Atmos. Chem. Phys. Discuss., 10, C5668–C5672, 2010
www.atmos-chem-phys-discuss.net/10/C5668/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Eyjafjallajökull eruption in April 2010 – detection of volcanic plume using in-situ measurements, ozone sondes and a new generation ceilometer network" *by* H. Flentje et al.

Anonymous Referee #1

Received and published: 23 July 2010

General

The paper contains original, very interesting observations of particulate and gaseous emissions of the Eyjafjoll volcano. The in situ measurement part is excellent. The material is appropriate for ACP.

However, my comments deal mostly with the ceilometer observations. This part of the paper needs to be improved. Revisions are necessary here.

Details.

Page 14948, ABSTRACT: The abstract shoud briefly summarize: What will be presented, what are the main results.

Page 14948, line 22: IES, 2010? Not explained, not in the references.

Page 14948, line 25: The volcano erupted on 14 April, so the airflow on 14/15 April is important, not on 16 April.

Page 14949, line 7: what means 'consequently' here. . ..? Any decision of VAAC leads to a stop of air traffic?

Page 14949, line 12-17: How do you know all these details? References? EARLINET? AERONET? Ceilometers? Satellites? Backward trajectory modelling? Please specify, provide references (including personal communication, if necessary).

Page 14949, line 18: . . .of this event . . .. Please add location: at Hophenpeissenberg and Zugspitz mountain. . . ..of ceilometers . . . add: in Germany.

Page 14949, line 26: I cannot find Flentje et al., 2010!

Page 14950, line 2: extinction coefficients to about 5x10-6 m-1 . . .. for what signal averaging period, signal smoothing length? If the ceilometer is that sensitive even for high temporal and vertical resolution, why does it then not detect the thin volcanic ash layers? With other words, if you show height-time displays of the range-corrected backscatter signal, and the traces of volcanic ash remain unresolved, you obviously do not detect all the layers with extinction down to 5x10-6 m-1. Only the rather thick layer on 16/17 April are obviously detected.

How large is the uncertainty of the retrieved 1064nm backscatter coefficient? 100 %? The backscatter coefficient is used to estimate the extinction coefficient! The extinction coefficient cannot be determined by a standard lidar? How large is the error of the conversion from backscatter to extinction? 50%? And this latter quantity (extinction) is then used to estimate the mass concentration? How large is the error in this conversion? 100%? These numbers may be too pessimistic, but they clearly say that a

discussion on errors (lidar signal calibration at reference height, impact of lidar ratio uncertainties), and on mass to extinction conversion is demanded.

Page 14950, line 5: ....solid state laser... include: transmitting radiation pulses

Page 14950, line 7: diameter of the primary mirror of the telescope?

Page 14950, line 13: The ceilometer provides molecular backscatter profiles?

Page 14950, line 15: You use the Klett 1981 method? So you completely ignore Rayleigh scattering? If not, the proper reference is Fernald, Appl. Opt., 1984.

Page 14950, line 17: Meanwhile, there should be ash-related lidar ratios available (even if they are only measured at 532nm). May be, you contact the Munich EARLINET group.

Page 14950, lines 19-22: These sentences are confusing You will always detect particle backscattering at 1064nm, and the situation becomes better and better with increasing amount of large particles. Please improve the text accordingly.

So, at the end of section 2.1. it seems to be clear that a discussion on the uncertainties in the Klett retrieval or Fernald retrieval is strongly required. There is no Rayleigh backscatter detectable at 1064 nm at the reference height in the clear upper troposphere, I guess. So how do you overcome this problem? How large are the remaining uncertainties.

Page 14952, lines 21: This section is not satisfactory, for several reasons. Figure 1 is not acceptable. It is impossible to read the axis text in the more than 30 height-time displays (please avoid the notation: curtain). One may show this plot to indicate THREE sites, and for these three sites (may be close to Hamburg, one in the middle part of Germany, and HP) one should present the results as given for HP in figure 2.

Page 14952, line 25: northern Germany (Fig.2) ? something is wrong here.

Page 14953, line 7: There were a lot of ash layers (not just one), sometimes up to the

tropopause, in central and western Europe on 17 April, and the following days. The ceilometer only detects the thickest one. The text is misleading, must be improved.

Page 14953, line 7: Can we trust the ceilometer? Does it really detect all the layers (even the rather weak ones in the upper troposphere)? I think a strong support would be if the structures seen by the HP ceilometer (or even better by the Munich ceilometer) are exactly the same as seen by the Munich EARLINET lidar? Furthermore, how do you know that the layer is of volcanic origin? What did the Munich EARLINET lidar detect? What was the depolarization at Munich? Is your interpretation of the slope ok (descending volcanic layer in the early morning of 17 April)? The Munich lidar data may help....

Page 14953, lines 11-12: extinction values, mass values.... What mass-to-extinction conversion numbers do you use? Provide reference! Uncertainty values are required in addition. ... You mention: ....based on co-located aerosol optical depth? Measured with sunphotometer? Nephelometer at ground cannot be used to interpret data measured above the boundary layer. Please provide more (detailed) information!

Figure 3: The ceilometer mainly detects clouds and boundary layer aerosols from 15-21 April. So, there was only one ash layer (volcanic plume) according to the ceilometer observation? The reader may draw this conclusion, what is certainly not consistent with the truth (when looking at all the EARLINET observations).

Page 14954, lines 2-4: Can you conclude that layers descend just by comparing Zugspitz and HP observations? May be there are just two different layers, seen at HP and Zugspitz.

Page 14955, line 2: 1-4 microns diameter.... Is that a typical size range of aged volcanic ash? Provide reference! I believe the particles were much larger? Can we characterize this ash already as aged aerosol? The ash and other emissions were just ejected a few days ago. Is that already aged ash...?

Page 14956, line 21-29: There may have been layers higher up (or even something like a volcanic background in the full troposphere). Again, one may check the Munich lidar data to obtain a sufficient overview of the event.

Page 14957, lines10-12: I am not totally convinced that the ceilometer network does an exhaustingly good job regarding the Eyjafjoll emission documentation. So, please be a bit more careful.

Page 14952, lines 19-27: The conclusions may be true (if there would be a convincing, thorough comparison with, e.g., the Munich lidar), but appear speculative at the moment as long as we do not know how sensitive the ceilometer actually is and what the uncertainties are.

Figure 2: Are these features (including the feature around 4 km between 0000 ands 0300 UTC) exactly the same as in the color plots of the Munich lidar? Please check the quick looks available in the internet. I saw a lot during these April days.

Figure 4: the same horizontal lines for the particle concentration would be fine. . .

Figure 8: at the top of the plot. . .... 2010 04 14 04:42 not . . .. 16 04:42. . .

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14947, 2010.