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Geoscientific Model Development Discussions
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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 RC3

We thank the reviewer for the careful reading of our manuscript and the many insightful comments and suggestions. Below we respond to the comments in detail, with reviewer comments in bold and our reply in italics. We also did all the other changes suggested in the annotated manuscript. 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.

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Section 2.1: The section is rather dense and I would recommend dividing it into several subsections.

We agree that the section is dense, but it is really focused on a single thing: how particle size distribution is treated in the model. We do not see how this could be split.

Section 2.1: Could you please consistent in the way the function eta is called. It is sometimes referred to as number density function, NDF, mass distribution, distribution of mass fraction...

We followed the suggestion of the reviewer and we used the term number density function, where possible.

Eq (5) onward: I don't understand why you suddenly dropped x and t from eta.

We wrote in the manuscript that the notation without x and t is used also when the moments change in space and/or time.

> *"We observe that in the notation of the moment on the left-hand side of the equation we dropped the explicit dependence from x and t. Also in the following of the paper, for the sake of simplicity, we will use without ambiguity the notation without x and t to denote the moments, even when they vary in space and time."*

Eq (8): Is there a citation you could add for these expressions?

We are not sure a reference is needed. A substitution of the linear relationship given by Eq. (8) in the left-hand side of Eq.(7) immediately gives Eq. (6), and thus the right-hand side of Eq. (8).

Eq (10): I don't understand in which situation each case is used. Is there a criterion that allows you to switch from case to case? Figure 2 actually didn't help me understand, and it would be nice to explicitly explain that in the text. By the way, you never explain clearly how you calculate the coefficients alpha, beta and gamma. This is really missing from the model description.

The reviewer is correct, because the criterion for the choice of the case and details of the calculations were not provided in the paper. Qualitatively, the cases are represented in Figure 2. Case 1 and case 3 are used at the left and right intervals of the support of the number density function, when a linear approximation cannot provide the correct moments, while case 2 is used for the internal intervals. In any case, we added a new appendix where all the details are provided.

Section 2.2: I am missing an equation for the plume size/radius. You should at least indicate somewhere how this quantity is determined.

There is no need for an additional equation for the plume size/radius. The equation for the moment is used to update the vertical velocity, and the equations for the particles and energy are used to update the density of the gas and the mass fractions of particles, and thus the mixture density. Once we have the new values of rho_mix and w, the new radius is computed from the updated value of the mass flow rate obtained with the integration of Eq. 22.

line 194: You state that the plume equations are solved in a 3D coordinate system, and yet, all equations are written in 1D. I believe all plume equations assume an axisymmetric plume and are derived in a cylindrical coordinate system. Could you please correct?

We are not using a cylindrical coordinate system, because we are not solving for radius and an angular coordinate. The plume has both vertical and horizontal components

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of the velocity, and because of the last ones (when wind is present) the plume (and thus its axis) moves also horizontally. Thus, the plume axis location is defined in a 3D coordinate system. For this reason, following the comment of the reviewer, we added to the manuscript the differential equations (new Eqs. 11) which describe the 3D location of the points of the plume center-line. In addition, at the beginning of Section 2.2, we added this sentence:

> *"The model we present, based on the buoyant plume theory of Morton et al. (1956), is a 1D integral model where plume properties are averaged over cross-sections."*

Now it is explicitly stated that the model is 1D also at the beginning of the abstract.

line 198: You define Phi as an angle, but you already introduced the notation previously for the Krumlein scale. Similarly, you later use w to denote the vertical plume velocity, sedimentation velocities and the specific humidity. Could you please use different notations to denote different variables?

We changed the notation for the angle.

Eq (13): Should be supported by a citation.

Done.

Eq (14)-(15): Shouldn't the integrals be replaced by sums since, after all, what you do is simply summing over bins.

Here we are writing the general aggregation/coagulation equation, as introduced by Von Smoluchowski. This is independent from the choice of partitioning the mass/size in bins. We also observe that it is not possible to derive the discrete equations, if we *don't write first the continuous version.*

Eq (17)-(18): The EÌĹE Ìĺ notation is not defined.

There is a problem with the fonts used by the reviewer, and this makes unclear what is the notation problem.

line 300: I would suggest explicitly stating that x_w is the mass fraction of total water, and that the various contributions from vapor, liquid and ice are determined as described in the appendix.

We modified the text in the following way:

> *"where x_w is the total mass fraction of water (which can be in either vapour, liquid and ice form) in the mixture. The partitioning of total water in the different phases is detailed in Section 2.3 and in Appendix B1."*

lines 333-335: I believe that you forgot to mention here all the extra steps described in Appendix A1 to partition condensed water between liquid and ice. This should be mentioned.

The reviewer is correct and now at the end of the paragraph we added:

> *"The details of the procedure employed to compute these values are given in Appendix B1."*

Eq (40): Several parameters are poorly defined. In particular, C_D, gamma, u_{nbl}, v_{nbl} and w_{nbl} (and nbl indices in general) are not defined in the text. I would also

suggest explaining the physical meaning the two terms on the right hand sides of the equations.

We defined all the missing parameters in the text and better explained the physical meaning of the terms. Thank you for noticing the problem.

Eq (41): Again, function chi is not defined.

We have now properly defined chi as the indicator function.

lines 409-419: How do you initial the atmospheric background profiles? Nothing is said here about where the data comes from. By the way, a longer description of atmospheric profiles determination is given in 3.2. I don't understand if the same method was used also to constrain the experiments run in 3.1. If yes, then the whole part should be moved to the beginning of 3.1. If no, then something is missing from3.1 anyway.

For the simulations of the Calbuco eruption we did not use the modified standard atmospheric profiles described in section 3.2, but we used atmospheric profiles coming from the ECMWF-ERA5 dataset. This part has been made clearer in the text by adding the following lines:

> *"The atmospheric profiles used to run the simulations derive from the ECMWF-ERA5 reanalysis dataset. For the two eruptive phases, we extracted geopotential height, atmospheric density, pressure, temperature, specific humidity and horizontal wind velocity components at the vent location and eruption starting time."*

lines 427-428 and fig 5: I am very very surprised to see so much ice formed all of a sudden while no liquid water is present. It seems like at the top of the plume,

all the water is found in ice form, there is no more water vapor. This points to a serious caveat of the model. Perhaps a mistake in the formulation of the liquid-ice partitioning method? In any case, you need to comment on that issue and discuss possible reasons for these extreme ice fractions.

We double-checked the formulation of the equations and the implementation of the procedure in the code, but we couldn't find any error. We also compared our results with those produced by other plume codes (FPLUME, PLUMERIA), and we have not found any appreciable difference. It is important to observe that for other test cases we have liquid water in the mixture, but this is not very common in volcanic plumes, while it is more common to have ice forming at high altitudes. A better understanding of the reason there is no liquid water in the simulation presented in Fig. 5 can be obtained by looking at the values of saturation pressures e_l, e_s *and the value of the partial pressure of water vapor, presented in the center plot of the figure at the end of the document. The two dashed lines indicate the region where ice, liquid water and water vapor can be present at the same time (case 3 in new appendix B1). We first observe that at lower altitudes, where only liquid water can form, the partial pressure of water vapor is well below the saturation pressure of vapour over liquid, also because the mass fraction is small compared to that of dry air (because of the large entrainment). This happens because the temperature of the mixture, despite the large entrainment, stays high because of the heat capacity of the solid phase. In fact, numerical tests performed with lower temperatures produce lower values of saturation pressures, and favour the formation of liquid water at lower altitudes. In any case, we observe that the mass fraction of water in the mixture, compared to that of dry air, is quite low. Additional simulations with a larger amount of water also produced results where liquid water forms in the volcanic column.*

Figure (6): I would suggest rewriting the caption like: "Same as figure 5 but for the second event."

Done.

Figure (7): Does the color scale represent time? You should add a title to the color bar to specify that.

The fact that multiple contours were plotted in Figure 7 was quite confusing. Now the right panels of Figure 7 only shows the outer contours of the outputs at the final time, for the different values of the drag coefficient. Please note that the range of drag coefficients has changed, accordingly with the suggestion of Reviewer 2. The contours of thickness at the end of the simulation, for the best value of the drag coefficient, are now shown in a new figure, where a title has been added to the colorbar specifying that the different colors represent different thickness values.

Section 3.2: As said previously, is the method described here to initialise the atmosphere similar to what was used in 3.1? If yes, this should come earlier. Also, this is absolutely not clear to me if the sensitivity experiments shown here are based on the same case study as in 3.1 or are purely idealized. The connection (or lack thereof) between the simulations in 3.1 and 3.2 should be more clearly stated.

We better clarified that the atmosphere profiles here are different than in Section 3.1:

> *"With respect to the previous section, where atmospheric profiles used to run the simulations were derived from the ECMWF-ERA5 reanalysis data, the simulations presented here employ atmospheric profiles of pressure, temperature and density modified from the International Standard Atmosphere (ISA) model."*

line 645: Which parameters have been constrained? Except perhaps for C_D

which was tuned to reproduce observations, we can't really say that model parameters have been constrained.

The paragraph cited by the reviewer has been revised following the suggestion of the reviewers, and now it reads in the following way:

> *"PLUME-MoM-TSM also accounts for phase change of water, resulting in the formation of a liquid water or ice inside the plume, and it includes a module for the spreading of the umbrella cloud as a gravity current. These new features have been tested by applying the model to the April 2015 eruption of Calbuco volcano in Chile, allowing to constrain model parameters and to quantify the effect of wet aggregation on model results. The analysis shows that aggregation has a minimal control on plume characteristics and on the loss of particles from its margins. 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. In addition, we remark that most aggregates that have been mapped in proximal fallout (Self, 1983;670Sisson, 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."*

equation (A3): e_l is not defined.

The variable e_l is the saturation pressure of vapour over liquid. This variable is defined at line before equation B2 (previously A2).

lines 704-705: Which of the two temperatures is used then? Or under which conditions one or the other is used? More generally, I found the whole part between equations (A8) and (A10) very confusing.

In the interval [Tref-40,Tref] we search for mixture temperature and water partitions (x_{wv}, x_{lw} and x_i) that satisfy the known value of equilibrium mixture enthalpy (H_{eq}). We show that x_{wv}, x_{lw} and x_i can be written as functions of mixture temperature (Eqs from A7 to A10, B7 to B10 in the revised paper), reducing our problem in finding mixture temperature only and then calculating water partitions through equations from B7 to B10. In more detail, Eqs. B7 and B8 allow the calculation of x_{wv} as a function of mixture temperature, while Eqs. B9 and B10 are for x_{lw} and x_i respectively. We first check the equilibrium conditions at Tref and Tref-40, and, if we find that equilibrium is possible in interval [Tref-40,Tref], we apply a bisection procedure to solve for mixture temperature. For this reason, depending on the investigated temperature, we solve equation B8 by using e_l if the investigated temperature is equal to Tref, while we use e_s for the other cases.

Eq (A9): So if understand the equation well, when mixed-phase conditions prevail, x_{lw} is first calculated at T=0C, and then corrected using a linear function of the temperature such that it would be 0 at -40C? Then, the ice fraction is simply the total condensed water fraction - x_{lw}?

Yes, in interval [Tref, Tref-40] we assume that x_{lw} varies linearly from the value computed at Tref (under the condition that no ice is present, but only vapour and liquid) to 0 at Tref-40 (only vapour and ice are present). The ice fraction is then calculated as the total water fraction (vapour+liquid-ice, known) minus vapour + liquid.

line 722: It says that several settling velocity models are implemented. But which one is used in your experiments then? This is never said. More generally, I would recommend only describing the one model that has actually been used.

It is stated in the text, in the first paragraph of section 3.1, that the Textor model for velocity settling is adopted for the simulations.

For the sensitivity analysis section, the following text has been added:

> *"As for the previous application, settling of particles from the plume edges is considered, and the settling velocity model from Textor at al. (2006b) is adopted (see Appendix B2 for more details)."*

We agree with the reviewer that usually it is better to describe only the models used for the applications presented in a paper, but we remark that in this paper, and in GMD in general, the main focus is on the presentation of a new model and not on the application. For this reason, we think it could be of interest to potential readers of the paper to know that more than one settling velocity model has been implemented in the code.

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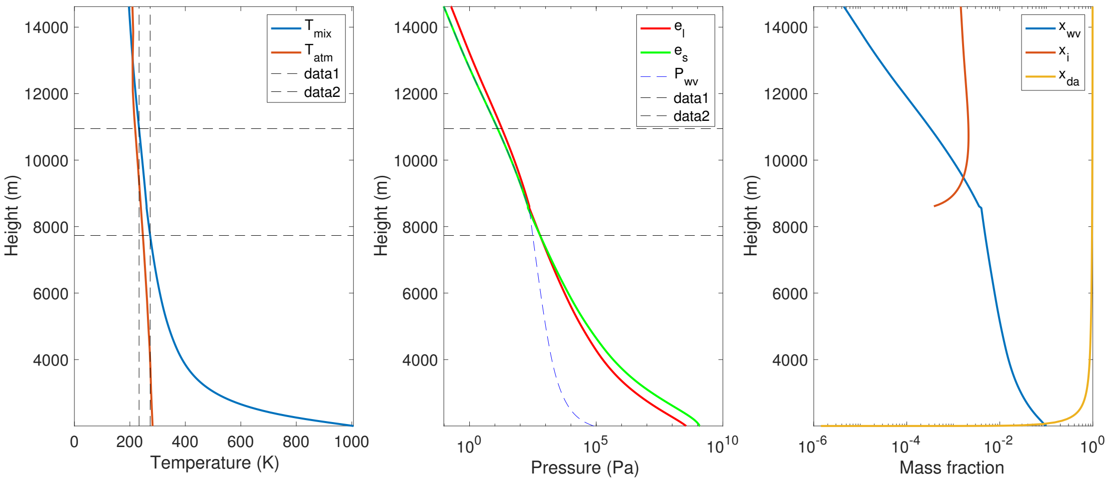

Fig. 1.

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