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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Investigating emission sources and transport of aerosols in Siberia using airborne and spaceborne LIDAR measurements" *by* Antonin Zabukovec et al.

Anonymous Referee #1

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Siberia covers a large area in Asia and little is known about the aerosols above Siberia. Siberian forest fire plumes are transported over large distances. Therefore, it is important to study the optical properties of smoke close to the source and after long-range transport and as well industrial pollution. Airborne lidar measurements offer the great possibility to study various scenarios with different aerosol types. However, the methods used in this paper do not reflect the state-of-the-art approaches and the contribution to the scientific advance is little. Therefore, I have to reject the current version of the manuscript and encourage the authors to stress other results from their campaign.

The reasons for the rejection are the following:

1. The extinction-to-backscatter ratio (lidar ratio) of different aerosol types is one of the main products of this study. However, it is not measured directly, but retrieved using the backscatter coefficient from the airborne lidar and the aerosol optical depth (AOD) from the satellite (MODIS). State-of-the-art would be to use a high spectral resolution lidar (HSRL) which performs direct measurements of the aerosol extinction coefficient profile and can be flown on an aircraft. A ground-based Raman lidar offers another state-of-the-art direct measurement of the extinction coefficient and provides vertical profiles of the lidar ratio. Even a ground-based backscatter lidar with a collocated sun photometer to constrain the extinction coefficient using the AOD would offer more accurate results. Therefore, the scientific advance by this indirect retrieval of the lidar ratio is little. And as the lidar ratio is not just a side product but the main product of the aerosol characterization in this study, it is not sufficiently well constrained.

2. The comparison of the airborne observations with the spaceborne CALIOP measurements is highly uncertain. The temporal and spatial distance is too large to draw valid conclusions. An aerosol plume 500 km away contains not necessarily the same aerosol. In the case of the shortest distance (105 km) the satellite passed more than two days (57 hours) prior to the aircraft over the area. In the last 14 years several much better comparisons have been presented. There are studies using ground-based lidar systems with a spatial distance < 100 km or airborne lidar systems flown along the track of CALIPSO. The flight patterns of the campaigns are not designed for a good comparison with CALIPSO. Therefore, the conclusions drawn with respect to CALIOP might be correct but are not based on a convincing comparison.

3. Additionally, a state-of-the-art aerosol characterization includes a proper measurement of the depolarization ratio (see Burton et al., 2012, or Groß et al., 2013). The depolarization ratio conveys important information about the particle shape and is therefore a key parameter in the aerosol classification presented in Burton et al., 2013, to which the authors refer in the manuscript. I wonder how the authors assign one of Burton's aerosol types in Tab. 1 to their observations by using just one out of the four

parameters used to separate different aerosol types. This is a very rough estimate and does not represent the state-of-the-art.

4. Two out of the six cases presented have significant ambiguities in the current version of the manuscript. Case 2 (Ob Valley gas flaring emissions) is a multilayer scenario. In Fig. 8b a CALIOP measurement is presented showing a dust layer above the Ob Valley emissions. The airborne lidar measurement does not capture this layer and it is not clear, if the dust layer is present above the aircraft. In that case the MODIS AOD is biased by this second layer and can not constrain the lidar ratio of the backscatter lidar. For case 6 (Long-range transport of Northern China emissions) the source appointment is not very convincing. The FLEXPART backward simulation (Fig. 18a) shows a large residence time in the area south of Yakutsk. It is possible that the aerosol originates from Harbin in Northern China, but Fig. 18a suggests an origin between lake Baikal and Yakutsk with a potential emission sensitivity of around 2000 s (shown in yellow). This is not discussed at all. The second ambiguity in this case is the top height of these aerosol layers. From the manuscript it can not be decided, if there are more aerosol layers above 5 km height. In the case of further layers above the aircraft, the extinction can not be constrained by the MODIS AOD.

Minor remarks:

i. Please use always the year when writing the date to avoid ambiguities (especially in Sec. 5).

ii. Comment on the speed of the aircraft and the translation from the temporal resolution of the lidar measurements to a horizontal resolution of the measurements.

iii. Looking at Fig. 4, 7, 10, 15, 18 the pink line in Fig. (b) shows less than the PES > 2000 s, according to the color scale in Fig. (a), especially in Fig. 18 where it is supposed to show PES > 1000 s.

iv. L344/345 “Since the lidar ratio is generally 10 sr higher at 355 nm than at 532 nm”

This is simply not true.

It is a challenging task to characterize well the different aerosol types without direct measurements of the lidar ratio (HSRL) or the depolarization ratio. However, Siberia is an interesting region and the effort put into this work should be appreciated. The method to determine the lidar ratios and the CALIOP comparison is not convincing and not state-of-the-art. However, I would encourage the authors to focus on different aspects of their campaign: The differences between fresh and aged forest fire smoke or between gas flaring emissions and urban pollution. Also, a stronger focus could be put on the comparison between the lidar and the in situ measurements.

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