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Interactive comment on "PLUME-MoM 1.0: a new 1-D model of volcanic plumes based on the method of moments" *by* M. de' Michieli Vitturi et al.

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General comments

The paper presents an extension of integral models for turbulent buoyant plumes carrying particles that allows the evolution of the particle distribution to be modelled conveniently. The formalism presented has the potential to be very powerful in describing plumes from volcanic eruptions as it allows a coupling of grain-scale processes to the bulk flow (although for volcanic plumes the grain-scale processes such as particle aggregation and comminution are not well understood, so this cannot be exploited in this contribution).

The model is demonstrated on three test cases (which differ in their atmospheric structure and volcanic source conditions). The analysis demonstrates that the particle-size distribution does not change substantially during the ascent of the plume, which can be anticipated from the model formulation as the particle size distribution changes only due to fall-out of solids from the plume.

In addition to presenting the mathematical model, the paper also presents the results of a partial sensitivity analysis to examine the effect of changes in the initial (mean and variance of the) particle size distribution on the plume dynamics, measured by four quantities at the plume top altitude. The sensitivity analysis focuses only on the mean and variance of the initial particle size distribution, so is not a full uncertainty quantification of the plume model. However, the analysis shows that small changes in the parameters for the initial particle distribution have only small effect on the plume dynamics.

The mathematical content of the paper is extensive, but it is very difficult to follow. In particular, there are numerous symbols that are not clearly defined, some quantities are denoted by more than one symbol, and several symbols are used to denote different quantities. The mathematical development of the model presented in the paper is a major contribution so must be more carefully considered. I would recommend moving the discussion of the method when applied to the particle size distribution to an appendix, as this is not used subsequently (although I can appreciate that the derivation here is more natural than the mass fraction distribution on the phi scale). Furthermore, there are numerous assumptions implicit in the model development that must be explicitly stated. I have attempted to highlight these in the specific comments below.

Throughout the manuscript there is a lack of reference to other works.

Finally, I would recommend a stronger emphasis on the advantage of the method of moments over binning of the particle distribution in terms of the ability to incorporate grain scale models. While this is mentioned briefly in the conclusion, comments in the rest of the manuscript tend to highlight the computational saving of the method of moments rather than the significant modelling advance that this approach facilitates.

Specific comments:

1. Title and pg 3746 ln 2: The description of the model as 1-D is misleading. The model gives three-dimensional results (the wind can change direction and, with a model for the radial variation of the fields, the three dimensional character of the plume can be reconstructed). Rather, the model has only 1 independent variable, the arc-length along the centreline. It would be better to describe the model as an integral model, as it is derived by integrating the point-wise conservation equations over the plume cross-section.

2. Pg 3749: How are the moments defined in equation (1) related to other descriptions of the shape of the distribution such as the mean, variance, skewness, kurtosis? Could central and standardized moments be used to make this connection between descriptions of the distributions?

3. Pg 3749 ln 17: Does the prefactor in $M^{(3)}$ assume spherical particles? Can the same interpretations of the moments be made if the particles are not spherical, as is typical for volcanic pyroclasts? I recommend writing $\pi/6M_j^{(3)} = \alpha_{s,j}$ as $\alpha_{s,j}$ is used subsequently.

4. Pg 3752 equation (7): The quantities k_1, k_2, k_3 are not defined and their values are not given. Dimensional analysis shows that their dimensions differ – do these quantities have a physical meaning? Also, C_D should be defined and the used value given.

5. Pg 3753 equation (10): It appears that there has been an implicit definition the square bracket notation as

$$[g(D)]^{(i)} = \frac{1}{M_j^{(i)}} \int_0^\infty g(D) f_j(D) D^i \mathrm{d}D.$$

It would useful to make this definition explicit, as it is used subsequently. Also, as the denominator in equation (10) doesn't depend on D it can be taken out of the integral.

6. Pg 3754: the use of two variants of the phi symbol (ϕ and φ) (one used for the phi-scale size and the other for the particle mass fraction distribution) is not a very convenient notation.

7. Pg 3754: it should be explicitly stated that $\Pi_j^{(0)} = x_{s,j}$

8. Pg 3754 equation (13): there is another use of the square bracket notation, but this is inconsistent with the previous use (see comment 5). In particular, the kernel and variable of integration differ in these definitions. A new notation is needed.

9. Pg 3755 ln 5: ϕ is used to denote an angle, but it has previously been used to denote the particle size. x has also been used previously.

10. Section 3: There are numerous assumptions implicit in the equations presented. These assumptions must be made explicit. In particular:

> equation (15) assumes all the particles are carried at the same speed, equal to the speed of the gas phase, and that the particles are distributed uniformly in the plume;
>
> equation (18) assumes the density of a particle class does not change in time (and so distance s in the steady setting). This is concerning as it is likely that aggregation increases the effective particle size but might reduce (or increase for wet aggregation) the density;
>
> equation (19) the model assumes there is no phase change of the gas phase (i.e. no condensation). Can moisture be included?
>
> equation (24) assumes the material in the plume all has the same temperature, and therefore that heat transfer is the same for all particles. This might not be

true, as e.g. particle size, shape and composition can influence the cooling rate of particles.

equations (26) and (30) assume all the water vapour in the plume is derived only from the erupted material, so the atmosphere is dry, and that there is no phase change of water vapour.

11. Pg 3757 ln 4: The symbol α has already been used.

12. Pg 3757 ln 7: The entrainment formulation given in equation (20) was first proposed by Hewett et al 1971 (http://dx.doi.org/10.1016/0004-6981(71)90028-X). This reference should be used.

13. Pg 3737 ln 19: The text states that the equations for the conservation of momentum (equations 22 and 23) can be derived from the conservation of mass (equation 21). This is not correct – independent information is required (Newton's second law). This also applies to the similar discussion on pg 3761.

14. Pg 3758 ln 18: The bulk densities ρ^B_{atm} and ρ^B_{wv} are not defined. Need to state how ρ_g is determined from the bulk densities.

15. Pg 3759: The derivation of the rate of change of the mixture specific heat capacity is not clear. Why are partial derivatives used (what variables are held fixed)? Why is the second term on the right-hand-side written in this way?

16. Pg 3760 lns 7-10: Does the use of ρ_{mix} and C_{mix} at s to find T at $s + ds$ result in a first-order scheme? I suggest removing these lines as the direct integration of equations (28) and (30) are preferred?

17. Section 4.1: There is extensive use of jargon in the description of this algorithm that results in a lack of clarity. In particular:
(i) the term "realizable moment set" should be avoided or defined.
(ii) the term "invalid moment set" requires clarification.

(iii) what is "moment corruption" and how can it be diagnosed?
(iv) Hankel-Hadamard determinants require definition or reference.
(v) why do nodes within the support of the distribution and positive weights guarantee accuracy of the algorithm? An explanation and reference are required.
(vi) it not clear at which point in the predictor-corrector numerical integration the algorithm is used. Is it in both the predictor and corrector step?

18. Pg 3765 ln 1: "The initial moments are evaluated analytically. . ." This can only be true if a probability model is provided for the initial particle distribution and the model is sufficiently integrable to calculate the required moments.

19. Pg 3765 lns 4-8: This paragraph is not clear. I'd recommend re-writing, perhaps including the equation for $\varphi(\phi)$ given in equation (53) moved to this point.

20. Pg 3768: The justification for using the formulation based on the moments of mass fraction distribution rather than the number distribution (i.e. that the first three moments can be combined to give the mean and variance of a normal distribution) seems weak. Surely it is natural to use the moments of the particle mass fraction distribution to track changes in quantities derived from and describing the mass fraction distribution.

21. Section 5: The value of all parameters used in the model should be given. A table of these would be useful.

22. Section 5.3: It should be noted that the sensitivity analysis performed is only a partial sensitivity analysis. Only a small subset of the model parameters and inputs are varied. The sensitivity results are therefore conditional on the values taken for the fixed parameters. In particular, it is possible that different inferences might be made if some of the fixed parameters take different values.

23. gPCE: The generalized Polynomial Chaos Expansion is more similar to a spectral method rather than a finite element method, as the expansion is a high order polynomial posed on the whole domain of the input variable rather than a series of low-order

polynomials each posed on a collection of subset (elements) covering the input domain. The multidimensional integral referred to on pg 3773 should be given or references provided – as written it is not clear how to determine the expansion coefficients, and there are several methods for doing this. A reference is needed for Clenshaw-Curtis quadrature. It should also be noted that the requirement to evaluate multidimensional integrals might result in a high computational cost when there are a large number of inputs and when the integrand is not well behaved.

24. Pg 3774: When comparing the LHS and gPCE approaches, there is a choice of using 1000 and 2000 samples for the LHS. It would be useful to show small sample sizes for the LHS, to give a suitable lower bound on the number of samples. In particular, as the response surfaces found here are very smooth and with small variation, it is possible the smaller LHS would give good results here.

25. Section 5.4: (i) The details of the sensitivity analysis should be given, or suitable references provided (e.g. Saltelli's Sensitivity Analysis book, Sobol' and Saltelli's papers on this topic). (ii) The technical definitions in terms of variance of the conditional expectation are not intuitive to non-statisticians.

26. Conclusion: Although only 91 simulations are required in the gPCE method applied to the plume model, I suspect that if the response function where not as smooth and well-behaved as for this model, many more model evaluations would be required. Furthermore, if more parameters were varied, the computational cost would increase for gPCE (and for LHS, although the advantage of gPCE might be reduced). Therefore, I feel there is an over-emphasis on the effectiveness of gPCE. In addition, is the computational saving enough to require the additional technical computations necessary? – there is a cost in code development time for gPCE that is significantly greater than LHS.

27. Figure 1: The cross-section of the plume is circular in a plane normal to the centreline of the plume. This isn't clear on the diagram.

28. Figure 2: The axes in the plots don't seem appropriate. The top plot has a range for the Particle number distribution from 10^{-50} to 10^{+50}, which seems extremely large. Furthermore, the top plot suggests there are particles erupted with diameter in excess of 10^7 m! I suspect the labelling of the axis has gone wrong (missing minus sign in the indices?) and the particle diameter decreases to the right? The phi scale labels are also wrong.

29. Figure 6: A fair comparison of the two sampling strategies would be to have 81 points in LHS sample as well as the Clenshaw-Curtis grid.

30. Figure 9: The sensitivity indices displayed for the Top σ panels cannot be correct. The response surfaces in figure 7 and 8 show that the contours of Top σ are not absolutely parallel Bottom μ, so there must be some sensitivity to the Bottom μ.

Technical corrections

Pg 3746

Ln 11: The term "space of properties" is vague. Clarify.

ln 2: Change to "and some model attempt to describe the multiphase nature of volcanic flows." Certainly not all models do this, and the approach taken by those that attempt it may not be complete. Also, add references here and for the examples of plume (ln 5) and conduit models (ln 6).

ln 9: Reference needed

ln 14: It should be noted that many properties are insensitive to the behaviour of the dispersed phase. References needed.

Pg 3747

ln 18: "Recently…" but there is a reference to a 1964 work.

Pg 3748

ln 4 (and subsequently): change “for unit volume” to “per unit volume”.

ln 5: change “transport equations” to “conservation equations”.

ln 7: “implementation of the quadrature” – the quadrature hasn’t been defined yet. Rephrase.

ln 23: References needed.

Pg 3749

ln 5: The units for D shouldn’t matter in the derivation.

ln 20: should be $L_{j,(i+1)i} = M_j^{(i+1)}/M_j^{(i)}$

Pg 3750

ln 3: both length of particles and the number of particles are per unit volume.

ln 4: delete “density function”.

ln 14: “. . . [such] as settling velocity”.

ln 15: “. . . function[s] of . . .”

ln 16: “. . . [particle size] distribution . . .”

ln 17: “. . . [that is a] function of the diameter . . .”

Pg 3751

ln 8: add “where δ is the Dirac-delta function”.

Pg 3752

ln 8: Rephrase – I think all you need to say is that you can use this method with different expressions for the settling velocity, and I’m sure Barsotti’s method could adopt different models for the settlings velocity could be used.

Pg 3754

ln 18: Take $x_{s,j}$ out of the integral as it doesn’t depend on ϕ.

Pg 3755

ln 7: The cross section is circular in the plane that is normal to the centreline trajectory.

ln 9: Both angles are needed for a weak plume, and the angle θ is only needed if the wind direction changes.

ln 15: r and U_{sc} must be more carefully defined. Is this the top-hat radius and velocity, or has another radial dependence of the axial velocity been assumed?

ln 16: “. . . p is [the] probability . . .”

ln 17: change “(see below)” to “as”.

Pg 3756

ln 8: “. . .both the terms [by] the mass. . .”

ln 19: delete “want to”

Pg 3758

ln 6: change “segregation” to “fall-out”. The particle effect on the momentum flux is due to fall-out. As all material in the plume moves with the same speed, there isn’t segregation of particles within the plume.

Pg 3759

ln 2: change “derive” to “differentiate” and remove “after some cancellation and algebra manipulations”.

ln 9: I think “Eq. (38)” should be Eq. (24).

Pg 3760

Equation (31): The partial derivatives should be ordinary derivatives.

ln 21: “...of the equation [by] ϕ^i...”

Pg 3761

ln 13: change “writes as” to “is”

Pg 3762

ln 2: change “writes as” to “is”

ln 9: “... ordinary differential equation[s] ...”

equation (40): change partial derivatives to ordinary derivatives.

Pg 3763

ln 13: remove “named after Carl Friedrich Gauss” and replace with a suitable reference. Remove “a quadrature rule”.

Pg 3765

ln 11: “satisfy the condition” – what condition? (This also applies to the same phrase on Pg 3768 ln 6.) Presumably that the integral of f over all D is equal to 1, so that $f(D)$ is a proper probability density?

Pg 3766

ln 4: change “from” to “for”.

ln 7: remove “also”.

ln 8: “Krumbein scale has a lognormal distribution” – but above the Krumbein scale is assumed to be a normal distribution.

ln 13: change “We observe that” to “The”

ln 17: change “play a major role” to “is a convenient descriptor”. It ought to be possible to use another characteristic diameter.

Pg 3767

ln 1: rewrite as “... more appropriate to use the Sauter mean diameter than the mean diameter when the particle size has a lognormal distribution.”

ln 9: the abbreviation SD should be given in full at the first use. As the variance is calculated below, perhaps remove “and the SD”.

ln 14: change “observe” to “note”.

equation (52): why not do the inversion rather than give the linear system to be inverted?

Pg 3768

equation (54): the double factorial function isn’t widely known, so the definition here would be useful. Also, the binomial coefficient and the integer part symbol used in the sum should be defined.

ln 12: change “being” to “as” and insert “moment [is] equal…”.

Pg 3769

Change to lower case k in kg.

Pg 3771

lns 3-6: I don’t understand this sentence. I think you mean that the distribution changes from a normal distribution so that the mean and variance are not the parameters of a normal distribution, but this is clear from the previous sentence. I would suggest removing this sentence.

ln 6: remove “we remark that these changes”

ln7: change “can be assumed to be” to “are”

ln 15: change “not always” to “often not” and remove “or are constrained with significant uncertainty”.

ln 17: change “The best” to “An alternative”. The “best” alternative would be to constrain all inputs very precisely, although this isn’t possible.

Pg 3772

ln 1: change “uncertainty in” to “sensitivity of”

ln 3: change “base” to “vent”

ln 27: the history of PCE is not really needed, and it would be better to replace with references to the method and applications.

Pg 3773

equation (55): as given this expansion has only 3 terms. I believe the final term should have subscripts k rather than 3.

ln 21: why 9, 36 and 91 simulations (should it be $9 \times 9 = 81$ rather than 91)?

Pg 3774

ln 3: “…results highlight that gPCE [here] represents a valid alternative…”

ln 29: should this be smaller mean sizes and larger dispersion? I recommend changing “dispersion” to “variance”.

Pg 3775

lns 3âĂŤ5: the text starting at “and, as previously mentioned” is repetition and can be removed.

Pg 3776

ln 3: the subscript in x_{-1} should be $--i$ rather than -1.

ln 7: change “obtain” to “approximate”. As the gPCE is a surrogate of the plume model, the sensitivity analysis obtained from the expansion are approximations of the true sensitivity indices.

ln 17: add “. . .are controlled [primarily] by . . .” as there is some effect of the initial TGSD SD on the TGSD mean at the plume top.

Pg 3777

ln 4: Bursik’s model builds on earlier work, which should be referenced here.

ln 22: add “. . . variations of the order of tens of meters [for a plume rising to several km’s].”

Pg 3788

Figure 7: add”. . .response functions of the four [output] variables. . .”

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