Comment on acp-2021-254

Larry Mastin (Referee)

Referee comment on "Modelling the size distribution of aggregated volcanic ash and implications for operational atmospheric dispersion modelling" by Frances Beckett et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-254-RC1, 2021

This manuscript presents a one-dimensional plume model that solves the Smoluchowski coagulation equations to calculate particle aggregation in plumes ascending from the 2010 Eyjafjallajökull eruption, and then uses results of the plume modeling, incorporated into the NAME atmospheric dispersion model, to examine the effect of aggregation on the areal extent of the ash cloud. For the conditions considered, the results show surprisingly little effect of aggregation on cloud area. Less than 20% change or so in cloud area over the first 24 hours, when aggregation effects are considered. The manuscript thoroughly examines the sensitivity of the calculations to some key aggregation parameters and includes thorough sections discussing uncertainties and limitations.

The manuscript is clearly written, equations and assumptions are well presented, citations are thorough, and the conclusions are for the most part well supported by the results. I also think this subject will be of great interest to atmospheric scientists and volcanologists who are concerned with uncertainty in ash cloud forecasts. I have only a few comments:

1) The plumes you model condense water only in the upper kilometer (Fig. 1a & b), or not at all (Fig. 1c & d). I would expect many other circumstances when more liquid water would be present, and aggregation would be more important. You include some discussion of this in lines 511-524, but I think you could add a few more sentences to quantify the water content and put it into context with precipitating clouds and wet plumes. Hail-forming clouds typically contain about 0.1-10 g/m3 water (e.g. Heymsfeld & Musil, 1982), and we know that some hail-forming columns remove a lot of fine ash (e.g. Van Eaton et al., 2015). The water content of your plumes are expressed in mass fraction of the plume (Fig. 1), which is a little hard to compare with meteorological cloud water data.

2) One reason, not discussed, for the small effect of aggregation on your results is that you keep the mass fraction of distal ash in the model at 5% of erupted mass in all cases (line 377). If you were to truly ignore aggregation in the control case (Fig. 8a), your ash

mass used as input would have been several times greater than 5%. This assumption should be discussed (Perhaps you account for that on lines 446-469, but I don't understand some of that discussion (see below)).

3) It would be worthwhile to add a short paragraph at the end of Section 4 summarizing some of the key aggregation relationships in Figures 4-6 in physical rather than mathematical terms. The analysis is meticulous but sometimes hard to follow when relationships are expressed in terms of, for example, St_cr or q rather than words.

Many less important comments are listed below. Overall I think this is a worthy manuscript and look forward to seeing it published.

Specific comments:

Line 16: change "modeled extent" to "modeled area".

Line 17: At what time after the eruption start is the modeled extent of the ash cloud reduced by 3%?

Line 18: add comma after "ash cloud".

Line 22: change "differs to" to "differs from" (or is this an English vs. American usage thing?)

Line 91: Consider adding a sentence that explains the exponents in equation 8. Most 1-D plume models do not use this exponent when calculating entrainment.

Line 95: add a comma after "water".

line 103: change "molecular mass" to "molecular mass ratio(?)"

line 112: what is "n_l,ice"? The sum of n_l and n_{ice}?

line 120: add a comma before "and solid phases"

Line 122: Are you assuming that there is no fallout of solids from the plume? I presume that this is the case, since you are setting $dn_s/ds=0$. If so, change "as there is no entrainment of solids" to "as there is no entrainment or fallout of solids". In a future version, it might be worthwhile calculating particle fallout from the column and using the remaining ash at the top of the column to initiate the NAME simulations. You may also be able to produce a physically based vertical distribution of mass for the NAME simulations.

Line 155 (and equations 25 and 26): it would be useful to describe physically what the terms for inertial turbulence, and laminar shear are, and why they are important. These terms are not included in some other coagulation kernels, such as those by Costa et al. (2010) and de' Michieli Vitturi et al. (2021).

Line 189: add a comma after "following a collision".

equation 36: Maybe I'm just dense, but I don't understand this equation. Equation 33 suggests that St_v should range from zero to infinity. In this St_{cr} appears to range from minus infinity to plus infinity.

Line 192: could you briefly define "surface asperity"? Most readers may be unfamiliar with this term.

Lines 197-207: this description of the state of knowledge of liquid bonding and its effect on the sticking efficiency looks reasonable to me. Although some current studies may improve our knowledge, (e.g. https://agu.confex.com/agu/fm20/meetingapp.cgi/Paper/688464), we are not there yet. Regarding eq. 38, I think that some studies (e.g. Telling & Dufek, 2013) may suggest that sticking efficiency is not a linear function of relative humidity, but the data are sparse.

Table 1: comments:

(a) the term "relative velocity" is not defined for u_p and u_s, although I'm pretty sure that you mean the velocity within the plume minus the velocity component of the ambient wind field in the same direction (parallel or perpendicular to the plume axis).

(b) The units of E are shown as kg m-2 s-1, but Q_m has the units kg s-1 and E=dQ_m/ds (eq. 5). So it seems that E should have the units kg m-1 s-1.

(c) I don't see a specific heat capacity of particles listed. Seems like it should be used somewhere.

Line 218: what do you mean by a "best-guess set of observations"?

Lines 223-224: "The mass is distributed uniformly across the bins such that 50% is on grains with diameter <= 125 um and 36% of the mass is on grains with diameter <= 32 um." I didn't understand this statement until I stumbled on the GSD plots in Figure S1. Citing them here would be very helpful. It would be easier to understand if you simply said that the GSD was uniform in mass between sizes of 1 um and 16000 um (I think that's what it looks like in Fig. S1. Also, how many bins are you using and what are the phi intervals between bins?

Line 227: I'm a little confused by your use of the word "mode", here and in Table 3. To me, the mode is the peak in a histogram of size bins. But you've shown no such histograms in this paper, so it's hard to visualize. (--oops, I see that histograms are plotted in Fig. 2. Perhaps refer to Fig. 2 when describing the mode)

Line 263: add a comma after "10".

Figure 1: Are the atmospheric soundings used in these simulations listed somewhere? I don't see them.

Figure 2: the amount of aggregation shown in these figures is surprisingly modest. Only two of the four scenarios in Fig. 1 show condensed liquid water in the plume, and the condensation starts less than 1 km below the plume top (assuming the top of the plot is the plume top, which is not clear to me).

Line 274: change "in the former case" to "in the presence of water" (assuming that's what you mean).

Line 275: change "appropriate for the formation of water" to "appropriate for wet conditions"

Figure 4: it might be worthwhile annotating one of the curves on this plot with labels that say, for example, "d_j=d_k" at the low point, and "beta_i,j@D*d_j^4" where it flattens out on the left-hand side.

Line 333: is the "q" after "4" an exponent?

Line 335: add a paragraph break before "Turning"

Line 365: "This explains why the mode of the AGSD in Figure 3 shifts to larger diameters with increasing St_cr". I can't tell where the mode of the AGSD is in this figure.

Line 378: You assume that 5% of the erupted mass goes into the distal cloud. This percentage should vary depending on the amount of aggregation. But you haven't considered that here. It would be good to acknowledge that in the Discussion section.

Lines 383-384: "Model particles are released with a uniform distribution over the depth of the modelled (bent-over) plume". Uniform from ground level to the plume top?

Lines 399-402: I'm starting to get lost here. For Figure 9, you assume that 25%, 50%, and 75% of the ash mass consists of aggregates. Aggregates of what size? Are you assuming that each light bar in Fig. 7 consists of 75% aggreagates?

Lines 399-402: Does the GSD calculated by the plume model distinguish between particles and aggregates? This isn't explained.

Line 424: delete "Whereas"

Line 430: add a comma after "eruption".

Lines 439-440: "in this case, the modelled ash cloud is more sensitive to the input GSD of the non-aggregated ash at the source, than due to any change to the GSD or density of the ash due to aggregation". I agree, in this case. But you could have chosen other cases where aggregation was more important. Adding water at the vent in the 1D plume model for example would have increased aggregation in the plume. Higher or colder plumes produce hail, which scavenges fine ash (Van Eaton et al., 2015)

Lines 462-463: "our simulated AGSD still has ~30% of the total mass on tephra (aggregates and single grains) with diameters <125 um, which, given their size and density, would travel further than Iceland before depositing due to sedimentation alone." I'm not sure what this means. Thirty percent of the total erupted mass has diameters <125 um? The GSD in Fig. 7 contains much more than 30% grains <125 um.

Lines 448-474: this is a long, rambling paragraph. I suggest breaking it up into shorter paragraphs.

Line 516: change "mass on smaller grains" to "mass of smaller grains" (or is this just an English vs. American usage thing?)

Line 530: Is viscous dissipation the right term here? I think surface tension is the force that binds particles together, just as it keeps droplets from breaking up in water sprays (Hinze, 1955). I think of viscous dissipation as the process that converts viscous shear to heat (e.g. Hardee & Larson, 1977). But perhaps there are other meanings.

References:

Costa A, Folch A, Macedonio G (2010) A model for wet aggregation of ash particles in volcanic plumes and clouds: 1. Theoretical formulation. Journal of Geophysical Research 115:doi:10.1029/2009JB007175

de' Michieli Vitturi M, Pardini F (2021) PLUME-MoM-TSM 1.0.0: a volcanic column and umbrella cloud spreading model. Geosci. Model Dev. 14(3):1345-1377

Hardee HC, Larson DW (1977) Viscous dissipation effects in magma conduits. Journal of Volcanology and Geothermal Research 2(3):299-308

Heymsfield A, Musil DJ (1982) The 22 July 1976 case study: Storm structure deduced from penetrating aircraft. In: Knight CA, Squires P (eds) Hailstorms of the Central High Plains. Colorado Associated University Press, Boulder, pp 163-180

Hinze JO (1955) Fundamentals of the Hydrodynamic Mechanism of Splitting in Dispersion Processes. A.I.Ch.E. Journal 1(3):289-295

Telling J, Dufek J, Shaikh A (2013) Ash aggregation in explosive volcanic eruptions. Geophysical Research Letters 40(10):2355-2360

Van Eaton AR, Mastin LG, Herzog M, Schwaiger HF, Schneider DJ, Wallace KL, Clarke AB (2015) Hail formation triggers rapid ash aggregation in volcanic plumes. Nature Communications 6:7860