it difficult to make a direct comparison between the two instruments. I assume that one is a plot of backscatter ratio (CORALNet lidar) and one is proportional to attenuated backscatter coefficient (ceilometer). Calibration of the ceilometer is reasonably straightforward (although it is not possible to use the molecular return), typical calibration scale-factors for the CL31 lie between 1 and $2e-8$ to convert the raw integer data to $[sr^{-1} m^{-1}]$. After calibration, direct comparison of the ceilometer and lidar data would then be possible if the two panels were converted to the same measurement, either backscatter ratio, or attenuated backscatter coefficient.

One premise of this manuscript appears to be that subsidence is of overriding importance in the interpretation of mountain top chemistry. However, since it is difficult to attribute the apparent lowering of the smoke layer unambiguously to subsidence, maybe it is more appropriate to stress the importance of detecting the interaction of elevated free-tropospheric smoke layers with the boundary layer and the subsequent effect this has on measurements taken at the surface. The diurnal evolution of the boundary layer is clear in the lidar plot, with the convective well-mixed boundary layer

C8893

probably reaching close to 1 km in altitude by midday on all three days. This is similar to the level of the mountain top station, and, as stated in the manuscript, any additional oscillation of the air within the valley basins could then allow it to mix with the elevated smoke layer, thus bringing smoke particles down to the surface.

The CORALNet lidar systems can provide depolarization ratio at 532 nm. The hygroscopic aerosol in the boundary layer usually has a low depolarization ratio because it is predominantly spherical, typically $< 3\%$. In contrast, the smoke particles aloft are often observed to have a slightly higher depolarization ratio, 5-8%. Cite e.g. Murayama, T., Müller, D., Wada, K., Shimizu, A., Sekiguchi M., and Tsukamoto, T.: Characterisation of Asian dust and Siberian smoke with multi-wavelength Raman lidar over Tokyo, Japan in spring 2003, *Geophys. Res. Lett.*, 31, L23103, doi:10.29/2004GL021105, 2004. One question that would be of interest to the scientific community, and could be addressed in this manuscript is the following: is the depolarization ratio alone able to provide evidence of smoke? Or is the color ratio (or Ångström exponent) between 532 and 1064 nm required as well? If the corresponding time-height plot of depolarization ratio at 532 nm (using a color scale from 0-15%) could be added to Figure 5, this would help answer this question. Potentially, the interaction of the smoke layer with the boundary layer could also be seen in the depolarization field, as the effects of mixing/dispersion take place.

Technical corrections

In Abstract line 1 and P 20306 line 3, spell out the first use of British Columbia (BC) for those not familiar with the continent.

No need for lidar to be in italics.

P 20307 line 7. Error in citation for Räsänen, Lönnqvist and Piironen (2000). The problem with some letters which should have diacritical marks not being displayed is probably a formatting issue as this occurs elsewhere in the manuscript (References - P 20317 line 27, P 20319 line 10 and line 22).

P 20310 line 4, almucanter should be almucantar.

C8894

P 20321 Table 1, use either Angstrom or Ångström.

Figure 5 (A) There are no units for the color scale. I assume, therefore, that this is a plot of backscatter ratio. This should be stated in the caption.

Figure 5 (B) Again there are no units for the color scale. Since this is from a ceilometer, I suspect that the measurement is of attenuated backscatter coefficient.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 10, 20303, 2010.

C8895