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Interactive comment on "Physical and mechanical rock properties of a heterogeneous volcano; the case of Mount Unzen, Japan" *by* Jackie E. Kendrick et al.

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This is very nice work, and a very interesting paper. The authors have collected a huge rock mechanics and rock physics dataset on volcanic rocks collected from Mt Unzen (Japan); the site of extensive activity in the early 1990's. It is always pleasant to see new datasets presented and published, as these data are hard to collect and will benefit the community for many years. I have only a few general comments, and some minor points for the authors to consider.

General comments:

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1. How were the blocks selected in the field? Was this opportunistic, or were the sites selected via some form of criteria? This could, of course, be as simple as to cover a range of rock physical properties in 'accessible' locations, but it'd be nice to have that directly stated.

2. Somewhat covered by the earlier review: Permeability is easily one of the most tricky parameters to measure and discuss, particularly in the field, and in terms of spatial variation. Many years ago a NERC scheme (micro to macro) identified that permeability needed a measurement every few metres to see such variations, compared to 100's of metres and km to resolve parameters like elastic wave velocity and conductivity. That's just one example, but perhaps this type of 'challenge' is worth reinforcing when introducing and discussing the general nature of heterogeneity inherent in volcanic deposits of all kinds.

Minor queries:

3. Line 137: Typo, "ultransonic" should be 'ultrasonic'.

4. Line 141: For a recent report on damage and Vp changes in volcanic rocks see, for example: - Harnett, C.E., P.M. Benson, P. Rowley, and M. Fazio (2018), Fracture and damage localization in volcanic edifice rocks from El Hierro, Stromboli and Tenerife, Scientific Reports, 8, 1942, doi: 10.1038/s41598-018-20442-w.

5. Line 300: A pore pressure differential of 1.1 to 1.5 is actually fairly high considering the confining pressures of 5.5-13.5 MPa. Leading to what is, in effect, an 'effective pressure differential' across the length of the sample (rule of thumb being dP of around 10% of Pc, so 1.3MPa for the 13.5MPa experiment). Might the author comment or add a few words here? I suspect this protocol was adopted simply due to the low permeabilities of the rock types investigated, but it'd be good to have this confirmed by the authors.

6. Line 375: I wouldn't call this sub-section heading "Acoustic emissions - active":

surely you mean simply “elastic wave velocity”? Or perhaps “active surveys”? Rather a minor quibble, but I do think the use of AE is implied as passive only and this is well established in the literature.

7. Lines 695 (figure 8): What is the error on the velocity changes? Apologies if it is in the text, and I missed it when reading up to this point.

8. Lines 935-945, and a few other places: Do the authors note any differences in the character of the AE with regards to the dry and saturated experiments? This is a well known phenomenon in volcanic systems with the inherent fluid-rock coupling, for example: - Fazio, M., P.M. Benson and S.V. Vinciguerra (2017), On the generation mechanisms of fluid-driven seismic signals related to volcano-tectonics, Geophysical Research Letters, 44, 734-742, doi:10.1002/2016GL070919. - Fazio, M., Salvatore Alparone, Philip M. Benson, Andrea Cannata, Sergio Vinciguerra (2019), Genesis and mechanisms controlling Tornillo seismo-volcanic events in volcanic areas. Scientific Reports, 9, 7338, doi: 10.1038/s41598-019-43842-y I leave it to the authors as to whether they think it is worth including, or out of the scope of their study.

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