Review of Mastin et al., submitted to ACP, April 2016

The manuscript aims to investigate the role and effect of particle aggregation in explosive eruptions. This is done by using a numerical model of ash dispersal and by adopting a simple parametrization of the aggregation process. Optimal parameters of such model are then derived by optimizing the comparion between model predictions and deposit evidence. The underlying hypothesis is that the effect of aggregation may be accounted for by a simple modification of the original grain-size distribution at source.

Based on the results and analysis presented the above hypothesis appears quite well justified. This is actually quite surprising given the wide range of eruptive conditions considered and the complexity of the aggregation process. On this basis, the study appears able to provide a first-order approximation of the effect of particle aggregation by simply modifying the grain-size distribution at source. This is quite relevant for improving the accuracy of operational ash disersal models.

I found the study very interesting, well-presented and certainly worth of publication after minor revision. The organization of the manuscript, as well as the figures and tables, are clear and informative. I suggest to further investigate just a few points listed below in order to make the outcomes of the study and its presentation even more robust and effective. A few minor technical points are also listed.

Main points:

- Section 3.1, lines 189-193: the Authors assume a constant particle shape factor for all particles and eruptions considered (except for the aggregates). This is probably a quite important assumption that should be acknowledged and commented given the main sensitivity of the dispersal process to such a parameter (see e.g. Scollo et al., JGR 2008; Bagheri et al., Pow. Tech. 2015; Pardini et al., JGR 2016). This is also quite evident from Fig. 6 where the shape factor of the aggregates has been varied. A similar assumption has been made for the density of the aggregates which, as explained in the text, also varies largely (lines 244-246). A brief discussion of the implications of these assumptions could be appropriate.
- Section 3.4, lines 258-261: the justification of the range of particle aggregate size and distribution (standard deviation) does not appear sufficiently clear as reported in the text. Why the assumption that most deposits fall in the region of interest is able to constrain the size of the aggregate? Is this valid/assumed just for the MSH case (Fig. 8) or for all the four eruptions? Also the extension of the mapped area is not clear. This key point should be better explain to me for both the mean and the standard deviation values. In particular the range of the standard deviation appears very narrow (i.e. 0.1-0.3) given the uncertainties involved and the results obtained, which, in some cases, indicate optimal values close to, or larger than, 0.3 (see Fig. 9).
- Section 4, lines 264-266 and Tab. 4. The way the aggregates are assigned to the various bins is not clear. In particular the distributions shown in

Tab. 4 are not Gaussian as expected. This should be corrected. It would be also interesting to see the effect of a different discretization of the Phi units of the aggreagates so to estimate the effects on the optimal parameters (units of 0.2 or 0.5 Phi instead of 0.1).

- Section 4, lines 291-297: in the description of the consistency with other studies the Authors could also mention the studies of Biass et al. (NHESS 2014) and Barsotti et al. (BV 2015) on Icelandic volcanoes and Vesuvius, respectively, that show similar optimal parameters of the aggregation process.
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Minor technical points:

- Line 374: D should be 3x10^2.
- Line 850: Fig. 6 should be replaced by Fig. 3?
- Line 853: Fig. 7a should be replaced by Fig. 3a?

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