Responses to Folch review

Dear Arnau:

Thank you for your thoughtful comments to this paper. In it, I am indicating how we are changing the manuscript in response to your critique. The revised manuscript will be posted after the Discussion period has ended.

Reviewer comments in black. *Reponses in blue italics*

- The parameterization scheme (section 3.2, lines 206---201) seems somehow arbitrary and could be better justified. On the other hand, if the TPSD is discretized on 1Φ intervals all the fines aggregate except for Φ=3, for which 50% of particles aggregate. This seems rather simplistic. To what extend the results depend on this choice? What if discretization is performed at 0.5Φ intervals and/or the limits are extended beyond Φ=4 (e.g. Φ=5 or 6)? Could the best---fit values (i.e. the conclusions of the paper) depend substantially on this?

  We are rewriting Section 3.2 to explain more clearly why we chose our parameterization scheme. It was based on experimental field observations of grain sizes that aggregate under different circumstances. Some explanation was already in an appendix, which has been deleted and its material moved into the main text. Figure A1 was also moved into the main paper and is now Figure 2.

  If we had chosen different particle-size thresholds for aggregation, the main effect would be to alter the mass that contributes to the secondary thickness maximum by several percent to tens of percent. For Mount St. Helens, about 10% of the erupted mass lies between phi=2 and phi=4. For Spurr, Ruapehu, and Redoubt, the percentages are 28%, 6% and 11%. These values reflect the variability in mass of the secondary maximum that could result from different choices of the aggregation-size threshold. In Section 3.2 we are adding a few sentences pointing this out.

- The authors find "optimal" values of aggregate density of about ρ_{aggr}=600 kg/m3 consistent with (but denser than) previous studies and observations. It is important to mention that this is also a consequence of the settling velocity model chosen. Note that, for fine particles, the Wilson and Huang model gives smaller settling velocities than other fits (see e.g. Figure 1 in Folch 2012; Journal of Volcanology and Geothermal Research 235---236, 96–115). In other words, other velocity model using a smaller aggregate density would give exactly the same fit…

  Actually, rho_agg was chosen, rather than being obtained by optimization as we did with mu_agg and sigma_agg. We chose 600 kg/m3 because it was toward the middle of the very large range of densities observed for aggregates. We are adding a paragraph to the end of Section 3.2 explaining this. We will emphasize that this choice may lead to an over- or underestimate of aggregate sizes. Our objective in this study is not to constrain the size of real aggregates, but to find a combination of parameters that can successfully replicate observed deposits. We will make this point more clearly in the first paragraph of the Discussion section.

  Thank you also for pointing our the dependence of our results on the chosen fall velocity. We will add a brief statement in Section 3.4 noting this.

- Figure 2 is misleading because (at the beginning of the paper) gives the impression that only one aggregated bin is considered, contradicting the text. It would be much clearer if the distribution of aggregates is shown as an inset.

  Yes, you're right. We will modify it to show real histogram insets.

- Line 241---242. Values of settling velocity for a given particle strongly vary with height. Are these values at sea level or averaged?

  As stated in the text and in the caption to Figure 5 (now 6), the fall velocities are averages. Specifically, they were averages of calculations made at 1-km intervals in the atmosphere, from 0 to 15 km. We will add this to the figure caption.

- It is unclear to me how the modeled "dispersal axis" is obtained and why topography causes the oscillations observed in Figures 10---13.

  The dispersal axes in Figures 10-13 are determined by finding the ground cell in each row (for Figs. 10, 11) or column (Figs. 12-13), with the highest mass load. The algorithm that finds this cell reads from an ASCII output file that give mass load at each cell center, in kg/m2, to three decimal places. At distal locations, the maximum load along a row or column may not be much greater than the precision of the output, causing a jagged appearance when spurious cells are picked. We will add an explanation to the caption of the new Fig. 11 that now explains the calculation.

- Line 320. Typo (And)

  Corrected—thanks.

- Figure 1 and lines 322---327. Th fact that diffusion can be ignore and still obtaining a reasonable fit is because the (Eulerian) model adds numerical diffusion. It is difficult to extract conclusions from here since this strongly depends on the numerical scheme, different from model to model.

  Good point. At the end of Section 4.1 we will add a sentence noting that these results may be different in other models or model configurations.

- Line 428. "hundreds to thousands"? sure?

  Changed to "many". (I was actually referring to the number of collisions per cubic meter per second, not per particle per second. But it wasn't clear).

- Lines 424 to 444 in the discussion are rather speculative but interesting. I understand that the proposed "empirical" aggregation scheme would hold to model the finer aggregates (i.e. formed during transport), not for the larger aggregates (mm size) formed in the plume. That would explain why so different eruption conditions end up with similar mean and dispersal. Right?

  If I understand your point, you seem to be suggesting that perhaps we're able to match these four deposits with similar aggregate sizes in part because we're excluding other, near-source processes, that could produce a more complicated and disparate outcome? If that's your point, I would agree. It doesn't appear that you are suggesting that anything needs to be changed in this passage; so no changes have been made.