Review of FPLUME-1.0: An integrated volcanic plume model accounting for ash aggregation, by A. Folch, A. Costa and G. Madedonio

This paper tackles the important problem of injection and dispersion of ash in the upper atmosphere by volcanic plumes. It focuses on the determination of the input parameters that are required by models of dispersion of ash in the atmosphere, and in particular the distribution of particle grain sizes. Two famous eruptions are then used to illustrate the model. I think the proposed model of volcanic plume represents a very useful step towards more robust integration of volcanic source parameters into atmospheric models of ash dispersion, the key aspect being the incorporation of particle aggregation in the plume. I consider the publication as a significant contribution to a subject that has become and remained quite active since the Eyjafjoll 2010 eruption in Iceland. I have only a few and mainly minor comments listed below.

Title: maybe the present title can be misunderstood; it could explicitly states that the model is accounting for other important phenomena rather than only ash aggregation. I admit it might be quite long then...

Abstract: as the 1.0 version of the model is presented here, it would be nice to end by a sentence announcing future/potential improvements.

Main text:

p8010-l19: maybe state that volcanic plume are turbulent flows

p8010-l23: "negatively buoyant basal thrust region"

p8011-l09: you may wish to add a couple of references here, e.g. Carazzo et al. 2014 (Laboratory experiments of forced plumes in a density-stratified crossflow and implications for volcanic plumes, Geophysical Research Letters 41 (24), 8759-8766)

p8011-l17: I would add that sophisticated 3D multiphase models have problems on their own related to the accurate description of the physical processes their are taking into account (e.g., closure equations, impact of spatial resolution, etc).

p8012-l5: the upcoming special issue of the Journal of volcanological and geothermal research might be cited (if time has come).

p8013-l2: for sure the TGSD is also depleted in large particles related to the source due to sedimentation.

p8014-l5: I suggest to define the mass, momentum, energy fluxes as well as s before giving the equations of conservation that will give their evolution with z. The parameters related to aggregation in the equations should be defined in the main text here (rather than in page 8016) as they are key in the paper (I mean not only in the table at that stage), as well as the rate of entrainment.

p8015-l25: this is a detail, but one may note that buoyancy main become positive in the basal gas-thrust region (i.e. before the source momentum has become negligible).

p8016-l18: is rho_p independent of the size of the particles?

p8019-equ(5): is this formula equivalent e.g. to the ones used in Girault et al. 2014 (The effect of total grain-size distribution on the dynamics of turbulent volcanic plumes,

Earth and Planetary Science Letters 394, 124-134)? If not, what are the implications of the choice made here?

p8021&8022: variable entrainment. I have two questions on that part:

- for sure a volcanic plume is a forced jet in the basal gas-thrust region. Hence I do not see why it is necessary to propose a function for A_plume(zs) for zs<10. I wonder also why A_jet(zs<10) cannot be taken as A_jet(zs=10) rather than proposing an unconstrained function. Does that choice really affect the results? I guess it does not, but if it is the case this as to be discussed as the model would then appear open-ended.

- I am not sure I understand why a sin(theta) is added in equation (19). Could you add a few sentences to explain that point in more details?

p8023-equ(23): isn't there more recent ways to determine H_t? I think there is at least one paper by Koyaguchi and Suzuki that compare the evolution of Ht and Hb with the eruptive flow rate.

This part of the model appears less convincing than the previous one adressing the dynamics of the plume below the NBL. Is there a way to show that the approach (i.e., the prediction of the total height Ht) is consistent with some results from 3D numerical models or lab-scale experiments?

p8024-l10: Plume wet aggregation model.

This part is the most difficult to read as many equations are presented that involved a large number of parameters. I wonder if it is possible to have an idea on the dependence of the model results on these various parameters. I understand that Df0 is the key parameter here, but it will be good to illustrate more its importance relative to other parameters. It will be good also to show a figure with the evolutions of the predictions of the model when starting from a model with no aggregation and then adding the different processes ending to the full variation of n_tot (equ 34).

p8034-l15: Eyjafjoll eruption: did you consider the possible presence of meteoritic water in the plume, and will this affect the results (aggregation made easier)?