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Geoscientific Model Development Discussions

Interactive comment on "PLUME-MoM-TSM 1.0.0: A volcanic columns and umbrella cloud spreading model" *by* Mattia de' Michieli Vitturi and Federica Pardini

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Reply to Reviewer RC1

We thank the reviewer for the careful reading of the manuscript and the constructive remarks. We have taken the comments on board to improve and clarify the manuscript. Please find below a detailed point-by-point response to all comments (reviewer comments in bold, our replies in italics). We are also providing in the GMD online discussion a revised manuscript that reflects the suggestions and comments of all the reviewers, where changes with respect to the original submission are highlighted. We feel that this has resulted in a stronger manuscript.

Line 108: why is it called the "two-size moment method"? Line 114: "the NDF is reconstructed with a linear function". Linear with re- gard to whet? particle mass?

The reviewer is correct and these points deserved a better explanation in the paper. We changed the text and now it reads in the following way:

> *"While in the original formulation the internal variable is the size (described by the volume or the radius) of the particles and in each section two moments of the size distribution are used to reconstruct the NDF with a linear function of the size (hence the name two-size moment), here the NDF is defined as a function of particle mass."*

Equation 10: there seems to be a subscripted left square bracket in each equation that should not be subscripted.

Done, thank you!

Line 195: It's interesting that your control volumes are cylindrical sections with a vertical axis, not with an axis paral- lel to the plume axis. This implies that the axes of stacked cylinders are not co-linear. Yet in the mass flux equation (eq. 12) you calculate the mass flux from one cylinder to another as something like pi*rĖĘ2*w, where w is the vertical velocity. What sort of approximations are implied in eq. (12) if the stacked cylinders are not co-linear?

The mass flux across the horizontal section, without approximations, is given by

$$Av \cdot n \qquad (1)$$

where A is the cross sectional area, v is the velocity vector and n is the vector normal to the considered section. In this case, because the section is horizontal, the scalar dot vn is equal to the vertical component of the velocity, and thus in the formulation expressed by Eq. 12 we are taking into account the correct mass flux, without approximations. We preferred this formulation, with respect to the one where sections are orthogonal to the bent plume axis for one main reason. In this way, when we reach the neutral buoyancy level, it is easier to define a horizontal source area for the umbrella cloud.

Line 215: Where does equation 13 come from? Can you provide a reference?

We added the reference to Bursik et al. 1992. Our equation is slightly different from that of the original reference, because of the presence of a multiplying factor 2 in the original one (Eq. 19 in Bursik et al. 1992). This is due to the fact that we have the factor 2 in our Eq. 13, while it is missing in Eq. 16a in Bursik et al. 1992. We preferred to keep it in the equation and not in the equation for p because this factor come from the integration along the edges of the plume, which results in the 2πr term on the right-hand side of Eq.13.

Line 220: You need a reference to the Von Smoluchowski equations

We added a reference to the original work of Von Smoluchowski.

Line 264: The equation for entrainment velocity U_e appears to assume a 2D geometry. but you are solving for a 3D system. Are the equations the same for 2D and 3D?

The equation accounts for the fact that the plume has both vertical and horizontal components of the velocity, which result in a velocity vector with magnitude U_{sc} and direction parallel to the bent axis of the plume (this is by definition). So, even if we are considering horizontal sections, we still have a 3D plume velocity. When computing the total

amount of entrainment, the two contributions need to be defined for the components parallel and orthogonal to the plume velocity (i.e. U_{sc}), and not to the velocity orthogonal to the sections. For this reason, also with our choice for the horizontal sections, the two terms in the equation still represent the total contributions to entrainment velocity.

Line 282: Where do you list the values of C and h0 that are used in equation 27? Also, I think there should be some discussion in the main body of the paper of the freezing temperature that you use in the model, and whether you consider a temperature range over which liquid and ice coexist. You explain this in Appendix A1 but I think it would be worth mentioning here as well.

The values of specific heat capacities and enthalpies at a reference temperature are listed in the Table in Appendix C. The freezing temperature is here assumed to be Tref= 273.15K, and we assumed that in the temperature range[Tref−40;Tref] vapour, liquid and ice form may coexist.

Line 285: I presume the specific heat capacities C are heat capacities at constant pressure (C_p), not at constant volume (C_v). This would be worth mentioning.

Done.

Equation 28: the term for enthalpy of dry air on the RHS of this equation is w_atm*(h_wv0-C_wv*T_atm). I thought it would be w_atm * (h_wv0+C_wv*(T_atm- T_0)). Am I missing something?

The reviewer is right. There was an error in the formulation of the enthalpy of dry air in the energy equation. We corrected the term (in the code and in the text) and we updated all the simulations presented in the paper (all the figures showing model results have been replaced with new ones). The new results are very close to the old ones, and negligible changes in the output quantities can be noticed. As an example, *for a reference eruption, plume height above the vent increases by 0.5% (from 36341m to 36513m), while differences at the third decimal place can be noticed for the majority of the output variables.*

Line 306: Change "A 4-5th order Dormand-Price . . . ". to "a 5th order Dormand-Price . . . "(?)

We changed the text in the following way:

> *"5th order 7-stage Dormand-Prince Runge-Kutta method (Dormand and Prince, 1980) is implemented in a Fortran 90 code for the numerical integration of the system of ordinary differential equations. This method is based on a 5th order method used to advance the solution, which is compared with a 4th order method to estimate the integration error and to automatically reduce or increase the integration step."*

Line 349: change "Appendix A" to "Appendix A1"

Done.

Lines 353-360: Here you mention that you use the buoyant plume equations to model plume rise up to the neutral buoyancy level, and then use the shallow water equations to model umbrella cloud spreading. Switching to the shallow water equations as soon as you reach the neutral buoyancy elevation would prevent you from modeling plume rise up to the maximum height of the overshooting top. Why not model plume rise up to the maximum height and then descent back to the neutral buoyancy elevation before switching to the shallow water equations?

The user of the model can decide to solve the equations and obtain of solution of

the plume model up to the maximum height, but in any case we decided to use the mass/volume flow rate at the neutral buoyancy level as source for the shallow water model for two reasons: (i) the entrainment above the NBL is minimal (for example, in other models as FPLUME there is air entrainment only below NBL), and thus the same flow rate feeds the plume above the NBL and the umbrella cloud; (ii) when modeling the descent back we should consider both the presence of the rising and descending flow, which makes the problem not easy to solve. In addition, we performed several tests varying the source area but keeping the total volume injection rate constant (by reducing the velocity) and the results, within reasonable ranges, are not sensitive to the source area. This result, coupled with the fact that the plume flow rate at the NBL does not change with further rise, should ensure that results are not different from those that could be obtained by considering the rise up to the maximum height and then descent back to the neutral buoyancy elevation.

Line 376: The calculation of N usually requires a numerical calculation of the gradient del(rho_atm)/del(z) over some finite height interval. Over what height interval are you calculating?

The vertical gradient is computed at each integration step of the plume model, by using the actual value of atmospheric density (at z) and the one from the previous integration step (thus at z-Deltaz). The integration step is automatically reduced to reach with the desired accuracy the neutral buoyancy level and not overshooting it. This, coupled with the adaptive step of the RK integration scheme, results in an integration step at the NBL which is generally of the order of less than one meter.

Figure 4 caption: "Above the neutral buoyancy level, the colored contours represent thickness levels of the steady solution computed by the transient umbrella model." This is con- fusing. “the steady solution computed by the transient umbrella model”? What does this mean?

We agree with the reviewer that the caption was confusing. "Steady solution" means that, while the downwind spreading of the umbrella cloud increases with time, the upwind spreading of the umbrella generally reaches a maximum value and then becomes steady (except when no wind is considered). Because here we are interested in the upwind spreading with respect to the vent, we plotted the solution obtained once this steady upwind condition was reached. We tried to make it clearer in the caption.

equation 40: How do you solve these equations? Do you set up an x-y grid in a horizontal plane at the umbrella-cloud height and solve these equations at each node?

We better specified this thing in the text now:

> *"The system of partial differential equations is solved on a uniform grid in the horizontal plane at the umbrella-cloud height with a transient finite-volume code"*

equations 41 and 42: I can't find a definition for chi

We have defined now chi as the indicator function which for a given subset A of X, has value 1 at points of A and 0 at points of $X - A$.

equation 42: where is the origin for x and y used in these equations? I think earlier you said it was the vent location, but in this case it would seem more appropriate to be the location of the plume axis at this elevation. Also, chi is written as CHI|(xËĘ2+yËĘ2>r_nbl). Shouldn't this be CHI|(sqrt(xËĘ2+yËĘ2)>r_nbl)?

The reviewer is right in both the cases. We explained better the origin used for x and y:

> *"the volumetric flux source derived from the vertical velocity at the vent and*

> *defined as a function of the horizontal coordinates(x,y), with the origin of the coordinate system fixed at the center of the plume at the neutral buoyancy level"*

We also corrected the argument of the indicator function Chi, which now is $x2 + y2 > r_{nbl}^2$.

Section 2.3: You need to cite Appendix A1 here for a description of water phase changes. It would help also to cite it near equation 27.

Done.

line 387: define CFL condition.

We modified the text in the following way:

> *"The solution is advanced in time until a steady upwind spreading is reached, with the integration time step controlled by a CFL condition, i.e.by a constraint on the ratio between the distance travelled in one timestep by the fastest waves in the solution and the size of the cells of the computational grid (Courant et al., 1967)."*

Section 3.1: April 2015 Calbuco eruption In the first paragraph of this section, you have listed most of the key model inputs, but a few are missing (or were difficult for me to find). In particular, the grain-size distribution, the vent elevation, and an atmospheric sounding. It would be useful to have two tables listing these inputs. Line 409: the information on grain-size distribution in this sentence does not provide the mass fractions of each grain size. It would be better to list it in a table.

We added a new table for the grain size distribution and we listed the other model inputs in the main text.

Figure 5 (and other figures with multiple plots): It would be useful labeling these plots "a", "b", "c", etc. And in the plot of relative density vs. height, it would be useful to have a black vertical dashed line indicating a relative density of zero. Also, it looks like these results are calculated using only the buoyant plume calculations, not the shallow- water equations. The curves of ascent velocity and relative density for example extend above the neutral buoyancy level, apparently all the way to the plume top, where ascent velocity reaches zero. Are the circles plotted at each elevation in the lower right, above the level of neutral buoyancy, also calculated from the buoyant plume model? Or are they calculated using the shallow water equations?

We added the labels to the panel as suggested by the reviewer. Furthermore, we tried to make it clearer that all the results presented in figure 5, including the 3D view, are obtained with the plume model only. For this reason, we added this sentence at the end of the caption:

> *"Please note that in all the panels values up to the neutral buoyancy level are plotted with solid lines, while those corresponding to plume model solutions above the neutral buoyancy level are plotted with dashed lines."*

Also in the text we tried to make it more clear:

> *"In Fig. 5 some of the plume model outputs obtained for the first phase are reported. In each panel, values below the neutral buoyancy level are plotted with a solid line, while dashed lines represent plume model results above it."*

Figure 5: it would be useful to have labels in each row: A="no aggregation", B="Costa", C="beta=1e-15", D="beta=1e-14", E="beta=1e-13".

We tried to add the labels, but they were confusing because it was not clear if they referred only to the left panels or to all the panels. So, to avoid any confusion, we preferred to keep the figure as it is.

line 469: what are the sizes of the initial particles?

We added the phi sizes -2 and -3 to the considered grain size. This is now specified in the main text.

Line 471: why did you choose an aggregate density of 1500 kg/m3? This seems dense for an aggregate, but perhaps it's appropriate for an ice aggregate containing dense particles.

This due to the fact that here we are considering wet aggregation. Density values for aggregates with water vapor and liquid water are presented in Costa et al. 2010, Fig. 2c and 2d, where values between 1000 kg/m3 and 2500 kg/m3 are shown. Furthermore, as the reviewer observed, it is possible to have ice aggregates, which could also produce relatively high densities. In any case, in further simulations with aggregation, it will be worth investigating more the effect of aggregate density. Considering the comment of the reviewer, we also added this text to the conclusions:

> *"We observe that dry aggregation could produce different results, because the aggregates would have lower densities and thus lower settling velocity, strongly affecting the deposition pattern from the rising plume."*

Figure 8 caption: I'm a little confused about what is shown in the left column. The caption says it is the GSD at the neutral buoyancy level but then it says that

the blue bars represent the particle size distribution released at the vent. Also, it would be useful to indicate the neutral buoyancy level in the plots on the right column. Lines 489-490: "It is important to note that the neutral buoyancy level differs from that obtained without aggregation by less than 0.1%,". I don't see the NBL height shown for the runs in Fig. 8.

The reviewer is right and the caption is confusing. Now we changed the text in the following way:

> *"The blue bars represent the amount of non-aggregated particles, while the maroon bars represent the amount of aggregates."*

We also added dashed lines in the panels on the right to represent the NBL height.

End of Section 3.1: I have the impression that most aggregates that have been mapped in proximal fallout (Self, 1983, Fig. 8; Sisson, 1995, Fig. 9; Wallace and others, 2013) and produced in shaker-pan experiments (Van Eaton and others, 2012) have a narrow size distribution, millimeters to a couple of centimeters in size; and nearly all fine ash (phi>âĹij4) is incorporated into aggregates. The aggregate size distributions you show in rows B-E show a wider range of aggregate sizes, and, in rows B-D, a large fraction of the finest ash survives as individual particles. How do you think the aggregation parameters would have to differ from those you use in order to produce a similar aggregate size distribution? Non-constant value of beta? Higher value of beta?

The point raised by the reviewer is really interesting, but at this stage it is not easy to speculate about it. Our main aim in this work was to present a methodology and a numerical approach to model the aggregation process by considering the full coagulation equation with a collision and a sticking kernel. The method we propose will easily allow

in the future to test different kernels, and for sure it will be interesting to calibrate the kernels with the results of laboratory experiments. In any case, we think it is worth to raise the point in the paper, and the following text has been added to the discussion:

> *"In addition, we remark that most aggregates that have been mapped in proximal fallout (Self, 1983; Sisson, 1995; Wallace et al., 2013) and produced in shaker-pan experiments (Van Eaton et al., 2012) have a size distribution narrower than that produced by the aggregation kernel we adopted. This suggests that in the future the model could be updated with new collision and sticking kernels, informed by laboratory experiments and data coming from fallout deposits."*

Line 522: what is orthometric height?

Orthometric height is the distance of a surface point along the plumb line to the geoid, which is taken as the reference surface. geopotential altitude is a vertical coordinate referenced to Earth's mean sea level. Now the definitions of orthometric and geopotential altitude have been added to the text.

line 535: "Moist unsaturated air cools with height at a slightly lower rate than dry air (Dutton, 2002)". Are you referring to the wet versus dry adiabatic lapse rate? I thought the environmental lapse rate (6.5 C/km for a standard atmosphere) was independent of humidity.

One of the main assumptions of the International Standard Atmosphere (ISA) is that air is assumed to be dry and clean and of constant composition. In fact, at each geopotential altitude, the model solves for the ideal gas law in molar form, which relates pressure, density, and temperature, by using the specific gas constant for dry air. The same is true for the U.S. Standard atmosphere, for which one the basic assumptions is *that "air is a clean, dry, perfect gas mixture (cp/cv = 1.40)".*

line 570: How did you adjust the mass eruption rate? By changing vent diameter?

The reviewer is correct, and we added it to the text.

Line 606: change "wincreasing" to "increasing"

Done.

Section 3.2, paragraph 1. It would be useful to have a table listing the properties of an ISA Standard Atmosphere.

A new table has been added.

equation 43C: should R be R_{air}? the table of notation in Appendix C1 indicates that R_{air} is the specific gas constant for air.

R_{air} *is the specific constant of dry air, while here R denotes the constant for the atmosphere, which varies with humidity. This is now written explicitly in the text.*

Figure 9: In the y- axis labels to all the plots, change "Hight" to "Height".

Done.

Appendix A1: nice description. A complicated, iterative procedure depending on temperature and saturation conditions seems like about the only way to solve for T .

Thank you!

Line 667: "For T>29.65 K". Should this be "for T>29.65 C"?

No, the temperature is in Kelvin degrees (see Eq.9 Folch et al. 2016). More than a thermodynamic/atmospheric constraint (temperature is well above this value in the altitudes of interest), this is a constraint needed to avoid singularities in the equation).

Line 688: "where P_{wv} and e_s are functions of x_{wv} only". Do you mean "where P_{wv} is a function of x_{wv} only"? I thought e_s was a function of T, as indicated in equation A5.

The reviewer is right when writing that e_s is a function of mixture temperature, but this quantity can be expressed as a function of water vapor mass fraction, through an opportune combination of the other equations. We tried to make it clearer in the text.

Line 753: I don't see any explanation of how you calculate Γ_s, the fluid shear, in equation A20.

We added the value we used and a reference.

Appendix B: Figures B2 and B3: in the plot of "liquid water and ice mass fraction", I don't see any curves representing ice. Also, it would be useful to label each plot in this figure "a", "b", "c", etc. In the curve of relative density, it would be useful to have a vertical black dashed line at a relative density of 0.

Figures B2 and B3 (Figures C2 and C3 in the revised paper) have been corrected according to the reviewer's suggestions. The panel for liquid water and ice mass fraction has been splitted into two different subplots, one for liquid water and one for ice. Labels have been added, together with a dashed black line marking the relative density of 0.

Appendix C list of terms is not complete. Missing for example are S terms,

OMEGA, the meaning of hats (ËĘ) and overbars, N_k and M_k from eq. (11), etc.

We renamed the table to "List of model variables" and we added the missing variables.

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