Referee#1 (T. Espositi Ongaro)

Referee#1 General Comments

- *I only have one remark on the title, where "integrated" seems to indicate the integration (collection), in the same tool, of different features.*

  Title has been changed

- *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.*

  It has been stated that code is available under request and the manual will be provided with the distribution package.

Referee#1 Specific Comments

Section 2.1

- *Equation 2c) lacks a term associated to the mass loss. If it is neglected, the reason should be specified.*

  The term is implicitly included in P (LHS).

- *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).*

  Right. The sentence has been removed

- *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.*

  We have added the sentence "Given that the Cd depends on Re (i.e. on us), Eq. 14 is solved iteratively using a bisection algorithm."

Section 2.2

- *Equation 19) Please explain why the sin(theta) factor in the second term in the RHS "generalizes" Eq. 18.*

  We have added the sentence "Beside the local Richardson number, the entrainment coefficient αs depends on plume orientation (e.g. Lee and Cheung, 1990; Bemporad, 1994), therefore we modify Eq. 18 as:" and 2 new references.

- *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 zs. It is also different, for high zs, 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 Equation 21) This is an unpublished results from a PhD thesis.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.*

In order to answer to this point (and also to the main comment from reviewer#2) we added a sentence at the end of section 2.2 where we stress the limitations of the entrainment parameterizations. Concerning the interpolation function, it is clearly stated that is an empirical function. Finally, eq (21) comes from a published work (the PhD Thesis is available online).

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

  Done. Figure 3 has been changed

Section 2.3

- *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 Ht simply by means of the Bernoulli equation along a plume streamline and for an adiabatic transformation.*

  We agree with the referee. We have added the sentence "In the umbrella region (from the NBL to the top of the column), we neglect air entrainment and assume that the mixture is homogeneous, i.e. the content of air, water vapor, liquid water, ice, and total mass of particles do not vary with z." Regarding the Bernoulli equation, we disagree with the reviewer, as the umbrella region is an open system (calculation by the reviewer were probably made assuming a homogeneous environment with constant density and pressure).

Section 3

- *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.*

  We have added the sentence "Obviously the assumption that all particles aggregate into a single particle class is simplistic and considering a range of aggregating classes would be more realistic. However, there are no quantitative data available for such a calibration."

- *Lines 7-17 p.8030) These considerations should be supported by evidences or references to previous works.*

  We have added the references (Costa et al., 2010; Folch et al., 2010)

- *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?).*

  Following referee's suggestion, we have moved the paragraph to the end of section 4. It is now clear that the package lsode is used for solving the system of equations.

Section 5

- *Lines 25/27, p.8033) Are the two values of Dfo inverted?*

  Yes, this has been corrected

- *Lines 16-18, p.8035) "Input values… vent coordinates". Move this sentence at line 29 after "...height".*

  ok

- *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.*

  We have added the sentence "Input parameters were fixed as in Table 5 varying only column height from 4 to 8.5 km (a.v.l.)" at the caption of Figure 10.

- *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.*

  We have added the sentence "However, it should be kept in mind that mass fraction of aggregates is not controlled by the eruptive column height but depends on several variables such as particle concentration (that is a function of the mass flow rate), presence of liquid water (that can form above a given level depending on the local meteorological conditions), etc." at the end of section 5. An axis showing the corresponding MER has been added to Figure 10.

Other corrections

Finally, we have accepted the minor corrections on the annotated manuscript.