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Geoscientific Model Development Discussions
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Interactive comment on "FPLUME-1.0: An integrated volcanic plume model accounting for ash aggregation" *by* A. Folch et al.

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Dear Editor, dear colleagues,

I have read the paper "FPLUME 1.0: An integrated volcanic plume model accounting for ash aggregation" with much interest and found it suitable for publication in GMD, pending the following minor revisions that I hope will contribute to clarify some of the model details.

The manuscript does represent a substantial contribution to modeling science within the scope of Geoscientific Model Development and addresses a relevant scientific question within the scope of EGU, namely the modeling of volcanic eruption columns and the development of computational tools to assess of their related hazards.

The model source code is not provided in the supplementary material. However, I could revise the code, that was sent to me by the authors upon my request. The code is well written (in a effective, modular structure), commented in detail and easy to run. I think that, if released, it will be very useful to the volcanological community.

General Comments

Scientific significance

Although not conceptually new (it is based on the the Morton's Buoyant Plume Theory and on various developments of the Woods' 1988 plume model) FPLUME1.0 represents a significant incremental step in the broad taxonomy of volcanic plume models and a substantial advance in modelling science. The code integrates most of the state-of-the-art volcanic plume models, including the effects of:

- plume tilting
- water phase transitions
- particle fallout and re-entrainment
- buoyancy-dependent air entrainment.

Additionally, it is the first plume integral model to include a specific treatment of the umbrella region to avoid the singularity at the plume top. The main novelty is represented by the original implementation of a particle aggregation model (whose theoretical formulation had been previously published by the authors).

Scientific quality

The scientific approach and the applied method for the formulation of the integral transport equations is valid and consolidated. However, some aspects concerning 1) the formulation of the mass loss and entrainment model for the tilted case and 2) the new model for the umbrella region, deserve some more thorough discussion (see Specific Comments below) . The treatment of particle aggregation is simplified (see Specific Comments) but it represents in any case a significant step forward in the plume modeling literature. All the other relevant hypotheses and assumptions are valid and clearly outlined or referenced.

In general, the paper is well balanced and discusses the most relevant aspects of the model with respect to previous works. Two independent applications to recent, well documented, volcanic eruptions (the 1982 El Chichon and the 2010 Eyjafjallajokull eruptions), provide evidence of the model reliability. Despite the limited number of presented test cases, the model has the potential to perform calculations, leading to significant scientific results, to many explosive eruption natural cases, provided that accurate observational data are available.

Scientific reproducibility

All model results are based on available data, they are reproducible and support the conclusions. The most relevant related works are referenced.

Presentation quality

The paper is generally well and concisely written in all parts. I have checked all mathematical formulae, whose symbols are correctly reported in Tables 1 and 2. I only have one remark on the title, where "integrated" seems to indicate the integration (collection), in the same tool, of different features. This could be a reasonable choice, but I am not sure whether the authors intend to refer instead to an "integral" model (referring to spatial integration). The numerical algorithm is not much detailed (see Specific Comment below), nor a manual is provided as a supplement, but a thoroughly commented source code can be obtained upon request.

Specific Comments

Section 2.1

- Equation 2c) lacks a term associated to the mass loss. If it is neglected, the reason should be specified.
- Line 20, p.8016) I see no reason to use such an approximate equation of state (even though the approximation is probably good "almost" everywhere).
- Equation 15) I would like to know whether this equation is inverted (and how) to compute the settling velocity (Eq. 14): the Reynolds number (Re) is indeed a function of the non-equilibrium velocity, and this makes the inversion not immediate.

Section 2.2

The extension of the buoyancy-dependent entrainment model of Carazzo et al. (2008) to the bent-over plume is problematic. Because it is known that the entrainment model has a first-order control on the plume dynamics, I fear that some of the authors' choices are not fully justified.

- Equation 19) Please explain why the sin(theta) factor in the second term in the RHS "generalizes" Eq. 18.
- Equation 20) The choice of the interpolation function is not justified enough. Looking at Figure 2, the interpolation function seems quite arbitrary, given that it extends over two orders of magnitudes of z_s. It is also different, for high z_s, to the interpolation function proposed by Carazzo et al. (2008). I understand the need of having an analytical expression in the whole range, but it would be useful to know how this choice impacts the results.
- Equation 21) This is an unpublished results from a PhD thesis. As for the previous point, I do not see here a major improvement with respect to a model with constant entrainment parameterization.

- Figure 3) If possible, plot the entrainment coefficients against the non-dimensional scale Zs.

Section 2.3

The empirical treatment of the umbrella region is effective to avoid the divergence of the plume radius at the top, and the results are reasonable. Nonetheless, Eq. 23) holds for a vertical plume with constant entrainment, so it seems difficult to justify it for bent-over plumes with variable entrainment.

Therefore, I am wondering whether it would not be better to compute H_t simply by means of the Bernoulli equation along a plume streamline and for an adiabatic transformation. Assuming that the mixture at NBL has about the thermodynamic properties of air and that atmospheric temperature variations do not strongly affect the pressure profile (which is generally true), using the Bernoulli equation I could obtain $(H_t - H_b) \sim 8400$ m in the case of the El Chichon test (using a velocity $v = 150$ m/s at the NBL), which is about the same value shown in Fig. 5b), without the need of using Eq. (23).

Section 3

In general, the assumption that all particles aggregate into a single particle class seems rather simplistic, although it is clear from the paper that more complex models would probably be poorly constrained by data. Although I understand that such an assumption strongly simplifies the computation, I would encourage the authors to discuss how the aggregation model would be modified if this hypothesis was relaxed and a spectrum of aggregates had to be considered.

- Lines 7-17 p.8030) These considerations should be supported by evidences or references to previous works.
- Last paragraph, p.8030-8031) I suggest to move this paragraph in Section 4

(model algorithm). In addition, the algorithm should be described in more detail: since A^+ and A^- (computed at step 8) affect the solution of the system of transport equations (solved at step 1), I would like to understand how is this dependency solved numerically (is it a predictor-corrector algorithm?).

Section 5

- Lines 25/27, p.8034) Are the two values of D_{fo} inverted?
- Lines 16-18, p.8035) “Input values . . . vent coordinates”. Move this sentence at line 29 after “. . . height”.
- Line 28, p.8035) To understand how Fig. 10 was constructed, the ranges of variability of the input parameters in the study should be specified.
- Fig. 10) This figure is interesting but might be misleading, since it seems to suggest a direct dependency or control of the mass fraction of aggregates of the column height, which would be surprising. The discussion of this figure (page 8036) should be extended, by commenting the main source of variability of the column height. If possible, also substitute the continuous line with symbols.

Technical Corrections

I have suggested a number of minor corrections in the attached pdf version of the manuscript.
Here I just report that the use of “bent over” as a noun is probably not correct in English (use “tilt” instead?).
I consider this optional, but I would also suggest, wherever possible, to use Black-and-White readable plots (e.g., Fig. 2, 3, 5, 6, 9, 10).

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C2921/2015/gmdd-8-C2921-2015-supplement.pdf

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Interactive comment on Geosci. Model Dev. Discuss., 8, 8009, 2015.