# Response to Mark Vaughan's review of our article F. Marenco, V. Amiridis, E. Marinou, A. Tsekeri, and J. Pelon

We wish to thank Mark Vaughan for his detailed review of our article. The reviewer supports our main conclusion, i.e. that this is a case where several factors contribute to make things difficult for CALIPSO: the low-level clouds increase the albedo of the scene, thus reducing the signal-to-noise ratio, and moreover their incorrect removal causes the aerosol subtype to oscillate between smoke and polluted dust, thus introducing distortion in the retrieved extinction retrievals. The reviewer, moreover, points out that validation experiments in the Southern hemisphere are rare, and thus that our underflight of the CALIPSO satellite is a good scientific opportunity.

We wish to point out that our aim is not to "criticise" the CALIPSO retrieval scheme: as a matter of fact, we are convinced that such an advanced and fully automated retrieval scheme is a great scientific achievement. Scientists working in the field of lidar know how difficult it can be to infer atmospheric properties from an elastic backscatter system when no additional information is available; more advanced systems, such as HSRL, would greatly simplify the task (this remark also applies to our aircraft lidar). It is therefore admirable that layer detection, cloud-aerosol discrimination, assignment of an aerosol subtype and lidar ratio, and profile inversion can be obtained automatically on a global scale. It is not our intent to diminish the work of the CALIPSO science team in any manner. Our only aim is to describe a critical scene that we have had the chance to observe, with the perspective that it may contribute to perfect the products in the future.

The reviewer points out that our paper is not well-structured and omits critical details. We hope that we can improve it by following all the points raised by him in the annotated manuscript.

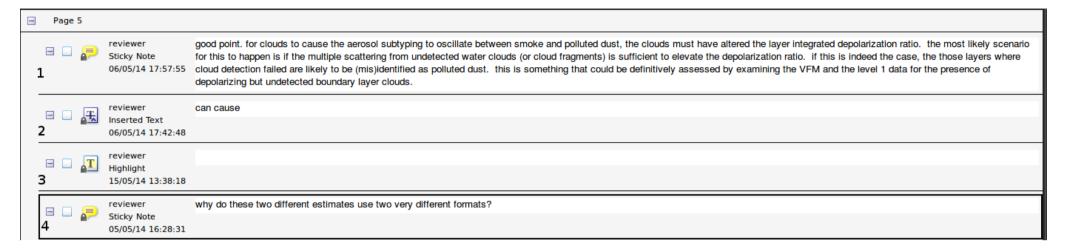
Results are shown before methods: the reviewer is correct that we have forgotten to add a description of the airborne lidar and of the data analysis framework before the presentation of results; we shall add this information in the revised version. This has given the reviewer the impression that we assume facts for which we do not have evidence; we thank him for pointing this out so that we can reformulate the paper in a way that does not leave such a negative impression.

The reviewer remarks that we do not adequately describe the airborne lidar: we will do so in more detail in the new version; however this system is commercial and the amount of details that we can give is limited. Moreover, he feels that our data processing methods are not described adequately. The latter does not reflect the fact that our algorithm has been documented in a previous paper; probably we have to point this out earlier in the article, so as not to leave such an impression.

The reviewer then gives a detailed discussion on the question of "unstable outward retrievals" for nadir-pointing lidars. We believe that this is an interesting discussion, and we have dedicated a previous author comment on this ACPD forum to express our thoughts about it. That author comment is to be considered as being part of this response; however the question of outward or inward retrievals is not the main point of our paper, and we are planning to reduce the emphasis on it.

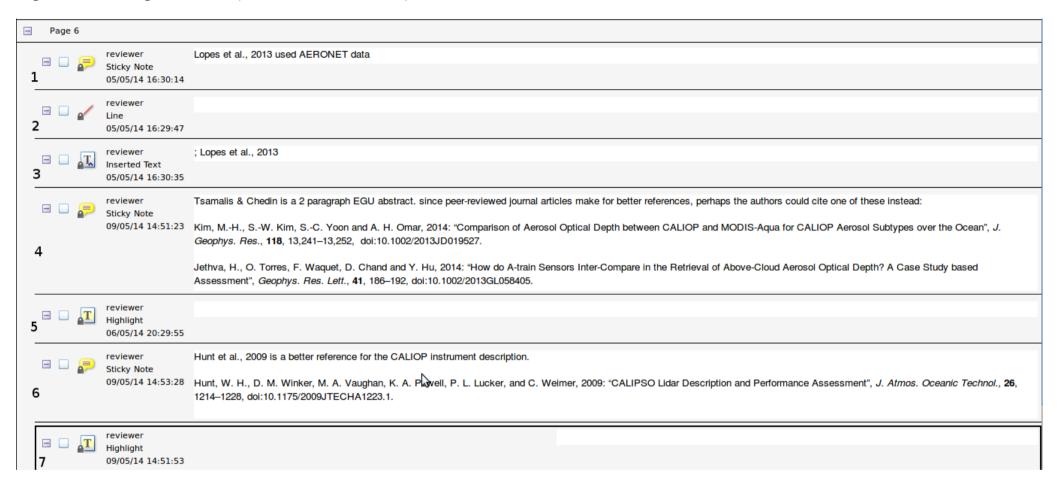
Finally, the reviewer returns an annotated manuscript with several comments. These are addressed in the following pages.

#### Page 5 of Mark Vaughan's review (comments to abstract)



- 1) We thank the reviewer for his comments which supports our main conclusion. We agree with the reviewer that multiple scattering in dense cloud can give depolarisation, and we actually see this very well in the level 1 volume depolarisation data (not displayed in the paper but available on the online quicklooks). From the level 2 data, we also see aerosol depolarisation in the smoke layer, at the same altitude where the clouds are detected (Figure 2e), and for this reason we infer that cloud depolarisation may have leaked into the aerosol product. A small fraction of undetected cloud can cause this effect. The consequence of this aerosol depolarisation is the mis-identification of the layers as polluted dust, which we observe in Figure 3e. We cannot think of a different cause for the assignment of this aerosol subtype in the CALIPSO product, and therefore we believe that this explanation is sensible. We are happy that the reviewer seems to agree to our main conclusion.
- 2) Ok.
- 3)
- 4) There must have been a typo in the figures that we have given for Assessment Report 4: AR4 indicates  $+0.03 \pm 0.12$  W m<sup>-2</sup> (see Chapter 2, Table 2.12); AR5 indicates  $-0.0 \pm 0.2$  W m<sup>-2</sup> (see Chapter 8, Table 8.4). Note that this is the estimated value of the global mean radiative forcing, and is not representative of any particular observing scene. We shall correct the value for AR4 in the reviewed version of the manuscript.

## Page 6 of Mark Vaughan's review (comments to introduction)

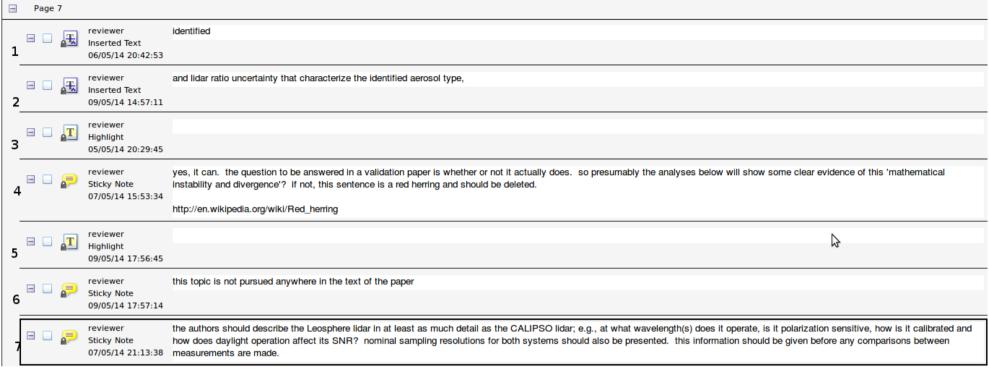


- 1) Thanks for this correction. We shall amend the paper.
- 2)3)

5)

- 4) Thanks. We shall add these references.
- 6) Thanks. We shall add this reference.

#### Page 7 of Mark Vaughan's review (comments to introduction and section 2)



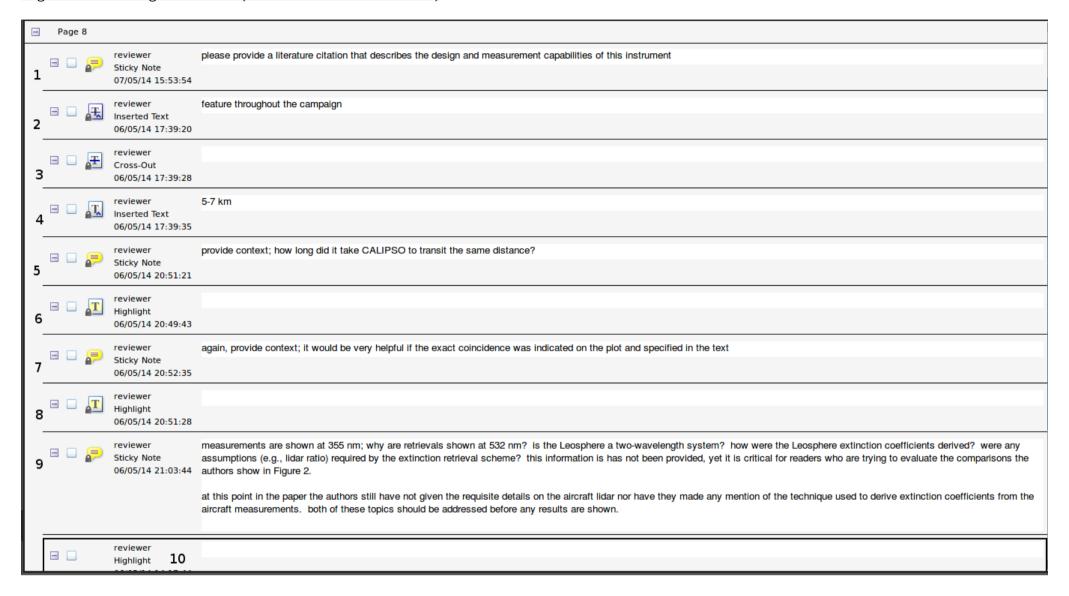
- 1) Ok.
- 2) Ok.

3)

4) We have moved the discussion on the instability of the outward solution to a separate online discussion comment. We shall in any case reformulate this sentence.

- 6) Thanks for pointing this out. We shall omit the highlighted sentence.
- 7) We shall add a table with the principal characteristics of the Leosphere ALS450. This lidar has been a success for the characterisation of clouds, volcanic ash, desert dust and smoke because it enjoys additional information arising from a full set of in situ and remote sensing instrumentation on the FAAM aircraft, which complement the lidar observations. However, we are not the manufacturers of this instrument and do not have access to its internals in detail to produce a thorough documentation like CALIPSO's. Unfortunately the instrument is not anymore under production, and we acknowledge that its datasheet seems to have been removed from the manufacturer's web site. For this reason, it will be useful to give more information here. The instrument is not calibrated, but calibration is not required for the data inversion method that we use (Marenco, 2013). The instrument is polarisation sensitive, but we have encountered difficulties into using the depolarisation channel in a quantitative sense; therefore we use depolarisation only qualitatively, and we are reluctant to display any plots of depolarisation since we are unable to calibrate it correctly (see Marenco et al, 2011, where this problem is already mentioned).

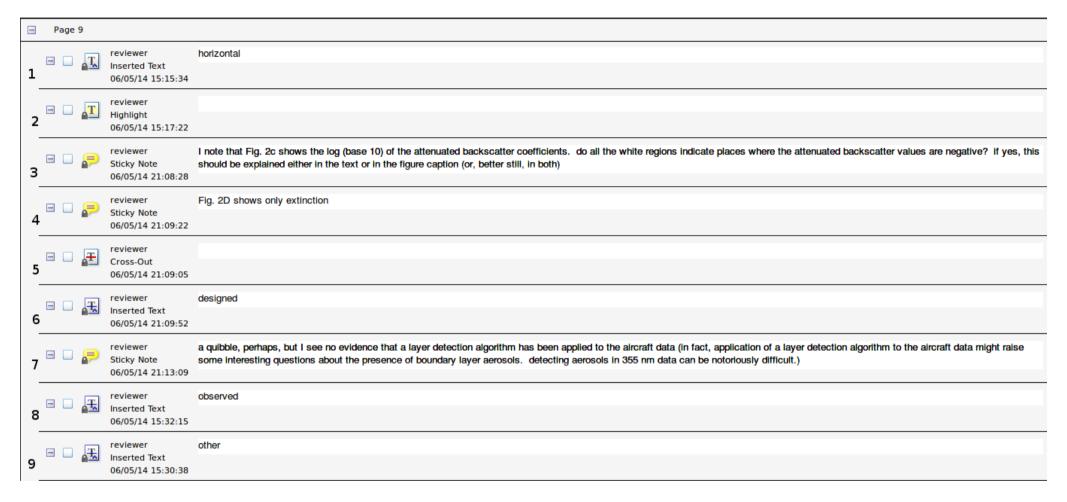
#### Page 8 of Mark Vaughan's review (comments to sections 2 and 3)



- 1) This instrument is documented in two JGR papers by Gerbig et al (1996 and 1999). References will be added in the revised paper.
- 2) Ok.
- 3)
- 4) Ok.

- 5) The CALIPSO footprint lasted 0.5 min; in the upgraded version, this shall be indicated.
- 7) The coincidence (i.e. when the aircraft is flying closest to CALIPSO) is at the latitude 10.36S. We shall add a vertical line showing it in the revised version of Figure 2. Thanks for the suggestion.
- 9) The Leosphere only measures at 355 nm. Aerosol extinction has been converted to 532 nm using the Angstrom formula for a better comparison with CALIPSO. The Angstrom exponent was derived from AERONET. This is explained in the text; in the revised version we will make sure that it is clearer. As already mentioned (our response to comment 7 on page 7 of Mark Vaughan's review), we shall add a paragraph on the Leosphere instrument, and will indicate the method used for the derivation of aerosol extinction and for wavelength scaling. This information was available in the paper at a later stage, but we agree with the reviewer that it should be explained before the results are discussed.
- 10)

## Page 9 of Mark Vaughan's review (comments to section 3)



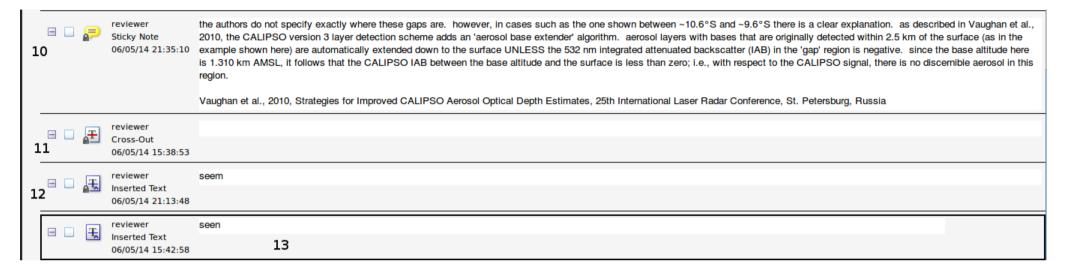
- 1) Ok.
- 3) The reviewer is correct: the white space in the discussion paper indicates negative data. In the revised version, we shall revert to a linear scale because we believe that the logarithmic scale does not improve the way the information is presented.
- 4) Correct. We shall amend the text.
- 6) Ok.

5)

7) No layer detection is applied because, unlike in the CALIPSO retrieval scheme, for the aircraft lidar the whole column is processed at once. As the scene shows broken clouds, we simply omit whole vertical profiles featuring a cloud (this is a solution ad hoc for this scene) but we do not require a layer

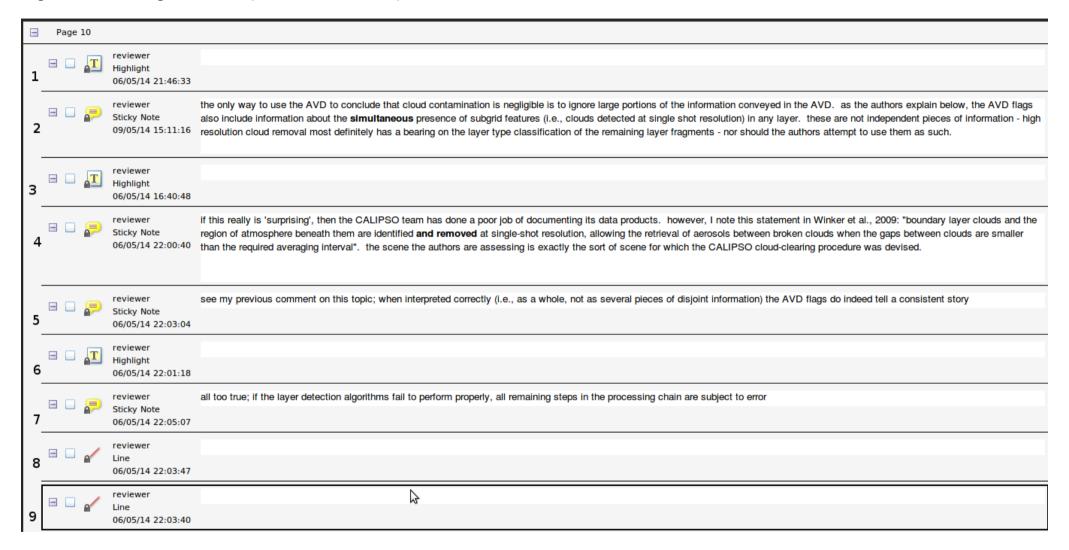
detection algorithm.

- 8) Ok.
- 9) Ok.



- 10) The gaps are shown in Figure 2d: from 11.35S to 11.1S (whole column); from 10.45S to 9.7S (surface to ~1300 m) and from 9.55S to 9.4S (whole column). We thank the reviewer for this explanation, and we shall add it to the revised paper.
- 11) Ok.
- 12) Ok.
- 13) Ok.

#### Page 10 of Mark Vaughan's review (comments to section 3)



1)

- 2) We thank the reviewer for this clarification, which we will add in the text.
- 4) We thank the reviewer for this clarification, and we shall amend the text accordingly.
- 5) We completely agree with the reviewer's comment and this is the idea that we were trying to convey. In the literature we have often seen these products used in isolation, and we were trying to show that this cannot always be done. We will try to give a more effective message in the new version of the paper.

6)7) We thank the reviewer for his comments which supports our main conclusion. See also our response to comment 1 on page 5 of Mark Vaughan's review.8)

perhaps, but not in the way the authors appear to think. see my comment in the abstract about the effects of undetected clouds on the aerosol subtyping algorithm. within the context of this scene, I suspect the aerosol subtyping is behaving exactly as it's supposed to, and the errors here are a case of garbage in, garbage out. (see Omar et al., 2009 and Liu et al., 2009; the primary inputs to both the cloud aerosol discrimination algorithm and the aerosol subtyping algorithm are layer properties calculated within the layer detection engine.)

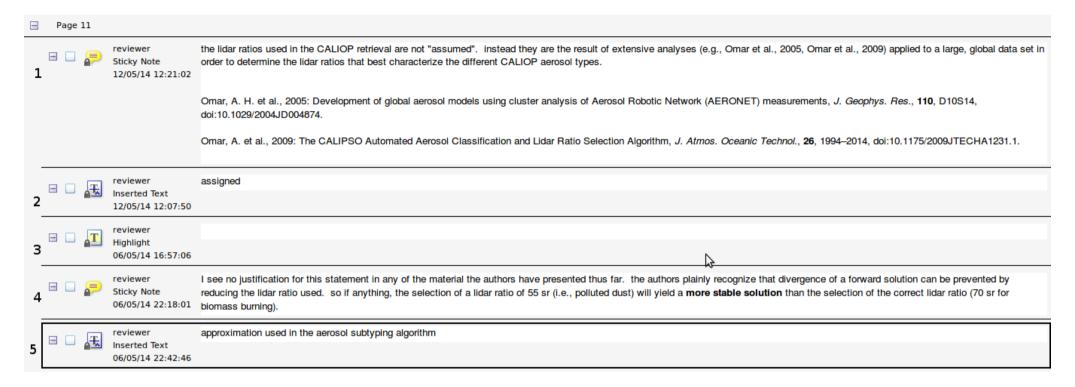
Liu, Z. et al., 2009: "The CALIPSO Lidar Cloud and Aerosol Discrimination: Version 2 Algorithm and Initial Assessment of Performance", *J. Atmos. Oceanic Technol.*, 26, 1198–1213, doi:10.1175/2009JTECHA1229.1.

Omar, A. et al., 2009: "The CALIPSO Automated Aerosol Classification and Lidar Ratio Selection Algorithm", *J. Atmos. Oceanic Technol.*, 26, 1994–2014, doi:10.1175/2009JTECHA1231.1.

10) We do not think in a particular way. We are just displaying the products and trying to connect them together so as to bring out a more complete picture that would come from taking them in isolation. What we observe is a highly variable CALIPSO extinction coefficient, with larger values near the top of the boundary layer. These are the layers where high resolution clouds are apparent in the Level 1 data, and the same layers where a "polluted dust" aerosol subtype is shown. The aerosol subtyping code is doing what it is expected to, given the imperfect removal of cloud signal at a previous stage in the retrieval chain, but this has to be considered within the context of the complete CALIPSO product (and not in isolation). The oscillation of the aerosol subtype does clearly indicate that the problem lies in the incomplete removal of cloud signal, because it is the only mechanism that could have introduced a spurious depolarisation.

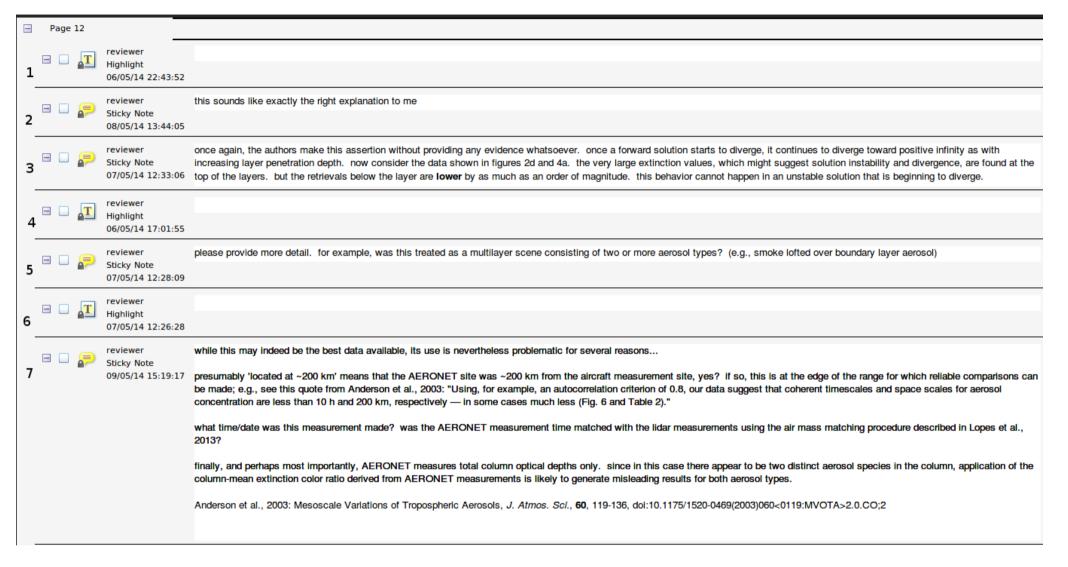
11)

## Page 11 of Mark Vaughan's review (comments to section 3)



- 1) The reviewer is correct. We shall use the more neutral word "assigned".
- 2) Ok.
- 3)
- 4) We have moved the discussion on the instability of the outward solution to a separate online discussion comment. We anyhow plan to remove this statement from the revised article, and leave the question of numerical stability until the conclusions.
- 5) Ok.

#### Page 12 of Mark Vaughan's review (comments to section 3)



1)

- 2) We thank the reviewer for his comments which supports our main conclusion. See also our response to comment 1 on page 5 of Mark Vaughan's review.
- 3) We have moved the discussion on the instability of the outward solution to a separate online discussion comment. We anyhow plan to remove this statement from the revised article, and leave the question of numerical stability until the conclusions.
- 5) No. It is treated as a single layer scene, because we believe (from evidence of a month-long campaign on a multi-instrumented aircraft) that all the

aerosols (in and above the boundary layer) are smoke, although possibly with different degrees of ageing. We shall add more details on how the aircraft data have been treated to section 2, in the next version of the paper.

6)

The reviewer is correct: the two measurements were separated by 200 km (see map in Figure 1). We agree that it would be better to have co-located measurements (unfortunately unavailable). However, we anticipate that on flights over the Amazon, where the distance covered was more than 200 km, a very large degree of horizontal coherence of the regional haze has been found during SAMBBA, except for fresh plumes just above a fire. Here, the AERONET measurement is only used to (a) verify the consistency of the Marenco (2013) method, where however AERONET is not a requirement of the method; and (b) infer an Angstrom exponent for wavelength conversion. The latter being an "intensive" property of the aerosols, it can be presumed to be consistent over a larger distance, more or less like the lidar ratio is often assumed consistent over scales much larger than 200 km. Concerning the question on timing, the AERONET data have been simply interpolated to the time of the CALIPSO overpass. Concerning the remark that two different aerosols are present in the scene, we repeat (see response to comment 5 above) that we have good reasons to believe that all the observed aerosols in this scene are smoke, although possibly with different degrees of ageing.

8		T	reviewer Highlight 06/05/14 23:06:24	
9			reviewer Highlight 07/05/14 12:48:54	<i>P</i>
10			reviewer Sticky Note 07/05/14 12:50:54	what does 'lowest layers' mean here? per an earlier comment, I see no evidence that the authors have applied a layer detection algorithm to the aircraft measurements. do the authors mean 'lowest range bins' instead of 'lowest layers'.
11		<b>,</b>	reviewer Sticky Note 07/05/14 12:53:33	be quantitative; was some fixed threshold of attenuated backscatter used to identify 'large peaks'? if so, what value was used?
12	= (		reviewer Highlight 07/05/14 12:51:58	

8)

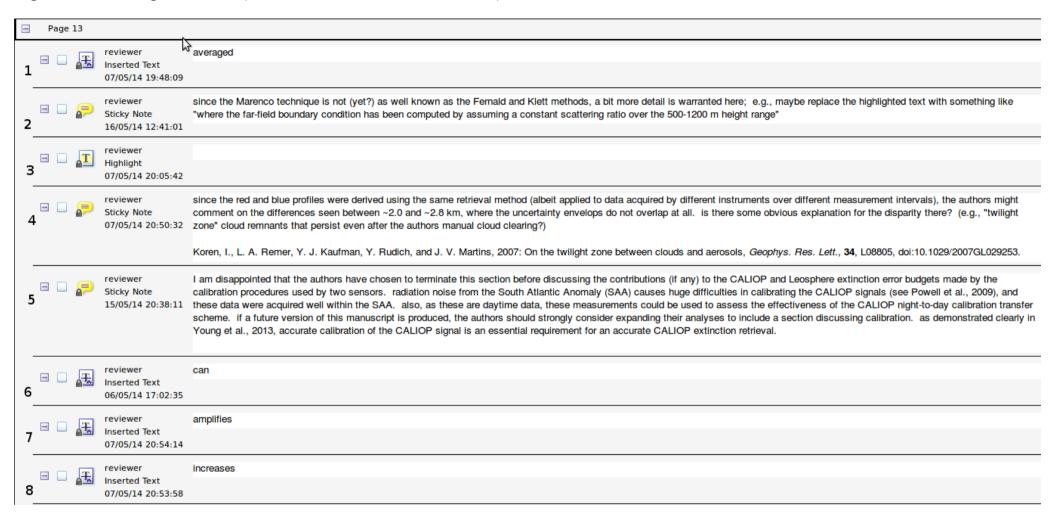
9

- 10) The method is described in Marenco (2013), with its limitations and uncertainties. As discussed above (response to comment 7 of Page 9 of Mark Vaughan's review), we do not apply a layer detection scheme because we treat the whole column at once. By lower layers, we mean the height range nearer the surface (we shall specify this better in the next version): this is where integration is started, and a large uncertainty on the starting values exists; when moving inward, however, this uncertainty quickly decreases due to the mathematical stability of the solution, and becomes independent of the boundary value and little dependent on the lidar ratio (this uncertainty is clearly depicted in Fig. 4B of the discussion paper).
- 11) Any profile presenting at least a point within the 1500-8000 m altitude range, that has attenuated backscatter larger than 60 Mm<sup>-1</sup> in both the 532 nm and the 1064 nm channels, is entirely removed before further processing. We could give the thresholds the revised paper if the reviewer feels it is useful, but

to be honest we do not believe that these are general thresholds, nor that they should be applied blindly to other scenes. This quantities have only been tested on the small scale of this experiment and not on a general basis, and this is why we are not so keen to release them.

12)

## Page 13 of Mark Vaughan's review (comments to section 3 and conclusions)



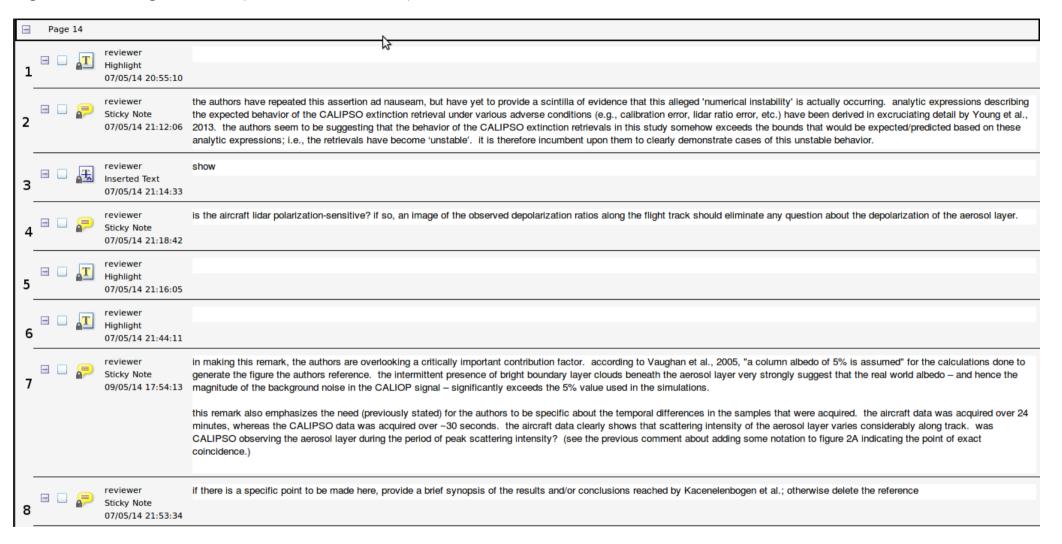
- 1) Ok
- 2) Ok

3)

4) We have been puzzled by this difference as much as the reviewer, but we have not found an explanation. The difference is much larger than the uncertainty of the retrieval method, and hence is not to be considered due to the choice of the far end reference. For both datasets, cloud screening is done by completely removing whole vertical profiles affected by cloud, and therefore we do not believe that the discrepancy results from this procedure. It is quite possible that this is due to the "twilight zone" consisting of hydrated aerosols with different optical properties than the rest (lidar ratio and wavelength dependence): in that case, it is possible that retrievals at 532 nm and 355 nm yield a profile looking different. We thanks the reviewer for

- pointing this out, and we shall add a comment in the revised paper.
- 5) We thank the reviewer for this comment; however, we have never expressed any doubt about CALIPSO's calibration procedure, which we consider very convincing. We have to point out, however, that data inversion using the Marenco (2013) method, similarly to data inversion with the traditional Fernald-Klett method, does not rely on calibrated lidar signals. If the signal is multiplied by a factor of 10, 100, or 1000, the derived extinction coefficient results identical. Therefore, of the three curves in Figure 4b, it is only the green curve that relies on calibration due to the way it is computed in HERA. Since the blue and green curves show quantitative agreement, where both are derived from the CALIOP dataset although with very different methods, and only the latter relies on calibration, we can say that the comparison tends to suggest that CALIPSO's calibration transfer is accurate. One more point has to be mentioned, however: when looking at layers near the surface, the lidar signal is affected by extinction in the overlying layers. In this scene, a layer of AOD 0.03 is missed by the CALIPSO layer classification scheme; this is equivalent to a 6% (e<sup>2\*</sup>) error on the instrument calibration when processing the lower layers; despite this, the green and blue curves show quantitative agreement. We do not believe that this brief comparison can reveal such small differences in the space lidar calibration, and our conclusion to the reviewer's question is that CALIPSO appears having a good calibration despite the SAA.
- 6) Ok.
- 7) Ok.
- 8) Ok.

## Page 14 of Mark Vaughan's review (comments to conclusions)



1)

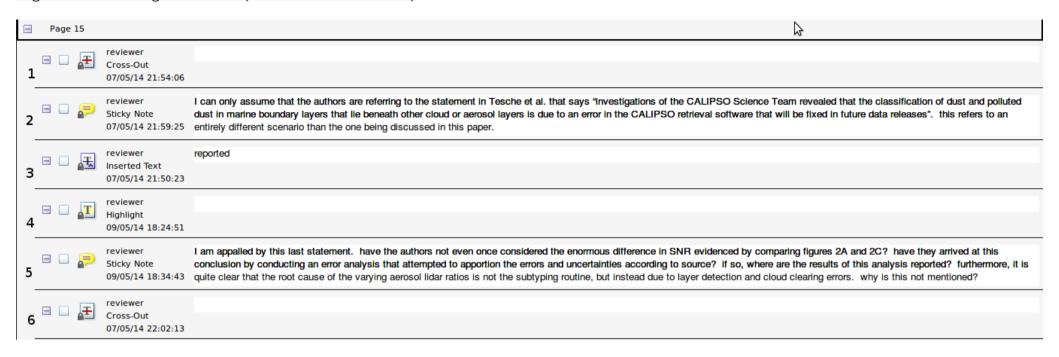
2) We have moved the discussion on the instability of the outward solution to a separate online discussion comment. We admire the CALIPSO data analysis framework (calibration, layer selection, cloud-aerosol discrimination, aerosol subtyping and inversion) because it is very powerful and it can be applied automatically to large global datasets; this is a capability that we do not have for the aircraft lidar, where we require human intervention in the data processing and interpretation. We have never intended to stir doubt about the hard efforts from the CALIPSO science team to get the best possible product. We have not said that the behaviour of the CALIPSO retrievals exceed the bounds of the estimated uncertainty; we had just added an element of discussion mentioning one of the potential origins of the large horizontal variability that is observed with CALIPSO (Fig. 2d) and is not picked up

with the airborne lidar (Fig. 2b). It has to be taken as an element for discussion rather than a criticism of the CALIPSO data analysis framework.

- 3) Ok.
- 4) The instrument is polarisation sensitive, but we have encountered difficulties into using the depolarisation channel in a quantitative sense; therefore we use depolarisation only qualitatively, and we are reluctant to display any plots of depolarisation since we are unable to calibrate it (see Marenco et al, 2011, where this is already mentioned; see also our response to comment 7 on page 7 of Mark Vaughan's review).
- 5) 6)
- 7) We thank the reviewer for pointing out that the specification for CALIPSO refers to an albedo of 0.05, and we shall add this information in the revised paper. We shall also ask a vertical line indicating coincidence in the figure.
- 8) Kacenelebogen et al (2011) have found a misclassification of fine and strongly absorbing aerosol as dust and polluted dust; this is similar to our finding, although probably driven by different causes. We feel therefore that it is ok to reference this paper. We shall re-phrase the sentence.



#### Page 15 of Mark Vaughan's review (comments to conclusions)

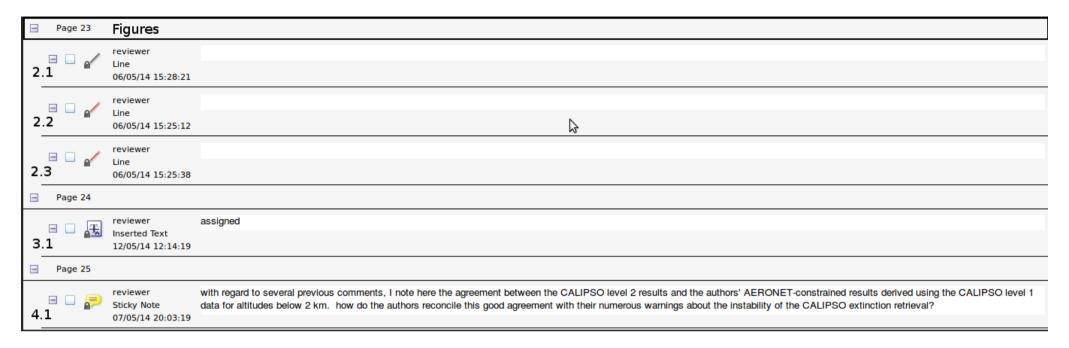


1)

- 2) Tesche et al have found cases of misclassification of marine aerosols as dust or polluted dust, in the presence of clouds. A software bug has been found responsible for this misclassification, and therefore that case, although apparently similar, is different than ours. This what we are saying, and we do not understand what is wrong with our statement. The discussion tries to put our findings into a broader context, and it is appropriate to quote cases that are related or similar, but not identical. We shall re-phrase the sentence.
- 3) Ok.

- 5) We do not understand the reviewer's comment. Please read again our "second remark" (the previous paragraph) where the causes of varying subtype are clearly analysed in the same direction that the reviewer indicates. The present paragraph is to be read together with the previous one. We agree that the subtyping routine is being mislead by a previous stage of the data analysis (the layer detection routine), and this is exactly the message that we are trying to give. We will also mention shot noise, as suggested by the reviewer.
- 6) We have moved the discussion on the instability of the outward solution to a separate online discussion comment.

Pages 23, 24 and 25 of Mark Vaughan's review (comments to figures)



2.1,2.2,2.3) The reviewer has added three lines to our figure. We presume that he wants to highlight the bit of the elevated layer that CALIPSO has detected, but it is unclear to us what message he tries to convey.

## 3.1) Ok.

4.1) Please note that this figure shows a horizontally averaged profile over an area ~200 km long, whereas individual profiles show a larger variability (Fig. 4a). We have moved the discussion on the instability of the outward solution to a separate online discussion comment; in that comment we also display the profiles in Fig. 4a in an expanded graphical form, so as to make them easier to read.