

Interactive comment on “Four-dimensional distribution of the 2010 Eyjafjallajökull volcanic cloud over Europe observed by EARLINET” by G. Pappalardo et al.

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The paper by Gelsomina Pappalardo *et al.* combines the measurements by different lidar groups within the EARLINET network to establish an extended dataset of volcanic ash observations over Europe in April and May 2010. Volcanic layers were observed in the free troposphere at nearly all EARLINET stations, at different times during the eruption of Eyjafjallajökull: over Northern and Central Europe at first, followed by the South and the Southeast at the end of April, and later in May over Spain, Portugal, the central Mediterranean region, and the Balkans. An aerosol typing methodology based on backtrajectories has been established, which permitted to

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isolate aerosol classes based on their origin, and in the end compile a map of volcanic layers. The aerosol mask determined in this way is shown for four example sites (Hamburg, Palaiseau, Granada, and Cabauw), and discussed in detail. Finally, the European sites are grouped into five clusters, based on their region, and the evolution of the plumes for each cluster is described. The full dataset is made available for scientists on the EARLINET website.

The main highlight of this article is the presentation of ground-based lidar observations on a continental scale for the full duration of the volcanic event. To my knowledge, this is the first time that such a detailed dataset is produced, and for this reason the paper deserves full attention: in fact, in the years to come such a detailed dataset could prove extremely precious for studies on dispersion modelling and on satellite retrievals.

I feel, however, that the article could be differently organised, that the material could be presented in a more efficient manner, and that the discussion could try to summarise better the evolution of the volcanic layer at a continental scale. Indeed, this is stated as the main purpose for this article, but is actually a little lost in the discussion of the five clusters; moreover, although the quantitative estimates are briefly described in the text in terms of peak backscatter coefficient and AOD, all figures are limited to qualitative (aerosol mask) and geometric (vertical layering) aerosol properties. Presenting the results in terms of maps would probably help the reader make sense of the results in a pan-European view. Finally, I believe that the technical details of the relational database should not belong to the paper.

In summary, very good scientific observations have been collected and summarised in one dataset, and they deserve just a slightly deeper insight in the form in which they are discussed, so as to give us the big picture and highlight the most interesting features of the volcanic ash plume over Europe.

Note that at the time of writing these comments I have not gone through the dataset on the website; only through the material presented in the article.

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Specific comments follow.

Major points:

1. We need some maps of the lidar observations to meet the claim of a study on a continental scale. Therefore, I suggest replacing figure 1 with four figures (1a–1d) as follows. Each figure represents the location of the plume as from the dispersion model at a given date and time, and over it the observations are overlayed with a colour code in terms of ash AOD for each ground station. Each of the four figures represents a different date, i.e. 16/4, 17/4, 19/4 and 21/4. Similar maps at later times in May could also be shown, where the authors believe that interesting features are to be highlighted.
2. Figures 2–10 contain a lot of useful information, but are lacking in terms of showing the quantitative estimates. Therefore, for each cluster in figures 6–10, I propose showing an additional time series for the ash optical depth at either: all sites in the cluster; the average of the cluster; or a representative site in the cluster. Moreover, a similar plot could also be shown, with peak backscatter or peak extinction instead of OD.
3. Section 3. This section seems to interrupt the flow of reasoning, and moreover overviewing the volcanic event is not an aim of the article. I would therefore break it as follows: (a) the paragraphs on p. 30212 could become part of the introduction; (b) the first paragraph of p. 30213 (good agreement between EURAD and EARLINET) would be better in the conclusions, with some more detailed supporting evidence given within the paper; (c) the remaining part of section 3 could be attached to section 2.
4. The last paragraph of section 5 (aim of the paper) could actually be moved to the introduction as well.

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5. Remove appendix and figures A1–A4. This material will be better placed in a data user manual on the EARLINET website.
6. p. 30209, line 4. The paper by Marengo *et al.* (2011) does not only show that airborne lidar observations are feasible, but also makes a detailed dataset available in a similar way to the present paper. Ash layers have been identified in time and space, and quantitative estimates of concentration and optical properties are given. This dataset has already proven useful in a series of modelling and remote sensing studies. I think therefore that it could be worth mentioning with a little more detail here that these two papers show complementary data, and maybe some general quantitative comparison could be given in the results and/or discussion, indicating similarities and differences between the datasets.
7. p. 30214, lines 16–18. (a) “Longest available wavelength”: add the words: “for each station”; (b) the wavelength used will depend from site to site, but it is worth specifying a range of longest available wavelengths, e.g. “532–1064”; (c) specify your detection criteria in quantitative terms, e.g. what thresholds are used on the backscattering coefficient and/or its derivative?
8. p. 30215, line 11. Specify how cloud screening is done, i.e. manual vs. automated, thresholds used, etc. In a similar way, indicate how you distinguish cirrus from aerosols (p. 30221, line 21). As a matter of fact, cirrus data could be left in the database if properly flagged as such, and could represent a valuable starting point for studies on the aerosol-cloud interaction. Several publications exist that highlight the formation of ice clouds within volcanic layers.
9. p. 30215, line 12 and following. Specify how you set the aerosol type based on backtrajectories. Is it an automated method or do you do it manually? Have you got predefined criteria? How universal do you think the criteria are?
10. Subsection 4.1. The Hamburg site shows a marked diurnal cycle, as opposed to

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other sites. I suggest commenting why this is. Is the effect real, or is it an instrumental effect due to how the alternance of day and night affects the background? Ancillary meteorological data should be able to confirm it (e.g. diurnal cycle of temperature near the surface).

11. Subsection 4.2. I believe that it could be quite hard to distinguish a volcanic ash layer from a Saharan dust layer, based on lidar observations, or to identify the boundary between the two; even more difficult would be to tell when two such layers are mixed together. On the other hand, are backtrajectories alone reliable enough as to be sufficient for separating these two air masses with certainty?
12. Subsection 4.3. The volcanic ash shown in figure 5 on the evening of 17 May has also been sampled by the DLR and FAAM aircrafts, and it has been studied in several papers (Turnbull *et al.*, 2012; Newman *et al.*, 2012). It is a very good case study, and I believe that the differences and similarities between results could be given.
13. Section 5. The figures are too small to be able to identify the features described in the text, e.g. the apparent descent of the aerosol layer (p. 30222, lines 27–29, and p. 30223, lines 9–12). This section is hard to follow with such small figures. I would try to lighten this section a bit and at the same time extend table 1 where all the highlights and differences for the clusters can be summarised.
14. Where quantitative information is given, is it worth attempting an estimation in terms of ash concentration as well? E.g. the features described on line 20 on page 30223 could be worth $\sim 2500\text{--}3000 \mu\text{g}/\text{m}^3$, which is a large concentration.

Minor points:

15. “four-dimensional” (see title, abstract, and article text). It is unclear what the article means with this term. I suggest to use more traditional terminology, such as e.g. aerosol mapping and layering.

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16. “centre of mass” (this expression is used in several places within the paper). I believe that this term is not the most appropriate, because sampling is only in the vertical direction. Moreover, the layers not being rigid bodies, I would probably not claim that their dynamics can be simply summarised in terms of the centre of mass (p. 30220, line 21).
17. “descent”: this expression is used several times in the paper to describe the decrease in altitude of the aerosol layers at a fixed location. It has however been shown (see, e.g., Dacre *et al.*, 2011) that these features are often the result of the advection of a sloping layer rather than by an actual vertical motion. I suggest therefore the term “apparent descent”.
18. Use a consistent time scale across figures to facilitate reading: either time and date as in figures 2–5 or hours since April 15 as in figures 6–10. Using both is rather confusing. Moreover, if you are going to use the date/time type of scale, it would be a bonus to have a consistent time across dates.
19. Colours used in figures 2–5. In my printed copy continental and medium ash content show in the same colour; the same can be said for PBL and unknown.
20. p. 30206, line 7, “Raman”. I believe that Raman channels are in general only used at night, and that a large part of the observations here pertain to elastic channels.
21. p. 30306, line 19, “lower stratosphere”. No stratospheric observations are shown in this paper.
22. p. 30297, line 12, “aerosol typing”. Replace with “proxies for aerosol type” (no direct measurement of composition is made).
23. p. 30209, line 23. A few words should be spent here to say how the ash mask is determined, e.g: “... aerosol mask, based on backtrajectories and supplemented

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with estimates of lidar ratio, depolarisation ratio and colour ratio (Mona *et al.*, 2012)".

24. p. 30218, lines 16–18: observation of ash layers within the PBL. State your criteria to say that ash is mixed within the PBL (I believe that you use depolarisation, but it should be stated).
25. p. 30227, line 25, "resolution". Add "vertical".

References:

- Dacre *et al.* (2011), J. Geophys. Res. 116, eid: D00U03, doi: 10.1029/2011JD015608.
- Marenco *et al.* (2012), J. Geophys. Res. 116, eid: D00U05, doi: 10.1029/2011JD016396.
- Newman *et al.* (2012), J. Geophys. Res. 117, eid: D00U13, doi: 10.1029/2011JD016780.
- Turnbull *et al.* (2012), J. Geophys. Res. 117, eid: D00U12, doi: 10.1029/2011JD016688.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 30203, 2012.