

***Interactive comment on “Airborne observations of the Eyjafjalla volcano ash cloud over Europe during air space closure in April and May 2010” by U. Schumann et al.***

**Anonymous Referee #2**

Received and published: 18 November 2010

**General**

The manuscript is well written, contains a rather dense set of information, and is thus clearly appropriate for ACP. However, the paper is, to my opinion, too long. Especially section 3.4 should/could be shortened. A paper should present the main findings and conclusions only. But section 3.4 is presented in form of a detailed field campaign report. Another point is that the retrieval of the ash mass concentration is obviously rather uncertain so that any support by other observations (may be by the ground based lidars, see the details below) should be presented in addition. Not to be misunderstood: I appreciate that a rigorous error analysis is presented. But at least some more efforts

C9958

are needed to fix the numbers, to decrease the range of uncertainty by intercomparisons. As I understood, the main goal of the FALCON mission was to measure ash mass concentrations!

Nevertheless, the overall impression is: very impressive! Minor revisions are required.

**Details**

Abstract: Long ...!

Introduction:

Page 22136, line9: Any reference for the 15.2 km altitude?

Page 22136, line 13: Add Mattis et al., JGR 2010. that paper provides an overview (height range of aerosol layering) of the aerosol situation in Europe after the eruptions in 2008 and 2009.

**Section 2.1.3**

Page 22143: What's about the idea that ash particles may not be well described by a spheroid model (same may hold for dust). Any idea about the impact of particle shape on the retrieval uncertainty..?

**Section 2.4**

Page 22150, line 20: Leibniz Institute FOR Tropospheric Research, IfT ... seems to be the most accurate name of the Leipzig institute.

**Section 3.1**

Page 22151, lines 22-24: This is speculation, or do trajectories or Flexpart results support this speculation, should be avoided .... (Mike Fromm, communication).

**Section 3.1**

Page 22152, line 16:  $D_{\text{eff}} = 3 V/(2A)$ , is that true? I guess it is:  $D_{\text{eff}} = 6 V/A$  because

C9959

what I remember is that  $r_{\text{eff}} = 3 \text{ times } \frac{4}{3} \pi r_3^3 / \frac{4}{3} \pi r_2^3 = 3 V/A$ ?

If I am right please check all the results in this respect.

### Section 3.3

Page 22156, lines 14-16: What's about this idea: calculate the extinction coefficients from the size distribution (by taking in addition the set of refractive index data with which the size distribution was obtained) and compare these extinction data with the ones measured with ground-based lidar at Leipzig! .... Or does that fail because the different size distributions together with respective refractive index data always produce the same extinction properties? If the solutions computed from the size distributions are very different the lidar could indicate the best solution.

Comment: It is stressed several times that the FALCON flew over all available lidars because of quality assurance issues (I guess). If so, then (at the end) it is quite unsatisfactory that no use is made of this huge advantage. I can understand, the list of authors is already very long and the lidar intercomparison can be done in followup papers, but this point remains to be solved somehow.

### Section 3.4

This section could be significantly shortened.

### Section 4.1

Page 22175, lines 3-10: Again, I would like to see some comparison (in terms of extinction coefficients) of FALCON observations (starting from the retrieved size distribution, Mie computations. ....) and ground based lidar observations. Even if lidar only provides backscatter (I don't know whether they can apply Raman lidar at daytime?), the backscatter can be well translated into extinction.

Page 22178, line 22179: The volume to extinction ratio in the GRL Leipzig paper is obviously 0.75 or so, whereas the AERONET data (I checked many April observations

C9960

of German stations) show conversion factors always close to 0.5. So it seems to me that 0.75 is already something like the upper limit rather than a lower limit of conversion factors as suggested by the authors. They suggest that conversion factors around 1.5 could also be likely. I'm not sure.

Figure 12: The region below 3 km is not well resolved. Please expand the x axis range a bit. According to Figure 8 there might have been also a lot of ash even at lower heights.

Well written paper!

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22131, 2010.

C9961