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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Reconstructing volcanic plume evolution integrating satellite and ground-based data: Application to the 23rd November 2013 Etna eruption" *by* Matthieu Poret et al.

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This paper combines deposit mass load data, radar data, and satellite data to reconstruct the grain-size distribution of erupted material from the 23rd November 2013 paroxysmal eruption of Etna volcano, Italy. Reconstruction of grain-size distributions during eruptions is important for modeling and forecasting tephra hazards. But it is laborious, requiring systematic sampling of a tephra deposit and integrating grain-size data. Grain-size data collected from a tephra deposit is also incomplete, because a significant (and usually unknown) fraction of the erupted mass drifts downwind, to dis-

tances far beyond the mapped deposit. Few studies have attempted to estimate the fraction of the deposit escapes the mapped area. Yet it is a key input in ash-cloud models used for aviation safety. This is one of very few studies that integrates deposit, radar, and ash-cloud data to derive a complete grain-size distribution (TGSD). For this reason, I support its publication. HOWEVER, before recommending publication I think it requires significant revision. In particular:

1. Explanations are too long, complicated, and frequently unclear. Many examples are flagged in the accompanying pdf. The revised manuscript should be reviewed again to ensure that it can be understood.

2. Much of the methodology is not clearly explained. Or at least I head trouble following it. Key issues are:

a. One or more tables should be added summarizing inputs, including model domain, nodal spacing, etc.

b. There should more explanation (perhaps in a table) of which parameters are changed in each simulation, and how they are changed. In Table 2 for example, I had assumed that the only parameters changed from one row to the next were the relative percentages of the deposit-based TGSD and the radar-based TGSD. But it appears that the total erupted mass is also changing. Why? Is the mass being adjusted to optimize the fit? Does each TGSD have an erupted mass associated with it?

c. Section 3.1 should more fully explain how the radar retrievals are combined to give a TGSD. The radar returns a reflectivity (which can be turned into GSD) for each volume, delineated by horizontal angle, vertical angle, range, and time. How are these combined to give a TGSD for the entire eruption?

d. Section 3.2 should clarify how fplume output feeds into Fall3d. A list of inputs for fplume, and another list of its outputs that go into Fall3d, would be helpful.

e. Please explain the Poret et al. (2017) method of inverting for alpha and beta (p.

6, line 12). Do you hold plume height and eruption rate constant (using independent information) and then optimize alpha and beta? What range of alpha and beta do you start with before you optimize? Your resulting values, 0.06-0.15 for alpha and 0.2 to 1.0 for beta, are so broad that I'm not sure the inversion is worth including.

f. What factor(s) determine the vertical distribution of mass with height in the Fall3d model? Is this an output from fplume, or do you use some parameterization, like a Suzuki curve?

3. It would be of great value to discuss some results in the context of other studies. Most significant (in my opinion) is your finding that the mass fraction of fine ash (PM20) is 3.6-9.0 wt% of the erupted mass. This fraction is a critical input into ash-cloud models used for aviation safety, because it is the mass that goes into the downwind cloud. Few studies have constrained it. NOAA's Hysplit model assumes for example that 5% of the erupted mass makes it into the downwind cloud, and uses this 5% as model input. At Spurr in 1992, Wen and Rose [1994] estimated about 2% of the erupted mass went into the distal cloud. At Eyja, the estimates range from ∼0.9% to 11% [Bonadonna et al., 2011; Dacre et al., 2011; Devenish et al., 2012]. Basaltic eruptions like Etna tend to be very poor in fine ash, hence I would expect a lower fraction. But your 3.6-9 wt% are in the middle of the previous numbers. Does this mean that we don't need to adjust downward when modeling a basaltic eruption? Additional comments are below, and in the attached pdf. Some duplicate points made above. Overall I think the paper merits publication. But it should be revised to shorten, clarify, improve readability, and explain methods. I look forward to seeing the final version.

Larry Mastin

Specific or technical points

Page 3, line 16, and Figure 2: Is there any evidence that the existence of a whitish plume on top and grayish below was persistent during the eruption? Or is this photo just recording a transient phenomenon? Also, the upper cloud looks light gray to me,

not exactly white, like the one in the lower right of Fig. 2.

p. 3, line 26. What method(s) does the Camsizer use for grain-size analysis? Laser? Settling rates? Some combination?

Section 2.1 (page 4): The number of samples collected (7) is pretty small. Thus there is a strong chance of bias in your results. Can describe where the samples were taken relative to the dispersal axis, and how (or whether) you know the overall distribution of the deposit?

p. 3, lines 31-32: The MER estimate of 4.5e06 kg/s for the climax phase comes from the mapped deposit and observed duration. Where does the estimate of 1e06 kg/s come from?

p. 4, line 21. How do you derive a mass eruption rate from radar? From plume height using fplume?

p. 4, lines 23-24. I'm getting a little confused about the release of the ice and ash clouds. On p. 3, you seemed to imply that ash and moisture (forming the ice cloud) were released at the same time but at different elevations. Here you seem to be saying that they came out at different times (but maybe overlapped in time).

p. 4, lines 10-28. This paragraph needs to be reorganized and reworded to improve its coherence. Equation 1: Does this equation assume that the plane of the block's trajectory is the same as that of the radar beam? If not, I don't see how that the angle between the two is considered. Also, why is ejection velocity important in this study? It is not really related to column height, or MER. Is it just a qualitative indicator of eruption intensity?

P. 5, lines 8-15. This explanation should be reworded to be clearer and more concise. I don't have specific suggestions. But I think you are saying that the field TGSD will be biased toward coarse ash, and that the radar will help constrain the mass of fine ash.

p. 5, lines 19-29. You talk about estimating TGSD from the radar. So you estimate two independent TGSD's? One from the deposit and another from radar? Perhaps this should be mentioned. It is not clear to me how you derive a TGSD

Section 3.1 (p. 5). There is some critical information I don't see in the explanation of TGSD derived from radar. I assume that the radar provides a TGSD for each volume, delineated by range, and horizontal and vertical angles, and time of the scan. Somehow these volumes are integrated to get a radar-based TGSD for the entire airborne mass. How is this done?

p. 5, lines 27-28: "To [integrate the radar and field TGSDs], we investigated the weights at regular intervals until we best-fit the field measurements maintaining the shape of the radar TGSD on the proper grain-size interval". After reading this sentence a couple of times, I'm still not sure exactly how you arrived at a best fit. You adjusted the relative amounts of radar ash and field ash until the heights of the histogram bars in the overlapping interval agreed?

p. 5, line 36. "X(phi_5) is the fraction obtained for phi=5". How do you obtain this fraction? By estimating the total mass of the cloud and dividing it by the total mass of the deposit+cloud? Also, there could be a little more explanation about how PM20 is estimated from the integrated TGSD. If PM20 is at the tail of the size distribution derived from radar, it seems that the mass of PM20 would be highly dependent on the size distribution assumed (i.e. gamma) from the radar retrievals. TGSD's tend to be polymodal, a fact that is not considered here.

p. 6, line 12: "alpha and beta are obtained empirically through the solution of an inverse problem (Poret et al., 2017)". A little more detail would be helpful. You took cases where both the plume height and eruption rate were known, and then adjusted alpha and beta until the modeled height matched the observed one?

p. 6, lines 24-42. The English in this paragraph really needs some work. And why are you are spending so much time comparing winds over the volcano with those in Tirana?

Section 3.3 (p. 6, lines 10+). I think it would be valuable to show a table listing variables that were varied in your optimizations.

p. 7, line 15. To quantify the goodness of fit, what observations were you comparing with model results? Cloud load? Deposit mass load? Was each observation, e.g. deposit mass load at each sample location, equally weighted with every other observation? Also, there is not enough explanation of K and k to know what they signify. It would be worthwhile to show the equations used to calculate K and k.

p. 7, line 25. It's not exactly clear to me what simulations you are doing that attempt to reproduce "sampled tephra loadings". Are you doing plume simulations, feeding the airborne tephra into Fall3d to simulate the deposit, then comparing the simulated deposit mass load with measured values? What were the setup parameters for the Fall3d runs? What was the model domain, model resolution etc.?

p. 9, lines 8-9. What fplume results were you comparing with observations during your simulations that constrained values of alpha and beta? The constrained ranges, alpha=0.06-0.15, beta-0.21-1.00, are so wide that I think the constraints aren't really meaningful.

p. 9, lines 25-30. There are several reasons why the model result could be inaccurate in the proximal region. One of them has to do with details of plume dynamics, and fallout of large clasts from the side of the rising column. How is fplume integrated into Fall3d? Does it consider fallout from the side of the column? How does it transfer mass from the column to Fall3d? How is fine ash distributed with height? Is this determined within fplume, and then transferred to Fall3d for lateral transport?

p. 10, line 8. You note here that the assumptions involved in the radar analysis add uncertainty. It would help to be specific about which assumptions are important, and how they would affect uncertainty.

Table 2: What are RMSE_1, RMSE_2, and RMSE_3? RMSE values for each of the

three eruption schemes illustrated in Fig. 7? Also, I'm a bit confused about why the total erupted mass (TEM) is changing in each row of the table. Are you adjusting it until the RMSE is minimized? Are you adjusting any other parameters? Are you somehow calculating the total mass in the air from radar measurements and using that as the model input? This reinforces my view that there is a lot that needs to be explained regarding the radar data.

Figure 5: Isn't the radar TGSD supposed to have a gamma distribution? It doesn't look like one. (or does the gamma distribution only apply to each volume in the radar?).

Figure S1: How were the SEVIRI data processed to give ash mass load? Who did the processing? Why does the animation look like a model output? Should it show pixels of ash? Why is there a brown airplane off the coast of Albania?

References:

Bonadonna, C., R. Genco, M. Gouhier, M. Pistolesi, R. Cioni, F. Alfano, A. Hoskuldsson, and M. Ripepe (2011), Tephra sedimentation during the 2010 Eyjafjallajökull eruption (Iceland) from deposit, radar, and satellite observations, J Geophys Res, 116,doi 10.1029/2011jb008462.

Dacre, H. F., A. L. M. Grant, R. J. Hogan, S. E. Belcher, D. J. Thomson, B. Devenish, F. Marenco, J. Haywood, A. Ansmann, and I. Mattis (2011), The structure and magnitude of the ash plume during the initial phase of the Eyjafjallajökull eruption, evaluated using lidar observations and NAME simulations, Journal of Geophysical Research, 116, D00U03,doi 10.1029/2011JD015608.

Devenish, B., P. N. Francis, B. T. Johnson, R. S. J. Sparks, and D. J. Thomson (2012), Sensitivity analysis of dispersion modeling of volcanic ash from Eyjafjallajökull in May 2010, Journal of Geophysical Research, 117(D00U21),doi doi:10.1029/2011JD016782.

Wen, S., and W. I. Rose (1994), Retrieval of sizes and total masses of particles in

volcanic clouds using AVHRR bands 4 and 5, Journal of Geophysical Research, 99(D3), 5421-5431.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-1146/acp-2017-1146-RC1-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1146, 2018.