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Atmospheric Measurement Techniques Discussions

Interactive comment on "Synergy between CALIOP and MODIS instruments for aerosol monitoring: application to the Po Valley" *by* P. Royer et al.

Anonymous Referee #1

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The paper is on the determination of aerosol extinction coefficient (α_p) profiles and aerosol optical depth τ_p derived from (typically) 200-shot averages of CALIOP data over the Po valley being constrained by the aerosol optical depth derived from MODIS Aqua and Terra data. From this constraint the authors derive a height-independent lidar ratio S_p and found out that in some/many cases it differs from the lidar ratios used in the operational algorithm of CALIPSO, and as a consequence, the extinction coefficient profiles differ as well. In summary, they state that the agreement between both S_p, however, is still acceptable.

The main weakness of the paper is, that it is not really clear what the purpose and

the benefit of this study is. Shall the retrieval of τ_p be improved, better than MODIS alone by means of CALIPSO data? Shall a climatology of S_p be established? Shall the CALIPSO-retrieval be improved by providing more realistic S_p (by means of lidar ratios independent of height and averaged over 70 km and no clearly attributable to aerosol types!)? It is certainly true that α_p from CALIPSO is sometimes questionable because of wrong lidar ratios. However, the synergistic approach has also "'question marks"': temporal difference between Terra and CALIPSO, nighttime interpolation, spatial variability, height-dependent S_p, uncertainties in τ_p from MODIS, and more (as the authors state by themselves). So, both retrievals (operational CALIPSO vs. MODIS-synergy) are subject to errors, and the authors present two solutions but it remains open, which is closer to the truth. So, what do we learn from the paper? What is the future benefit, what might be a future strategy? Here, clear statements must be added.

Furthermore, it becomes not clear, how the synergistic approach really works; it is never clearly described, one can only read between the lines, what the authors might have done. Probably, addition of a small paragraph will suffice.

Another weakness of the paper is the imbalance between relevant and irrelevant parts, at least it is not convincing that some sections contribute to the overall purpose of the paper:

(a) parts are apparently of marginal importance (e.g. most parts of 4.2),

(b) significant parts cover a lot of details with respect to τ_p-intercomparisons (IS-PRA radiometer [wavelength-interpolation required!], MODIS-Aqua and MODIS-Terra, AERONET) but it is not clear what this is good for as the authors use all (btw: last paragraph in 3.2 is hard to understand from the figure; maybe the regression lines help; from visual inspection one would expect just the opposite conclusion),

(c) several parts are not precise (page 1337, line 7: what means "'more important"'? (1339/10): depolarization data from which instrument? conclusions for PBL-optical depth? (1340/18): polarization again; (1334,27): given SNR-range not found in Table,

etc.),

(d) whereas relevant topics are not covered in detail (description of the synergistic method as already mentioned, comments/explanations to the figures [examples see below], strategies for validation, etc.).

A few comments to the figures:

Fig. 7: what do the blocks mean, e.g. Fig. 7a, second from left: 15 cases of BER from 0.01 to 0.014, with nighttime BER larger than daytime BER? What about lidarratios larger than 100 sr of even larger than 200 sr (Fig. 7b). What kind of aerosol is this (never seen before)? Such a finding needs comments in the text.

Fig. 8: I don't see an annual cycle. There are almost no measurements in winter (only one per year)! This is certainly over-interpreted.

Finally, the authors should avoid BER. Everybody uses the lidar ratio, and it makes no sense to use a more or less private nomenclature. It seems that the authors are aware of this problem, as they include S_p in addition to BER in the last part of the paper. So, they can directly omit BER. By the way: "'BER"'-inclusion in Eq. 1 causes pain.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 1323, 2010.