

Interactive comment on “The RAMNI airborne lidar for cloud and aerosol research” by F. Cairo et al.

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In the following, the reviewers' comments have been reported (and tagged with "R:"), and answers are specifically provided (and tagged with "A:"). Additions to the manuscript have been reported and tagged with respect to the corresponding (page/line) on the previous discussion paper, when possible. Changes to the manuscripts to not limit to those hereby reported. The new version of the manuscript is attached.

Response to Anonymous Referee #2

R:Introductory remark: I cannot remember that I have ever seen a paper submitted with such a large number of typos, wrong units, inconsistent labels/captions of the figures, errors in equations, confusion of left and right, references not in alphabetic order,

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and more. Do the authors expect that the reviewer spends hours of her time to make all the corrections? There is no doubt that it is the obligation of the authors to have the manuscript carefully checked before submission! As a consequence, I will only comment on scientific issue; maybe some might be obsolete if the text would have been in a better shape. The paper introduces a lidar system for airborne operation that shall be used for the observation of possibly hazardous aerosol layers, e.g., from volcanic eruptions. The specifications of the system, the basics of an error analysis, and the way how the data are evaluated are described. Then, four test flights – one of which being a demonstration of the technical feasibility – are discussed in view of the possibility to characterize (volcanic) aerosols. The data are restricted to one wavelength (unfortunately the infrared channel did not work properly) and the volume depolarization ratio. There is no doubt that an airborne lidar such as RAMNI is urgently needed to have a “moving aerosol platform” in case of civil contingency. RAMNI can be a valuable tool to do the job, but at the present state the results are not fully convincing (see below), maybe it would have been better to postpone the publication until more consolidated results are available. Parts of the paper are “poor” but with “major revisions” it might be possible to improve the paper so that can be published.

A: We take the severe but honest reproaches of the referee as a (most needed, it seems) stimulus for doing better. We thank the referee for the punctual remarks: we addressed them and tried to improve the paper according to the suggestions. The paper, that aims at presenting the new instrument after its first field deployment, has been deeply changed. The main change of the new version of the manuscript is a rearrangement and reformulation of the Uncertainty Analysis paragraph, and a more comprehensive description of the uncertainties to be attributed to the data. Figure 4 now reports error curves on the actual data as well. Point-by-point answers are provided below. Modifications to the manuscript do not limit to those explicitly reported hereafter.

R: Some more specific details: 1254/18: the statement that “aerosol optical properties”

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can be achieved with a resolution of seconds is a little bit optimistic (if the accuracy should be high).

A:Such sentence is general. Of course the results the lidar technique can deliver, depend crucially on the specific system performances. However there are already examples in literature (see, as instance, Pal S., Behrendt A., Wulfmeyer V.: Elastic-backscatter-lidar-based characterization of the convective boundary layer and investigation of related statistics, *Ann. Geophys.*, 28, 825-847, 2010) where high performance lidars have been used at such high resolution. Anyway, we do not think it is a crucial issue here, maybe our sentence can be left as it is.

R:1254/24ff: “Rengel” does not exist! In the list of references, the flights of the Falcon of the DLR, Germany, should be mentioned: the Falcon made a lot of flights during the Eyafjallajökull eruption and several aerosol campaigns. Airborne lidars have also frequently been used by scientists from the US.

A:The reference to “Rengel et al., 1997”, has been corrected to “Renger et al., 1997”. We apologize for that. A reference to the Schumann et al. 2011 paper reporting the flights of the DLR Falcon during the EyafjallaJokull eruption was already present in the discussion version of our manuscript (1255/8), we are not aware of others. Two more topical references have been added, documenting U.S. activity in airborne lidar research (Browell et al., 1991, McGill et al., 2002)

R:1255/14: sort of a review is given in Wiegner et al. (*J. Phys. Chem. Earth*, 2011).

A:The suggested reference is still in press, but available from ScienceDirect and quotable. It has been added as so.

R:1255/17: a description, more detailed than in the reference given, can be found in Gasteiger et al. (*ACP*, 2011).

A:The suggested reference has been added. The same reference has also been cited in the conclusion.

R:1256/17: the mass of the system would be interesting as well.

A:This has now been reported in the text.

R:1257/24: How is the “gray photochromic glass” chosen? Before each flight? Can it be removed/changed during the flight, if necessary? Is this a critical issue?

A:There has been no particular selection for this glass. Is an ordinary “off the shelf” plane photochromic glass able to reduce its transmission in the visible, from 0.95 to 0.25, on ultraviolet light exposure . Even if it is in principle relatively easy to remove it from the system, it auto adjusts its transmission to the background light intensity, so in fact there’s no real need to remove it. In the text, the sentence has thus been changed to: “A gray photochromic glass is placed in the telescope focal plane. The glass darkens on exposure to ultraviolet (UV) radiation, and gradually returns clear when UV is removed, on a timescale of some minutes. The system efficiency then decrease by a factor of 4, under conditions of strong sky background light, i.e. when used in daylight, while maximum sensitivity is achieved during nighttime; this allows to moderate the effects of saturation and non linearity on the light detectors under strong light exposure. The photochromic glass response to fast background light changes - as those that may occur when white clouds cross the telescope FOV - is considered slow enough to deem the glass attenuation constant over the time a single lidar profile is averaged.”

R:1258/2: What is the purpose of the Raman channel? The authors state that the lidar cannot be used during daytime. Are nighttime flights possible? Is it planned to operate the lidar from ground as well? How long are the time averages required? If the required time periods are in the order of 30 minutes or more, it is certainly not reasonable to use it during flights (horizontal resolution of 200 km or so).

A:Nitrogen Raman channel is operative only at night, and requires time integrations of several minutes, so although nighttime flight can be envisaged, for airborne research is probably of limited usefulness, as stated by the referee. However we have added a

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ground based Raman profile to fig. 1 – a 25 min. integrated measurement session – for the sake of completeness. As stated earlier in our manuscript (1256/9) the RAMNI is usually deployed as a ground based instrument. During nighttime ground operation, the Raman channel is used for a better constrain of the lidar ratio, for PBL aerosol – its range does not extend much further than few kms. The text has been edited as follow: (1265/16 onward): “Figure 1 displays the atmospheric elastic, polarization preserving, backscatter signal acquired on a clear night with 300 s integration time. The figure reports the atmospheric return as photoncounting rates per single laser shot, for the photoncounting mode acquisition, and in Analog to Digital Converter digit units, ranging from 0 to 255, for the current mode acquisition. The photocounting mode acquisition is presented before (black line) and after (blue line) the application of the dead time correction, the current mode acquisition is displayed before (red line) and after (purple line) the application of the partial overlap correction. Also displayed are the altitude regions where the overlapping photoncounting and current signals are merged. The inelastic Nitrogen Raman signal, acquired with 1500s integration time (green solid line), is also displayed. The Raman signal is acquired in photoncounting mode only.” . The figure 1 caption has been changed accordingly.

R:1259/2ff: The numbers for the maximum range and the resolution seem to be inconsistent with respect to the fixed number of 1024 range-bins. Please clarify.

A:The range-bins number is fixed to 1024 – the first 24 collected before the laser shot and used for measuring background light .The duration of the single bin can be varied from 12.5 ns to 100 ns for current acquisition, and, separately, from 25 ns to 1000 ns for photoncounting acquisition. This delivers a spatial resolution spanning from 1.875 m to 15 m for the current mode, and from 3.75 m to 150 m for photoncounting mode. Accordingly, the total range can be set from 1.875 to 15 km in current mode, and from 3.75 to 150 km in photoncounting mode. The text was unclear and wrong, and has been corrected.

R:1259/15: “several tens of hours”: what is this good for?

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A: We have been using such systems with high energy-per-pulse laser at extremely low pulse repetition rates (0.1 Hz or less). In order to acquire sufficient signal statistics in unattended operation, averaging times extended for the whole day, and daily average profiles were acquired, for seasonal monitoring. These lines may be regarded as fulfilling a curiosity, we do not think it is worthwhile going too much into this specific in the manuscript.

R:1260/8: The way how the range for the merging of the signals (current and photoncounting mode) is selected should be discussed in more detail. Is it possible to define the range by means of distance from the laser? Isn't it better to define the range by considering the signal strength. This could vary depending on the aerosol extinction and backscattering, so that the range where the signals could be merged could be at different distances.

A: Absolutely so. What we meant in our previous text (1265/12), "...The current and photoncounting profiles are then superimposed and merged together in a region where both acquisition modes are considered sensitive and accurate, generally above 2-3 km in daylight..." was that the merging altitude is not fixed but may vary according to the signal strength. The text was unclear and has been modified as: "The current and photoncounting profiles are then superimposed and merged together in a region where both acquisition modes are considered sensitive and accurate. This region may vary according to the background light level and to the amount and distribution of aerosol. Generally, we privilege the photoncounting acquisition, as it is known to be superior to the current one in terms of stability, detection efficiency, and signal to noise ratio (Tull, 1968) and is less affected by nonlinearities arising from the extensive dynamical range of the atmospheric backscatter signal (Cairo et al., 1996). Therefore the current acquisition is used only when the photoncounting starts showing saturation effects, i.e. when the photoncounting rate exceeds 10 MHz. Henceforth, for the polarized channel, current mode is used generally below 2-4 km in daylight, and below 1-2 km during nighttime..."

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R:1260/15ff: I am not sure that the equations are necessary; it is just a repetition of the Russell-paper! Moreover, there are some squares missing so that the equations are not correct in the present form (Eq.2, 6, 9).

A:Eqs. (2) to (11) (now (3) to (12)) follow the seminal Russell et al. paper, we slightly changed the notation, simplified, summarized and reformulated the equations. We do not think they are excessively burdensome, and would prefer to keep them in. The statement “there are some squares missing . . . (Eq. 2, 6, 9)” stems from a misunderstanding. We denoted with $T(r)$ the total transmission term due to the double passage from the lidar to the scattering region and back. The often used notation $T2(r)$ explicitly divides it in two passages, and is most useful when the extinction over the two passages is different – as when you send one wavelength and receive a different one from inelastic scattering processes. In our context, it appears an unnecessary complication, so we prefer to keep our notation consistently throughout the formulae (please note that consistency in eq. (12) of the earlier version of the manuscript, now (13), where the factor 4 takes into account the double passage), and now explicitly state that “the term $T(r)$ expresses the atmospheric transmission from the lidar to the scattering region at distance r , and back”.

R:It should be explained where the “ R_0 ” comes from (Eq. 10; missing in Eq. 7).

A:Eq. 7 (now (8)) has been corrected. Eq. 10 (now (11)) as well (there, a square was missing in the ratio of the ranges.)

R:It is not obvious how Eq. 11 follows from the previous equations (one has to read the Russell-paper), the text does not help here. The authors should rather spend some more words on explaining the meaning of Eq. 11 and Eq. 12.

A:We have now introduced eq. (11) (now (12)) as: “After rearranging the usual error propagation formula and neglecting covariances between the measured quantities, and the uncertainty on the altitude x , we get to: (12) Showing how the errors in the aerosol backscatter coefficient retrieval come from the signal measured, the estima-

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tion of transmission and density, and on the assumed value for the backscatter ratio at the calibration altitude.” We have discussed eq. (12) (now (13)) more deeply: “. . . is the error on the transmission due to both molecular and aerosol extinction. The molecular extinction can be evaluated from Rayleigh theory once the air density profile is obtained from measurements or from a suitable atmospheric model, while in absence of an independent measurement, β_a can be calculated from (11) only if a “piori” assumptions are made on the relation between aerosol extinction and backscatter coefficients (the so-called “lidar ratio”). In such assumptions lie the largest source of unaccuracy in lidar retrievals. We follow the standard Klett approach (Klett, 1981) and choose to fix the lidar ratio to piecewise constant values in regions where clouds or aerosols are present. Such regions are automatically identified by iteratively inspecting the values of backscatter ratio, depolarization ratio and altitude during the data processing, and recursively adjusting the lidar ratio accordingly. As instance, when thin liquid or ice clouds are identified in a given altitude range, the lidar ratio there is set to values known from literature (Chen et al., 2002, O’ Connor et al., 2004). The lidar ratio for aerosol may easily range from 30-50 sr⁻¹ in the case of dust (Mattis et al., 2002, Immler et al., 2003) to 80 sr⁻¹ for biomass burning aerosol (Wandinger et al., 2002), and reported values for volcanic ashes are in the range 45-60 (Ansmann et al., 2010, Gross et al., 2011). Although our data process allow to constrain the aerosol lidar ratio value when additional Aerosol Optical Depth measurements from sunphotometers are available (as in the San Pietro Capofiume station) (Marenco et al., 1997), or to provide an altitude dependent aerosol lidar ratio when the nitrogen Raman signal (Ansmann et al., 1990) is available during nighttime, these opportunities were not attainable during the flight tests. Hence a constant aerosol lidar ratio was set to 50 sr⁻¹ everywhere, except when cirrus (30 sr⁻¹) or thin water clouds (19 sr⁻¹) were identified. To give an estimation of the uncertainty induced by such choice, following the literature (Russell et al., 1979, Bockmann et al., 2004) we write: (13) where t_a, m indicate the optical depths due to particulates and molecules, respectively.

R:The main shortcoming of this section is that it mainly consists of general remarks,

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whereas the actual errors of the retrieved optical parameters (beta, delta; see Section 3) are missing.

A:The whole section has been largely reformulated. Please refer also to the discussion introducing Fig. 4, which now displays also the uncertainties to attribute to the measurements for the 9 Dec.2010 flight, in 3.1.

R:1266/7: “lidar ratio” is already defined on page 1263.

A:The text has been changed as: “assume a value for the lidar ratio,”

R:1266/15: the authors should include some of the recent publications on lidar ratios (see e.g., Gross et al., 2011, Atmos. Env.; Weinzierl et al. 2011, Tellus; see also JGR) or several papers from the IfT Leipzig group.

A:The suggested references have been added.

R:1266/19: How can sun photometer measurement of the AOD help airborne lidar measurements? They are never “co-located”.

A:The remark is correct, as the two instruments measure path extinctions with different geometries, resulting in AOD averaged over different portions of the atmosphere. However the constrains such different measurement approach poses on the simultaneous use of the two instruments, depends much on the horizontal homogeneity of the aerosol layer and, for airborne instruments, on the flight path. There are several studies on the synergic use of the two instruments, either one or the other airborne. A recent study (Rogers, R. R., Hair, J. W., Hostetler, C. A., Ferrare, R. A., Obland, M. D., Cook, A. L., Harper, D. B., Burton, S. P., Shinozuka, Y., McNaughton, C. S., Clarke, A. D., Redemann, J., Russell, P. B., Livingston, J. M., and Kleinman, L. I.: NASA LaRC airborne high spectral resolution lidar aerosol measurements during MILAGRO: observations and validation, Atmos. Chem. Phys., 9, 4811-4826, doi:10.5194/acp-9-4811-2009, 2009) compares independent measurements of aerosol extinction from airborne HSRL lidar and airborne (although on a different aircraft) and ground-based sunphotometers,

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and looks promising for a synergic simultaneous deployment of airborne elastic lidar and sunphotometers. However, in our manuscript the sentence the reviewer pointed the attention on, really quotes “en passant” such topic. We do not feel worthwhile dwelling too much on that subject, there.

R:1267/3: Again, some recent papers on the benefit of depolarization measurements should be included; see the special issues on the Icelandic volcano or search the papers on SAMUM.

A:A reference to Wiegner, M., Gasteiger J., Kandler, K., Weinzierl, B., Rasp, K., Es-selborn, M., Freudenthaler V., Heese, B., Toledano, C., Tesche, M., Althausen, D.: Numerical simulations of optical properties of Saharian dust aerosols with emphasis on lidar applications, *Tellus B*, 61, 180-194, 2009 has been added.

R:1267/7ff: The calibration method described is questionable, in particular, as the SNR from the “Rayleigh-range” is very low. A more accurate method is described in e.g. Freudenthaler et al., 2009, (*Tellus*).

A:There are indeed absolute calibration methods relying on assessing the relative responses of the two channels, as the one described in the aforementioned article, or in those in the manuscripts quoted in our discussion paper a few lines below (1267/15-18). In fact our retrieval algorithm allows for an alternative determination of the depolarization based on an a priori assessment of K, retrieved with algorithms that are similar to those described in the suggested Freudenthaler et al. 2009 (see as instance Snels et al., 2009). However, such absolute K determination should be repeated everytime changes may occur in the instrument response (photomultiplier gain, photoncounting threshold levels, and everything may influence them, optics cleanings, even a change in the laser emitted power, that may affect both the state of polarization of the emitted light in some lasers, and the linear response of the acquisition chain, and so on and on. . .). Such instrumental parameters had been changed, and we did not assess the K factor before the flights. However, our experience was that the “quick and dirty”

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“Rayleigh range” depolarization calibration (otherwise said, as in Freudenthaler et al. 2009, “0° calibration”), based on the assumption of a pure molecular depolarization in a fixed region of the atmosphere, often gives results that are consistent with the more accurate ones based on the absolute K determination. With “consistent” we mean that the attribution of the aerosol class based on depolarization is substantially not hampered by the less accurate “Rayleigh range” procedure. In any case, the reviewer is right in asking a more thoughtful description of the depolarization calibration procedure. After introducing the operative definition of Volume depolarization (eq. (14), now (15)), we have added: “In (15) it is apparent that, apart from the coefficient K, a calibration constant accounting for the difference in the responses of the two channels, only the measured signals contribute to its random error. However, an incorrect determination of K leads to significant systematic errors severely affecting the accuracy of the measurements. This coefficient can be directly measured by a variety of procedures (Freudenthaler et al., 2009, Alvarez et al., 2006) that exploit a controlled splitting of the backscattered light into its parallel and cross polarized components, to be fed into the receiving channels. In the data presented hereafter, a different depolarization calibration approach has been used, the so called 0° calibration. In this approach, K is chosen in order for the depolarization to obtain the theoretical value to be expected for the atmospheric backscattering from a region where the aerosol presence can be considered negligible, and the observed depolarization is assumed to come from molecules alone (Young, 1980). In our case, this theoretical value was set to 0.014 (Behrendt et al, 2002). To determine K, a mean atmospheric profile with reduced SNR was created by averaging the measurements for several minutes, and an atmospheric region, namely the “Rayleigh range”, where particulate scattering could be considered negligible was determined around 8 km. This procedure offer itself to criticism, as even a small amount of depolarizing aerosol in the Rayleigh range leads to an uncorrected bias, inducing a severe underestimation of the aerosol depolarization throughout the profile. An absolute determination of the K coefficient of our system was performed after the flights, by illuminating the lidar telescope, covered with a thick slab of Teflon,

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with a collimated beam from a high power quartz lamp. The diffuse transmission in the forward direction, resulting completely unpolarized, allowed an absolute determination of the channel gain ratio. The agreement of the absolute determination of the K coefficient with the value retrieved with the Rayleigh range approach confirmed the correctness of our previous assumptions. “

R:1268/5ff: it should be explicitly stated that the lidar measurements are pointing to the zenith.

A:We have added to our text: “The system was placed in the aircraft, pointing to the zenith through an open hatch on the ceiling of the fuselage.”

R:1269/14: Times in “hours:min” would be more convenient.

A:Done.

R:1269/20: Please give more details how Fig.4 is calculated. Is beta_p set constant with range? What is the reason for including the “molecular backscatter coefficient”.

A:New text has been added, as quoted hereabove. The molecular backscatter coefficient line just gives a reference. From there, with little visualization skill, one could infer the backscatter ratio corresponding to the reported aerosol backscatter coefficients.

R:1270/15: It is indeed difficult to recognize any feature in the MODIS images (Fig. 5; exchange left and right!); so it does not really help the reader. If the cloud was “subvisible” (line 23) it cannot be seen by MODIS (per definitionem). Which spectral range is used for the Angström coefficient.

A:The Figure caption has been completed and corrected. The aim of presenting the figure was both to geographically locate our measurements, and to show the (low) intensity of the event, under both respects the figure is valuable.

R:1271/1ff: Fig. 7: from the figure it seems to be impossible to get a signal from the upper free troposphere: how is it possible under these conditions to calibrate the

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depolarization measurements?

A:Please refer to what exposed hereabove, when describing the depolarization calibration.

R:1271/14ff: Fig. 8: what is the reason for the “white spot” at 55000 s in 1.5 km? According to the text the PBL height was 1-1.5 km. Is this consistent with Fig. 8 and the fact that the flight altitude was already 1 km? In general: it should be clearly stated throughout the paper, what “height” is: above ground, or above flight level, or . . .

A:We have dropped out of the text any reference to PBL height, as its determination would have needed much more work than a simple visual inspection of the aerosol distribution. The “clean air” region is of interest the reviewer underline is surely of interest, we have no reason to consider that as an instrumental artefact. We use the term “altitude and “height” when referring from the ground. We use the term “range” or “distance” when from the lidar.

R:1272/9ff: it does not make much sense to mix “mega-meter” and “kilo-meter”.

A:We conformed to km-1.

R:1272/22ff: If e.g. volcanic ash shall be distinguished from desert dust, the knowledge of the volume depolarization ratio is not sufficient. Will it be possible to derive the particle depolarization ratio? Which accuracy is expected (considering the calibration of the depolarization channels)? Is this accuracy sufficient? Please comment on this issue.

A:A comment on the aerosol depolarization has been included in 3.2: “An average particulate depolarization of 40-50 % throughout the layer could be inferred. The uncertainty on the particulate depolarization is severely affected by the extremely low value of the aerosol backscattering, hence in our case the error to attribute to this parameter exceeds 100 %. The inferred value would be consistent with what expected for mineral dust, but a 5-days backtrajectory analysis show no sign of origin from dust

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source regions, hence do not support such attribution. In any case, the measurement uncertainty due to the paucity of aerosol do not allow a reliable classification.” The sensitivity for aerosol depolarization measurements is of course dependent on the amount of aerosol. We thought it might have been to state in the paper our ideas on the use of our lidar observations taken alone for volcanic ash discrimination, hence we have added to the conclusion of the manuscript the following: “. . . while the availability of airborne lidars as the one here presented, and the effort to improve the accuracy of its aerosol parameter retrieval is undoubtedly worth of, any improvement will probably never result - alone - in a totally unambiguous classification of the aerosol, and a quantitative assessment of the aerosol mass concentration through extinction-to-mass or backscatter-to-mass coefficients. The depolarizing properties of the volcanic cloud - and its mass-to backscatter ratio - depend on the particular volcano, on the particular eruption, on the age of the ash cloud, on the thermo dynamical conditions encountered along its trajectory and so on and on, the microphysics of volcanic clouds being largely unknown, and there is still a relatively poor database of in-situ and remote sensing measurement comparisons to support the results of lidar inversions. The unequivocal attribution of the type of particles observed and a reliable estimate of their mass concentration will have to be based on ancillary information from transport models, and from the synergic use of other remote sensing (Gasteiger et al., 2011, Ansmann et al., 2010) and in situ measurements (Flentje2010).”

R:1274/10ff: What is the meaning of the numbers added to all references?

A:These number did not appear in our submission, we do not know from where they come. Maybe an editing procedure of the journal.

R:Tables 2 and 3 are never mentioned in the manuscript!

A:They are mentioned in the Appendix. In the previous version, the numbering was incorrect.

R:Fig. 9: nice photo, but not really necessary.

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A:For us is fine to remove it, we leave the decision to the Editor.

R:Figs.10/11: in Fig. 11 a pronounced layer can be seen in 6 km, but not in Fig. 10. How trustworthy is Fig. 11?

A:The values of volume depolarization ratio of the aerosol layer appearing in a layer at 6 km in fig. 11, are around 2-3%. For highly depolarizing particulate, whose aerosol depolarization is 50%, this corresponds to an aerosol backscatter ratio around 0.05, which in turn would render an aerosol backscatter coefficient around $0.05 \cdot 10^{-3} \text{ km}^{-1} \text{ sr}^{-1}$. We think that the data shown in figure 10, depicting a POLARIZED aerosol backscatter coefficient (sorry to admit another fault in the figure and caption, of the many already pointed out by the reviewer) close to $0.05 \cdot 10^{-3} \text{ km}^{-1} \text{ sr}^{-1}$, are consistent with the observed depolarization.

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/5/C1089/2012/amtd-5-C1089-2012-supplement.pdf>

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 1253, 2012.

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