Dear Mattia:

Thank you for your extensive comments to our paper. Some of your key points have prompted us to re-do all the calculations and re-formulate most of the figures. The details are below. My comments are in blue italics. Yours are in black.

Larry Mastin

The paper presents a study of the modelling of volcanic ash in the atmosphere, with a particular focus on the effect of ash aggregation on depositional pattern. Several eruptions are investigated in order to find the parameters controlling aggregation, which give best fits of the deposits. To this aim, the authors employed Ash3d, an Eulerian model that calculates tephra transport and deposition through a 3-D, time-changing wind field.

Despite the differences in the magnitude and styles of the eruptions studied, the parameters describing ash aggregates are found to be similar for all the events.

The phenomenon investigated is interesting and very relevant for the volcanic hazard associated with ash dispersal in the atmosphere and it presents important novelties for operational model forecast. For this reason, I think that the manuscript falls into the scope of Atmospheric Chemistry and Physics and it is scientifically sufficiently sound to be published, once some points detailed below are clarified, in particular concerning the way the grain size distribution has been discretized.

• Lines 174-179. While in most of the literature the Suzuki relation is described as the distribution of mass in the column, in the original paper it is defined as "probability density diffusion". This probability is related to the mass concentration of particles leaving the column at height z in the unit time, and it is different from the concentration of particles along the column.

Thank you for pointing this out. We will modify the text to indicate that we are using a modified version of the Suzuki equation, and that we are using this formula as a simple parameterization of mass distribution with height, with no attempt to relate it to physical process. The difference between a probability density function (which would not apply to our Eulerian model) and a function defining mass distribution in the column seems minor to me, unless I am misunderstanding something.

• Lines 189-193. In Wilson and Huang a, b and c are the principal axial lengths and not the semi-axes, and the values were measured for more than 155 particles. I am also not sure that the average value of the shape factor of 0.44 is reported in the Wilson and Huang paper.

Thank you. This was a typo, not an error in our calculations. We have corrected it in the text.

You're right that the average shape factor of 0.44 was not reported in Wilson and Huang. We used their data to calculate an average shape factor. We will reword the relevant sentence in section 3.1 to make this clearer.

• Section 3.2. It is not clear to me the choice of the bins for the discretization of the TPSD. Why bins of 0.5phi are used for the non-aggregated particles and bins of 0.1phi are used for the aggregated? If the settling velocity and the depositional process is sensitive to bins of 0.1phi for the aggregates, I think this should be true also for the non-aggregated particles.

Bins of 0.5 phi or coarser were used for the non-aggregated particles based on what was available in the published literature for these deposits. The finer, 0.1 phi bins were used for aggregates because, as shown in Figs. 5 and 8, where the aggregates land is highly sensitive to aggregate size, for the rather narrow range of sizes and densities that would put fine ash at medial distances. For non-aggregated grains, this high sensitivity is only true for particles ~50-100 microns, as illustrated in Fig. 5. Most particles of this size have already aggregated. We will add a paragraph to Section 3.4 pointing out these constraints.

It is also reported that aggregates are described by a Gaussian size distribution, but the amount of fine ash assigned to different size bins, reported in Table 4, is not representative of a Gaussian distribution. The values should be computed using the error function:

F(mu+x sigma)-F(mu-x sigma) = erf(x/sqrt(2))

You're totally right (gasp!); our distribution is not strictly Gaussian. And the values of sigma_agg were inaccurate for the distributions given. We are modifying the values in Table 4 and re-running all the simulations so that truly Gaussian distributions are represented. In the caption to Table 4 we will also describe exactly how these values are derived (i.e. using a Gaussian formula). This change required us to re-derive Figures 10-13, 15, 16, all the supplementary figures. It also changed the results slightly, requiring slight rewording in the Results section.

• Section 3.3. I think that the first and third indexes, defined in Table 3, should not have the square root (exponent ½).

Thank you. This has been changed, and the error indexes recalculated.

• Section 3.4. Aggregate size. Why is the range for sigma_agg so small? Is it supported by observations or experiments? This doubt is also due to the results, showing a small sensitivity of the results with such a small range.

The small range that we use is a consequence of the high sensitivity between aggregate size and distance traveled (Fig. 5). For each simulation, we wanted to use a size distribution such that the range of distances traveled between the smallest and largest aggregates was a few hundred kilometers, as illustrated in Fig. 8. This limited the range of aggregate sizes to tenths of a phi unit. Broadening the size range would have caused a large fraction of aggregates to deposit outside the range of distances we were studying. This point was made in Section 3.4.

However to accommodate this concern, we have slightly broadened our range of sizes by adding a size distribution to Table 4 that spans 0.8 phi units. The sigma_agg value is still small (0.3 phi), but larger than previously. When calculated properly using a Gaussian best-fit, our old maximum sigma_agg value was 0.12.

With this new analysis, we can show that none of the optimal fits in Fig. 9 occur at the maximum value of sigma_agg, suggesting that the range is now large enough to include the optimal value. Also, in the supplementary figures we will show that, when sigma_agg=0.3, the secondary thickness maximum is broader and less thick than observed, for example, at Mount St. Helens (Fig. S028) and Ruapehu (Fig. S128). At Spurr the secondary maximum is too poorly mapped to make this comparison, and at Redoubt no secondary maximum was mapped.

If we had compared the model result with more proximal sample locations, it is likely we would have obtained a wider optimal range of aggregate sizes. We chose not to include more proximal locations because the indexes we used, particularly delta^2, can be overwhelmed by proximal sample points, since their importance is directly proportional to the absolute value of the difference in mass load between the model and the measured deposit. Proximal deposition also involves processes such as hail-forming aggregation or fallout from the vertical column, that are not accurately simulated in a widespread fallout model like Ash3d. Finally, if we had included these proximal sample locations, the optimal aggregate-size distribution would probably not have produced a secondary thickness maximum. This is a key feature of three of these deposits. Not reproducing it would have yielded an unacceptable result in our opinion.

We are substantially revising Section 3.1 and now emphasize some of these points in that section.

• Section 4.1. It is not clear why some points are excluded from the analysis in Figure 10b and 10c. In the caption it is written that for panel (b) "grey dots lay outside the range of downwind distances covered by trend lines in Fig. 6", and are excluded from the calculation of Delta^2. I don't understand why the trend lines are involved in the point-by-point index, and also why Figure 6 should be used.

Values of Delta^2 can be dominated by differences in proximal locations, where mass per unit area is greatest, and where processes such as fallout from the vertical column are not accurately simulated. Therefore we exclude these proximal points from the calculation. At the beginning of Section 3.4 we noted that we ignore proximal fallout, but perhaps didn't do an adequate job explain why. We will modify the explanation of the point-by-point method in Section 3.3 to add this explanation.

Also for panel (c) the caption is not clear, referring to Delta^2_area, while the figure is reporting a value for Delta^2_downwind.

Thanks for pointing out this typographical error. It's now corrected.

In any case, I think that the criteria to exclude points from the measures of the fit should be discussed more in the main text.

I think the above-mentioned changes to Section 3.3 address this.

• Lines 322-325. It is stated that adding turbulent diffusion "visually improve the fit". For this reason, I think it would be useful to quantify how much the fit is improved, through the different statistical measures of fit presented in the paper.

Thanks for the suggestion. We tried this, and found that delta^2 actually shows a worse fit for the MSH case when diffusion is turned on! Apparently, the improved fit on the margins of the deposit is more than offset but poorer fit along the dispersal axis. We will note that in the new version.

It is also interesting to note that the numerical results seems to show a diffusion in the results, and this is probably due to a numerical diffusion associated with the Eulerian approach. Is it possible to quantify or discuss the effects such diffusion, in relation with the grid-size?

I'm not sure. At the moment, I can't think of how this would be done.

• The choice to neglect diffusion in the model is justified by the decrease in run time from 30 to 10 minutes for operational conditions. It would be interesting to compare this time with the characteristic timing of the depositional process.

This might be beyond the scope of the paper, but an interesting problem.