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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lidar observation and model simulation of a volcanic-ash-induced cirrus cloud during the Eyjafjallajökull eruption" *by* C. Rolf et al.

Anonymous Referee #1

Received and published: 6 August 2012

General:

The paper is based on original work and combines lidar observations of volcanic aerosol with modelling of the impact of volcanic IN on cirrus formation. The paper is appropriate for ACP.

Minor revisions are required.

Details

Abstract: I expected that the volcanic aerosol provided a high IN concentration in the upper troposphere shortly after the Eyjafjallajökull eruption in mid April 2010, much more than just a factor of 10 compared to clean background conditions. May be the given background value (0.01 cm-3) is too high? Please check Richardson et al. (JGR 2007) and other papers dealing with particle concentration in the upper free troposphere.

1 Introduction

Page 15676, line 21: the first large eruption occurred on 14 April 2010.

Line 24: The second paragraph is a bit misleading. Seifert et al. (2011) is introduced too late. Right in the beginning of the paragraph, there should be more information regarding recent observations of clouds and Eyjafjallajökull volcanic aerosols. . .. There are several special issues (ACP, Atmos. Environ, JGR) available.

Page 15677, line 29 (last line): increasing depolarization is found also by others over central Europe. However, at 355 nm, it is hard to see that based on volume depolarization ratios. Could you provide some numbers for the particle depolarization ratio (i.e., depolarization ratios after removing the dominating Rayleigh effects)?

2 Instrumentation

Page 15678, line 21: Leo-Lidar sounds like a nick name, please provide full name of the lidar type, and may be provide manufactory information. How large is the receiver telescope (primary mirror, 20cm?, 30cm?). What is the receiver field of view? Was the lidar pointing to the zenith or not? This has a strong impact on cirrus observations (backscatter, depol, because of specular reflection, multiple scattering). Should thus be mentioned.

Page 15679, lines 4-5, please check Gross et al. (Atmos. Env. 2012) for further depolarization values, and others: Miffre (GRL 2011, Atmos. Env. 2012) , Chazette (JGR 2012). Gross et al. present high quality lidar observations at 355 nm (your wavelength!). I believe, Miffre also shows observations for 355nm.

Page 15679, line 6: I learned (from discussions with Chazette and Marenco, running

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a leosphere lidar as well) that depolarization ratio observations with this lidar type are crucial, because the calibration constant is always changing with temperature. This can be a significant source of uncertainty. What are your findings here. Please provide more information on depol. observations with the lidar, and respective uncertainties.

Line 24: lidar ratios can also be found in the paper of Gross et al. (Atmos. Env. 2012). They show observations at 355 and 532 nm!

Page 15680, line 8: What do you mean with 'iterative procedure'. The Wandinger MS approach is to my opinion based on approximations and straight forward computations. Iterative procedures are not used, I believe. Please check!

3 The Eyjafjallajökull ash plume

Page 15683, line 20: I would be interested in, at least, one height profile with the PARTICLE depolarization ratio (may be computed from mean signal profiles for the two hour period before the cirrus layer was measured, as shown in Figure 4). The volume depolarization ratio at 355 nm is dominated by Rayleigh depolarization and suppresses all the information concerning aerosol depolarization. Please mention also, the zenith pointing of the lidar. It is essential for interpreting the results (specular reflection issue).

Line 21: Klett method, please specify in the text at which height the calibration height is set. Forward Klett intergration or backward Klett integration makes a strong difference. In the optimum case both solutions should match, and then you have the cirrus AOT and the cirrus lidar ratio (of course not corrected for multiple scattering).

Page 15684, line 5...... The zenith pointing of the lidar is mentioned too late here...

Page 15684, line 5.... The discussion on specular reflection is too speculative. There may be some hints that specular reflection preferably takes place at temperatures higher then -25C, but specular reflection occurs almost always and at any time when zenith pointing is used. Ice virga form always, and they form disregarding higher or lower temperatures, and it is never very clear how large the crystals are which are

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detected as falls strikes.

Again, without showing the particle depolarization ratio instead of the volume depolarization ratio, the discussion of polarization observations (especially at 355nm) is almost useless. Only in case of particle depolarization ratios different studies including this study can be compared (i.e., removal of the strong Rayleigh effect is a request!).

Line 18: The second paragraph is again just speculation (who knows?), and thus should be skipped.

Page 15685, line 1: The final paragraph is confusing and speculative, too, and should also be removed.

The most convincing reason for low PARTICLE depolarization ratios in case of vertically pointing lidars is clearly the specular reflection effect. However,

Lines 13 and 25: This is confusing! Did you measure aerosol effects with the lidar or not? That must be clearly mentioned. And then one may argue why trajectories are needed in addition? So, it clear now: We need a profile of the particle backscatter coefficient (similar to the profiles in Figure 4) already here. Figure 4 is introduced too late. And we need particle depolarization ratios in addition! Longer signal periods have probably to be averaged and smoothed vertically to obtain reliable lidar products.

Line 26: Please check the Dacre et al. paper (JGR, Eyja. Special issue, 2012) for ash transport times as a function of height.

Page 15687, line 19: The derived particle number concentrations should be compared with other available estimations such as from Seifert et al. (2011), or from aircraft observations during the volcanic event (please check the special issues mentioned above).

Line 23: The comparison with FALCON data (taken below 5 km) and the measurements presented here for the upper free troposphere makes no sense. Schumann et al analyzed the microphysical properties within a lofted pronounced ash layer. The

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cirrus discussed here formed in the upper troposphere at ash background conditions.

Page 15688, I am surprised that these ash particles are so bad IN. Just one out of hundred particles serve as IN? What would follow if you would assume that 10% are IN? Should be discussed, Note that ash particles were partly large during the first days after the eruptions, and this may not be considered in the laboratory investigations.

Line 26: I can not be believe that such a significant disturbance of the upper free troposphere produces an aerosol concentration that is just a factor of 10 higher than for clean background conditions? Check Richardson et al. (JGR 2007).

Page 15689, line 10: As mentioned, one should check the literature for other ash size distribution observations (check for example all the aircraft observations in the JGR 2012 special issue). And may be there are further AERONET observations.

Page 15690, line 24: You do not know the exact extinction-to-IN relationship! Please, avoid speculations.

Page 15691, line 16: The last paragraph is a bit confusing, please improve explanations. And what happens if the IN concentration is increased by another factor of 10 (as discussed above when 0.1 instead of 0.01 of the ash particles are IN). Would that then be in contradiction with the lidar observations?

Figure 1: The lowering of the base height and the diagonal structures clearly indicate falling ice crystals (at least during the second half of the cirrus period). So, particles were large enough to produce coherent structures and should also be large enough to cause specular reflection. Note, that only a few aligned crystals are already sufficient to produce a significantly enhanced backscatter signal. Volume depolarization ratio is misleading, depends on particle concentration. It is impossible here to decide whether there is significantly lower depolarization during the first half of the cirrus period then during the second half. Only after correction of Rayleigh depolarization effects this can be done! And must be done!

Therefore: Please show height profiles of the particle depolarization ratio for the first and second half in addition.

What is the temperature at 10 km before 20 UTC?

Figure 2: Again, temperature scale (information) would be helpful.

Figure 3: Please give color information in the caption... radiosonde profiles in blue, ECMWF data in red...

Figure 4: The profiles obviously need more vertical smoothing. Most of the shown aerosol structures are caused by a low signal-to-noise ratio. Should be mentioned.

Figure 5: Caption text must be improved.

Figure 6: Points are too small so that colors are hard to separate

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15675, 2012.