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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of aerosol and cloud layer structures from the space-borne lidar CALIOP using Seoul National University ground-based lidar" *by* S.-W. Kim et al.

Anonymous Referee #3

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This is a useful paper, but not an impressive one. It compares CALIOP lidar profiles and designation of cloud and aerosol layers therefrom with similar profiles obtained from a ground-based lidar. The paper contains no surprises – it basically says that CALIOP seems to be doing a good job. However, the paper doesn't discuss or indicate how accurate or good the CALIOP results are. The comparison is at a rather elementary level.

The authors should give attention to the following before publication:

1. Shorten the title. Perhaps "Validation of aerosol and cloud layer structures from

using a ground-based lidar in Seoul"

2. Calibration of the SNU lidar was not described. The reader cannot understand the differences in the two lidar's profiles or the discussion in the paper without knowing the method and accuracy of the calibration of the ground-based lidar.

3. Remove the claim of "first observationally based validation." How do the authors know theirs is the first? Even if it is, being first is not important for the purpose of this paper. They are substantiating only the expected, not discovering any previously undetected problem. There is no new theory or understanding, where "first" might matter.

4. Fig. 2 caption. State range gate resolution for SNU lidar as well.

5. The reader needs to know the answers to these questions. Is the SNU analysis method equivalent to that for CALIOP? Is the comparison for measurement quality, processing algorithm, or both?

6. The time series in both the CALIOP and SNU data streams give information about the spatial variability. This should be examined and discussed to establish the degree of this variability and how it influences that comparison.

7. 11213 line 12: What does "excellent" mean? 11215 line 2: What does "sound" mean? The authors need to describe the kinds of errors and their magnitude that might occur from either the measurements or analysis methods, and state whether the comparisons reveal anything about those potential errors. The conclusion may well be that the results for layers are identical within the uncertainties from spatial/temporal changes. A validation exercise that compares only a few cases that appear relatively simple is not enough to say the CALIOP method is "excellent" or "sound." The comparison must look at difficult cases, even those where the algorithm is expected to have problems, before one can make such strong statements.

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8. 11209 line 24: Unclear whether the CALIOP switches polarization in the transmitter, or whether discrimination is in the receiver (simultaneous or switching?) Most likely it is in the receiver with simultaneous measurement of both polarizations.

9. 11215 line 1: . . . "study, we validated in an approximate manner the space-. . . ." would be better. The comparisons weren't quantitative or comprehensive enough to say that the CALIOP profiles and layer products were validated.

Additional considerations I urge the authors also to address, resources permitting, before publication. The editor might decide whether to make any of these a requirement.

1. English usage has quite a few problems. However, the paper is adequately readable. I encourage the authors to have it edited for English usage and style.

2. Averaging the SNU lidar at higher altitudes, up to the 600 m average used by CALIOP, would permit useful comparison for heights $>$ 10 km.

3. A suggestion for all comparisons of this kind (zenith and nadir lidars): Correct for the molecular attenuation, which can be easily calculated from a meteorological sounding or weather model output. If those are not easily available, a standard atmosphere air density profile will suffice very nicely. This will reduce the opposite trends in lidar data slope, and make clearer the differences due to backscatter and extinction from aerosol particles and clouds.

4. The authors qualitatively discuss differences due to extinction for zenith and nadir views. They could go one step farther and quantitatively test this difference to see if the results are reasonable (and thus more deeply evaluate the performance of CALIOP). This would be important if users desire to infer aerosol backscatter or extinction profiles from CALIOP, or optical depths of layers.

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5. It would also be good to consider advection of the air where CALIOP observed and match that as closely to the SNU data as possible. This might mean choosing SNU profiles staggered a little in time, but observing closer to the same air mass. This might be done after seeing where the layers are – the advection of the cloud layer may be very different than advection of the boundary-layer aerosol layer. This could be done in combination with # 6 above.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 11207, 2007.