Authors' response to the second review of

"Airborne verification of CALIPSO products over the Amazon: a case study of daytime observations in a complex atmospheric scene"

<u>Reviewer 1</u>:

We very much thank reviewer 1, for his or her appreciation of our manuscript, and for his or her recommendation for it to be "accepted as is".

<u>Reviewer 2</u>: see the following pages

Second Review of "Airborne verification of CALIPSO products over the Amazon: a case study of daytime observations in a complex atmospheric scene" by F. Marenco, V. Amiridis, E. Marinou, A. Tsekeri, and J. Pelon

In the revised version of this paper the authors have gone to great lengths to respond to the many suggestions and change requests that I provided in my initial review, and as a result have, I believe, greatly improved the manuscript. Nevertheless, I also believe that some further changes are required before the paper is published. As with my earlier review, I am once again returning an annotated version of the manuscript that contains a number of comments that address very specific points. Below I offer a list of some more general comments that I hope will provide context for understanding many of the specific points in the annotated manuscripts.

(a) Throughout the paper, the authors' descriptions of their data handling procedures are too vague. Furthermore, there is a general lack of specificity in the descriptions of the figures.

This is a topic that I mentioned in various places in my initial review. For example, on the last line on page 9211 of the paper published in ACPD (where the authors describe their cloud clearing procedure) I make this comment: "be quantitative; was some fixed threshold of attenuated backscatter used to identify 'large peaks'? if so, what value was used?". (The authors will find a very similar comment in the revised manuscript at line 248.) The authors respond as follows:

"We could give the thresholds [in] 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 to keen to release them."

I certainly understand the very valid concerns they raise. On the other hand, readers (or reviewers!) wanting to unambiguously replicate the authors' findings must have the exact information required to do so. My view recommended solution for this conundrum is to fully specify the method used for cloud clearing (including the threshold values used) *and* to include in the text all necessary caveats about the appropriate of the method that was used. For example, after describing their cloud clearing procedure the authors might say that "while this simple thresholding scheme is demonstrably effective for this specific data segment, we cannot and do not advocate its general use in more complex scenes."

While I've used cloud clearing as an example, there are a number of other places where I believe the readers' comprehension of the manuscript will be improved by the inclusion of additional detail. In particular, please see lines 94, 104–119 (this section needs lots of attention), 123, 240 and 248.

(b) The authors are, I think, far too hasty in attributing variabilities in the CALIOP extinction coefficients to misclassification of aerosol type and hence incorrect assignment of lidar ratio (e.g., see lines 151–152, 201, 277 and elsewhere). I suggest that a far more important contributor to the observed variability is the underlying SNR of the CALIOP level 1 data. To illustrate my point, I have filtered the CALIOP level 1 data using to the author's cloud clearing technique and plotted the results below in Figure 1. A visual inspection of these plots, along with an investigation into the averaging required for layer detection in the extinction retrieval (see the figure caption for details) is certainly sufficient to make a prima

facie argument for signal variability being the dominant cause of variability in the derived extinction profiles. So if the authors wish to make the case that (lines 201–203) "it is rather evident [...] that the classification of what is a homogeneous smoke layer into different aerosol subtypes is connected to the large inhomogeneity in the retrieved backscatter and extinction coefficients" – i.e., that the CALIOP level 2 algorithms are somehow responsible for the "excessive spatial variability" they observe (line 277) – they should first evaluate the contributions made by other sources (e.g., SNR and calibration).



Figure 1: left panel shows CALIOP level 1 data filtered using the authors' cloud clearing criteria then averaged to a 20-km horizontal resolution. The CALIOP data is from the daytime orbit segment on 2012-09-20 beginning at 17:39:12 UTC and extends from ~11.5°S to ~9.3°S. The right panel shows the same CALIOP level 1 data averaged to an 80-km horizontal resolution. For comparison, the center panel shows all extinction profiles retrieved for this same orbit segment. For the extinction retrievals, 4% of the aerosols were detected at a 5-km horizontal averaging resolution, 34% were detected at 20-km, and 62% were detected at 80-km.

(c) There are still some places where the authors make assertions without offering any evidence to convince the reader of the truth of their statements. For example, see lines 323–324 and line 336. These are both important considerations, and if demonstrated to be true (or even highly likely) are worthy of attention by all prospective CALIOP data users. However, the authors do not present evidence to support either one.

In closing I will make two final comments. First I would like to thank the authors for their indepth and insightful discussion of the relative merits of forward (outward) solutions versus backward (inward) solutions. And finally and most importantly, in case the authors (and/or the editors) should have any doubt, let me be clear: I very firmly believe that (a slightly revised version of!) this paper should – indeed, must – eventually be published. Because CALIOP provides the only global, long-term measurements of the vertical distribution of atmospheric aerosol loading, validation of the CALIOP aerosol profile products is a critically important task. At the same time, however, the logistics and expense of making the required validation measurements make this a very difficult and challenging undertaking. Although admittedly sparse, the SAMBBA data set provides a fine opportunity to make meaningful comparisons and the authors have made impressive progress in doing so. We thank Mark Vaughan for this second, very detailed review of our article. We are also grateful for his strong words in appreciation of our work, and of the importance of validation of the CALIOP aerosol product. Mark Vaughan believes that we have greatly improved our manuscript, and that a slight revision is needed before publication. Most of the corrections in the annotated manuscript have been directly implemented; however, some of them require a clarification which is given in this response document. We have dealt with all reviewer points.

General comments:

- (a) We have now clarified our data handling procedure following the reviewer's direct advice in the annotated manuscript. In particular, for cloud screening, we have followed the reviewer's advice.
- (b) We believe this comment not to be completely fair. We acknowledge that one of our sentences was using the ambiguous word "connected" (see response to comment 4 on page 7), but we have clearly not indicated the lidar ratio selection as the single cause of variability in CALIPSO for this scene. Mention of the low SNR (due to daytime operation and the underlying cloud field) is certainly not missing in our article; for instance see our "third remark" in the conclusions (and this point is repeated throughout the paper when necessary). On the other hand, it cannot be denied that varying the lidar ratio has a relevant effect on the retrieved extinction profile (see e.g. our figure attached to our response to comment 10, page 10). Therefore, we maintain that a concurrence of causes has to be considered for the discussion to be complete.
- (c) We have dealt with the two points indicated by the reviewer, and hope that they are now clarified.

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Airborne verification of CALIPSO products over the Amazon: a case study of daytime observations in a complex atmospheric scene

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- 5 layer is topped with small broken stratocumulus clouds. In this complex scene, a comparison of observations from the airborne and spaceborne lidars reveals a few discrepancies. The CALIPSO detection scheme tends to miss the elevated thin layer, and also shows several gaps ($\sim 30\%$) in the boundary layer. The small clouds are not correctly removed from the signals; this can cause the CALIPSO aerosol subtype to oscillate between smoke and polluted dust and may introduce distor-
- 10 sion in the aerosol retrieval scheme. The magnitude of the average extinction coefficient estimated from CALIPSO Level 2 data in the boundary layer is as expected, when compared to the aircraft lidar and accounting for wavelength scaling. However, when the gaps in aerosol detection mentioned above are accounted for, we are left with an overall estimate of aerosol extinction for this particular scene that is of the order of two thirds of that determined with the airborne lidar.

Summary of Comments on acp-2014-192-refereereport-2.pdf

Page: 1

| Number: 1 | Author: reviewer | Subject: Sticky Note | Date: 11/08/2014 14:19:06 | | | |
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| suggest characterizing both layers the same way; i.e., either in terms of optical depth or in terms of extinction, but not a mixture of the two. if | | | | | | |
| extinction is used, please specify the type of metric (e.g., max, median, mean, etc.) | | | | | | |
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Number: 3 optical depth

15 1 Introduction

Biomass burning is the second largest source of anthropogenic aerosols on Earth (Houghton et al., 2001). The Fourth Assessment Report of the Intergovernmental Panel on Climate Change reports a global radiative forcing (RF) contribution of roughly $+0.03 \pm 0.12$ W/m² for biomass burning aerosols (Forster et al., 2007), whereas the Fifth Assessment Report estimates this contribution to be

- 20 $\pm 0.2 \text{ W/m}^2$ (Stocker et al., 2013). Textor et al. (2006) showed that there are still significant uncertainties in the aerosol vertical distribution in global models, whereas this information is critical in assessing the magnitude and even the sign of the direct RF. Of particular interest are the distribution of lofted layers (Mattis et al., 2003; Müller et al., 2005; Baars et al., 2012) and the identification of complex scenes involving both aerosols and clouds (Chand et al., 2008). The large amount of
- 25 heat released by forest fires can generate strong updrafts and deep convection in their vicinity, with a rapid transport of aerosols to upper layers (Freitas et al., 2007; Labonne et al., 2007; Sofiev et al., 2012). These aerosols, in turn, have an impact on cloud formation, convection, and precipitation patterns (Andreae et al., 2004; Koren et al., 2008).
- Since 2006 the Cloud-Aerosol Lidar with Orthogonal Polarization (CALIOP), on-board the Cloud30 Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO) satellite, has provided an invaluable global dataset on the vertical structure of the atmosphere (Winker et al., 2010, 2013). Several studies have appeared recently, with the goal of evaluating CALIPSO products using ground-based lidar (Kim et al., 2008; Pappalardo et al., 2010; Tesche et al., 2013), AERONET (Mielonen et al., 2009; Schuster et al., 2012; Omar et al., 2013; Lopes et al., 2013), other satellite sensors
- 35 (Kittaka et al., 2011; Redemann et al., 2012; Kim et al., 2013; Jethva et al., 2014), research aircraft (Burton et al., 2013; Amiridis et al., 2012), or comprehensive multi-platform experiments (Kacenelenbogen et al., 2011; Amiridis et al., 2013).

CALIOP has two operational wavelengths: 532 nm and 1064 nm, and at the first one it has dual polarisation capability (Hunt et al., 2009; Winker et al., 2010). Accurate nighttime calibration of the

40 principal channel at 532 nm is obtained via molecular normalisation at stratospheric levels, and the calibration is then transferred to the other channels (Powell et al., 2009). As for most lidars, daylight acts as a disturbance to the signal returns, and hence reduces the signal-to-noise ratio (SNR), with the consequence that CALIPSO's nighttime data have a superior quality to the daytime data. Scenes with a large planetary albedo, as e.g. those with cloud cover, will be dominated by a larger amount of daylight entering the detectors, and thus will present an even poorer SNR.

For the first time, a global and fully automated lidar data inversion procedure has been designed. CALIOP's data analysis package automatically identifies aerosol and cloud layers, and this information is stored as the vertical feature mask (VFM) and atmospheric volume description (AVD) flags (Liu et al., 2009). For aerosol layers, one of six aerosol subtypes is identified (clean ma-

50 rine, dust, polluted continental, clean continental, polluted dust, and smoke), and they determine the extinction-to-backscatter ratio (lidar ratio) based on a look-up table (Omar et al., 2009). Using

the lidar ratio (and its uncertainty associated with the identified aerosol subtype), extinction and backscatter profiles are computed using the Hybrid Extinction Retrieval Algorithms, HERA (Young and Vaughan, 2009; Young et al., 2013). This is an iterative method that solves the lidar equation

- 55 for a two-component atmosphere, with an integration that starts at the top of the atmosphere and works its way down to the surface. However, the outward solution of the lidar equation can lead to mathematical instability and divergence (Fernald, 1984; Marenco, 2013), and in the attempt to keep these unwanted effects under control, a mechanism for iterative adaptation of the lidar ratio is applied when such instabilities are detected (Young and Vaughan, 2009; Young et al., 2013). This
- 60 marks a difference with the classical outward solution with a pre-assigned lidar ratio, and the latter is decreased as is needed to reach stability, and offers the advantage of exploting the forward inversion down to the surface, in terms of vertical extension. This procedure, although stable, may present some signature of the unstable one.

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In this paper we examine an underpass of the CALIPSO satellite by the Facility for Airborne At-

65 mospheric Measurements (FAAM) BAe-146 research aircraft, during a daytime flight in the Amazon basin during the biomass burning season. Although limited, dis dataset gives a good insight on some critical aspects that may be associated with CALIPSO retrievals and the characterisation of aerosol subtypes.

2 Aircraft observations

- 70 In September and October 2012 the South AMerican Biomass Burning Analysis (SAMBBA) campaign was carried out in Brazil, and several observations were made during 20 science flights using both *in situ* and remote sensing techniques Ingelo, 2012)
- 75 wing-mounted optical particle counters (PCASP and CDP; see, e.g., Liu et al., 1992; Lance et al., 2010) showed a predominance of fine mode particles. Moreover, measurements with the on-board AL 5002 VUV Fast Fluorescence CO Analyser (Gerbig et al., 1996, 1999; Palmer et al., 2013) showed high carbon monoxide concentrations. No strong depolarisation signal has been observed in the aircraft lidar returns, except when observing optically thick layers where multiple scattering
- 80 is non-negligible (clouds and very thick smoke). A general feature throughout the campaign was the persistence of aerosols above the boundary layer, with thin plumes up to altitudes of 5–7 km, presumably due to lifting via deep convection.

On 20 September a complex flight was carried out, taking off from Porto Velho, Brazil, and overflying the Amazon for three hours and 45 minutes (flight number B737, see Fig. 1). Most of

85 the flight was devoted to characterising a large natural wildfire, but towards the end a 230 km long underpass of CALIPSO was performed (this distance was covered in 33 seconds by CALIPSO, and

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| ſ | how does a stable | solution 'present so | me signature of [an] unstable | one'. if this is possible, then can the stable "Marenco method" also present | |
| | some signature of | an instable solution | ? if not, why not? please expl | lain. | |
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| ſ | here's what I'll be le | ooking for as I read | the remainder of this new dra | ft: do the authors attempt to put the flaws they identify in the retrieval of | |
| | this one scene into some larger context? that is, do they provide any estimates for how likely a data user is to be confronted by these flaws, and | | | | |
| | what are the down | stream enects likely | to be in/when a user is so con | infonced? | |
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| | this appears to be a news release, not a peer-reviewed journal article or even a conference proceedings paper. is there no journal or conference paper that describes the overarching goals of the SAMBBA campaign and details the instrumentation carried aboard the aircraft? (I must admit that my quick search using Google Scholar did not turn up any promising candidates.) | | | | |
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Page 3 - response to the reviewer's comments

- 2. The forward method is numerically unstable when no feedback is included. However, in the very obvious cases of instability, in the CALIPSO retrievals the lidar ratio is adjusted until stability is reached, so that major instabilities are avoided. Elements of minor instability may still be present, but to facilitate the present review, since this sentence is "controversial" and not essential for the manuscript, we have preferred omitting it.
- 5. We agree that there is sufficient CALIPSO data, to offer the chance of working out a climatology of cases of smoke below a broken cloud field in the Amazon in the biomass burning season, and their representation in the level 2 product. This work is however outside of the scope of the present paper, and cannot be allocated within our current resource plan. Please see also comment 13 on page 10.
- 6. The paper is precisely being submitted to the special issue related to the SAMBBA project. However, at present no campaign overview paper exists to the authors' knowledge. A conference proceeding abstract has been added in the references.

24.5 minutes by the aircraft). This paper focuses on the latter part of the flight (Run 19), when clouds and aerosol layers have been mapped with the airborne lidar looking down from 6500 m.

An ALS450 lidar system, manufactured by Leosphere, was used on-board the aircraft, looking 90 down at nadir (Marenco et al., 2011). For a description of the lidar system, see Chazette et al. (2012); see also Table 1 for the system's specifications. The receiver implements two channels, for the detection of the elastic backscatter in both the co-polar and the cross-polar planes, relatively to the emitted radiation. Unfortunately, the system suffers large temperature variations during a research flight, which affect the depolarisation signal strongly this reason it is not possible to

95 use depolarisation quantitatively (it cannot be calibrated) and depolarisation information is used qualitatively.

Lidar signals have been acquired with an integration time of 2 s (40 laser shots) and a vertical resolution of 1.5 m; to reduce random noise, all vertical profiles have been further smoothed with a 30-point running average. The range-corrected lidar signal that is displayed in the present paper has

100 therefore a horizontal resolution of 0.3 km (2 s at ~ 150 m s⁻¹, speed of the aircraft) and a vertical resolution of 45 m. For this product, the signal-to-noise ratio (SNR) is larger than ~ 5 on the whole atmospheric column, for a daytime cloud-free profile with moderate aerosol load (aerosol optical depth, AOD ~ 0.3), when looking down from an altitude of 6500 m.

Lidar signals have been integrated to a 10 s resolution (1.5 km footprint) for further analysis.

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- 105 Cloud screening has been done by discarding whole vertical profiles at the 10 s resolution, they involved oud signal. The lidar signals have then been processed with the method described in Marenco (2013), signal lidar ratio of 75 sr, appropriate for biomass burning aerosols (Groß et al., 2012, Fig. 14); this processing is achieved for whole vertical profiles at once.
- Finally, to offer a better comparison with the CALIPSO product, we have converted the the extinction coefficient obtained with the aircraft lidar to 532 nm; the conversion is achieved by applying 12 olour ratio ved from the nearby AERONET station in Porto Velho. This wavelength conversion has to be considered approximate, because the spectral absorption properties of the aerosols may vary; moreover, the AERONET site is located ~ 200 km to the Northwest (see, e.g., Anderson et al., 2003 for the coherent spatial scales for aerosol magnetic for the Amazon have shown a large degree of coherence of the regional haze over distances of several hundreds of kilometers, and (ii) colour ratio

is an intensive property of the aerosols, and thus presumed to be consistent over large scales (better

- 3 Results
- 120 Fig. 2(a) shows the range corrected signal measured from the airborne lidar at 355 nm. A thin elevated aerosol layer is highlighted at 4500–5000 m with some other thinner layers underneath it

than extensive properties such as concentration and AOD).

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| suggest adding m determine any dov | ore description here wnstream conseque | . without some explanation o nces for the total (i.e., parallel | f how the depolarization measurements are affected, it's hard for readers to plus perpendicular) signal measurement | |
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| now is cloud sign | | stovide a quantinable metric. | | |
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| figure 14 in Groß e appropriate refere | et al. shows lidar rati nce for what is presi | os for a mixture of smoke and umably a dust-free smoke laye | dust that shows appreciable depolarization (15-20%), and thus is not an er that shows "no strong depolarization signal". | |
| I would think that burning in the Am | the measurements a azon (e.g., see sectio | and retrievals in the Baars et al on 3.1 and Figure 5). | . 2012 paper would provide a much better estimate for lidar ratio of biomass | |
| 👝 Number: 10 | Author: reviewer | Subject: Sticky Note | Date: 12/08/2014 17:51:35 | |
| what is the value of were these data ad | of this color ratio? (i cquired? is the value | e., $\chi \pm \Delta \chi$) how many AERON e used consistent with extinction | ET measurements were used to compute this value, and over how many days on color ratios derived from smokes measured by other AERONET stations? | |
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| the authors need the parameters. | to provide more det | ails in this section about their | data handling procedures and their calculations of important conversion | |
| TNumber: 12 | Author: reviewer | Subject: Highlight Date: 11 | /08/2014 18:09:44 | |
| Number: 13 there are several r | Author: reviewer | Subject: Sticky Note | Date: 12/08/2014 17:55:18 | |
| there are several p | ioblems with this. | | | |
| first, because the authors are examining an isolated example, the spatial coherence of other flights has no obvious bearing on this one. is there some metric the authors can use to assess the spatial coherence of the aerosol layers in this flight? | | | | |
| second, 200 km is approaching the outer limit of the spatial coherence criteria suggested by Anderson et al. (see section 4: "Using, for example, an autocorrelation criterion of 0.8, our data suggest that coherent timescales and space scales for aerosol concentration are less than 10 h and 200 km, respectively — in some cases much less (Fig. 6 and Table 2).") | | | | |
| finally, if only "some of [the] flights" show long coherence lengths, then presumably others do not. the authors should offer some evidence to convince the reader(s) that this particular flight is one of the long coherence ones. | | | | |

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Page 4 - response to the reviewer's comments

- 1. When temperature in the aircraft hold varies (due to the aircraft changing altitudes), the ratio of depolarised signal to the non-depolarised signal varies: this is already stated in the manuscript. For non-depolarising aerosol layers (as those observed here), it is believed to affect the total signal measurement in overall magnitude, but it is believed to have little impact on the retrieved extinction profile (inversion method not requiring calibration). A sentence has been added at the end of the paragraph on the latter.
- 5. Information has been added as requested. Note that a profile-by-profile review of the data is done manually in this case for the aircraft lidar data, where the criteria can be overridden.
- 9. We thank the reviewer for pointing out the Baars et al reference, which we have added. It is true that Groß et al refer to a smoke mixture.
- 10. We have now better specified how the colour ratio is computed and have given a standard deviation for it.
- 11. The points above should address the concerns expressed by the reviewer.
- 13. We thought to have already answered this point in our previous response to the reviewer (online discussion, response to point 7 on page 12 of Mark Vaughan's review, <u>http://www.atmos-chem-phys-discuss.net/14/C4357/2014/acpd-14-C4357-2014-supplement.pdf</u>.

We'll try to be more detailed in the present response. The paper by Anderson et al. discusses the autocorrelation of aerosol <u>extinction</u>, <u>optical depth</u> and <u>radiative forcing</u> on a global scale, for the purpose of their climatic representation. In general, aerosol properties are divided into extensive and intensive. Extensive properties depend mainly on the "quantity" of aerosols and are, e.g., concentration, extinction, optical depth, radiative forcing, etc. Intensive properties depend mainly on the microphysical properties of the aerosols and are generally independent from "quantity". Among the intensive properties of aerosols, we have their composition, size-distribution, lidar ratio, colour ratio, etc. The paper by Anderson et al characterizes the autocorrelation of the <u>extensive</u> properties of the aerosols (see their footnote number 1), and relies on measurements of extinction by nephelometer and of optical depth by lidar.

It is generally assumed that intensive properties of aerosols show a much smaller variability in time and space than extensive properties; in other words, that their autocorrelation distance and autocorrelation time are larger. This is fortunate for people working with remote sensing, because they can exploit an "average" value of the intensive properties to derive the extensive properties. In the present case, we rely on the colour ratio for a wavelength conversion (because it is useful for a comparison). Colour ratio is an <u>intensive property</u>; we have also put all the necessary caveats in the paper, and we state that the conversion is approximate. To make an example of direct knowledge to Mark Vaughan, for instance, the CALIPSO algorithms rely on lidar ratios, observed at a finite number of ground-based sites, where AERONET is available, to characterize all the aerosols in the globe (see Omar et al, 2009). This is an even bigger generalization of aerosol properties, and it goes far beyond the 200 km autocorrelation distance from those specific sites: in our opinion, it is fortunate that this generalisation can be made. We therefore do not understand the objection.

The reason, why the spatial coherence from other flights had been added to the manuscript, is precisely to respond to the reviewer's remark on this point. The reviewer is also misinterpreting our sentence on "some of our flights", and we have now omitted the word "some" to make things clearer. The reason why only "some" flights showed evidence of spatial coherence is that not all flights travelled a long distance. We don't have any metric to offer for the time being, but a more general characterization of the properties of the Amazonian haze during SAMBBA will be the object of future papers.

We hope that this point is now clearer for the reviewer.

but well above the boundary layer. The elevated layer has actually been is served by lidar during all the high altitude portions of this flight. At the top of the boundary laye is served by lidar during all clouds can be noticed (stratocumulus), displayed in dark red since their lidar returns are very large

- 125 and saturate the colour scale. The size of the clouds can be estimated from the airborne lidar: their along-track horizontal extent ranges from ~ 0.3 to 5 km (median 1.2 km), except for a wider cloudy area at the Northern end that has a horizontal extent of 20 km. Cloud cover is estimated to be 36% (fraction of aircraft lidar profiles where a cloud is detected). Low returns are found in the boundary layer (blue colour): one could be mislead into thinking that they could be indicative of a clean layer;
- 130 however, the opposite is true. The low returns are triggered by attenuation through a moderately turbid layer, and are indicative of aerosol load. The information on the aerosol distribution can be better visualised in Fig. 2(b), in terms of extinction coefficient, which can be interpreted in a more straightforward way. The aerosol signal shows an overall horizontal homogeneity over the area under study, but a weak horizontal gradient can be observed for the elevated layer (thicker at the Southern 135 end, and nearly undiscernible in the North).
 - It is interesting to compare this atmospheric structure to the CALIPSO returns, displayed in Fig. 2(c) in terms of the 532 nm attenuated backscatter (Level 1 dataset). One is surprised to notice that none of the aerosol layers detected by airborne lidar is evident, and indeed only the cloud returns are apparent. We will show, however, that information about the atmospheric layers is not lost, but
- 140 when it is displayed in this plot, the aerosol signal is hidden by the amplitude of shot noise. Fig. 2(d) shows the result of the inversion into extinction coefficient, spectively, as computed with the CALIPSO algorithms (Level 2 dataset, version 3.02). This product is designed to yield aerosol properties only, after the removal of cloud signals from the lidar returns. The following observations can be made:
- An elevated layer at 4000-4500 m is observed at the Southern end. However, this layer is not detected at the other latitudes where the aircraft has observed it.
 - Boundary layer aerosols are detected, but with some gaps that do not find a justification in comparison with a earborne dataset. The gaps can be observed in Fig. 2(d) 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). They represent ~ 30% of the boundary layer during the underflight.
- 150
- Garge horizontal variations of the backscatter and extinction coefficient are observed.

The first two points can be understood in relation with CALIOP team presentations (Vaughan 155 et al., 2009) and a comment in Pappalardo et al. (2010), where it is stated that not all structures in the CALIPSO Level 1 attenuated backscatter profiles get a representation in terms of Level 2 products, since the identification of features depends on their optical and geometrical properties as well as

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| specify altitude; e. | .g., at ~2.4 km | | | | |
| Thumber: 3 | Author: reviewer | Subject: Cross-Out Date: 11 | /08/2014 18:40:14 | | |
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| TNumber: 4 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 18:41:25 | | |
| are not observed | in | | | | |
| 画 Number: 5 | Author: reviewer | Subject: Sticky Note | Date: 11/08/2014 21:45:18 | | |
| I'm assuming this observation applies only at the top of the boundary layer. if it is meant to apply more globally, then the variations in the | | | | | |
| retrieved backscatter and extinction coefficients should be compared to the variations in the measured attenuated backscatter coefficients. if the latter are "large", then the former will also and necessarily be "large". | | | | | |
| TNumber: 6 | Author: reviewer | Subject: Highlight Date: 11 | /08/2014 21:42:45 | | |
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at the top of the boundary layer

Page 5 – response to the reviewer's comments

5. We confirm that the horizontal variability is mostly seen at the top of the boundary layer, as the reviewer has himself been able to verify from our figures; this has now been made clearer in the text.

the signal-to-noise ratio. The signal-to-noise ratio could be for instance reduced by cirrus above the aerosol layer (Kim et al., 2008); we have verified the dataset, however, and $2 \operatorname{rrus}$ is not seen be

- 160 latitudes of the underflight with the research aircraft. A thin high cirrus (not shown here) is observed instead at the Southern latitudes, where the elevated layer is actually found in the Level 2 data as well. The gap between 10.45S and 9.7S (below ~ 1300 m) can be better understood in connection with the findings of Vaughan et al. (2010): the CALIPSO version 3 layer detection scheme adds an aerosol base extender algorithm. If the base of an aerosol layer is within 2500 m from ge surface,
- 165 it is automatically extended down to the curfate, unless the 532 nm integrated backscatter for the 'gap' region is negative. We must deduce for the present scene, the integrated backscatter must be less than zero, and therefore with respect to the CALIPSO signal there is no discernible aerosol in this region.

Note that the aerosol layers in the CALIPSO Level 2 dataset generally show good quality indices for this scene. For all aerosol layers shown here, the extinction quality control flag is zero, meaning quality assured trieval (geconstrained and not requiring prative adaptation of the lidar ratio and 11 extinction uncertainty is less than 0.5 km⁻¹. For ever, the cloud-aerosol discrimination (CAD) scores, Fig. 3(a), suggest that there is little doubt about the layer classification as aerosol. The more negative the CAD score (the closer to -100) and the higher the confidence that the observed

175 layers should be treated as aerosols. All CAD scores for this scene fall below -93, except for the layer displayed in orange colour for which CAD = -74. Cloud contamination of the profiles is therefore apparently negligible, as also highlighted in the feature type given in the atmospheric volume description (AVD) flag, as shown in Fig. 3(b).

 \mathcal{O}^{12}

It has to be reminded however that $\underline{13}$ absence of clouds in the AVD feature type at 5 km resolution is apparent and misleading. Indeed, low-level clouds were detected by the airborne lidar,

- Fig. 2(a), and are also evident in the Level 1 dataset, Fig. 2(c). The clouds were also detected 14 the vertical feature mask (VFM), Fig. 3(c), which is a high-resolution (single shot) version of the AVD product. Moreover, when looking at the CALIPSO wide-field camera (WFC) the underlying cloud field is evident, see Fig. 2(f). Also, if one examines the AVD product on horizontal averaging,
- 185 Fig. 3(d), the detection of subgrid features at the single-shot level suggests the presence of a highly variable cloud field; this is not independent information, and it must be taken into account together with the feature type. Detected clouds are normally 15 hoved from the Level 2 product before the computation of aerosol signals (Vaughan et al., 2009). In Winker et al. (2009) it is specified that boundary-layer clouds and the region of the atmosphere beneath them are identified and removed at
- 190 single-shot resolution, allowing the retrieval of aerosols when the gaps between clouds are smaller than the required averaging interval. However, if clouds are imperfectly removed, fightificant discrepancies can be expected: imperfections of the layer detection algorithms will in general affect all the subsequent steps of the processing chain.

Concerning the large variablity of the backscatter and extinction coefficient, mentioned above,

| Number: 1 | Author: reviewer | Subject: Sticky Note | Date: 11/08/2014 18:46:54 | | |
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| ambiguous. was t | here an uplooking li | dar or other sensor aboard the | e aircraft? or should readers assume that the CALIOP data was used to | | |
| determine cirrus p | determine cirrus presence. please state clearly how cirrus identification was done. | | | | |
| Number: 2 | Author: roviowor | Subject: Highlight Date: 11 | /08/2014 19:44:54 | | |
| T INUITIDEL. 2 | Author. reviewer | Subject. Highlight Date. 11 | ./00/2014 10.44.54 | | |
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| of | | - | | | |
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| or download the I | evel 1 data and chec | ck to be sure. (It could be - he | eaven forbid! - that there are errors in the base extender code) | | |
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| <u> </u> | | | | | |
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| that the | / autor. reviewer | Subject. Inserted Text | Dute: 11/00/2011/22.55.22 | | |
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| was | | | | | |
| m Number: 9 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 22:55:56 | | |
| did not require | Aution. reviewer | Subject. Inserted Text | Date: 11/00/2014 22:35:50 | | |
| did not require | | | | | |
| Number: 10 | Author: reviewer | Subject: Sticky Note | Date: 12/08/2014 17:59:16 | | |
| given that CALIO | extinction uncertaint | ies are dominated by uncerta | inties in the modeled lidar ratio, perhaps a more useful metric would be to | | |
| say that 'the relative uncertainty in the extinction coefficients is less than X% for lidar ratios in the range of Y% to Z%". | | | | | |
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| | Aution. Teviewei | Subject. Inginight Date. 11 | ./00/2014 10.50.50 | | |
| | | | | | |
| Number: 12 | Author: reviewer | Subject: Sticky Note | Date: 11/08/2014 19:34:58 | | |
| as written (and as | I commented in my | initial review) this sentence is | highly misleading. I very strongly suggest that the authors consider (and | | |
| accept!) the modifications I have proposed here. | | | | | |
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| m Number: 13 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 19:30:42 | | |
| Evamining only th | e feature type given | in the atmospheric volume d | escription (AVD) flag (i.e., as shown in Fig. 3(b)) one might conclude that | | |
| cloud contaminat | ion of the profiles is | apparently negligible Howey | /er | | |
| | ion of the promes is | apparently negligible. Howev | | | |
| T Number: 14 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 19:32:54 | | |
| by the CALIOP lay | er detection algorith | nm and reported | | | |
| Number 15 | Authors reviewer | Subject Incented Text | Date: 11/08/2014 10:20-E0 | | |
| | Author: reviewer | Subject: Inserted Text | Dale. 11/00/2014 13.20.33 | | |
| Ciouds delected at single snot resolution below 4 km are | | | | | |
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| in cases where the | e cloud detection rou | utine fails to identify a cloud, t | these clouds will not be removed from the surrounding aerosols layer. In | | |
| these cases, | | | с , | | |

Page 6 - response to the reviewer's comments

- 1. We have now specified that the presence of cirrus was examined in the CALIPSO dataset.
- 4. The requirements for extending downward the first layer base altitude, provided in Vaugan et al. (2010), are as follows: (a) the surface is detected below; (b) the initial layer base is within 2500 m of the surface; and (c) the 532 nm integrated attenuated backscatter in the region between the initial base altitude and the top of the surface echo is positive. For the case of the gap between 10.45S and 9.7S, below ~1300m, all three requirements are true, thus the layer should have been detected. More specifically, we have verified the Level 1 dataset for the third requirement, as advised by the reviewer, and we have found that the attenuated backscatter (averaged over 80km) is positive (see following figure). Thus, the integrated attenuated backscatter is also positive. We have amended the text of the manuscript accordingly.



- 10. The reviewer provides a very good idea on a new metric for extinction. However, we prefer to maintain the same metrics that can be found in the CALIPSO files.
- 12. We do not understand the reviewer's objection. In his previous online review, he did not suggest a modification to this sentence, but rather he clarified the CALIPSO product to us: "the only way to use the AVD to conclude that cloud contamination is negligible is to ignore large portions of the information conveyed in the AVD. as the authors explain below, the AVD flags also include information about the simultaneous presence of subgrid features (i.e., clouds detected at single shot resolution) in any layer. these are not independent pieces of information high resolution cloud removal most definitely has a bearing on the layer type classification of the remaining layer fragments nor should the authors attempt to use them as such." We had completely accepted this comment. Note that our sentence, on cloud contamination being negligible, contained the word "apparently". The 20 lines below explained the situation in detail, and were in line with the reviewer's comment. We had also added text: "this is not independent information, and it must be taken into account together with the feature type" (line 186-187 of the previous version of the manuscript).

In any case, we have integrated the reviewer's suggestions on how to further reformulate this, because these suggestions do not alter the meaning of the message that we were trying to convey. We hope that this is now clarified.

- some insight can be given by the aerosol subtype, displayed in Fig. 3(e). Part of the observed layers are correctly attributed as smoke, but for some layers the CALIPSO retrieval scheme 'thinks' that it is in the presence of polluted dust. For each aerosol subtype, a different lidar ratio is assigned, as displayed in Fig. 3(f): 70,2 for hoke and 55,3 for polluted dust (Omar et al., 2009; Lopes et al., 2013). The actual lidar ratio used in the retrieval may in principle be different than the initial one, due
- to the iterative adaptation applied in HERA in order to prevent divergent solutions; however, for this 200 scene such an adaptation has not been applied. 5 is rather evident bomparison with Fig. 2(d), that the classification of what is a homogeneous smoke layer into different aerosol subtypes is connected to the large inhomogeneity in the retrieved backscatter and extinction coefficients. The smoke plume is surprisingly classified as smoke and as polluted dust. As a matter of fact, each layer is solved 205 independently and finally this surprising result is found



According to Omar et al. (2009, Fig. 2) the polluted dust type can only occur if the aerosol displays a depolarisation signal. An approximate particle depolarisation quantity is used, derived from the Level 1 volume depolarisation, and this approximation could lead to overestimation of the actual particle depolarisation and to corresponding classification uncertainties. Recent validation results using

- 210 airborne High Spectral Resolution Lidar (HSRL) co-located measurements show that CALIPSO's dust layers correspond to a classification of either dust or dust mixtures by the HSRL, and that the polluted dust type is overused due to an attenuation-related depolarization bias (Burton et al., 2013). In our case, depolarisation returns from the FAAM lidar show that aerosols observed in the Amazon basin during SAMBBA are non-depolarising; these observations seem confirmed in the CALIPSO
- 215 Level 1 depolarisation product, although signal-to-noise ratio is poor (not shown here). Examining the Level 2 particle depolarisation product, presented in Fig. 2(e) and which is considered more accurate than the approximation used in the aerosol subtyping algorithm, we find however high depolarisation values. Even recomputing depolarisation according to Tesche et al. (2013) does not substantially alter the picture, and therefore particle depolarisation is in this case 18 thought to
- 220 be dominated by the software bug highlighted in that paper. A large aerosol depolarisation signal is mainly found in the altitude range dominated by the broken low-level clouds, suggesting that the incorrect removal of the cloud signal has left some depolarisation signal in the aerosol product, causing its misclassification as polluted dust. In other words, the aerosol subtyping algorithm is affected by the previous steps in the CALIPSO data processing chain, and these errors are a case of 'garbage
- 225 in, garbage out' (Omar et al., 2009; Liu et al., 2009). Moreover, this is a daytime observation and shot noise is certainly a major source of uncertainty.

In Fig. 4(a) all the extinction coefficient profiles are shown for the scene under study, as derived from the CALIPSO Level 2 profile product. This information is equivalent to Fig. 2(d), and 10 bws the very large variation in the retrieved profiles discussed above. The mean profile, resulting from



spatially averaging the profiles, is shown in black; note that the profiles in this figure all have differ-230 ent horizontal extent, and hence a different weight in the averaging (they are weighed by horizintal

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|---|---|--|---|
| add lidar ratio v ± 28 and 55 ± 2 | ariability estimates (i.e 22. | ., standard deviations repre | senting the natural variation of the lidar ratio within any given aerosol type): 70 |
| T Number: 2 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 19:39:24 |
| ± 28 | | | |
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| ± 22 | | | |
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| disagree; the ac suspect the prin coefficients mus are too many ot the primary culp | tual value of the aeros nary drivers are SNR as st also be considered. ther possible explanati prit here. | ol lidar ratio likely has very nd cloud detection failures (have the authors looked a ons for it to be 'rather evid | little to do with the inhomogeneities seen in the extinction profiles. instead I (certainly this is true for vertical inhomogeneities). changes in calibration t a time history of the CALIOP calibration over this region?) in any case, there ent' that aerosol classification (and hence changes in the assigned lidar ratio) is |
| TNumber: 5 | Author: reviewer | Subject: Highlight Date: | 11/08/2014 23:00:16 |
| Number: 6 just how 'surpris layers. ~12% of Omar et al., 200 | Author: reviewer sing' this result is woul f the aerosol detected 9) the resulting estima | Subject: Sticky Note d depend on the measured between 9.5°S and 11.5°S h ite of the particulate depola | Date: 12/08/2014 12:48:18 Value of the layer integrated volume depolarization ratio (VDR) for the detected has a VDR > 7%. when corrected for molecular scattering (see equation 10 in arization ratio is likely to exceed the 7.5% threshold used in the CALIOP aerosol |
| subtyping algor | ithm to identify pollut | ed dust. so while the result | s may be incorrect, i don't find them especially surprising. |
| Number: 7 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 19:44:56 |
| unexpected resu | ult is found. | as partiy as smoke and par | ty as politiced dust, and when each layer is solved independently this final, |
| T Number: 8 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 20:03:12 |
| is | | | |
| pNumber: 9 | Author: reviewer | Subject: Sticky Note | Date: 12/08/2014 12:16:26 |
| the question the retrieval artifact | e authors should addre s. | ess is whether this large var | iability primarily due to variability in the measurements or is it the result of |
| TNumber: 10 | Author: reviewer | Subject: Highlight Date: | 11/08/2014 21:46:58 |

Page 7 - response to the reviewer's comments

- 4. We were not expressing causality between the lidar ratio and the inhomogeneities (it is rather obvious that both have a common cause in the incorrectly detected cloud field), but simply a co-location. We have now made it clearer by changing "connected" into "co-located".
- 6. We appreciate that from a purely remote sensing point of view this is not surprising; it is however surprising on a geophysical point of view to see dust represented in an area that is genuinely dust-free.
- 9. We do not understand the reviewer's objection: we believe that we have sufficiently discussed the point, and that we have suggested that we are in the presence of a retrieval artefact (i.e. comparison with a more homogeneous layer determined from the aircraft lidar), without any need to repeat the same discussion once more. Please refer to the words "discussed above" in the highlighted sentence.

extent). The mean profile is also shown in 22 en blour in Fig. 4(b), and is compared to the extinction profile derived from the mean aircraft lidar range corrected signal (indicated in red). The aircraft extinction coefficient shown in Fig. 2(b) and 4(b) was determined using the Marenco (2013)

- 355 method, and has been multiplied by 4 to convert it from 355 to 532 nm. This conversion factor was determined from the Porto Velho AERONET site (8°50'S, 63°56'W, located at ~ 200 km) where aerosol optical depth (AOD) at 18:00 UTC, interpolated for the 355 and 532 nm wavelengths yields 0.55 and 0.33, respectively. The uncertainty range in Fig. 4(b) indicates the effect of assumed $\pm 50\%$ error on the far end reference to the lida
- face, Derification has been done using AERONET as a constrain in red thick line indicates the lidar profile that matches the AERONET aerosol optical depth. Note that the constrained retrieval is compatible with the unconstrained one: constraining to AERONET is however not a requirement of the method, but it helps reducing the uncertainty. In the boundary layer, the mean of the CALIPSO Level 2 profiles is generally in good agreement with the aerosol extinction coefficient derived with the aircraft lidar after wavelength conversion.
 - We have also attempted another approach to the CALIPSO extinction retrieval, starting directly from the Level 1 dataset shown in Fig. 2(c), 12 starting has been cloud screening: all vertical profiles 13 htaining a large peak in the attenuated or excesscatter have been removed. The remaining profiles (524 out of 671, i.e. 80%) have been averaged together to determine a mean attenuated backscatter
- 250 for the scene, and this profile has been smoothed with a 6-point running average (resulting vertical resolution: 180 m). Then the signal has been inverted into aerosol extinction coefficient using the Marenco (2013) method, where the far-field boundary condition has been computed by assuming a constant scattering ratio over the 500-1200 m height interval, and the lidar ratio has been assumed to be 70 sr. The result of this procedure is shown in blue, and we can notice that it offers a reasonable agreement with the latitudinally averaged level 2 data, when uncertainties are accounted for.
- Note that, for both the airborne and the spaceborne lidar, the retrieval constrained with AERONET falls well within the stated uncertainty lines obtained without a constraint 14 s expected with this method when unconstrained, 15 certainty is large near the ground but it decreases when moving upwards.
- 260 Note also that between 2,000 and 2,800 m the extinction obtained for CALIPSO is larger than 17 that obtained for the airborne lidar. A hypothesis is that it could be ascribed to the 'twilight zone' consisting of hydrated aerosols inbetween the boundary-layer clouds (Koren et al., 2007): these hydrated aerosols could have different optical properties (lidar ratio and colour ratio) so as to introduce this discrepancy.

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| a color ratio of | | | | |
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| per a previous co uncertainty estim | omment, this informat nate. | ion should be provided earlie | r in the manuscript. The AERONE I-derived color ratio should also include an | |
| the authors migh lidar data to deri | t also note that a colore a 355-532 extinction | or ratio of 0.6 is in good agree on Ångström exponent of 1.17 | ment with the color ratio retrieved by Baars et al. (2012), who use Raman \pm 0.44. | |
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| from the aircraft | measurements | | | |
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| what is the basis | for this assumption? | can this assumption be replace | ed with an empirically derived uncertainty estimate? | |
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| when AERONET is used as a constraint, what quantity is optimized: the far-field boundary value or the lidar ratio? this should be explained in this manuscript. if the lidar ratio is optimized, the final value (and its uncertainty) after completing the constrained retrieval should be given. | | | | |
| also, over what vertical extent is the solution constrained? i.e., the mixed layer only or the entire column from the surface to the lidar altitude? | | | | |
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| be specific; 'a larg | ge peak' is qualitative | ; what's needed here is a quar | ntitative description of the cloud clearing criteria. | |
| TNumber: 13 | Author: reviewer | Subject: Highlight Date: 11 | /08/2014 20:33:04 | |
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| constraint | | | | |
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| in this respect, th | e error propagation of | characteristics of the Marenco | method are identical to those of the CALIOP standard method. | |
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 The differences between the bold red, bold blue and bold green curves is especially interesting given that the blue curve is created from CALIOP level 1 data that has been manually cloud-cleared by the authors. With the exception of a peak in the CALIOP retrieval (bold green curve) at ~2.1 km, the CALIOP standard method and the Marenco method applied to CALIOP data (bold blue curve) match very closely. This suggests that in general the CALIOP automated cloud clearing technique is, in this one instance, nearly the equal of the authors' manual technique. Of equal importance perhaps is the implication that, even when the authors apply a rigorous, manual cloud clearing scheme, twilight zone effects still remain.

I remain very puzzled, however, by the differences between the bold blue curve and the bold red curve (i.e., the Marenco method applied to the aircraft lidar measurements). To understand them it would be very helpful to compare cloud-cleared averages of the attenuated backscatter

Comments from page 8 continued on next page

coefficients from CALIOP and the aircraft lidar. A clear mismatch in the shape of these level 1 profiles could indicate (among other possibilities) that the meteorological conditions in the region had changed substantially between the (very short) time of the CALIOP measurements and the (much longer) time of the aircraft flight.

Page 8 - response to the reviewer's comments

- 5. This has been addressed earlier (response to comment 10, page 4 of the reviewer's comment). The comparison with Baars et al has been added at that same point. Thanks for pointing out the reference.
- 7. We have reformulated our sentence, and hope that it is clearer that we are attempting to quantify the uncertainty in the retrieval assumptions.
- 9. This is explained in the following line "the red thick line indicates the lidar profile that matches the AERONET aerosol optical depth". The lidar ratio is <u>not</u> changed. We have shown in Marenco (2013) that changing the lidar ratio does not have a large effect on the retrieved aerosol extinction; see also the figure attached to comment 10, page 10.
- 12. Information has been added.
- 16. In a qualitative sense, certainly. However, Figure 1 of Marenco (2013) shows how differently uncertainties in the assumed parameters propagate with an inward and an outward retrieval. In the case of CALIPSO this is still different, due to the iterative adaptation of the lidar ratio and the use of a layer detection scheme. We would not want to draw conclusions at this stage, on the quantitative similarities or differences of the uncertainties of retrieval methods, since we do not have access to the CALIPSO central processing code.
- 18. We agree that these profiles match well (this is already stated earlier in the paper), and this is reassuring. As we said in our previous review, we are also puzzled by the discrepancy at the top of the boundary layer; however we do not believe that the short time of this comparison can be compatible with a large variation of the meteorology as suggested by the reviewer. The cloud-clearing done by identifying "peaks" actually does not remove hydrated aerosols near cloud boundaries, hence does not prevent twilight zone effects. Note that the attenuated backscatter from CALIPSO and the range-corrected signal from the aircraft lidar differ substantially in both magnitude (due to the aircraft lidar being uncalibrated) and in general shape (due to the different Rayleigh backscattering and extinction).

265 4 Conclusions

We believe that the dataset presented here is a useful comparison, and that it may help identify some critical points and develop further verification experiments. We have highlighted a particular type of scene, which yields retrieval problems in CALIPSO: the case of broken clouds embedded in a regional haze field, observed in daytime. Problems arisd possibly due to the large amount

- of ambient daylight, limiting CALIOP's signal-to-noise ratio. Reflection of light by the clouds amplifies the upwelling radiation and thus increases this effect; CALIOP's detection sensitivity may have been reduced below specifications for this reason, and this could explain why an aerosol layer was missed groblems arise? as well because of uncertainties in the cloud-aerosol discrimination and aerosol subtype and lidar ratio selection algorithms: in this case, depolarisation by the same the same transmission by the same transmission.
- 275 may have mislead the algorithms into believing that dust is present over the Amazon, whereas the region was dominated by smoke. Moreover, be retrieved aerosol extinction showed an excessive spatial variability. As determined

with the aircraft instrument, however, the observed aerosols did not show a large horizontal inhomogeneity. A thin elevated aerosol layer (600 m deep, FWHM) was observed at an altitude of ~ 5 km,

- with an aerosol optical depth of 0.03; a 2 2 be deep boundary layer was also observed, featuring an served extinction coefficient of 110 Mm and topped with broken clouds (stratocumulus). The air layer between the boundary layer top and the elevated layer also showed aerosol content. From the observations gathered during SAMBBA, evidence exists that the aerosol layers are smoke from biomass burning, and that they do not depolarise backscatter lidar returns.
- In this scene, the first remark is that CALIPSO does not detect the thin elevated layer. According to the aircraft dataset, this layer has a peak backscatter coefficient of 0.8 Mm^{-1} sr⁻¹ at 532 nm (horizontally averaged profile). This has to be compared to Winker et al. (2009, Fig. 4) and Vaughan et al. (2005, Fig. 2.4), where the CALIPSO detection sensitivity for the 532 nm backscatter coefficient at 5 km altitude in daytime is set g_{10} , 5, 0.8, and 0.35 Mm^{-1} sr⁻¹ for horizontal resolution [11]
- 290 5, 20, and 80 km, respectively: according to these specifications, the layer should have been detected at the coarser resolutions. Note that the daytime sensitivity thresholds for feature detection are larger than the nighttime ones; this is an effect of the background radiation due to daylight, which acts as a disturbance to the lidar $system_{x_{12}}$ he clouds underneath may have played a role in this failure to detect, as they increase the diffuse daylight background, reducing CALIOP's SNR and hence detectors.
- 295 tion sensitivity: as a matter of fact, Vaughan et al. (2005) specify that the above specifications on detection sensitivity apply for a 5% columnar albedo; in the present scene, dominated by low-level clouds, the average albedo is most probably larger.

The second remark is that the CALIPSO dataset displays a 13ry-variable aerosol subtype 147e believe that the presence of broken clouds at the top of the boundary layer misleads the CALIPSO

300 automated processing scheme: if the clouds are incorrectly removed, an apparent aerosol depolarisation is detected and the aerosol layer receives a classification as polluted dust, and thus a reduced

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| Tumber: 3 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 21:51:30 |
| portions of aeroso | l layers visible in the | e aircraft data were not detect | ed |
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| —undetected bound | lary layer | | |
|) Number: 5 | Author: reviewer | Subject: Sticky Note | Date: 11/08/2014 21:55:52 |
| but (to echo an ea causes (e.g., lidar r ratios? | rlier comment) is thi atio). have the auth | is "excessive spatial variability" ors attempted to quantify spa | ' due primarily to noise in the measurements, or are there other contributing tial variability in the extinction profiles as a function of the applied lidar |
| TNumber: 6 | Author: reviewer | Subject: Highlight Date: 11 | /08/2014 21:52:25 |
| Number: 7 | Author: reviewer | Subject: Sticky Note | Date: 12/08/2014 12:53:35 |
| mean value? med metric (i.e., either e | ian? max? mode? extinction or optical | in any case, it would be better depth, but not a mix of both) | to characterize both the lofted and the boundary layer using the same |
| TNumber: 8 | Author: reviewer | Subject: Highlight Date: 11 | /08/2014 21:56:50 |
| T Number: 9 | Author: reviewer | Subject: Cross-Out Date: 11 | /08/2014 21:59:07 |
| Thumber: 10 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 21:58:57 |
| estimated | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 21:59:10 |
| s Number: 12 | Author: reviewer | Subject: Inserted Text | Date: 12/08/2014 12:27:50 |
| measurements Image: Provide the second se | Author: reviewer | Subject: Cross-Out Date: 11 | /08/2014 22:00:46 |
| TNumber: 14 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 22:01:07 |

in what appears to be a homogeneous scene.

Page 9 - response to the reviewer's comments

- 5. In this sentence, we are not expressing a single cause. The previous paragraph indicates a few potential causes, such as the daytime SNR and the uncertainty in the aerosol classification scheme (and hence lidar ratio). The reviewer's question could be assessed by accessing the CALIPSO central processing code and playing with its input; this is however outside of the scope of our research.
- 7. As it can be seen from figure 4b, the extinction coefficient in the boundary layer is more or less constant, but it experiences a large measurement uncertainty due to the way the boundary value is selected, for the unconstrained retrieval (as displayed in Fig. 4b). With this uncertainty in mind, we do not think that specifying whether 110 Mm⁻¹ is the mean or the median of the vertical profile is really useful information (but, to satisfy the reviewer's curiosity, <u>both</u> the mean and the median of the <u>constrained</u> retrieval vertical profile yield 110 Mm⁻¹). We have now indicated the AOD too.

lidar ratio and a lower extinction. Cases of aerosols being misclassified as dust or polluted dust have also been reported in the literature, but in those studies classification errors have a different explanation than in the present case. Kacenelenbogen et al. (2011) have identified an underestimate of the

- 305 lidar ratio assigned for retrievals in HERA, due to a misclassification of fine absorbing aerosols as dust or polluted dust, when compared to HSRL; as however no coincidence with clouds is reported, we believe that the causes of misclassification in that article should be different than the ones we report here. In Tesche et al. (2013) a similar misclassification of marine aerosols has been observed in the presence of clouds, but the reason for this was identified to be a software bug, and hence
- was not ascribed to an incorrect removal of the cloud field. The incorporation of the WFC radiance in the cloud detection scheme is being contemplated for a future CALIPSO data version, and the case illustrated here suggests that this pull lead to a potential improvement of the final product.
 The subgrid features already reported by the AVD product, Fig. 3(d), also look promising for cloud identification.
- The third remark is that the boundary layer extinction coefficient determined in the CALIPSO dataset yields a consistent average field, when compared to the aircraft lidar and accounting for the longer wavelength. However, taking into account that the boundary layer aerosol detection misses its extent by $\sim 30\%$, we have to conclude that the overall estimate of aerosol extinction by the Level 2 data for this particular scene is boundary to thirds of what is expected. The CALIPSO ex-
- 320 tinction dataset also shows a large spatial variability in both the horizontal and vertical directions, which is not reflected in the aircraft dataset. D'e believe that this is due on one hand to the large shot noise for these daytime measurements, and on the other hand to the variable aerosol subtype and subsequently to the different lidar ratios used. D is also possible that the potential mathematical instabilities introduced by the outward integration scheme used in HERA may have played a role.

325

Finally, we note that CALIPSO observations can be reprocessed from the Level 1 data (attenuated backscatter data), using published methods for backscatter lidar; this has also been done in Kacenelenbogen et al. (2011), although in that article an outward integration scheme is used. A reprocessing of this kind can't be easily automated and requires interaction by an expert for tasks such as inte-

330 gration, cloud filtering, selection of a reference layer and a lidar ratio, etc.; but in specific scenarios it can help get insight into the aerosol vertical distribution, and it permits comparing results with an inward solution scheme, which represents 12 table mathematical solution. 11

Space-borne lidar is a great advance for science, and in the last seven years CALIPSO has given researchers a very useful dataset, mapping global aerosols in 3-D at high resolution. It is therefore important to identify critical issues, so as to enable improving the data products. Scenes, such as the one highlighted here, are not infrequent misrepresentations such as the one highlighted will yield an incorrect evaluation of the regional radiative forcing and of the aerosol indirect effect. We have also tried to indicate a few ideas for improving the exploitation of the CALIPSO dataset.

| Thumber: 1 | Author: reviewer | Subject: Cross-Out Date: 11 | /08/2014 22:06:00 | |
|---|--|---|--|--|
| | | | | |
| TNumber: 2 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 22:07:02 | |
| —incorporation of V | VFC radiances in the | cloud detection scheme | | |
| pNumber: 3 | Author: reviewer | Subject: Sticky Note | Date: 12/08/2014 17:27:37 | |
| I'm afraid I have n | o idea what this sent | tence means. High resolution | (single shot) cloud identification simply cannot be done using a level 2 data | |
| product that is av | eraged to 5-km nom | contaily (at minimum) and ou | -III vertically. | |
| Perhaps the sente | nce can be revised to | o say something like this? (as | suming this is consistent with the authors' original intent) | |
| "Those range bins data users could c extinction and bac | for which clouds ha conceivably apply thi ckscatter profiles." | ve been detected and remove s information to derive more i | d at single shot resolution are identified in AVD product (e.g., Fig. 3d), and igorous quality assurance screening criteria for using the CALIOP aerosol | |
| T Number: 4 | Author: reviewer | Subject: Cross-Out Date: 11 | /08/2014 22:08:00 | |
| | | | | |
| ∓ Number: 5 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 22:19:40 | |
| along-track aeros | ol optical depth estir | nates | | |
| TNumber: 6 | Author: reviewer | Subject: Inserted Text | Date: 11/08/2014 22:19:46 | |
| are | | | | |
| TNumber: 7 | Author: reviewer | Subject: Highlight Date: 11 | /08/2014 22:20:25 | |
| | | | | |
| Mumber: 8 | Author: reviewer | Subject: Sticky Note | Date: 12/08/2014 17:34:14 | |
| but how much of the spatial variability is due to the one, and how much is due to the other. this is something that can be quantified, and thus | | | | |
| should not be sor | nething to simply ac | cept on faith or "believe". | | |
| | | | (00/2014/22/21/20 | |
| Tinumber: 9 | Author: reviewer | Subject: Cross-Out Date: 11 | /08/2014 22:21:26 | |
| — Number 10 | | Subjects Sticles Note | Data: 12/00/2014 12:20-E0 | |
| present evidence | or delete. to my eve | , figure 4b shows no sign of re | etrieval instability. | |
| | | | | |
| if the authors have | e looked for evidence | e of instabilities in the CALIOP | extinction retrievals they should first of all explain the technique they used | |
| solutions, they sh | ould report the occu | rrence frequency and describe | the retrieval conditions that differentiate unstable solutions from stable | |
| solutions. If they did not find unstable solutions, they should report that instead. | | | | |
| | | | | |
| line 55 is entirely | i, if no organized sea sufficient. | irch was conducted, sentences | such as this not appropriate. The explanation given by the authors around | |
| Number 11 | Ath | Cultingto Chieles Matte | D-+ 11 (00 /2014 22-20-02 | |
| but stability does | not imply correctnes | Subject: Sticky Note | Date: 11/08/2014 22.28.02 | |
| | | | 100 1201 4 22 27 15 | |
| T Number: 12 | Author: reviewer | Subject: Highlight Date: 11 | /08/2014 22:27:15 | |
| ■Number: 13 | Author: reviewer | Subject: Sticky Note | Date: 12/08/2014 18:06:17 | |
| how do the autho | rs know this? and w | hat does 'not infrequent' mea | n? 1%? 5%? 20%? have the authors done any analysis to quantify this | |
| statement in any v | way? e.g., see sectio | n 3.3 in Kanitz et al., 2014. wh | ile Kanitz et al. also use limited direct comparisons as the basis for reaching | |
| somewhat broade they observed in 1 | er conclusions, they c their comparisons als | to so by presenting additional so exist in other, similar regior | Instance of CALIPSO data that demonstrate that conditions similar to what is/scenes around the planet. the authors of the current manuscript have not | |

(yet?) been so diligent. while the authors may well be correct in their statement (and I expect they are), they still need to provide evidence that it is indeed a plausible assertion. (this small, almost parenthetical statement really does go right to the heart of what, in my opinion, CALIPSO validation papers should try to

(this small, almost parenthetical statement really does go right to the heart of what, in my opinion, CALIPSO validation papers should try to address; i.e., how broadly applicable are the results derived from any single study?)

Kanitz et al., 2014: Surface matters: limitations of CALIPSO V3 aerosol typing in coastal regions, Atmos. Meas. Tech., 7, 2061-2072, doi:10.5194/ amt-7-2061-2014.

Comments from page 10 continued on next page

Page 10 - response to the reviewer's comments

- 8. Please see our response to comment 5 on page 9.
- 10. We have never intended to stir doubt about the hard efforts from the CALIPSO science team to get the best possible product. We think that adding an element of discussion, mentioning one of the potential origins of the large horizontal variability that is observed with CALIPSO can be useful. We have never said that we have **certainly** found occurrences of unstable or diverging solutions: please note that this sentence contained the words "it is also possible" and "may have played a role". Further research on this is possible, by accessing the CALIPSO central processing code and playing with its input to find what exactly causes the variability: this is outside of the scope of our research, but we do not understand why it should not be mentioned as a possibility, since we have good reasons as already discussed. As suggested by Mark Vaughan in the previous review, we had strongly reduced the emphasis on the point of instability, without however removing it completely.

We remind that: (a) the theoretical framework suggests that such an instability is possible and even likely; (b) it is unproven that the lidar ratio adjustment in CALIPSO completely removes all instances of instability, and it is reasonable to believe that small instability effects might not be noticed by the HERA algorithm; (c) some of the profiles show signs which can potentially indicate instability, as discussed in our online comment, where a figure shows them in detail. To exemplify the concept once more, we hereby plot the vertical profile computed using the Fernald-Klett method for lidar ratio 70 ± 28 (this is the stated uncertainty for the CALIPSO lidar ratio) in both the cases of inward and outward retrieval. This is obtained for the same vertical profile used for the blue line in Fig. 4b (i.e. the cloud-screened 200 km average of CALIPSO L1 signals). The reviewer can verify how much more sensitive to the lidar ratio is the outward retrieval, compared to the inward one.

However, to facilitate the present review, since the highlighted sentence is "controversial" for the reviewer, we have preferred omitting it.



11. The reviewer is right: stable does not necessarily mean correct. That is why just a couple of lines above we insist on the necessity of a scientist reviewing assumptions on a case-by-case basis. The word "stable" strictly refers to how uncertainties are propagated via the solution of the lidar equation, and it is a purely mathematical concept. See e.g.

http://en.wikipedia.org/wiki/Stability_theory

http://en.wikipedia.org/wiki/Numerical_stability

13. Please see our response to comment 5 on page 3, above. Work to quantify this using the CALIPSO dataset greatly exceeds the scope of this paper and cannot be allocated within our resource plan. However, from a meteorological point of view, we know that broken stratocumulus clouds that build up at the top of a boundary layer are very common. To replicate such a scene, we need a frequent phenomenon, such as broken cloud fields, to develop in a region of the world where the boundary layer has many aerosols, such as the Amazon in the biomass burning season. This reasoning indicates clearly that this will happen again, although quantifying it in the way that the reviewer requests would mean a much larger effort, which we cannot afford at the moment.