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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Transport of aerosol to the Arctic: analysis of CALIOP and French aircraft data during the spring 2008 POLARCAT campaign" *by* G. Ancellet et al.

Anonymous Referee #1

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The article report a study of tropospheric aerosol distribution and transport pathways in the Arctic region. In situ and lidar data from an aircraft field campaign that took place on April 2008 are used. Additional data were provided by the satellite borne CALIOP lidar. The datasets are presented, and their statistical comparison allowed for both a recalibration of the satellite lidar dataset, and an estimation of the representativeness of the aircraft campaign outcomes. Aerosol optical parameters from lidars are then interpreted with the aid of a lagrangian transport model to assess transport mechanisms. The altitude and geographical distribution of the optical characteristics of the aerosol are provided and discussed in terms of source regions, transport pathways and aerosol

processes. The above analysis is restricted to the limited period of April 2008.

This is a comprehensive study that surely deserves publication in ACP. The article is properly laid down, well referenced, with science questions clearly exposed, methods properly explained and results adequately discussed and interpreted. I consider it a valuable contribution both under the methodological issues of of analysis of satellite lidar data, and to the more scientifically oriented study of tropospheric aerosol in polar region, both in term of methodology and of results. I would encourage its publication, after some minor comment reported hereafter have been addressed.

(5728,10-13) Here, the second mode the authors claim to be apparent in the airborne lidar data histogram is REALLY difficult to discern. Less so is the bimodality in the CR histogram panel. As these different modes in the histograms are so difficult to imagine, and their manifestation or not, does not hamper any of the subsequent analysis, I would suggest to discuss the data simply in term of existences of relatively high left tails of the histograms, attributable to different particle populations, mirrored in the two modes of the CR histograms.

In subparagraph 3.2, a review on how the aerosol backscattering at 1064 nm has been calibrated - there's ample literature on similar studies – in the molecular backscattering is too noisy, should be quoted. Moreover, as the CALIOP lidar calibration constant at 1064 nm is operationally evaluated by comparing 1064 and 532 nm signals in high cirrus clouds, some word should be spent on how the recalibration carried out in the present study may change the cirrus color ratios. I aknowledge the fact that the authors make no use of any CALIOP cirrus product, nevertheless I think it is worthwhile to discuss how would the CR behave on cirrus clouds after the proposed recalibration.

(5736, 18) and successive. As one of the possible causes for the small differences in CALIOP and airborne lidar, could this be the different spatial averaging for the lidar profiles, that would tend to lower the optical parameter values for the more spatially average profile, when patchy (both vertical and horizontal) aerosol are encountered?

(5738, 24) and (5740,10) I do not understand here what is the role of the CALIOP cloud screening the authors are suggesting in order to explain the decrease of CALIOP high aerosol concentration at low latitudes in the lower troposphere, and in general in the 1.5-4 km range. According to my understanding of the authors' explainations, a cloud screening may affect the computation of the average of the optical parameters only if the "screened" aerosol layers have, on the average, higher backscatter ratios than the "not-screened" ones. I can't see why that should be the case.

Fig.3 please rescale the axis for CR in order to use the full graph.

Fig.7 Regression line is quoted in the caption but is missing in the graph.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 5721, 2014.