Final author comment on "Big grains go far: reconciling tephrochronology with atmospheric measurements of volcanic ash" by J.A. Stevenson et al.

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1 Anonymous Referee #1

In a few places I think it would be beneficial to have bullet-point summaries of the main points, to better signpost the main messages.

RESPONSE: We have revised the abstract and conclusions, as well as making minor updates throughout the text, to better highlight our main findings.

I would also appreciate some more discussion about the in situ data and its associated uncertainties – from my reading it appears that the authors believe that the remote sensing data is what is at fault here?

RESPONSE: This has been added to the tephra sections in the introduction and in the methods section 2.1.

The authors should be more clear in the definition and more explicit and careful in their usage of the term. What size counts as 'cryptotephra'? Additionally, I tried to search for definitions online and am unsure why the term 'cryptotephra' is used and not simply 'tephra'?

RESPONSE: We chose 'cryptotephra' as it is an existing term that is used by the tephrochronology community. We also felt that it was more descriptive than 'very fine ash', which has been defined differently by a number of different workers. Cryptotephra refers to tephra from deposits that are too thin to recognise with the naked eye. For this reason, we refer to airborne particles as 'cryptotephra-sized'. I we have clarified the definition of 'tephra' and 'cryptotephra' in the introductory sections 1 and 1.1.

If 'basaltic' and 'rhyolitc' are the two main classes of volcanic eruption, it would be good to add an introductory sentence mentioning this (e.g. 'There are two classes of eruptions: basaltic and rhyolitic.'), if it is relevant.

RESPONSE: Added some sentences describing the difference between basalt and rhyolite magmas and tephra and how this affects the fall velocity of ash grains of each type.

This is the first use of the term 'BTD' and it should therefore be defined.

RESPONSE: BTD replaced by 'size most easily recognised by satellite infrared remote sensing' here. The definition is given within the satellites section.

> *I infer here that 'proximal' refers to locations < 500 km (based on 'distal' being > 500 km) but again it would be good to mention this explicitly here.*

RESPONSE: There are no formal definitions of proximal and distal in the literature, as it often depends on context. Here we use 'distal' to mean >500 km and 'proximal' to mean <200 km. The manuscript has been altered to explicitly define this.

> *How do we know that it's not a case that the large particles fell out while the smaller ones remain aloft? Or smaller grains that fell were either aggregated into larger particles, or just not detected/sampled (for whatever reason) in the ash measured on the ground? Surely it could be that there is not in reality a discrepancy, just the two techniques are measuring different things? How do we know there is a problem that needs to be 'reconciled' in this way?*

RESPONSE: We have added description on cryptotephra and the sampling technique to sections 1.1 and 2.1. The tephra measurements are restricted to particles >10-15 microns, but the satellite data are most sensitive to particles that are much smaller. Here we are emphasising that airborne clouds contain both sizes.

> *I would suggest inserting a new subsection after the current 1.2 (satellite data) to give an overview of techniques and uncertainties for estimating ash particle sizes from these deposits.*

RESPONSE: Section 1.1 and the Methods of Section 1.2 have been improved to better-describe the methods and the uncertainties so a whole new section is not necessary.

> *One option would be to use irregular particle optical properties when simulating the radiance (rather than Mie theory). Then do retrievals on this simulated data using both irregular particles and spheres, and see how the detected area and retrievals change. This is the logical next step to try to solve the problem rather than just illustrating it. Otherwise the simulations seem set up to fail, i.e. if a BTD is observed then Mie theory says it must be from a particle size distribution which is not very large.*

RESPONSE: We agree that this is the next logical step. However it is also a significant piece of work. As the prime aim of this paper *is* to illustrate the problem and to bring it to the attention of the atmospheric science community, this should be saved for later. The methods and assumptions used here are those currently used within the literature. We have changed the title from 'reconciling' to 'understanding' to remove the suggestion that we have solved the problem.

> *The second point that comes up in the discussion is that, because the BTD tends to zero as particle size increases, additional channels are needed for ash detection in some cases (the example given on P83L21 is 8.7 μm). However the authors do not seem to directly assess this. My suggestion is to add this wavelength to the simulation and see how much that changes the results. Or perhaps this is what is meant by 'additional tests' in the caption of Figure 10 – this should be made clear.*

RESPONSE: The 8.7 μm channel additional test is described in Section 4.1 and in detail in Francis et al. It is used to detect extra ash-containing pixels that were missed by the BTD test but is not used in the inversion. The wording in sections 4.1 and 4.2 has been improved to clarify this.

In situations where there is limited sensitivity to a parameter (for this case, as the authors note, the largest grain sizes), this type of retrieval scheme will be sensitive to first guesses and a priori information. So this will influence the statistics overall, and in this sense the analysis presented is quite algorithm-specific (since the retrieval solution is sensitive not only to the underlying physics but also the minimisation method). Changing the first guess at the solution or a priori value/constraint strength may change the results. ... This seems like it would be something fairly straightforward to test. The Optimal Estimation framework the authors are using provides additional statistics (averaging kernels and state vector uncertainty estimates) which the authors could analyse to see whether in fact the 'true' value is bounded by the retrieval uncertainty estimates or not.

RESPONSE: This was also raised in comments by Smith et al. We have added a plot showing the effect of changing the *a priori* effective radius on the retrieved grainsize, and incorporated extra discussion of this. We also calculated the averaging kernels for the retrievals. Together, these illustrate the sensitivity of the retrieval to the *a priori* estimates and show how it varies with particle size and mass loading.

Conclusion: This is a place where I think it would be clearer to present the take-home messages as bullet points.

RESPONSE: Although they are not presented as bullet points, we have numbered our three main conclusions.

2 Anonymous Referee #4

In addition to the BTD-based retrieval technique, are there other techniques for satellite-based characterization of volcanic ash plumes?

RESPONSE: A summary paragraph outlining other methods has been added to the start of the satellites section. Also, the title of the manuscript has been changed to specify that we are using the satellite infrared method.

It would be helpful to include a note on the effects of applying Mie theory to non-spherical particles. Would the findings likely change substantially if ash grain radiative properties were calculated by a method that considers the non-spherical shape of particles?

RESPONSE: We agree that this is an important avenue for future work. This was also picked out by Reviewer 1. See our response there.

I wonder if this sentence suggests that the lognormal approximation of the size distribution may not be accurate. In either case, it would help to clarify both this sentence and the last sentence of the paragraph.

RESPONSE: This is a note directed at tephrochronologists, suggesting that they use lognormal statistics to describe the grainsize distributions that they measure. It has now been clarified.

It would help to clarify what is meant by "preferable". Does the definition of the cost function imply that smaller particle sizes tend to have lower cost function values? If so, could a different cost function or incorporating additional observations (for example at much shorter or longer wavelengths) help?

RESPONSE: The cost function was also brought up in Interactive Comments from A. J. Smith. See discussion in the response there.

> *Page 69, Line 4: At this point readers are not yet familiar with the expression "BTD-active", and so deleting or clarifying this would help.*

RESPONSE: This has been changed, and BTD-active defined later in the document.

> *It would help to mention why the rhyolite grains fall slower (and thus travel farther) than basaltic ones do.*

RESPONSE: Explanation added. See also response to Reviewer 1.

> *Page 81. Lines 4-6: While the simulations assume no water or ice clouds, Lines 17-19 in Page 86 say that in reality there were some clouds around the ash plume. Thus it might help to point out here whether and how including water and ice clouds into the simulations may change the results.*

RESPONSE: A few sentences were added to the discussion, quoting Kylling et al (2013), who looked into the effect of clouds in simulated images.

> *Page 83, Line 6: The acronym VAAC should be defined.*

RESPONSE: Definition added at first use of VAAC.

> *Page 83, Line 15: The word "reducing" should be replaced by an adjective, for example "reduced" or "weak".*

RESPONSE: Changed to reduced.

> *Page 90, Line 12: It would help to mention why the geometric mean may be more appropriate for the comparisons than the arithmetic mean. If the paper used geometric as opposed to arithmetic mean values throughout, the manuscript could indicate this just as it points out that the used standard deviations are geometric.*

RESPONSE: See earlier discussion. This comment was primarily aimed at tephrochronologists, who use arithmetic mean when reporting PSDs of deposits.

3 A. J. A. Smith

> *Our main issue is that this work does not discuss generic remote sensing, but rather the BTD method which is a far more specific technique.*

RESPONSE: We have changed the title of the article to reflect the focus on satellite infrared methods and added a paragraph detailing other remote sensing methods for measuring atmospheric volcanic ash.

> *Additionally, there is a lack of precision in the discussions of BTD retrievals. Many of the comments on systematic bias and insensitivity to larger particle sizes would be more convincing if framed in terms of the information content, degrees of freedom, and the averaging kernels used. In Fig. 9, it would be informative to discuss or show how many degrees of freedom are available for each pixel, and where in the averaging kernel the information is partitioned. For example, what happens if the altitude of the ash cloud is held constant so that only the column mass loading and radius are allowed to vary?*

RESPONSE: We have calculated the averaging kernels and presented the degrees of freedom of signal and effective radius element as plots in the Supplementary material. These give much improved quantification of how the information is partitioned and show how the importance of the *a priori* value increases as the particle size increases and as the mass loading decreases. An extra paragraph has been added to the main document that summarises these results.

> *We would strongly disagree with the statement that the cost of a retrieval "can be used as a measure of uncertainty" [P82L11–15; P82L23; P118 figure caption]. The cost is a measure of how well a measurement is fitted by a specific solution i.e. how well satisfied with a retrieval's convergence one is, whereas the uncertainty is a measure of the error-bar on a retrieved parameter i.e. the reasonable range within which we believe our solution lies.*

RESPONSE: The text was poorly worded. We agree with this comment and have changed the text accordingly.

> *While non-sphericity certainly has an effect on the light scattering of volcanic ash, particularly the larger particles, this is not in itself going to change the fundamen- tal issue of the very similar light scattering properties of large particles observed between a very narrow range of frequencies. Additionally, since this is not ad- dressed at any point in the text, with the exception of in references to other work, we don't consider it a conclusion of this paper.*

RESPONSE: We have revised the conclusions section to better separate the discussion of large irregular particles from our own results. We have also slightly re-framed the discussion in terms of the upper limit on retrieved r_{eff} that results from the dense spheres assumption and the possibility that taking BTD-active large particles into account could increase this.

> *It would be instructive to compare the various size metrics used in the paper. Perhaps with some conversion examples. e.g. For spheres, an effective radius of 15 μm with a lognormal spread of 2 is equivalent to a volume average radius of 19 μm.*

RESPONSE: This information is already given in Figure 1, where the legend describes the parameters of different distributions. A note is added to highlight this.

> *Though Devenish et al. [2012] is an excellent comparison of lidar observations to the NAME model, we do not feel it suits the overall argument of this paper to simply cite its estimate of the fraction of ash remaining in the distal plume.*

RESPONSE: We have removed this estimate of ash fraction in the distal plume.

> *Figure captions are extremely long, and often contain scientific content that should be included in the main text.*

RESPONSE: Some text has been removed from most figure captions, particularly Figure 7. The length of some remaining captions is necessary to explain the different features of the plots.