

We thank the reviewers for thorough consideration of the manuscript and the constructive comments, which contributed to the improvement of this manuscript, responses are presented below.

Referee #3 (RC C1207), Received and published: 30 March 2015

Review of the manuscript #acp-2014-998 by T.Grigas, M.Hervo, G.Gimmestad, H.Forrister, P.Schneider, J.Preißler, L.Tarrason, and C.O'Dowd "CALIOP near-real-time backscatter products compared to EARLINET data" Atmospheric Chemistry and Physics Discussion (ACPD)

Summary:

This article describes an evaluation of (lev 1.5) attenuated backscatter profiles from the CALIOP space lidar with EARLINET ground-based lidar profiles, based on a 3 years dataset from 2010-2012. It investigates the agreement of profiles depending on the ground-track vs EARLINET station distance, the altitude of aerosol layers, the aerosol type and separate for the planetary boundary layer (defined as the lowest 2.5km) and the free troposphere. Two specific cases are discussed in detail.

Significance:

The topic is relevant and interesting for ACP readers, because this CALIOP data product at present is the only mature operationally available near-real-time aerosol profile information with global coverage, suitable for assimilation into global forecast models. The article is well structured, understandable and fits well into the MACC special issue. Many of the conclusions are sound but some of them are not yet convincing. Particularly the discussion requires considerable improvement, stating the significance, consequences and applicability of the results for the development of NRT aerosol profile assimilation.

Evaluation:

Therefore, I recommend that some significant revisions are done before it is published in ACP (minor revisions).

Major comments:

1. I understand that you adapt CALIOP and EARLINET profiles in the vertical (60m layers) but not in the horizontal and that 'synchronicity' means that EARLINET and CALIOP profiles are at the same time, neglecting wind drift between both. CALIOP is averaged over ~20-100 x 20 km, but I do not find a specification over how many minutes (??) EARLINET profiles are averaged.

Response: We followed up on this in the revised manuscript to describe over how many minutes EARLINET profiles were averaged.

2. Air-masses and scales would probably be closer comparable if the EARLINET profiles were drift-corrected and averaged over a similar scale as CALIOP, both with wind speed (e.g. estimated from the trajectories) see discussion. For example 50km average spatial distance corresponds to ~1.5h temporally at 10 m/s wind-component along the station track-center axis. Keep in mind that EARLINET lidars are often located in or close to large towns with corresponding horizontal gradients. Regarding the limited number of 48/27 coincident overpasses, this seems feasible.

Response: We agree that the reviewer's proposed method would make the comparison more feasible; however we do not have the access to EARLINET non-averaged measurements because they are non-publically accessible.

3. The section about data filtering addresses a relevant question but is not yet convincing. The results from this approach are not really discussed and I wonder whether they are significant. Attenuation errors should clearly reduce when attenuation (layers) are excluded. But I can't really take this message from figs 8-11. It seems to me that simply the data at small values are missing when large values (layers) are filtered out.

Response: We partly agree with the reviewer's point and we included a concluding statement that statistical improvements due to data filtering were not significant.

4. You often use the acronym 'FT' where I'm not sure whether the stratosphere isn't included. I found no other indication of the upper altitude range than on P6047L19, where it is 20km.

Response: We thank the reviewer for pointing this out. We have included this detail (20km) in the revised manuscript.

Specific comments:

5. Abstract L7: should this read '...included a valid aerosol type classification...'?

Response: We have changed this part of the sentence in the abstract.

6. P6044L20pp.: To most readers it won't be obvious how a product, which is available within 6-30h, is used for operational forecasting. There is little information on that in (Powell et al, 2013). Any other reference. If not, please explain briefly.

Response: Assimilations related questions were the motivation for the study, however answering them was not the main output of this study. Additional reference about using the CALIOP measurements for assimilations is Vaughan, M., C. Trepte, D. Winker, M. Avery, J. Campbell, R. Hoff, S. Young, B. Getzewich, J. Tackett, and J. Kar, 2011: "Adapting CALIPSO Climate Measurements for Near Real Time Analyses and Forecasting", Proceedings of the 34th International Symposium on Remote Sensing of Environment, which can be found here http://www-calipso.larc.nasa.gov/resources/pdfs/VaughanM_211104015final00251.pdf

7. P6045L15: level ->levels

Response: The expression "processing level" (P6045L15) was changed into "Level 0".

8. P6048L7: tempus: correspond

Response: The verb form was corrected.

9. P6050L15/16 and annotation Fig2: colors for different aerosol types can hardly be distinguished spread the color scale.

Response: The annotation and the colours were changed.

10. P6051, L9pp: another reason for the discrepancy could be an invalid type classification (e.g. some Sahara dust plumes are mixed with fire smoke) which affects the lidar ratio and the calculated attenuation. The layers' AOD can be roughly calculated to ~ 0.2 (with a dust LR 50-60), but ~ 0.3 with a dust/smoke LR around 60-100). This would result in too small attenuation re-scaling of the EARLINET profile. Do the EARLINET observations exclude this possibility?

Response: For this specific case, CALIOP detected the layer as dust layer and the lidar ratio provided in EARLINET file was equal to 55 (dust). That eliminates the possibility of invalid type classification for this case. The paragraph P6051L9-15 was updated to include this.

11. P6051, L11: can a cross-track variability of this layer be confirmed by the EARLINET observation, as variability in time, due to the cross-wind component that is evident in the trajectory plot?

Response: We do not have access to EARLINET non-averaged measurements. For this reason, we cannot check "be confirmed EARLINET observation, as variability in time".

12. P6051, L13: wouldn't difficulty to detect layers at all require an enormous AOD of the upper layer? Was there a thick Ci cloud above? Which AOD can typically be transmitted by CALIOP @ 532 nm?

Response: Indeed, we agree with that. The statement starting with "Second, the CALIOP measurements.." on P6051 L13 and ending on P6051 L14 was deleted.

13. P6052, L8pp: This should be expected, but for me is not obvious from Fig 6, where many points with large distance (red) match quite well but particularly around CALIOP=2Mm-1sr-1/EARLINET=3Mm-1sr-1 and also below the 1:1 line several data points with small distance show large deviation as well.

Response: We think that the problem is simply in the way how the program plots the 7405 data points. In this case, the statistical results tell the true story.

14. P6052 L16: Does this mean your 'FT' includes the stratosphere or what is the upper altitude range?

Response: We included this detail in the revised manuscript. See the reply to the comment Nr.4.

15. The choice of the threshold separating FT and PBL domains has considerable impact on the correlation of profiles, because errors due to the deviations of the highly variable PBL height are assigned to either the FT or the PBL analysis. 2.5 km will mostly be too high (particularly during night), such that the majority of PBL height issues will feed into the PBL analysis and reduce the correlation.

Response: The PBL aerosols can certainly extend as high as 2.5 km, as it was shown in Mona et al.2009. The definition & variability of the PBL height are not the most important issue; it is rather the vertical extend of locally-produced aerosols.

16. P6053 L23pp: The larger neg bias of CALIOP profile averages vs. (more locally influenced) EARLINET profiles seems to me a matter of representativeness. (see comment on averaging above)

Response: We think that the representativeness of the stations might be only a part of the problem; another part (potentially even more important) is related to the fundamental differences between the measurements by CALIOP (flying at 7-8km/s speed) and ground-based averaged lidar.

17. P6053 L21: The 'corespondingly' only holds for the first part of the sentence not the second e.g. replace 'and they decreased...' by 'but they decreased...'

Response: We corrected the phrase by 'but they decreased...'

18. P6053 L24-28: doesn't this only repeat (with zero criterion) the previous filtering according to the threshold?

Response: We agree with that, the lines (P6053 L24-L28) were deleted.

19. P6054 L21/22: why should the quantifyability of a particle layer depend on its mixing state? Because of the uncertain lidar ratio?

Response: We think that yes.

20. P6055 L14-17: I'm not convinced of that by the analysis you show (cf. comment on methodology 2)

Response: We answered related aspects to this comment in our replies to the comments Nr. 3 and Nr. 13.

21. Figs 5-11: move statistical parameters into the plot instead as headline (I needed some time to recognize them)

Response: The statistical parameters were moved into the plot.

22. Is there any more specific source of information about the Lev 1.5 data product than Powell, 2010?

Response: The reference provided in reply to Nr.6 comment delivers additional information on how the CALIOP level 1.5 is produced.

Referee #2 (RC C1278), Received and published: 2 April 2015

Review of the manuscript #acp-2014-998 by T.Grigas, M.Hervo, G.Gimmestad, H.Forrister, P.Schneider, J.Preißler, L.Tarrason, and C.O'Dowd "CALIOP near-real-time backscatter products compared to EARLINET data" Atmospheric Chemistry and Physics Discussion (ACPD)

I have little criticisms about the clearness of the content and its description. There are few inaccuracies that I point out in my technical comments below.

My major concern is the relatively low scientific relevance that this paper can have on the state of the art.

Evaluation:

I have wondered whether this concern should be a reason to reject a paper and I think it is not. I am then in favor of the publication of this paper as it brings anyway a clear description about how to improve the interpretation of the CALIOP data by highlighting the limitations related to the downward measurements and helps separating the PBL from the FT advected aerosol layers.

The abstract could be improved, the last part where the authors state the relevance of their work should highlight clearly the novel/important aspects of their study. A relative increase of 5% in the correlation coefficient would probably not be enough.

Summary:

The paper "CALIOP near-real-time backscatter products compared to EARLINET data", by T.Grigas, M.Hervo, G.Gimmestad, H.Forrister, P.Schneider, J.Preißler, L.Tarrason, C.O'Dowd describe a technique to filter the data and improve the correlation between EARLINET and CALIOP backscatter data over a period of three years. The proposed technique deals with a straightforward separation of the Troposphere into a PBL and a FT region in order to distinguish the aerosol contributions in the two regions. The technique allows improving the understanding of the different top-down and down-top signal attenuations experienced by the satellite and ground-based lidar systems. I have little criticisms about the clearness of the content and its description. There are few inaccuracies that I point out in my technical comments below.

Significance:

My major concern is the relatively low scientific relevance that this paper can have on the state of the art. I am then in favour of the publication of this paper as it brings anyway a clear description about how to improve the interpretation of the CALIOP data by highlighting the limitations related to the downward measurements and helps separating the PBL from the FT advected aerosol layers.

Evaluation:

I have wondered whether this concern (the relatively low scientific relevance) should be a reason to reject a paper and I think it is not.

23. Pg 1, In 28 (P6042, L24) : incoming and reflected solar Radiation

Response: The sentence was corrected according to the comment.

24. Pg 2, In 2 (P6043, L2): from the depolarization channel...

Response: The expression "polarization signal" was replaced by "depolarization channel".

25. Pg 2, In 21 (P6043, L22): several comparison of ground-based LIDAR data with...

Response: The expression "several comparisons" was left.

26. Pg 2, In 26 (P6043, L27): remove the brackets at the beginning of the sentence

Response: The brackets were removed in latest version of the manuscript.

27. Pg 2, In 28-29 (P6044, L1-L2): explain why only measurements with independent extinction calculation were retained for the study.

Response: The manuscript was updated to address this by adding a sentence at (P6044, L2): "That allowed (a) calculating the lidar ratio and (b) converting EARLINET backscatter into attenuated backscatter as seen from space at 532 nm without any assumptions."

28. Pg 2, In 33 (P6044, L6): remove the brackets at the end of the line.

Response: The citation was corrected.

29. Pg 2, In 32-33 (P6044, L5-L7): Please state which kind of EARLINET product was compared to CALIOP, Attenuated Backscatter?

Response: The manuscript was updated to describe the EARLINET product by adding additional sentence (P6055, L7): "In this study, the measurements were averaged approximately for two hours and were centred on the CALIOP overpass time."

30. Pg 3, In 6 (P6044, L12): remove brackets when you refer directly to a citation throughout the entire manuscript.

Response: It was fixed in latest version of the manuscript. The rest of the manuscript was reviewed and the places with such citations were accordingly corrected.

31. Pg 3, In 25 (P6045,L3): dropped by 54% ...

Response: The word "declined" was replaced by "dropped".

32. Pg 4, eq.1 (P6046, eq 1): if represents the uncertainty of the attenuated backscatter at the bin, than N should be the number of individual Level 1 lidar profiles, no?

Response: Thank you for mentioning about this aspect. No, the uncertainties are calculated per bin and not per profile. N is the number of level 1 profile range bins within the 20 km x 60 m interval remaining after clouds, overcast, surface, subsurface, totally attenuated, and invalid features were screened out (CALIPSO Quality Statements Lidar Level 1.5 Data Product Version Release: 3.02).

33. Pg 5, In 8 (P6046, L23-L25): provide the definition of total as sum of aerosol plus molecular rather at the beginning of section 2 than here.

Response: The manuscript was changed to include the total backscatter definition at the beginning (P6045 L9-10): "... of the total (molecular plus aerosol) attenuated backscatter as seen from ...".

34. Pg 5, In 20 (P6047, L9): from the lidar to the outer atmosphere and back down ...

Response: The original expression was left.

35. Pg 6, In 13 (P 6048, L9): are the LIDAR ratios values used in eq.6 to calculate the EARLINET extinction coming from CALIOP or from independent calculation of the EARLINET algorithm?

Response: The LIDAR ratio values were extracted from CALIOP level 1.5 files. See also the response to the comment Nr.75

36. Pg 6, ln 27 (P 6048, L23): As the authors compare two LIDAR measurements I think the word “comparison” is more appropriate.

Response: The end of the sentence “are inter-compared” was replaced with “are compared”.

37. Pg 7, eq.9 (P 6049, eq.9): what is the advantage of including a -0.5 term? Could not the FoE simply vary within [0-1]?

Response: The FoE value could be in different range, however keeping FoE in the range of [-0.5;0.5] gives some advantages. It gives more intuitively obvious way to understand either the CALIOP were higher (and how much more in percent compared to EARLINET) or they were lower (and how much less in percent compared to EARLINET).

38. Pg 8, ln 1-2 (P6050, L10-11): this has been said already on pg 7, ln 21-22 (P6049,L6-7).

Response: The lines (P6050, L10-11) of the manuscript were changed: “The CALIOP overpass map for the first case study (Barcelona) is shown in Figure 1.”

39. Pg 9 ln 8: I suggest to slightly modify the structure of Sections 3.2, 3.3 and 3.4 to a more straightforward structure. Section 3.2 deals with all the dataset for the overpasses with distances < 100km, then a separation of the dataset is performed in Sect. 3.3 in order to separate the contribution of PBL and FT always keeping $d < 100\text{km}$ and finally in 3.4 a filtering of the separated PBL and FT dataset is performed. As I see this, it would make more sense to have Sect. 3.2 “EARLINET-CALIOP comparison with ground track distance 100 km”, Sect. 3.2.1 “PBL and FT with ground track distance 100 km” and Sect. 3.2.2 “Filtered PBL and FT with ground track distance 100 km”.

Response: We thank the reviewer for advice and we agree that it would become better structured manuscript. The manuscript sections 3.2, 3.3 and 3.4 were modified accordingly:

Sect. 3.2 “EARLINET-CALIOP comparison with ground track distance 100 km”

Sect. 3.2.1 “PBL and FT with ground track distance 100 km”

Sect. 3.2.2 “Filtered PBL and FT with ground track distance 100 km”.

40. Pg 9-10, ln 29-2 (P6052,L18-19): no need to repeat the criteria of selection, these are the same as before.

Response: The paragraph of the manuscript was changed (P6052, L16-20) into the following:

“The PBL height was assumed to always be 2.5 km for this analysis (Mattis et al., 2004; Pappalardo et al., 2004). The scatterplots for the separated PBL and FT datasets are shown in Figs. 8 and 9 and characterized by R, MB and FoE parameters (Table 2).”

41. Pg 10, ln 9-10 (P6052, L27,28-P6053, L1): replace by “The aerosol layers in the free troposphere are often characterized by smaller horizontal variability compared to the PBL, it is then likely that a higher EARLINET-CALIOP correlation can occur in the FT”.

Response: The lines in the manuscript (P6052, L27,28 - P6053, L1) were changed according to the suggestion.

42. Pg 10, ln 11 (P6053, L1): one may argue this statement simply based on the definition of the PBL as the atmospheric region where aerosols get homogeneously mixed. I suggest to replace by “On the other hand, the boundary layer, especially during convective periods, undergoes higher temporal and spatial variability due to continuous PBL updraft and FT downdraft. Moreover, local sources of aerosols inside the PBL may not appear in the CALIOP profile due to its distance from the source.”

Response: Thanks the reviewer for the suggestion, the part of the paragraph (P6053, L1-4) was rewritten and ‘homogenous’ is not used anymore to describe the correlation within the PBL.

43. Pg 10, ln 11-12 (P6053, L1-2): I don’t see the relation with the considerations made in ln 9-11. I suggest to cut this sentence and replace with “When an aerosol layer occurs in the FT, it attenuates the CALIOP lidar signal that will have less energy to penetrate further down into the PBL.”

Response: The part of original paragraph (P6053, L1-4) was rewritten; please also see the reply to the comment Nr.42.

44. Pg 10, ln 21 (P6053, L11-12): the author statement “with aerosol layers present in both the PBL and FT” is redundant, the PBL is by definition the region with aerosols. I’d change it to “with aerosol layer occurring in the FT above the PBL”

Response: The statement (P6053, L11-12) was replaced by “with aerosol layer occurring in the FT above the PBL”

Short comment by Jason Tackett (SC C1465), Received and published: 9 April 2015

45. The CALIOP level 1.5 expedited products is derived from expedited level 1 and level 2 CALIOP data. It would help to emphasize that expedited products (a) use a simplified calibration scheme and (b) that the GMAO molecular model number densities used to derive molecular attenuated backscatter coefficients are slightly out of date (sometimes by as much as two days). These two effects degrade the science quality of the expedited data compared to the standard CALIOP products which have a more robust calibration scheme and use the most current GMAO molecular model. The following paper details these issues and their consequences on expedited level 1 and level 2 CALIOP data products (and ultimately, expedited level 1.5). It would be worth referencing this paper at a minimum since changes in CALIOP calibration could impact comparisons with EARLINET. Vaughan, M., C. Trepte, D. Winker, M. Avery, J. Campbell, R. Hoff, S. Young, B. Getzewich, J. Tackett, and J. Kar, 2011: "Adapting CALIPSO Climate Measurements for Near Real Time Analyses and Forecasting", Proceedings of the 34th International Symposium on Remote Sensing of Environment, http://www-calipso.larc.nasa.gov/resources/pdfs/VaughanM_211104015final00251.pdf

Response: Thanks the reviewer for providing very useful information and suggestions about the CALIOP level 1.5. It will help to improve the manuscript. We have included Vaughan. et. al (2011)

reference in the manuscript. Also, above mentioned differences between level 1.5, level 1 and 2 were included in replying for the comment Nr.47 in this document.

46. Equation 3 and page 6047, lines 15-16: "...the molecular backscatter coefficient β_{mol} is provided as a Level 1.5 data product." The level 1.5 product actually provides the molecular attenuated backscatter coefficient; i.e., the molecular backscatter coefficient multiplied by the two-way molecular transmittance (CALIPSO Quality Statements, 2011, pgs. 6-7). If the molecular attenuated backscatter coefficient is used rather than the molecular backscatter coefficient (as equation 3 expects), then the molecular extinction coefficient will be in error (equation 5). It follows then that the EARLINET total attenuated backscatter computations will also be in error. If this has already been taken into account, perhaps text could be added to clarify.

Response: We thank the reviewer for bringing attention to this aspect. We updated the manuscript to address this issue.

47. Page 6044, lines 22-24: "This (Level 1.5) product is derived (Powell et al., 2013) by spatially averaging the Level 1 profiles and merging them with the Level 2 vertical feature mask product." A clearer way to describe the level 1.5 product would be something like: "Level 1.5 is derived by cloud-clearing level 1 attenuated backscatter profiles using the level 2 vertical feature masks, and then spatially averaging the cloud-cleared profiles." In fact, the paper does not mention that level 1.5 is a cloud-cleared product. Adding this clarification would make this important point.

Response: We have rewritten the lines (Page 6044, lines 22-24) of the manuscript: "Level 1.5 is derived by cloud-clearing level 1 attenuated backscatter profiles using the Level 2 vertical feature masks, and then spatially averaging the cloud-cleared profiles. Level 1.5 expedited products uses a simplified calibration scheme compared to Levels 1 and 2. Also, it is derived by using the Global Modelling and Assimilation Office (GMAO) molecular model number densities, which can be out of date (sometimes by as much as two days). As a result, the scientific quality of the expedited data compared to the standard CALIOP products can be degraded."

48. Page 6045, lines 20-22: "The (aerosol classification) algorithm detects eight main aerosol types: clean air, clean marine, polluted dust, dust, polluted continental, clean continental, smoke/burning biomass and mixed aerosols." The paragraph in which this statement appears describes the CALIOP level 2 aerosol subtyping algorithm which only classifies six aerosol types: clean marine, polluted dust, dust, polluted continental, clean continental and smoke. The aerosol subtyping algorithm does not detect clean air or identify mixed aerosol. However, the level 1.5 product does report feature types having the designation clear air and mixed aerosol to describe range bins absent of detected features ("clear air", not "clean air") or 20 km horizontal averages containing more than one of the six CALIOP aerosol types (mixed aerosol). I think this paragraph and the following paragraph which describes the level 1.5 product should be revised to make clear that six aerosol types are detected by the level 2 aerosol subtyping algorithm and the categories "clear air" and mixed aerosol are specific to how level 1.5 describes the features (or lack thereof) in each level 20 km x 60 meter (horizontal x vertical) range bin. The level 1.5 product does report feature types having the designation clear air and mixed aerosol to describe range bins absent of detected features ("clear air", not "clean air") or 20 km horizontal averages containing more than one of the six CALIOP aerosol types (mixed aerosol).

Response: The paragraph (P6045, L18-L24) was rewritten to address the comment: “The algorithm detects six main aerosol types: clean marine, polluted dust, dust, polluted continental, clean continental and smoke/burning biomass. Such aerosol type detection is implemented in Level 2 aerosol subtyping algorithm. Level 1.5 product does report feature types having the designation “clear air” and “mixed aerosol”. The first type is used to describe range bins absent of detected features while the second type is used if the 20 km horizontal averages contain more than one of the six CALIOP aerosol types.” Using only the type “clear air” through the manuscript has been also implemented.

49. Page 6045, lines 25-26: “The Level 1.5 product is derived by spatially averaging 60 individual Level 1 lidar profiles and merging them with the Level 2 vertical feature mask product.” Same as comment #3 above (#47).

Response: Thanks the reviewer for paying the attention to this. We described the required details in the reply to the comment Nr.47 (P6044, L/22-24).

50. Figures 12 and 13: Labels are missing for the horizontal and vertical axes from all panels.

Response: Thanks for pointing this out. The labels were restored in Fig. 12 and 13.

51. Figure 12 and 13 captions: Consistent with comment #4 above, “clean air” is not a detected aerosol type. A better description would be, “Eight level 1.5 feature types ...” rather than “Eight aerosol types ...”. For that matter, there are only six panels, so why does the caption say eight types?

Response: The captions in Fig. 12 and 13 were corrected according to the comment; the number of different aerosol types was also addressed.

52. It is important to mention that CALIOP version 3 data products are being used.

Response: This detail was added to the manuscript (P6042, L3): “... with Orthogonal Polarization (CALIOP) products version 3 were evaluated ...”.

Short comment by L. Mona (SC C1872), Received and published: 24 April 2015

53. 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.

Response: We disagree. The general term “backscatter products” certainly includes total attenuated backscatter profiles. “Near-real-time” is important in the title; it is the reason that the level 1.5 data products were created.

54. “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.

Response: Thanks for this valid point, we included an additional paragraph about this in section 2 "Data and methodology".

55. 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 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 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?

Response: Thanks for good points. The study was motivated by the desire for data assimilation, but the outcome is a description of a methodology that we developed for doing a large statistical study and applying it to a level 1.5 data product, along with statistical results.

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

Response: See responses to the comments Nr.62 – 68.

57. Title: misleading (see above)

Response: See answer to the comment Nr.53, above.

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

Response: Thanks for comment. The PBL and FT acronyms were explained at first their use in the abstract.

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

Response: See the response to the comment Nr.58.

60. "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?

Response: Thanks for the suggestion, however it is impractical to implement it at the moment.

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

Response: "location of the dominant aerosol layer" refers to the data filtering outcomes; "aerosol type" refers to Figs. 12 – 13. It is qualitative because it is a general comment, which we feel is appropriate in an abstract.

62. (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.

Response: We thank the reviewer for several helpful suggestions (Nr.62 – 68) and we implement many of them. We addressed Nr. 67 by adding another CALIOP reference.

63. "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.

64. "... 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.

65. Bockmann et al 2014 is not appropriate (see above)

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

67. 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.

68. "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.

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

Response: The important thing is to explain clearly what level 1.5 is, and we have added that. See also our responses to the comments Nr.45 and Nr.47.

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

Response: It was a typing error, which was corrected in new version of the manuscript.

71. 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?

Response: See response to the comment Nr.48

72. Page 6046, line 3: SD stands for?

Response: The acronym was explained in new edition of the manuscript.

73. 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.

Response: There was limited amount of published data on the EARLINET portal to match CALIOP measurements, which can be seen on found 48 overpasses (with detected any aerosol type) during the period from November 2010 to December 2012.

74. 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.

Response: See response to the comment Nr.54.

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

Response: We thank the reviewer for mentioning about this. This aspect was explained in new edition of the manuscript that the extinction coefficients were estimated by using the EARLINET backscatter coefficient and the lidar ratios extracted from CALIOP.

76. 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?

Response: Interesting question, but we are plotting the data from CALIOP here and we have no way of knowing whether there is a classification problem in this particular overpass.

77. 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.

Response: We thank for this useful comment. The manuscript was updated to reflect this.

78. 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.

Response: We called it PBL for simplicity and clarity in the paper. We divided the atmosphere into two regions, defined by a boundary at 2.5 km, and gave them the names.

79. 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?

Response: We prefer FoE instead of RMSE. See the reply to the comment Nr.37.

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

Response: See our replies to the comments Nr.41 and Nr.42.

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

Response: See the response to the comment Nr.60.

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

Response: See the response to the comment Nr.50

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

Response: That is the data set which we had.

84. Page 6055, line 15-17: “majority of the outliers” this is not supported by the showed results

Response: See the response to the comment Nr.13

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

Response: They were discussed in section 3.4 “PBL and FT using data filtering”.

86. Table 1 is never referenced.

Response: We thank the reviewer for mentioning about this. The reference was added in new edition of the manuscript.