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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Aerosol monitoring in Siberia using an 808 nm automatic compact lidar" *by* Gerard Ancellet et al.

Anonymous Referee #2

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The paper deals with the analysis of a micropulse lidar measurements at 808 nm near the city of Tomsk (Siberia) for the period April 2015 – September 2016. The papers uses an hybrid methodology of air-mass analysis and aerosol sources to inferr aerosol lidar ratio. The model used for air-mass backward computation is the well-known FLEXPART model. Authors evaluate the consistency or their methodology using MODIS and CALIPSO spaceborne measurements for three particular months.

The study of aerosol vertical distribution is very important generally and particularly for Siberia where there are not many studies, as the authors brilliantly highlight in the introduction section. The topic is sound and of interest for its publication in Atmospheric measurement Techniques. However, the current paper need further revisions before its final publication in Atmospheric Measurement Techniques.

MAJOR CONCERNS

FLEXPART is mainly developed for the analysis of air-masses, both in backward and forward models. The authors propose FLEXPART use in combination with hyphotesis of aerosol sources over different regions. However, I wonder why not using more sophisticated models that already include aerosol modules. An example could be the NASA Goddard Earth Observing System Model, Version 5 (GEOS-5, https://gmao.gsfc.nasa.gov/GEOS/) or other from ECWMF. These model already include aerosol emissions and depositions.

It is not clear the novelty proposed about the analysis of micropulse lidar data. Actually, authors claim in the conclusion section that they analyze extinction and backscattering measurements, which is not true because a micropulse system needs the assumption of an aerosol lidar ratio. This give a general ambiguety to the scientific discussion. The authors must clarify the novelty they propose. For example, when FLEXPART is used for obtaining lidar ratios, Are consistent with the cases when sun-photometer are also available? Also, nighttime retrievals of lidar ratio are not clear. Appendices 'A' and 'B' do not provide any novelty to what is already known in lidar aerosol optical depth retrieval and in CALIOP depolarization ratio computation. Finally, the evaluation or the new methodology would need of correlative measurements of Raman/HSRL systems. If these measurements are not available, at least reference them in the text.

The authors selected three study cases and three months to present their analysis and the links with satellite observations. But to me it is not clear why these cases are representative.

MINOR CONCERNS

Pag. 9: Authors say that Russia emissions are not well-known but they use ECLIPSEv4 dataset for emissions. That seems a contradiction. Please clarify

Pag. 2, Line 13: EARLINET network posses more sophisticated instruments such as

Raman lidar, which provide further information on aerosol vertical-profiles. The authors should include this in the introduction. Also, there are many measurements of Raman lidars in North America, Asia and Latin America. The authors should not ignore that.

Pag. 2, Line 15: Please define what is CIF.

Pag. 2, Line 26: The best estimates of Angström exponent are provided by MISR satellite, not by MODIS. Please correct.

Pag. 2, Lines 29-30: CALIPSO does not provide direct estimates of aerosol extinction. CALIPSO is backscattering lidar. Please correct.

Pag. 3, Line 8: Why not complementing your study with VIIRS satellite?

Section 2.1: Is the lidar system operating continously?

Section 2.1: What is the vertical resolution of your lidar system?

Pag. 4, Line 33: Please, provide references for ERA-Interim.

Pag. 5: Please provide a better explanation of your iterative method for computing lidar ratio. Why starting at 60 sr? What happen with dust cases?

Pag. 5: Please give a complete definitions of the variables in Line 2 and in equation 1.

Pag. 7, Line 6: It is difficult to understand how you obtain the final accuracy on calibration factor. Please give a better description.

Pag. 8 and 9: Please provide the link for MODIS and VIIRS data.

Pag. 9: Please, provide a link for ECLIPSEv4 database.

Pag. 10, Line 1: This statement is incorrect. Compation of AOD requires vertical-profile of lidar ratio which is possible with Raman and HSRL systems but not with micropulse lidars. Your approach assumes constant lidar ratio. Please correct.

Pag. 11, Line 3: This statement is incorrect. Currently, it is possible to obtain AOD dur-

ing the night by star and moon photometry (see ACTRIS project for example). Please correct.

Figure 5: Please, explain better how you compute your nighttime AOD. Particularly, how do you obtain nighttime lidar ratios.

Figure 7: It is difficult to follow how was the aerosol load during the different periods you claim. Why not adding AOD temporal evolution?

Section 5.1.1 must be strenghten. The scientific discussion is poor. It is not clear the large diurnal variability in lidar values for Case C. Are each of the cases selected illustrative of the different atmospheric conditions over the study area?

Pag 18, Line 6: It is not clear if you use standard CALIPSO data or your own computations, particularly for depolarization measurements.

Pag 20, Line 13: The PBL is a region in your profile not a source of aerosol emissions. Please correct this.

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