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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "French airborne lidar measurements for Eyjafjallajökull ash plume survey" *by* P. Chazette et al.

**Anonymous Referee #2**

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General

The paper contains original material (lidar observations of Eyjafjallajökull volcanic aerosol) and is thus appropriate for ACP. However, major revisions are required.

The most critical point is the depolarization ratio retrieval and the presented results. The values obtained and shown are not in the range of other published volcanic aerosol depolarization ratios (Ansmann, JGR 2010, Gross, Atmos Env. 2012, Miffre, GRL2011, Miffre, Atmos. Env. 2012). So, in such a case a reviewer has to ask: Was everything ok with the observation? ...from the technical point of view? ... from the data processing point of view? Only after a rigorous quality-assurance discussion and after quantifying the error bars (range of bias) one may conclude: Yes, our measurements show indeed trustworthy values outside the published range of other Eyjafjalla observations.



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Details

Introduction:

Page 6625, line 18: The topic of the paper is volcanic ash in the troposphere, not in the stratosphere. A review of the literature regarding tropospheric lidar observations of volcanic aerosols would be more appropriate (e.g., Pappalardo, GRL 2004, Wang, Atmos. Env. 2008, Sassen, GRL 2007, Mattis, JGR 2010).

Page 6626, line 7: The Marenco JGR 2011 paper points to the direction that something was (or better 'is') completely wrong with the depolarization measurement with the Leosphere lidar. At least, they state that depolarization measurements were NOT possible because of high temperatures in the FAAM aircraft. And the problems are the same here, I assume!

Page 6627, line 14: With a field of view of 2.3 mrad one may already run into problems with multiple scattering in case of large volcanic particles. Any comment?

Page 6627, line 24: I am not sure that you can use climatological profiles of air density to estimate 355 nm Rayleigh scattering effects. At least in optically weak traces of volcanic aerosol layers in the free troposphere such a rough estimation will cause significant errors. Should be checked and discussed!

Page 6628, line 6: The volume depol ratio VDR is directly proportional to R-c, and this quantity is obviously highly temperature sensitive (as Marenco et al. 2011 state). In the present paper, R-c is 16 (at ground, unfortunately no temperature is mentioned), and then it is 6.4 (almost a factor of THREE!!!! lower) at about 30 degree C, and about 10.5 at about 18 C, the authors tell us. Although there is such a strong dependence on temperature, the temperature was obviously not recorded during flight (in the receiver unit) as a function of time. As a consequence, it remains unkown, whether the selcted R-c values are ok or not, and thus whether the depolarization ratio values are ok or not. May be the temperature was 21 C in the receiver unit later on, and the R-c values decreased to 8-9? This may already explain the large bias in the depolarization ratios, shown in the last figure. To be clear, it is not just my role to criticize the observational findings, my role is to ask questions so that the authors get the chance to re-check their results and at the end discuss their findings at least more carefully. And this seems to be necessary here. The uncertainties are obviously very high, and probably explain the (too?) high depolarization ratios. The statistically trustworthy values in the publications mentioned above are at all below PRD=40%. Do you know the reason for the strong impact of temperature in the beam separation unit? Marenco et al (2011) were obviously surprised by the fact that all the depolarization observations turned out to be useless after their first flights. They obviously had no idea about an apparent temperature impact.

Page 6629, lofted layer approach: Note that the lidar ratio or BER you obtain by using the assumption of pure Rayleigh scattering below and above an extended aerosol layer is the column mean BER. But BER may vary considerably from 40-70 as a function of changing sulfat/ash concentration ratio.

Page 6630, line 3: According to one of the latest publications (Wynn Eberhard , Appl. Opt., 2010?) the King factor may be more close to 1.05.

Result section

Page 6631: Why not presenting the 19 April observations in addition?

Page 6631, line 11: When the AOT is so low, less than 0.03, it seems impossible to observe a VDR of 5% at 355 nm. Even in a subvisible, highly depolarizing cirrus, the 355 nm VDR is lower than 5%, I guess, when the AOT is less than 0.03. Please check. Unfortunately the 21 April case is not further investigated here (not considered).

Page 6631, line 17: Please provide the information that this flight was on 12 May 2010 in the text right in the beginning of the discussion.

Page 6631: Note that the Eyjafjalla papers with aircraft observations over the North Sea



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suggest that the volcanic aerosol layers in April were dominated by ash (sufate formed a few days later), and that the eruptions in May showed, right from the beginning, always a sulfate-ash mixture. This circumstance makes the questionsmark regarding the rather high depolarization ratios presented in the last figure even larger!

Page 6632, line 6: When there were clouds on 16 May, then the humidity was certainly very high and affected the observations (even after cloud screening, I guess). If RH is larger then 80% water uptake may lead to a decrease of the depolarization ratio. . . ....

Page 6633, line 15: Unfortunately another R is introduced. . . The influence of a wrong assumption for the backscatter ratio R (assumed to be one above and below the cloud) is studied by looking at solutions for R=1.05 and R=1.1 at the lower height. But, in such volcanic times, is it unrealistic to assume that was more likely between R=1 and 1.2 to 1.5 instead of 1.05 to 1.1 (even at 355 nm)?

Page 6634, line 11: Now cirrus comes into play! To explain the almost not explainable findings. Are there RH values (at cirrus level available (even if taken from ECMWF model runs). And when there was a cirrus at such a low height (of 5 km (!)) that should have been noticed in the repective backscatter color plots. A cirrus produces certainly an abrupt change in the backscatter properties. Was that the case? The presence of large sulfate particles may also reduce the lidar ratio, increase the BER, who knows? So, there are more possibilities that must be mentioned and discussed. But an increase in BER together with the high depolarization ratio of course points to ice crystals (. . .if these questionable depolarization ratios show us the truth!!).

Page 6634, line 21: As mentioned, the April eruptions were different from the May eruptions (more sulfate). So, the VDR (PDR) values should have been larger in April. This is what the Cabauw (Netherlands) lidar team observed (as found on their web page).

Page 6635, line 2: To my opinion, these high depolarization values are not trustworthy and are in contradiction not only to previous papers but also with the fact that the May



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eruptions produced more sulfate mixed with ash, as mentioned. So, the May 2010 depolarization ratios should be lower than the April 2010 values (that would at least be understandable) rather than considerably higher.

If the authors want to show these questionable results they have at least to provide a rather careful discussion and they have to clearly mention these very large uncertainties caused by the temperature dependence of R-c. One should put large error bars to the PDR values in Figure 10.

Page 6636, line 14: One has to know the sulfate contribution to the measured backscatter coefficient. As Ansmann (JGR 2011) show, before one can estimate ash mass concentrations, one must subtarct the sulfate mass. That must be mentioned clearly.

All in all, there are so many serious sources of uncertainty that, at the end, I am not sure whether this paper is publishable.

Comments to the figures:

Figure 2, I would like to see the VDR and PDR for this case with a rather strong volcanic aerosol load, and because there was a Raman lidar that may have measured some BER values. Please use different colors (green, blue, red) for the different profiles in Figure 2.

Figure 8, left top: To assume that pure Rayleigh scattering occurred at the base of the volcanic layer (in a just 100 m thick layer!) before large aerosol scattering below this layer was detected, is almost not acceptable. As a consequence, the AOT and BER values are just estimates with very large uncertainties. Should be mentioned.

Figure 8, right, top: Here, the situation is much better (assumption of an isolated lofted aerosol layer is justified). The same is true for the other two cases. However, at 355 nm the unkown contribution of volcanic particles to backscattering below the main layer remains always a problem.

Figure 8: Please move the AOT values into the plots (remove them from the x axis



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text).

Figure 10, left, top: Because of the problems with the Rayleigh assumption (mentioned in Figure 8 comment) the particle backscatter coefficients are not trustworthy (can be increased significantly), so the PDR values are not trustworthy. The PDR values decrease with increasing particle backscatter coefficient, and can thus be easily moved into the range of values published in other papers (PDR below 0.4).

Figure 10, right, top: Here the R-c temperature uncertainty remains as serious error source. Only the lower layer produces (noisy) PDR values that look quite reasonable, but the systematic increase with height indicates that noise is strong (and the depol ratio is determined from a ratio of noisy signals. . .). So, this case is at all not trustworthy to me.

Figure 10, bottom panels: Noise impact is comparably low. R-c bias remains as the only strong error source? Rayleigh calibration (0.0039 in layers without aerosol) may be another source, because signals are noisy above the volcanic layers. But no VDR is shown for the aerosol-free upper troposphere or for the pure Rayleigh layers below the volcanic layers. Should be done to convince the reader, that depol observations are not that bad. Why is the depol ratio increasing from realistic values at the base of the volcanic to unrealistic values in the center, and then back to more realistic values. I would expect an almost straight line (almost height independent depol ratio). All in all, I do not trust the depolarization observations.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 6623, 2012.