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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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 #1

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Review of Airborne observations of the Eyjafjalla volcano ash cloud over Europe during air space closure in April and May 2010 by Schumann et al

General comments: The paper is very long and left me feeling rather exhausted. However, I like the paper and feel that it provides a wealth of data that will be of use to the scientific community in assessing the in-situ properties of volcanic ash and the models for predicting ash concentrations. Given that the data will probably be worked on further (e.g. there are a whole host of useful measurements that the ground based research community will make use of), it is important not to state definitively anything too strongly, given that further research may prove some of the assumptions unfounded.

Derivation of mass concentration is difficult from the instruments particularly because the large ash particles are difficult to sample accurately. The authors point out that the mass concentrations that are derived are very strongly dependent on the choice of the imaginary part of the refractive index. There is obviously some absorption in the ash as the ash layers appear quite dark grey, not white, which one would expect for a conservative scattering medium.

Some idea of the optical parameters derived from the measurements (e.g. specific extinction, asymmetry and single scattering albedo) would be useful. The authors seem to suggest that they favour the non-absorbing large particles using fairly simple terminal fall velocity arguments. The authors need to be quite careful here: there are several observations inferred from lidars where the peak concentrations exceed 1000μgm-3 (e.g. Ansmann et al. and several other papers that are being worked on). This could be due to a variety of factors. It would be prudent to make a comparison of the mass derived from the aircraft in-situ measurements with those derived from e.g. the Leipzig lidar, which was overflown on 19th April.

The size distribution from the FSSP leaves me wondering how well this instrument performs in measuring volcanic ash. The fact that the size distribution does not show a maximum but keeps increasing in mass as a function of particle diameter leads me to think that it is not suitable for measuring ash. I know that this instrument has been used for measuring dust during SAMUM, but I have the same concerns that the size distribution measured for mineral dust is also not well characterised by the FSSP. The size distributions certainly bare little resemblance to e.g. the coarse mode size distributions for either mineral dust or for ash that has been inferred from AERONET almucantar scans or indeed from other airborne observations such as those from the BAe146 aircraft which have been made available to the authors.

Charging of particles becomes a significant factor in mineral dust plumes and tends to increase the particle residence time for large particles. This really needs to be explicitly stated as the simple model takes no account of these factors.

Specific Comments:

P2, l14. weakly or moderately -> non- or moderately. I would suggest that the single scattering albedo at 550nm be indicated here as it is this that will give the reader an idea of the absorption, not the imaginary part of the refractive index. P2 l16. Are the sedimentation results really accurate enough to definitively state that the ash is non-absorbing? There is some absorption as the ash appears grey, not white.

P2 l19. damages -> damage.

P2 l25. dark layers -> dark layers when viewed side on. Again the 'darkness' of the layer suggests that there is absorption, which goes against the sedimentation arguments of l16 and l17.

P2 l31-32. "The volcano ejected 40Tg of ash and 10Tg SO2". It is cavalier to include these figures without any error bars. You say on page 13 that the 10-100Tg ash and on page 40 that these are 'order of magnitude estimates'. You should change the sentence accordingly to something like "Order of magnitude estimates suggest that the volcano ejected 40Tg of ash and 10Tg SO2, but these estimates are uncertain to approximately an order of magnitude given the simple extrapolation of the aircraft measurements using volcanic activity data."

P4 l18. As you're looking at the recent record of large eruptions, I suggest including the following at the end of line 18. "More recently still, Sarychev erupted in June 2009 injecting around 1.2TgSO2 into the stratosphere. The sulphur dioxide was tracked by the IASI instrument and the resultant sulphuric acid aerosol was detected by surface and ground-based lidars, and sun-photometers (Haywood et al., 2010; d'Amico et al., 2010).

d'Amico, G., A. Amodeo, A. Boselli, A. Giunta, F. Madonna, L. Mona, G. Pappalardo, J. Haywood, A. Jones, N. Bellouin, P. Telford, Stratospheric aerosol layers over southern Italy during the summer of 2009: Lidar observations and model comparison, International Laser Radar Conference Proceedings, St Petersburg, June 2010.

Haywood, J.M., A. Jones, L. Clarisse, A. Bourassa, J. Barnes, P. Telford, N. Bellouin, O. Boucher, P. Agnew, C. Clerbaux, P. Coheur, D. Degenstein, P. Braesicke, Observations of the eruption of the Sarychev volcano and simulations using the HadGEM2 climate model, J. Geophys. Res., in press.

P5 l17-l26. Change ‘plume height’ to ‘plume height, plume depth’.

P6 l21. The fact that the Falcon has been used in desert dust regions without engine damage should be qualified. Once volcanic ash has been melted and crystalised the melting point is significantly lower than if is has not been previously melted. The melting point for mineral dust is typically 1500-1700C while the melting point for volcanic ash is ∼1100C. The typical operating temperature of modern jet engines is about 1400C. So it’s not surprising that the Falcon can make measurements of mineral dust without any damage to the engines.

P13, l28-29. Here you are much more realistic about the uncertainty in the ash emission – 10-100TG. Yet you quote 40Tg with no error estimate in the abstract – the abstract should not be used to over-state the results from the body of the paper.

P13, l33. SEVIRI should be mentioned initially. Sugegst changing to “In order to monitor the VA plume, we utilise the Spinning Enhanced Visible and Infrared Imager (SEVIRI) ‘dust’ image which is a . . .. . .”

P14, l1-9. This paragraph is not well written. I suggest “This imagery allows identification and tracking of the ash cloud 24-hours a day and indicates regions where the ash might be obscured by ice on the volcanic ash particles or by overlying clouds. The frequency of these images is every 15 minutes which allows . . .. Therefore, we have developed a variant of this method for ash.”

P14, l16. Change judgement -> inspection.

P15, l20-21. Change “with high probability were free” to “which were forecast to be

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free”

P15, l23-29. It is a shame that no comparison with lidars is shown – you just try and describe them – a Figure would be useful here.

Section 3.2. Fig 5b. The caption looks wrong to me. Is the large particle really ammonium sulphate? 2micron ammonium sulphate particles must be extremely uncommon …... Any inference of ‘trend’ between two points (2 May and 17 May) are impossible to determine. There could certainly be changes in the volcanic material ejected during the course of the eruption.

Section 3.3. There are some independent measurements made by the PSAP. While these are inboard measurements presumably they show that the ash has some absorption – how do these relate to the assumption that the particles are entirely non-absorbing? Is there really no detection of absorption at all?

Section 3.3. It seems to me that there has been some pretty serious averaging across bins here. It looks like there are only 8 bins reported for the PCASP (nominally 15 bins on a PCASP-100X), and 6 bins for the FSSP.

Section 3.3. Figure 7. Why use such poor scales for dS and dV? This compresses the information too much. Is it because your size distributions don’t look quite right? I worry about the functionality of the FSSP instrument for measuring ash particles. In our experience, the FSSP has proven rather poor at measuring the small particle end of the size spectrum for mineral dust. The rather ad-hoc method of adjusting the FSSP to match the PCASP only works if the sensitivity of the FSSP is constant across all of the smallest sizes. If the FSSP Section 3.3. P21, l19. Can you really say that the mass derived is uncertain to 40%? Does this mean the uncertainties solely to the overlapping procedure? I don’t think you can ……. You’ve identified that uncertainties in the refractive index lead to changes in the mass by a factor of 2 to 7!

It would be very useful to see if you can provide log-normal fits to the size distributions with estimates of the single scattering albedo, asymmetry factor and the specific extinction coefficient.

Figure 12 & Fig 18. While I may have doubts about some of the details of the aerosol mass derived from the FSSP instrument, the concentrations that are coming out of the CO and, SO2, O3 all look in reasonable agreement with measurements made by the BAe146 aircraft – this is reassuring.

Section 4.1. There should at least be some acknowledgement that the particles are charged (Harrison et al., 2010). R G Harrison et al 2010 Environ. Res. Lett. 5 024004. The impact of charging will extend the lifetime of large particles in the atmosphere (http://strc.herts.ac.uk/ls/Ulanowski_et_al_DUST08.pdf). When you combine the results from these papers with the fact that the volcanic ash is more charged than mineral dust, you get an alternative explanation that large particles can indeed exists in the atmosphere for longer than your relatively simple modelling suggests. This would then suggest that the more absorbing solution may infact be more appropriate than the non-absorbing solution. At the very least this caveat needs to be included . . .. . . I would suggest that it may be wiser to suggest in the conclusions that “Determination of the mass of large aerosol particles such as volcanic ash from optical particle counters remains very challenging owing primarily to the uncertainties in the imaginary part of the refractive index. While our modelling study suggests that large particles >15μm should not be present in plumes over 2 days old, our simple model takes no account of the effect of charging of the particles. Harrison et al. (2010) showed that the volcanic ash particles were significantly charged, and Ulanowski et al (2008) shows that charged particles remain in the atmosphere for significantly longer than non-charged particles.”

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