Atmospheric
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Discussions

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Interactive comment on "CALIOP near-real-time backscatter products compared to EARLINET data" *by* T. Grigas et al.

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Dear Editor and Authors

I am sending these comments for improving the clearness of this paper, in particular respect to EARLINET data use and related references. This paper deals with a very important topic: the assessment of global aerosol profiles ready for the assimilation. This is a very timely and interesting topic, and data used in the paper are very valuable coming from the state-of-the-art platforms for aerosol profiling: CALIPSO and EARLINET. However, it is important to avoid misunderstanding and misleading sentences. I hope my comments would help authors to improve the paper.

Regards.

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Interactive Discussion

Discussion Paper

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General comments

1. The title "CALIOP near-real-time backscatter products compared to EARLINET data", is misleading: NRT backscatter product is not a correct wording. Even if, after reading the Data and methodology section, an expert can understand, that total attenuated backscatter profiles are compared, this title gives the impression that aerosol backscatter profiles are compared and this is definitively not true.

2. "CALIOP near-real-time backscatter products compared to EARLINET data", the comparison methodology is not a CALIOP vs EARLINET independent comparison, because the methodology described in section 2 uses CALIOP derived information into EARLINET backscatter to total attenuated backscatter conversion, so that EARLINET derived products are not independent from CALIPSO ones. This has some relevant outcomes: it is nowadays well known that CALIPSO typing has some troubles for marine type and in coastal regions (Kanitz et al., 2014; Winker et al., 2013), that polluted dust is oversampled (Burton et al., 2013) and also that dust lidar ratio value should be adjusted (Amiridis et al., ACPD, 2015). The impact of using these assumptions in this comparison for assessing the effectiveness of CALIOP lev 1.5 data assimilation is not considered at all. Authors should at least discuss these main critical aspects.

3. Reviewer 2 reports some doubts about the scientific relevance of the paper in this shape. Probably this is related to the lack of some quantitative information. The main point of the paper should be assessing the lev 1.5 quality , however at the present stage they are compared to external ones (EARLINET) but strongly contaminating them with CALIOP assumptions (because typing and lidar ratio values are assumptions for CALIOP algorithm) and without providing quantitative estimation of the Lev 1.5 accuracy. Reading this paper one cannot answer to the question: which is the error on Lev 1.5 over Europe on average? Is this dataset useful for the assimilation purposes at continental level and at which extend? Authors underline already in the

abstract that CALIOP could record signal with a too low SNR in case of strong layers in the free troposphere. This is actually a very important point, to be addressed in a more quantitative way. As reviewer 2 wonders, are the differences in R significant? This is the first point. A further really important point is: filtering out data with a layer in FT and in the PBL means going towards clean air, background conditions. Over a highly populated continent as Europe is, one would expect very often the presence of high aerosol content in the PBL but also in the FT for the presence of long range transported aerosol from surrounding areas (Sahara desert, Eastern developing countries, biomass burning, fires from the US-Canada and so on). In fact the authors have filtered out more than $\frac{1}{2}$ of the data (page 6053, line 7) and the result of this filtering is that 45% of the cases are clean air in the PBL and 97% of the cases for the FT are clean air. Is this representative for the European continent? My impression is that in less of $\frac{1}{2}$ of the cases over Europe you have this clean air condition, so that if there is an improvement of Lev1.5 reliability for the filtered cases, they would be representative in case just for one half of the situation observed over Europe. Is this sufficient for the assimilation purposes?

4. References are not properly included. Some important ones are missing and in other points (see detailed comment below) others are not relevant.

Detailed comments:

Title: misleading (see above)

Abstract PBL , FT not clear here the meaning but misleading in the abstract.

PBL and FT acronyms are explicitly reported at the end of the abstract and not at first appearance

"The presence of FT. . .", this presence should be reported in AOD which is what makes the difference for CALIOP SNR

These differences in the correlation coefficient are really relevant and significant?

"The results. . ." this sentence is not supported by this paper and it is also very qualitative (different levels???)

Introduction

Here references are very bad. This list reports some examples but it is far to be exhaustive:

"Lidar is a very useful technique. . ..(gross et al., 2010; Papayannis et al., 2002)" Many papers about lidar aerosol observations demonstrate its capability for aerosol profiling both from ground based and space-borne lidars. Here just 2 are reported that probably are not the most important (the e.g. or for example wording should be included at least), and nothing from satellite .

"Several research programme. . ." Giannakaki and Mattis are both from EARLINET , which are the SEVERAL research programmes which authors refer to? Moreover, authors report that "Several research programme performed routine long-term observations. . .however such studies are limited to single geographical locations. In order to study . . .on a larger spatial scale, lidar networks are deployed" in this sentence publications from EARLINET, which IS a network, are reported.

". . .lidar networks are deployed (Pappalardo et al 2009b) " Pappalardo et al, 2009 b, reports something about EARLINET for CALIPSO validation purposes. As reference for EARLINET Bosenberg et al, 2003 and Pappalardo et al., AMT 2014 should be used. However EARLINET is not the only network around the globe. The others should be mentioned as well.

Bockmann et al 2014 is not appropriate (see above)

"At present, 28 European. . ..(Sawamura et al., 2012)" Sawamura is for sure not a reference for EARLINET status, even if EARLINET is there involved.

Also for CALIPSO references they are not well included. Only one reference for CALIPSO and one for A-train are reported. Neither Vaughan et al., 2011 reported

on Level 1.5 data as main reference is reported.

"The EARLINET community…." Several but you included just 2.Moreover, EARLINET database and in particular for the purposes of this paper, EARLINET correlative measurements (<100km) for CALIPSO are published. This reference should be included.

Page 6043, line 25: Level 1 and Level 2, it should explain what they are

Page 6044, line 16: Level 1B, what is?

Data and methodology

Page 6045, line 23: mixed aerosols? Level 2 VFM reports clean marine, dust, polluted continental, clean continental, polluted dust, smoke and other. Is this mixed a new product?

Page 6046, line 3: SD stands for?

Page 6046, line 14: "The ground-based lidar measurements used in this study were acquired from the EARLINET portal www.EARLINET.org for the period from November 2010 to December 2012 as well as for several days in April and May 2010 during the Eyjafjallajökull volcano eruption." Why have the authors left out some of the EARLINET sites and did not include all which are available at the data base in their study? How did the authors choose their locations? How many profiles from each station are available (could be included in Table 1) and should show the representativeness of the study.

Page 6048, line 7: this means (see above) that the well-known problem of typing/lidar ratio assumptions in CALIPSO data are not addressed at all. This should be mentioned in the discussion for correctness and intellectual honesty.

Page 6048, line 9: these are not EARLINET extinction coefficient. This sentence is wrong from the scientific point of view.

Results

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Page 6050, Figure 2 discussion: in the clean marine layer the total attenuated backscatter is higher than for Polluted Dust... is this feasible or is it related to problems on the clean marine identification? Page 6052, discussion in figure 6 and 7: what one could say from these figs is that the larger discrepancies are observed for low altitudes. This is also in agreement with Moan et al., 2009 and Pappalardo et al., 2010.

Page 6052, line 16: The PBL is assumed to be always 2.5km. This is not correct, the authors could refer to low troposphere (below 2.5km) and middle troposphere explaining the 2.5km reference point from EARLINET observations.

Page 6052: why not using the RMSE which do not consider the sign of the difference since both mean bias and FoE have it inside?

Page 6053, line 2-3: "that could be...time" something is missing

Page 6053, lines 12-13: instead of column backscatter, AOD should be used.

Figure 12 and 13: what is reported on the axis?

Page 6054, 5: fit on only 5 pt, is this reasonable?

Page 6055, line 15-17: "majority of the outliers" this is not supported by the showed results

Page 6055, lines18-20: the aerosol typing is not discussed at all.

Table 1 is never referenced.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 6041, 2015.