Dear Editor,

In this document we have collected point-by-point responses to the reviews, where all relevant changes made in the manuscript are described. In particular, following the suggestion of one of the reviewer, we explored a larger range of values for the estimation of the Drag Coefficient, and we better discussed the outcomes in the context of previous literature results.

At the end of the document, you can find a marked-up manuscript version. We believe that we addressed all the points highlighted by the reviewers, and that this revision contributed to improve the manuscript.

Kind regards,
Mattia de' Michieli Vitturi
(on behalf of the authors)

Reviewer 1

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: Its 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*r2*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

$$Av \cdot n \tag{1}$$

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 2r 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[Tref40;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 equa-

tion 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 can 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 cant 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—(x2+y2¿r_nbl). Shouldnt this be CHI—(sqrt(x2+y2)¿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 its 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: Im 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 dont 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¿4) 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 dont 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 dont 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 reviewers 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 () 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.

Reply to Reviewer RC2

We thank the reviewer for the comments and the suggestions, which we believe contributed to improve the manuscript. In particular, the comments about the value of drag coefficient previously estimated let us consider a wider range of values to investigate, and a different criteria to select the best value, which is now more consistent with literature results. We collected below the main comments of the reviewer in several points (in bold), to which we replied in detail (our replies in italics). In addition, the reviewer can find in the GMD online discussion a pdf of the revised manuscript, where the changes with respect to the previous version have been highlighted.

As the authors state, the depth-integrated umbrella cloud model (equation 40) closely follows the approach of Baines (2013) and Johnson et al. (2015). In particular, the model for drag between atmosphere and the umbrella cloud used in both these papers is also used here to couple the ambient wind field to the motion of the umbrella cloud. The value of the drag coefficient CD is inferred in this paper by comparing model outputs with measurements of the equivalent radius of the plume of the April 2015 Calbuco eruption, and a value CD = 1 is selected on this basis. This value is extremely large between 100 and 1000 larger than previous studies of gravity currents and intrusions into stratified fluids indicate. It is worth noting that CD is not a free parameter that is expected to be O(1) (such as the parameter λ in the scaling of Woods & Kienle (1994)), but arises from a physical process, namely the formation of shear layers between the ambient and umbrella cloud. As such it has been constrained experimentally (e.g. https://doi.org/10.1080/00221687909499572). From such experiments, Baines (2013) and Johnson et al. (2015) both infer coefficients in the range CD = 0.001 to 0.01. These small values are reflected in the fact that drag forces are usually neglected entirely in models of spreading intrusions (https://doi.org/10.12012F9781584889045). As a consequence of the small drag coefficient, both Baines (2013) and Johnson et al. (2015) reason that drag becomes significant only late into the eruption (after at least 3 hours for CD = 0.01, 30 hours for CD = 0.001) and very far downwind from the source. This contrasts with the present model, where the much higher drag coefficient results a drag-dominated flow everywhere, with drag nearly arresting the upwind flow after 30 minutes.

- **Can the authors comment on why the inferred drag CD = 1 is so much larger than previous studies indicate?**
- **What physical mechanism is anticipated that could lead to such a large drag force in a high Reynolds number turbulent flow?**
- **What would be the effect of choosing a drag coefficient in the range CD = 0.001 to 0.01 as previous authors have?**

- **What changes to the mass flux, or other parameters, would be needed for the model to fit observations with these smaller drag coefficients?With a drag coefficient consistent with previous works, is the PLUME-MoMTSM model able to predict both the altitude and horizontal extent of the Calbuco umbrella cloud simultaneously?**

The drag coefficient here is inferred from comparison between the model predictions and the umbrella cloud of the second phase of the April 2015 Calbuco eruption (figure 7). The measurements of the Calbuco plume used are the equivalent radii reported by van Eaton et al. (2016), in which the area of the umbrella cloud as viewed from the GOES-13 satellite, is converted into the radius of a circle of the same area. This equivalent circle methodology is appropriate for van Eaton et al. (2016), where the umbrella cloud is assumed to be a circle, but is less suitable for the present paper, which makes predictions of the full cloud shape. A comparison of the full predicted shape of the cloud with the GOES-13 observations (available in the supplementary material of van Eaton et al. (2016)) would provide a much more convincing validation of the model. In particular, the upwind spreading distance is a focus of this paper (in 3.2), but this quantity not validated by the comparison of equivalent radius alone in figure 7.

- **How does the predicted upwind spreading distance in figure 7 compare with measurements of the upwind spread from GOES-13 data?**
- **Are the model predictions of the cross-wind plume width and downwind spreading distance consistent with the observations?**

We thank the reviewer for letting us notice the problem with the high value of the drag coefficient. For this reason, we performed new simulations and compared the results obtained with CD=1, 0.1 and 0.01. With respect to the version of the manuscript originally submitted, we compared not only the "equivalent radius", but also the upwind spreading, manually extracted from the Van Eaton et al. 216 paper. This quantity provides a better comparison between observation and numerical results for two reasons. The first is that the upwind spreading is the main quantity of interest of our work, and in particular it is the one for which we try to provide an analytical relationship with some characteristic quantities of the rising plume. The second is that in our simulations of the umbrella cloud spreading we use a constant wind, extracted at the vent coordinates from the ECMWF-ERA5 reanalysis dataset. Thus, upwind spreading, which occurs close to the vent, is less affected by this approximation than the umbrella cloud area, which covers locations far from the vent, and where wind could be potentially quite different. Thus, we completely agree with the reviewer that the *equivalent radii reported by van Eaton et al. (2016) are not the best data to use to calibrate the drag coefficient.*

Indeed, the new calibration, based on observation of upwind spreading, shows that CD=0.1 gives better results than CD=1 and CD=0.01 for both the phases (see plots above). This result is consistent with the values obtained in Pouget et al. 2016 for several eruptions. In the new version of the manuscript we present these new results (see new Fig. 7) and we discuss more in detail previous results on the value of the drag coefficient. The text has been modified in the following way:

> *"For both phases, the outputs at the neutral buoyancy level of the plume model of PLUME-MoM-TSM (volumetric flow rate, radius and horizontal velocities) were used as input of the umbrella cloud module. For this application, we varied the drag coefficient C_D of the umbrella cloud model in order to find the value which best reproduced the spreading in the atmosphere observed during the eruption. From the experiments of Abraham et al. (1979), Baines (2013) and Johnson (2015) both inferred coefficients in the range C_D=0.001 to 0.01, while Pouget et al. (2016), by comparing the results of the shallow-water intrusion model developed by Johnson (2015) with satellite data from seven eruptions, better reproduced observations of umbrella cloud structure and morphological evolution with a value C_D=0.1, which was the largest value of the investigated range. Here, three values were tested (C_D=0.01, 0.1 and 1) by carrying on the simulations for 1.5 hour and 2 hours for the first and second phase, respectively. At each time step we computed: (i) the upwind spreading, defined here as the maximum horizontal distance from the vent in the opposite direction to that of the wind at the neutral buoyancy level; (ii) the radius of a circle with area equivalent to that of the modeled umbrella cloud."*

> *"These values are plotted, at intervals of 300 s, in the left panels of Fig. 7, with blue lines for the equivalent radius and red lines for the upwind distances. In the right panels of Fig. 7 the edges of the umbrella cloud at the end of the simulations are plotted, with different lines for the different values of C_D. From both the left and right panels we can observe the expected larger spreading of the umbrella cloud for smaller values of C_D. These results highlight the fact that intrusive gravity current dominates in the initial stages of the dispersion of tephra at the neutral buoyancy level and large upwind and crosswind spreadings of the umbrella cloud with respect to the vent location (denoted with a yellow star in the right panels of Fig. 7) are produced also for subplinian eruptions. In order to constrain the value of the drag coefficient, the blue lines in the left panels of Fig. 7 are compared with the series of values for the umbrella radius reported in Van Eaton et al. (2016), obtained detecting the edge of the*

umbrella from GOES-13 satellite images and here plotted with blue markers. From the results plotted in the figure, we can see that the values obtained with C_D=1 seem to better match the observations, with a small overestimate at the beginning and a small underestimate at the end of the simulation.

A different result is obtained by analyzing the modeled upwind spreading of the umbrella cloud. First of all, we can see from the red lines in the left panels of Fig. 7 that for values of C_D greater than 0.1, a steady upwind distance is approached at the end of the simulation, and that by changing (increasing or decreasing) the drag coefficient by an order of magnitude, the upwind spreading at the final time changes approximately by a factor 2. In this case, when model results are compared with the upwind spreading derived from the processing of the observations presented in Van Eaton et al. (2016), represented by the red crosses in the left panels of Fig. 7, we see that the drag coefficient value C_D=0.1 produces the best results. The two different values of C_D obtained by comparing the equivalent radius and the upwind spreading with observational data can be due to the several approximations in the numerical simulations. In particular, the depth-averaged umbrella cloud model uses a constant wind (in time and space), extracted at the vent coordinates and at the neutral buoyancy level from the ECMWF-ERA5 reanalysis data. Thus, while upwind spreading, which is measured close to the vent, is slightly affected by this approximation, the spreading of the whole umbrella cloud (controlling the equivalent radius) could be affected by larger errors, because downwind wind far from the vent can be different from the assumed constant value. Furthermore, we notice that the value C_D=0.1 better agrees with the results presented in Pouget et al. (2016). For these reasons, and because in the following of the paper we are mostly interested in quantifying the upwind spreading of the umbrella cloud, we use the value C_D=0.1 as reference value. For this value of the drag coefficient, umbrella cloud thickness at the end of the simulations is plotted for both the phases in Fig. 8. The larger volumetric flow rate injected at the neutral buoyancy level for the second phase resulted in a thicker cloud, with a total height of column and umbrella of approximately 15.5 km and 17.5 km above sea level for the first and second phase, respectively. These values compare well with those reported in Van Eaton et al. (2016)."

In addition, because of the new value of CD, all the simulations for the sensitivity analysis and the fitting of the analytical relationship have been redone, providing new results where the upwind spreading increases by almost a factor 2, and in consistent way for all the input parameters, with respect to the results presented in the first version of the manuscript. In any case, we observe that the value of CD obtained with the comparison of Calbuco observations is still *higher than those inferred by Baines (2013) and Johnson et al. (2015). This is now discussed also at the end of Section 3.2:*

> *"We also remark that the values of the upwind spreading presented here are strongly dependent on the drag coefficient CD, in this analysis fixed at 0.1, and that the lower values suggested by (Baines, 2013) and (Johnson et al., 2015) would produce a larger upwind spreading of the umbrella cloud. Additional simulations we performed (not shown here) suggest that a decrease of the drag coefficient of one order of magnitude results approximately in doubling the upwind spreading distance."*

We also remark that the reviewer could notice in the plots some very small differences in the results obtained for the case C_D=1, with respect to the results presented in the previous version of the manuscript for the same value. This is because Reviewer 1 noticed a minor thing to fix in the plume model, which can also affect the predicted neutral buoyancy level and thus the input parameters of the umbrella cloud model.

REF. Pouget, S., Bursik, M., Johnson, C. G., Hogg, A. J., Phillips, J. C., & Sparks, R. S. J. (2016). Interpretation of umbrella cloud growth and morphology: implications for flow regimes of short-lived and long-lived eruptions. Bulletin of Volcanology, 78(1), 1.

In 3.2, an approximate expression for predicted upwind spreading distance is given (47). Similar predictions of the upwind spreading distance, in the absence of a rising plume, have been obtained by Baines (2013). How do the predictions of this paper compare to those of Baines (2013)? The expression (47) has some pathological properties for example, a finite upwind spreading distance is predicted even if there is no wind, and the cloud is assumed to spread upwind (positive upwind spreading distance) even for arbitrarily small mass fluxes and high velocities, where a weak plume with no upwind spread is expected. As noted in the previous two points, I have serious concerns about the usefulness of the prediction (47), as is obtained using a drag coefficient that is likely far outside the range of validity of the model equations (40), and is not directly validated. This notwithstanding, equations (47) and (48) need a clear statement of the range of parameters over which they can be used.

As previously written, the comment of the reviewer helped us in making a more convincing calibration of the drag coefficient, which provided a lower value which agrees with that presented in previous works. The reviewer is also correct when he states that the original expression had some pathological properties, in particular for the finite upwind spreading without wind. We really thank him because his observation suggested to us to search for a different expression, for which the correct behaviour with no wind is predicted:

$$z = a\frac{x^b}{y^c} \tag{1}$$

where z is the upwind spreading, x is the mass flow rate and y is the wind velocity, and a,b,c are three fitting parameters.

The new expression still provides a good coefficient of determination (¿0.98) and small relative errors over the whole range of parameters investigated. The same expression has been used for the other fitting, because the downwind distance acts as the wind, and upwind distance cannot be finite when it goes to zero. Also in this case good coefficient of determination and small relative errors in the predicted upwind spreading are obtained. As already written in the reply to the previous comment, we remark that the reviewer could notice in the plots some very small differences with respect to the results presented in the previous version of the manuscript for the same values. This is because Reviewer 1 noticed a minor thing to fix in the plume model, which can also affect the predicted neutral buoyancy level and thus the input parameters of the umbrella cloud model.

Other points:

line 10: "to compute" → "computation of"

Done.

line 31: "quantity" → "quantities"

Done.

line 58: "sometime" → "sometimes"

Done.

line 107: "context" → "contexts"

Done.

line 120: "detail" → "details"; remove "of the".

Done.

What is the origin of equation (13)?

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 2r term on the right-hand side of Eq.13.

line 227: "collisions" → "collision"

Done.

line 355: Strictly, the transition between rising plume and umbrella cloud is not properly described by either the rising plume or the shallow water model for the umbrella cloud, because the aspect ratio of the flow in this region is not small in either direction. This region, characterised by the plume rising to an overshoot and then descending to the neutral buoyancy level, is likely to be highly turbulent. Implicit in the source terms used in (40) is the assumption that energy associated with horizontal momentum is conserved, but energy associated vertical momentum is lost, in the transition from rising plume to umbrella cloud. Some comment about the approximations made in this region, and their potential effect on the model results, would be valuable.

Following the suggestion of the reviewer, we added the following text after the description of the link between plume model and umbrella cloud model:

> *"It is important to observe that the transitional region between rising plume and umbrella cloud, characterised by the plume overshoot and then the descent to the neutral buoyancy level, has an aspect ratio of the flow not small in either direction, and is likely to be highly turbulent. For these reasons, this transition region is not properly described by either the rising plume or the shallow water model for the umbrella cloud, and the derivation of the inflow of the umbrella cloud model directly from the output of the plume model at the neutral buoyancy level represents a simplification of the real dynamics."*

As regards the kinetic energy associated with horizontal and vertical momentum, we observe that even without the radial contribution associated to the term dr/dz and to the plume horizontal velocity at the nbl, at the boundary of the source area we still have a kinetic energy which is due only to the source term in the mass equation. In fact, the source term produces a thickness in the source area, which spreads laterally because of gravity, with a volumetric flow rate consistent with that of the plume at the nbl.

Figure 4 is missing a scale for contour colours. Are the displacements of contours in the lateral and oblique views representing half the thickness of the intrusion h (which should be centered around neutral buoyancy level)?

Thank you for the comment, we now specified in the caption that this plot and the different colors are not meant to represent quantitatively any property of the umbrella, but only to distinguish the different contours.

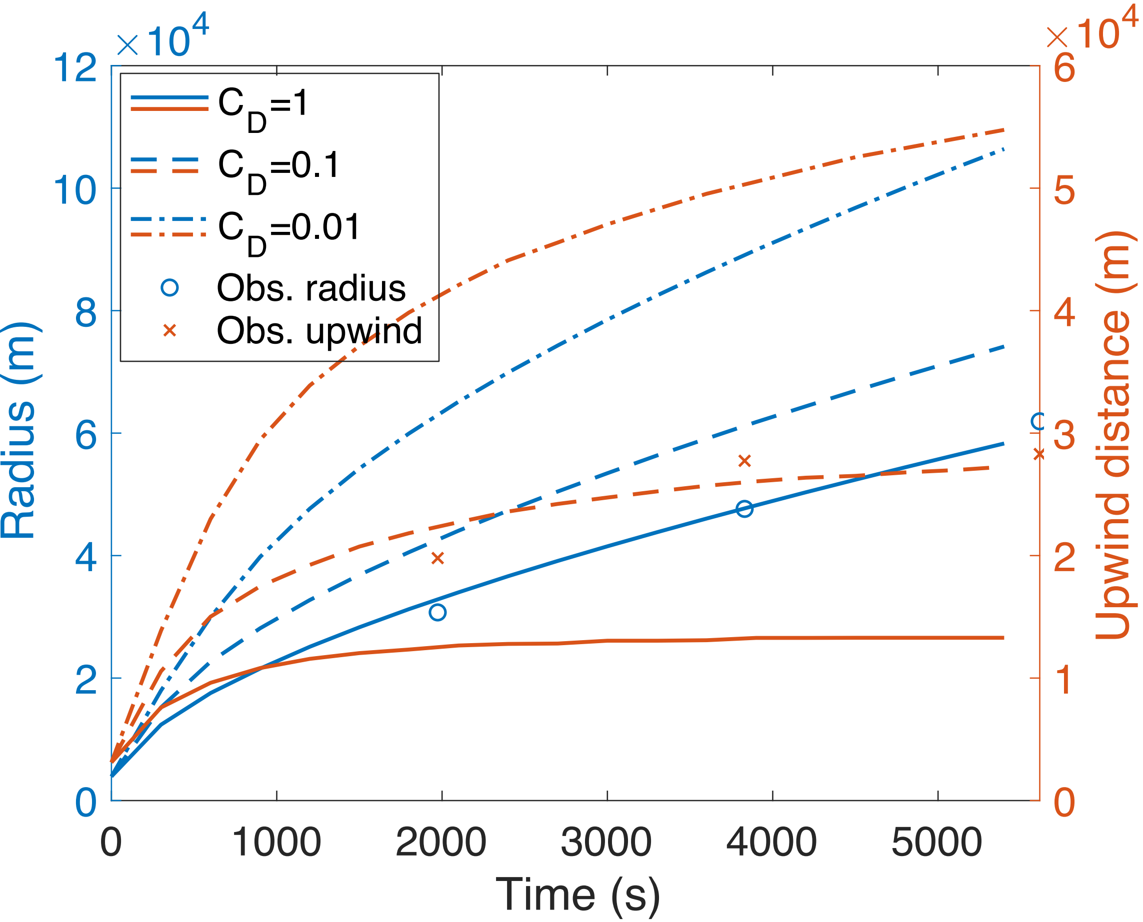
In the computations of figure 8, the grain size distribution is divided into a fairly large number of bins (12) with a constant kernel. What are the advantages in this case of the TSM method over a sectional/discrete description of the particle size distribution? In the limit of a very large number of bins in the grain size distribution,

would the TSM method produce the same result as a section/discrete description?

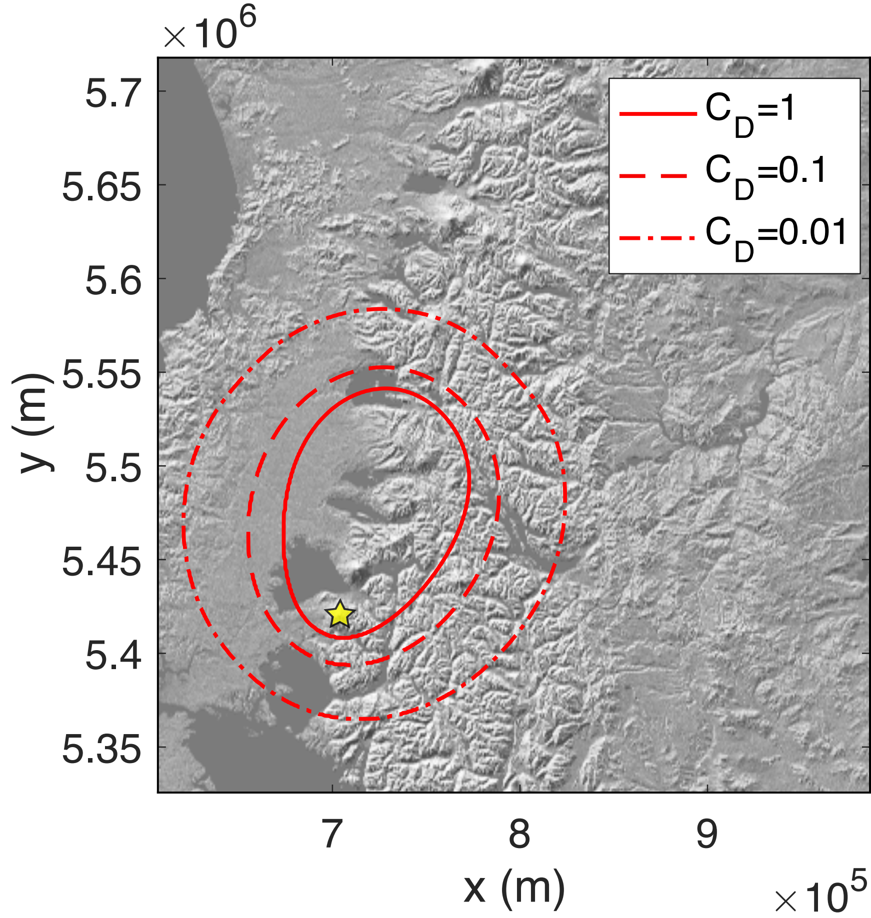
The problem with a sectional/discrete description is that aggregation could produce sizes which do not correspond to the sizes represented by the discrete bins. For example, if the characteristic sizes of the bins are 1,2 and 4 mm, then their volumes are of the order of 1,8 and 64 cubic mm. If two particles of 1mm aggregate, they would produce an aggregate of 2 cubic mm. Now, if we choose to assign to the newly formed aggregate the size which has the closer volume among the characteristic sizes, it will remain in the 1mm bin. So, we would ignore the aggregation. The TSM method allows to have a distribution within each bin, and the distribution changes also with intra-bin aggregation.

line 606: "wincreasing" → "increasing"

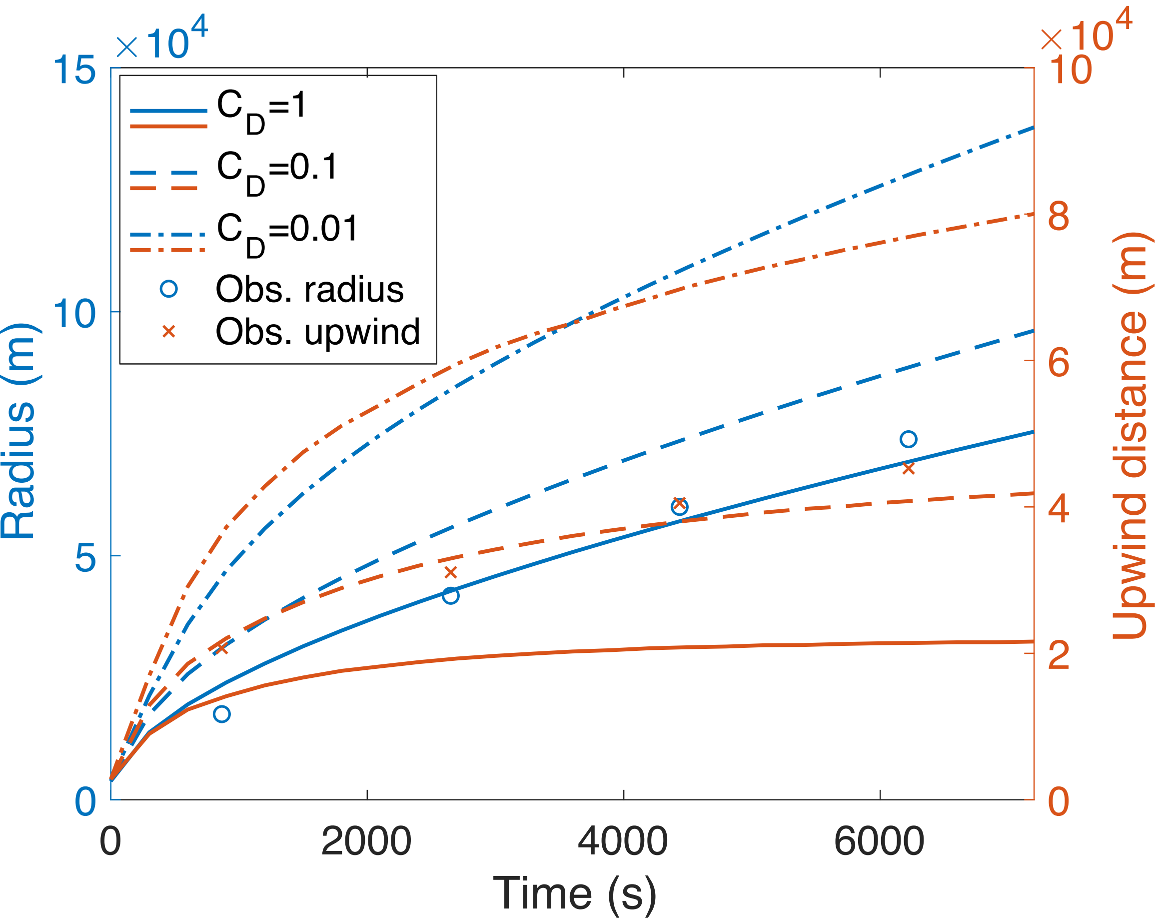
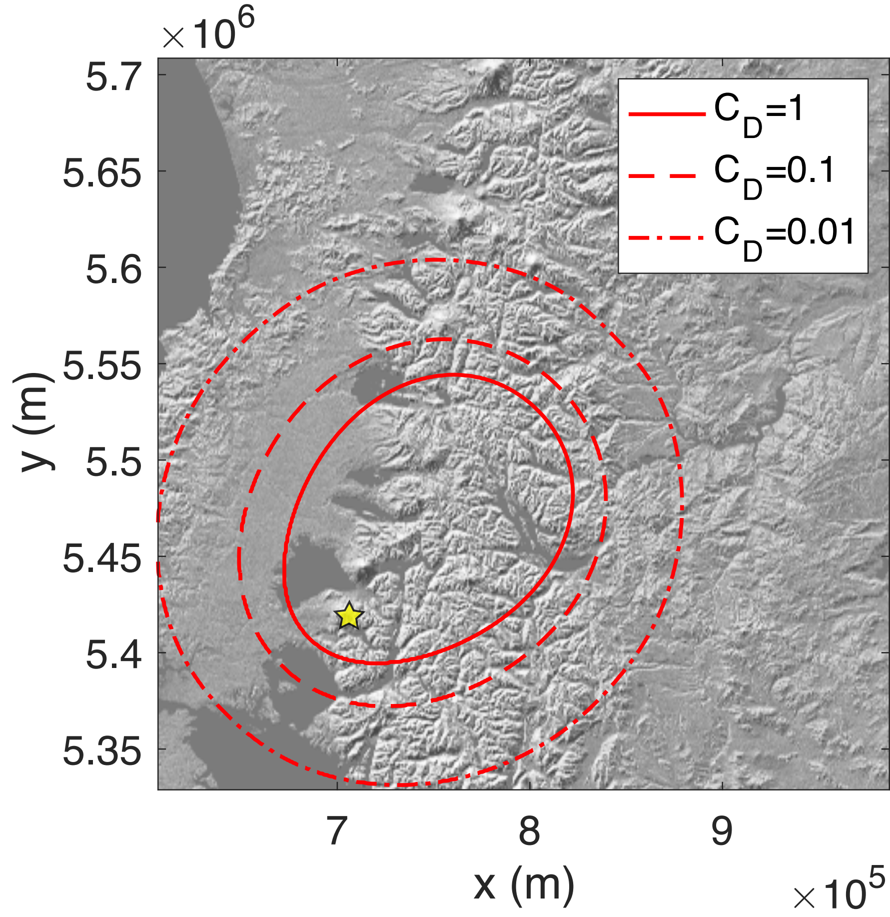
Done.



×10^4
Radius (m)
C_D=1
C_D=0.1
C_D=0.01
Obs. radius
Obs. upwind
×10^4
Upwind distance (m)
Time (s)



×10^6
y (m)
C_D=1
C_D=0.1
C_D=0.01
x (m)
×10^5


Radius (m)
×10^4
15
10
5
0
C_D=1
C_D=0.1
C_D=0.01
Obs. radius
Obs. upwind
0
2000
4000
6000
Time (s)
×10^4
10
8
6
4
2
0
Upwind distance (m)

×10^6
5.7
5.65
5.6
5.55
5.5
5.45
5.4
5.35
y (m)
C_D=1
C_D=0.1
C_D=0.01
7
8
9
x (m)
×10^5

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.

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 dont 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 dont understand in which situation each case is used. Is there a criterion that allows you to switch from case to case? Figure 2 actually didnt 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 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 centerline. 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): Shouldnt 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 EE 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 dont 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 couldnt 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 attached. 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 cant 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.

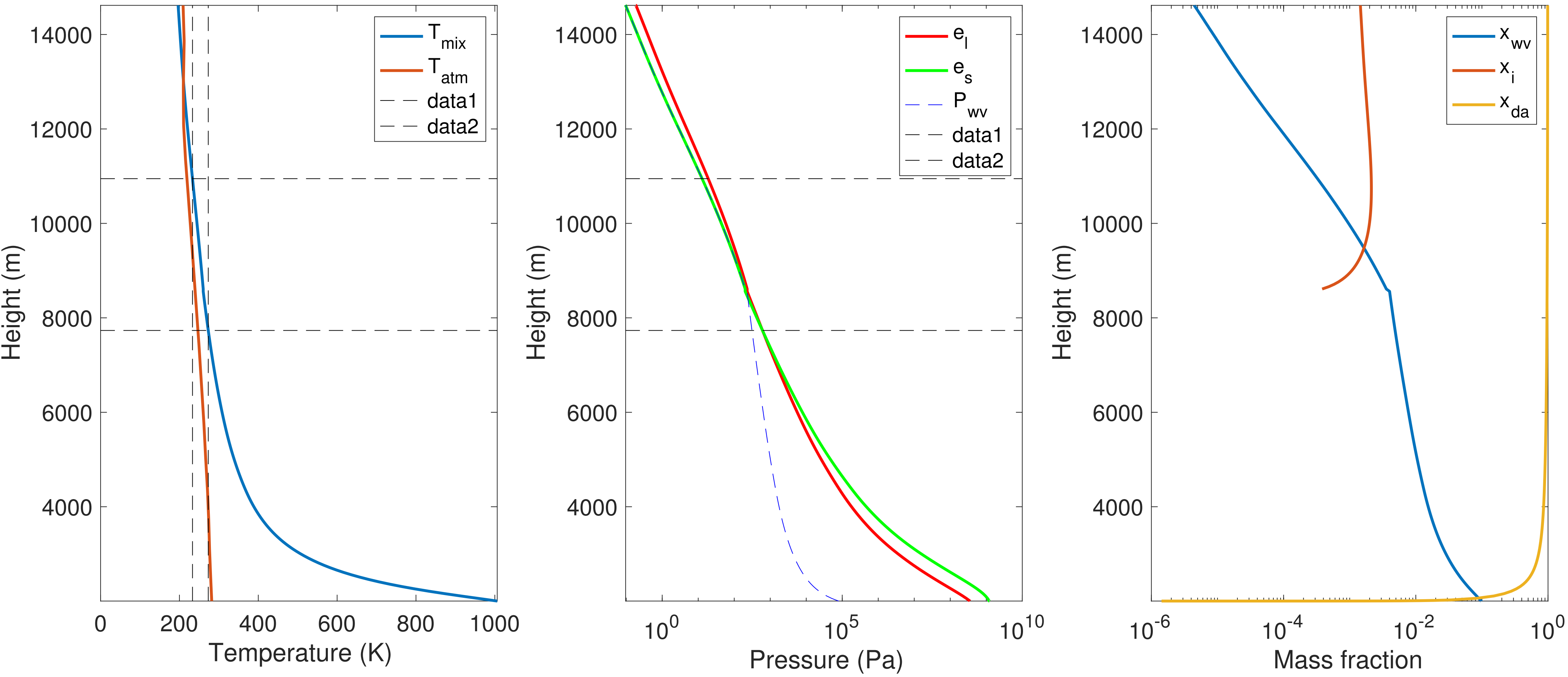
line 722: It says that several settling velocity models are implemented. But which one is used in your experiments then? This is never said. More generally, I would recommend only describing the one model that has actually been used.

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.



Height (m)
Temperature (K)
T_{mix}
T_{atm}
data1
data2
Pressure (Pa)
e_l
e_s
P_{wv}
Mass fraction
x_{wv}
x_i
x_{da}