Author's response to Referees concerning the revised manuscript "Simulations and parameterisation of shallow volcanic plumes of Piton de la Fournaise, La Réunion Island using Méso-NH version 4-9-3", by S.G. Sivia et al.

<div align="right">2 April 2015</div>

As contact author and on behalf of all co-authors, I (F. Gheusi) gratefully thank P. Bechtold and the other Anonymous Referee for their careful and constructive reviews on our GMDD paper. We have the feeling they helped us to significantly improve the revised manuscript submitted for consideration of publication in GMD.

Most of their comments are minor and have been addressed point by point in our revised manuscript as detailed below. Yet, three major points arose from their comments, which deserve more discussion:

1. **the question why we used modified entrainment/detrainment only at the model base layer of depth Δz (concern shared by both Referees)**

Anonymous Referee #2 wrote:
"if I understand well the modified EMDF model, the parameters of entrainment and detrainment are modified at the base layer (with a thickness Delta z). I did not really get why these parameters had to be modified only at the base of the layer and not over the whole height of the plume. Furthermore I wonder if there is a trend of between, for example, Delta z and the respective values of entrainment and detrainment required to find the same results as in the LES simulation."

Peter Bechtold wrote:
"clarify in text why you decided to entrain all at the first model level at 40 m and if you formulation is vertical resolution dependent"

2. **the sensitivity of modified-EDMF simulations to entrainment and detrainement (from Referee #2)**

Anonymous Referee #2 wrote:
"the results of the modified EDMF simulation are shown to be mainly sensitive to alpha, the amount of air entrained at the basal layer of the plume. Alpha itself is a function of the rates of entrainment and detrainment as illustrated figures 7 & 8. While informative, I found the result of the figures a bit too restricted, and, as alpha is not an explicit parameter of the model, I suggest the authors show two contours plots giving as a function of entrainment and detrainment (i) the altitude of detrainment and (ii) the concentration of SO2 at the level of detrainment. This will allow the reader to better estimate the sensibility of the model to the two input parameters."

3. **the formulation of surface fluxes in the LES to represent the volcanic source (from P. Bechtold)**

Peter Bechtold wrote:

"-page 8373 and 8374: The definition of the fluxes in (10) and (11) is incorrect. It should write (q_u-q) and (T_u-T) as there isn't any flux if there is no difference in scalar value"

Before all, it is important to note that the above point 3 led us to modify the formulation of the surface heat flux in the LES, and therefore to rerun this reference LES simulation. The obtained maximum injection height of the plume (1.0 km above the ground) is slightly lower than formerly (1.3 km). As consequence, a new adjustment had to be found for our experimental modified-EDMF single column model (thereafter "M.EDMF SCM") to fit the reference LES simulation at best. The new value found for the parameter alpha (fraction of entrained fresh air at the top of the first model layer) is 0.838, slightly above the former one (0.834). All the figures impacted by the new simulation results have been updated (Fig. 6, 8 and 9 in the revised manuscript).

Note also that Figure 6 from the GMDD paper has been split into two figures (2 and 7 in the revised paper). The reason is that the infrared imagery (revised Figure 2) is mentioned earlier in the revised text. For a similar reason, the tables have been also reordered.

Major concerns and manuscript changes

1. Modified entrainment/detrainment in the model ground layer

The question of fresh air entrainment at the base of highly buoyant plumes is actually an open question, which is relevant for all types of high-temperature surface sources inducing convection in the atmosphere, i.e volcanoes but also combustions, and in particular biomass fires (Rio et al., 2010). Volcanic or combustion gases are extremely buoyant and without entrainment of a large part of fresh air at the base of the buoyant updraft, this would accelerate dramatically, and by need of vertical mass conservation, its section would become much thinner than the area of the ground heat source at some height above the ground. This is clearly not what is observed in reality, neither in volcanic or fire plumes. Thus, the concept of a feeding layer with strong entrainment of fresh air could be introduced to account for actually observed plumes. The main questions are how deep is this feeding layer, and how to model the entrainment in this layer.

Rio et al. (2010) proposed the simple idea that the entrainment in the feeding layer is such that the updraft section remains constant (sic in their paper: *"we assume that the lateral entrainment of environmental air exactly compensates the narrowing of the plume coverage due to acceleration"*). They apply this constraint over the full depth of the atmospheric well-mixed boundary layer – but unfortunately, the authors give no reason for this choice.

On our side, we decided to be as simple as possible. We started from the simple observation that a few tens of metres above the ground, a large part of fresh air has been entrained into the plume – as proved e.g., by the infrared imagery in Fig.2 (revised manuscript), which shows a rapid temperature decrease. The simplest solution we found was to prescribe a desired fraction of entrained fresh air at the top of the first model layer (here 40 m). But this choice is purely arbitrary, and actually has no other justification than simplicity.

To compare our approach with Rio et al.'s one, we estimated the fraction alpha of entrained fresh air at 40 m above the ground, using Rio et al.'s assumption (constant updraft section

between the ground and 40 m). The result is 68%, which agrees qualitatively with the value of aplha for our best adjustment (83.8%), and supports the idea that a dominant fraction of fresh air is entrained into the updraft within a few tens of metres.

To address a question raised commonly by both referees – whether alpha is resolution dependent – we also performed a sensitivity M.EDMF SCM simulation with doubled Δz (= 80 m) but keeping alpha constant, such that the fraction of fresh air at the top of the first model layer (here 80 m) is the same as in the 40m-resolution simulation (83.8%). The result is shown in the revised Figure 9. Interestingly, both simulations provide SO_2 and water vapor profiles which are quite close to each other. Therefore, this result suggests that the required alpha value is not (or weakly) resolution-dependent, and that the plume height is primarily driven by its initial buoyancy near the surface (see also point 2 below).

Note that we also performed M.EDMF SCM simulations with modified entrainment / detrainment in the first two or three model layers (not shown in the paper) but without benefit in term of agreement with the LES reference.

Of course, the best would be to find a universal formulation of entrainment and detrainment, which would be valid at any levels of the updraft, and suitable for a wide range of eruption characteristics. We have the feeling that this question deserves much further investigation, but also that it is beyond the scope of the present paper which is mainly a first demonstration of our model capability.

Section 3.2 in the revised manuscript has been almost completely rewritten, and a specific discussion on this topic has been added.

(Added bibliography entry: Rio C., Hourdin F. and Chédin, A., Numerical simulation of tropospheric injection of biomass burning products by pyro-thermal plumes, Atmos. Chem. Phys., 10, 3463-3478, 2010.)

2. Sensitivity of modified-EDMF simulations to entrainment and detrainement

The Referee #2 asked for a new figure exploring more systematically the sensitivity of the plume dynamics to basal entrainment and detrainement. As required, we built a new Figure 8 (replacing the former Figure 7) showing (in the form of contour plots) the plume maximum injection height and the tracer concentration at this altitude, as function of both entrainment and detrainement. This figure mainly shows that:

    (i)   the vertical plume development is mostly independent of detrainement, but in contrary strongly sensitive to entrainment – this result is not unexpected, since the maximum altitude reached by the plume is in great part driven by its initial buoyancy, the latter being affected only by entrainment but not by detrainement;

    (ii)   the best adjustment also corresponds to the highest tracer concentration that can obtained at the maximum injection level from a given volcanic surface source.

With this new Figure 8, the former Figures 8b and 8c appeared no longer useful to us and therefore have been removed. Instead, the former Figure 8a and 9 have been merged into a single new Figure 9, which contains more or less the same information.

3. Surface flux formulation in the LES

We thank P. Bechtold for this comment. After having further thought to the question of the surface flux formulation for the LES (equations 10-12 in the discussion paper), it appeared that we agree with this Referee concerning the sensible heat flux, but not concerning the water vapour mass flux (and the SO_2 flux in the same way). As consequence for the revised paper, only Eq.11 (sensible heat flux) has been corrected and the LES recomputed with the new flux formulation. A detailed appendix has also been added where detailed demonstrations of Eq. 10 to 12 are given. Our rationale is summarized below.

The surface fluxes corresponding to those from the volcanic updraft at surface level in the M.EDMF SCM simulations, occur in the LES over one whole grid cell, and hence the surface to consider for budget calculations is that of this grid-cell, i.e. $S = \Delta x \cdot \Delta y$ (NB. The ad-hoc surface correction factor 1.2 mentionned in the paper is here omitted since it is not the subject of this discussion).

H_2O (or equivalently SO_2) mass flux: the question is to know what mass of water vapor (m_v) is added per unit time into the model lowest grid cell, such that $dm_v / dt = F_v S$. Clearly, this is the H_2O mass contained in the volcanic gas melange injected into the atmosphere. This does not depend on the water vapor content of ambient atmospheric air (and added H_2O would be still injected into the model even if there was no difference in scalar value between q_u and q). The mass of H_2O injected into the model between t and t+dt reads

$$dm_v = rho\_mix \cdot q\_u \cdot S \cdot w\_u \cdot dt,$$

and this yields in turn

$$F\_v = rho\_mix \cdot q\_u \cdot w\_u \text{ (eq. 10 in the discussion paper).}$$

The same rationale is also valid for SO_2, yielding equation (12).

Sensible heat flux: The rationale is more subtle. In general, the surface sensible heat flux is basically the energy quantity brought per unit time and surface which is efficient to cause a temperature change at constant pressure in the lowest atmosphere layer. Therefore, enthalpy should be considered. The enthalpy change dH of ambient air between t and t+dt in the lowest model grid cell is related to the sensible heat flux F_s, such that

$$dH = F\_s \cdot S \cdot dt.$$

In our specific case, we therefore want to know what enthalpy change is caused in the atmosphere by injection of a mass $dm\_u = rho\_mix \cdot S \cdot w\_u \cdot dt$ of volcanic gas.

The total enthalpy of this volcanic gas mass (assumed to be a perfect gas of specific heat capacity at constant pressure Cp_u) writes $dm\_u \cdot Cp\_u \cdot T\_u$. However, not all this enthalpy amount is available to heat the atmosphere. Indeed, when two bodies at different temperatures come in contact with each other, their respective final equilibrium temperatures match at an intermediate value (as consequence of the second law of thermodynamics). Assuming that dm_u is small with respect to the air mass contained in the lowest model grid cell (which is the case in our LES with small time step), the final temperature is close to the atmosphere

initial temperature T. Therefore, the enthalpy transferred from the hot volcanic gas mass to the atmosphere is $dH = dm\_u \cdot Cp\_u \cdot (T\_u - T)$.

This yields finally $F\_s = rho\_mix \cdot w\_u \cdot Cp\_u \cdot (T\_u - T)$.

Note however that sensitivity tests (not shown in the paper) were performed against volcanic gaz temperature T_u, with weak sensitivity found for the plume height in a range 1000-1400 K for T_u. Hence, we did not expect a great change in the simulated plume height, and the new LES simulation results confirm this expectation (revised Figures 6b and 9).

Minor changes

Minor comments from P. Bechtold

-page 8364, lines 14-15: saying '1 dimensional (single column) model with 1 km resolution' is odd. Clarify, you might use something like 'quasi-1D model using 3x3 columns'...

This has been clarified in the revised introduction.

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-clarify in text why you decided to entrain all at the first model level at 40 m and if your formulation is vertical resolution dependent, also if your mass you wish to entrain can be larger than Delta p of first model level.

The reason why entraining all in the first model level is mainly simplicity, and a sensitivity experiment suggests that the approach is not resolution dependent (see major point 1 above). This has been clarified in a discussion added into Section 3.2 in the revised manuscript.

It is not clear to us what the referee meant in the second part of his comment, but we may understand that his concern is whether the entrained air mass (we assume within a model time step = 1s here) can be larger than the total air mass in the grid cell. In our best-fitted M.EDMF SCM simulation, the updraft total mass flux at the ground (volcanic source) is 432 kg/s, while at the top of the first model level, it is 2901 kg/s. Hence, the entrained mass flux is the difference, namely 2469 kg/s. The total mass entrained within 1 s is therefore quite small compared to the total air mass in the grid cell, which is of the order of ~1 kg/m3 * 1e6 m2 * 40 m = 4e7 kg.

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-page 8364: change title '2 Materials' to something more sensible

The section title has been changed into 'Volcanic plume parameterisation and model configurations'.

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-page 8369: change notation for r_mix for the gas constant as one can easily think this is a mixing ratio

The notation has been changed to R_{mix} (upper-case R) throughout the revised paper. In the same time, the water vapor mass mixing ratio is now noted [H_2O].

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-page 8373 and 8374: The definition of the fluxes in (10) and (11) is incorrect. It should write (q_u-q) and (T_u-T) as there isn't any flux if there is no difference in scalar value

See major point 3 above.

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Typos:
-page 8362 line 25, add 'comma' after 'convection'
-page 8373, line 13;'atleast'

These typos have been corrected.

Minor comments from Referee #2

p8362 - l17: the eruptive mass flux is the main parameter controlling the height reached by the volcanic plume.

A mention to this has been added in the introduction first sentence.

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p8363 - l10: is it possible to show a map of the area affected by the pollution?

Unfortunately, we have no such map to show. However, details on the island areas affected by SO_2 pollution are given in Bhuwant et al. 2009 and Viane et al. 2009. This has been specified in the revised text.

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p8362 - l8363: the study of Kaminski et al., Journal of Geophysical Research, 2011, could be cited as an attempt of a study coupling volcanic plume dynamics (1D model) and atmospheric circulation models.

We thank the referee for this interesting suggestion. This idea and a citation to this reference have been added in the very first paragraph of the introduction.

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p8364 - l1: if I'm not mistaken, the study of Suzuki cited in the reference list shows how a model of entrainment in volcanic turbulent plumes depends on the resolution of the grid (the finer the resolution the more efficient the entrainment) and could be cited as a further argument for sub grid modeling.

We are sorry but we cannot find such discussion on the influence of resolution on the entrainment efficiency in any cited studies by Suzuki et al. Furthermore, we do not clearly understand how this result could be an argument for sub-grid modelling. We would appreciate if the Referee could clarify his point of view.

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p8364 - l13: the term "initialised" may not be understood correctly for readers from different backgrounds and should be explicitly defined.

The sentence has been rephrased in a more explicit manner, in order to avoid misinterpretation.

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p8366 - l14: I'm sorry I dot not understand the idea beyond the notion that " vertical motions dominate the vertical sub-grid transport".

The confusion is presumably because we erroneously used the word "vertical" twice in this sentence. This has been corrected simply into "vertical motions usually dominate the sub-grid transport".

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p8367 - equ. 2: perhaps use a different label than a in the equation (in order to avoid any confusion with the relative area of the plume).

The coefficient notations for a and b in Eq.2 have been changed to c_1 and c_2.

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p8369 - equ5: If I understand well there is no solid fraction in the plume. It could be useful to state that more clearly.

An explicit mention on the absence of solid fraction in the plume has been added at the end of Sect. 2.2.2.

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p8369 - l20: It might be more relevant to cite Woods 1988 here.

The citation has been changed.

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p8371 - l8: I think this sentence might rather be placed in the introduction.

The lines 9-17 have been moved and merged into the last two paragraphs of Section 1.

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p8372 - l7: It might be good to explain what "deep convection option" means for readers of different scientific background.

The sentence has been reformulated (Sect.2.3.1, last paragraph).

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p8374 - l10: I find this paragraph a bit confusing. Did you use or not a wind profile in the different simulations?

The paragraph has been reformulated to gain in clarity, and also moved to the end of Section 2.3.2 as the prescribed uniform wind profile is a common feature to both LES and SCM simulations.

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p8376 - l27: the sentence does not seem grammatically correct.

The sentence has been rephrased.

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Table 3: I think the third line of the table (H_2O by SO_2 ratio) is not very useful.

This line has been removed. Beyond this, the table has been updated with some changed notations and the corrected formula for the sensible heat flux. Note also that the tables have been reordered, this table being now Table 1.

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Figure 6: is it possible to show the temperature scale for the thermal image?

We are not able to trace back these specific images to their temperature scale, sorry. In the revised manuscript however, we instead provide a different but similar infrared image accompanied with a temperature color bar (Figure 2). This new image allows for drawing the same conclusion as the images formerly shown in Fig.6b and c.