

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

Anonymous Referee #2

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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, 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

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(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.

Some more specific details:

1254/18: the statement that “aerosol optical properties” can be achieved with a resolution of seconds is a little bit optimistic (if the accuracy should be high).

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 Eyjafjallajökull eruption and several aerosol campaigns. Airborne lidars have also frequently been used by scientists from the US.

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

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

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

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?

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

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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).

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.

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

1260/8: The way how the range for the merging of the signals (current and photo counting 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.

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). It should be explained where the “R₀” comes from (Eq. 10; missing in Eq. 7). 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. The main shortcoming of this section is that it mainly consists of general remarks, whereas the actual errors of the retrieved optical parameters (beta, delta; see Section 3) are missing.

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

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.

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

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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.

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).

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

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

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

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.

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 depolarization measurements?

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 ...

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

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

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particle depolarization ratio? Which accuracy is expected (considering the calibration of the depolarization channels)? Is this accuracy sufficient? Please comment on this issue.

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

Tables 2 and 3 are never mentioned in the manuscript!

Fig. 9: nice photo, but not really necessary

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?

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

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