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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 #1

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This paper describes the airborne measurements of volcanic ash with a Leosphere instrument on board the Falcon 20 aircraft during three flights over France, the Atlantic, and England, respectively. The analysis of the lidar signals permitted evaluating the lidar ratio (33-48 sr) and the depolarization ratio (45 +/- 7%), and evaluate the ash concentration by applying a specific cross-section.

I think that the most surprising finding in the article is the large ash depolarization ratio, 45%, accompanied by the low estimate of lidar ratio (down to 33 sr). This has to be compared for instance to Ansmann et al (2010, 2011), were a depolarization ratio of 35-36% is documented, accompanied by a lidar ratio of 50-60 sr.

The authors attempt a possible explanation for this difference in the following terms: "The ash plumes may evolve during transport by particle settling and their optical properties may be affected. [...] Moreover, the ash plume is very heterogeneous and the ash properties could be different from an eruption to another. This may explain that the ash optical properties are not necessarily the same from a location to another, and from a time to another."

Whereas such an explanation is not to be ruled out, I think that it is of paramount importance to prove that a correct experimental methodology has been followed. In particular, the evaluation of the imperfect separation of the two light polarizations at the receiver should get more attention, as any error there would have repercussions on the depolarization ratio and on the lidar ratio. In the present form, the paper does not convince on such a rigorous experimental methodology.

I recommend for this paper a thorough major revision, with a full rewriting of how depolarization is treated and characterized.

MAJOR COMMENTS:

1) The separation of lidar signals in two channels, based on depolarization, is in general affected by the following undesired effects: [a] laser output not fully polarized; [b] laser polarization not fully aligned to the receiver channels, i.e. a rotation; and [c] imperfect separation of signals at the receiver. In the present form of the paper (section 2.2), only the latter aspect is taken into account. The authors should discuss and quantify the contribution of effects [a] and [b], describe how they are minimized during instrument design, and provide the corrections that need to be used to cancel them during data analysis.

2) Equations 2 and 3 are formally correct (although they only account for the effect [c] discussed above). However, it is easy to verify that some terms are negligible. In equation 2, the second term in the denominator is less than 0.1% of the first term, and in equation 3 the first term in the denominator is less than 0.1% compared to the second term (try substituting VDR=0.5, for instance). Similarly, (1-T1perp)(1-T2perp) is almost 1 (0.2% error). You should therefore either simplify the equations, or at least

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say clearly in the text that those terms are negligible. Otherwise you confuse the reader on what is really important and what is not.

3) Once your equations are simplified as discussed above, it is easy to see that the important term controlling the imperfect separation of your channels is (1-T1par)(1-T2par) and is equal to ∼0.05 and ∼0.01 for the two systems respectively. This evaluation is based on the transmission of each Brewster plate, considered separately. However, it will be affected by the angle of incidence of light onto each plate. I would assume that the transmission values Tpar and Tperp given in the paper are valid for a perfect positioning of the plates (Tperp minimized). How can you be sure that your Brewster plates are correctly positioned? Please describe the method used to position and align the plates, and how the transmissions Tpar and Tperp are determined.

4) Atmospheric calibration of the depolarization channel (determination of Rc - lines 9-10 after equation 3): is this done on each vertical profile separately, or do you determine this constant once and assume it does not vary after that during each flight? Later (bottom of page) you say that Rc varies with laser temperature; what is your uncertainty on Rc after accounting for these thermal variations? Are you monitoring temperature constantly?

5) If you can prove that all the above have been done correctly, then we can start believing your findings. If you can't prove it, however, it is easy to show what may be the consequence on an incorrect evaluation. Let me first write equation 3 in a simplified form: VDR = [(T1par S2) / (Rc S1)] - (1-T1par)(1-T2par). When you do your atmospheric calibration, therefore: Rc = (T1par S2_m) / {S1_m [VDR_m + (1-T1par)(1-T2par)]}. If for instance you have overestimated (1-T1par)(1-T2par), you will underestimate Rc. In return, this underestimated Rc will give you an overestimated depolarization. Moreover, you can see from equation 2 that this will also result in a larger signal in the ash layer (but not in the molecular layers). Therefore you will overestimate backscattering but not extinction (extinction is estimated from the attenuation of the molecular layers), i.e. you will get an underestimated lidar ratio. I do not say that this is necessarily happening,

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but it is a another possible explanation of your results. It is therefore really important to prove that everything has been done for a correct evaluation of the (1-T1par)(1-T2par) term.

6) In section 4.1.1 you assess the potential bias for aerosols at the lower reference altitude, but ignore the possible bias at the higher altitude (line 15). The latter would probably result in a larger bias because it is at the near side. I recommend assessing this uncertainty as well.

7) Conclusions need to be expanded. Compare your estimated concentrations to dispersion models and/or simultaneous satellite estimates, and explain the impacts that your methods may have on future research. Is there any synergy with data from other instrumented aircrafts?

MINOR COMMENTS:

8) Introduction, p. 6625, lines 18-28. It sounds as though volcanic aerosols in the stratosphere are the same as volcanic ash. Please stress the difference between these sulfuric aerosols and ash particles.

9) Section 2.3, p. 6630, lines 7-8. The BER is assumed constant for the entired ash layer: specify whether you assume it to be constant in the vertical direction only, or whether you use a constant BER in time and space for a whole flight.

10) Equation 7. A similar equation is given in Freudenthaler et al (2009, eq. 20) and references therein. Please cite appropriately.

11) Section 3, p. 6631, line 13. How is the AOT derived? Is it from the airborne lidar? Please specify. Line 19: "high density"; you probably want to say that you have a large lidar signal (you have at this stage no information on density).

12) Section 4, p. 6632, line 16, "mean profiles". Indicate in figures 3, 4, and 6 which is the interval on which you average (vertical bars on the height-time plot should do). It would actually be really worth it to apply the method to single vertical profiles (rather

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than averages), to assess the variability (minimum/maximum) of the AOT and concentration.

13) Section 4.1.1, p. 6633, line 8. Specify what is the altitude interval on which you calculate AOT. I would assume that it is between the normalization heights in figure 8, but it needs to be specified.

14) Section 4.1.2, p. 6633, line 25. I do not understand what is a dichotomy approach. P. 6634, line 2, "maximum": specify that you speak about a maximum in the vertical direction (not in time or in the horizontal direction).

15) Section 4.1.4, p. 6635, lines 6-7. Besides gravitational settling of larger particles, other transformations may happen as volcanic aerosols age; for instance the formation of fine particles by oxidation of SO2. Line 9, residence time of 3 days: if you believe that this is the reason for your results, you should compare this to the residence time of ash observations of the other papers that you cite.

16) Section 4.2, p. 6636, lines 8-11, "the AMC varies from 190 to 2160". The concentration does not vary, but there is an uncertainty (quite a different concept).

17) In figure 3, a PBL aerosol layer is shown at around 7E longitude. Specify which criteria you have used to assess that it contains no volcanic ash. Depolarization?

18) In figure 8, indicate the mean time, latitude and longitude for each vertical profile, so that they can be checked against, e.g., Meteosat images.

19) In figure 9, add the zero extinction line.

20) Figures 2 and 10 would benefit from using colored curves. At present, distinguishing the curves is quite hard.

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