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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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The submitted manuscript presents an impressive collection of petrophysical data for four blocks collected from Mt Unzen volcano in Japan. The manuscript is well organised, well written, and the vast (in terms of type and quantity) dataset presented will surely pique the interest of the volcano community. The theme of my main comments on the manuscript, see below, is that I think that the authors should add some caveats to some of their experimental data. I recommend publication after the following main comments and line-by-line comments (minor revisions) have been addressed to the satisfaction of the editor.

Mike Heap (University of Strasbourg)

Main comments

1. The volume of rock investigated by a hand-held permeameter depends strongly on the radius of the nozzle: the larger the radius of the nozzle, the larger the volume of rock investigated. Theory highlights that the zone of investigation is approximately four times the inner radius of the nozzle (see, for example, Goggin et al. (1986) "A Theoretical and Experimental Analysis of Minipermeameter Response Including Gas Slippage and High Velocity Flow Effects"). Given that the nozzle of the hand-held permeameter used in the submitted study has a radius of 4 mm, the minimum radius of a sample that can be measured is 16 mm. However, although some of the samples have a radius of 20 mm (the Brazil-disc samples), the majority of the prepared cores have a radius of 10 mm. Therefore, the 20 mm-diameter cores are too small to be measured by the hand-held permeameter. Further, if measurements were taken at multiple different positions on the flat end-face of these samples, the nozzle was likely positioned, at least for some of these measurements, very close to the edge of the sample. I'm not necessarily suggesting that the authors remove these data from the manuscript. However, if they are to remain in the manuscript, I strongly suggest that the authors clearly state that the hand-held permeameter measurements on the 20 mm-diameter samples do not conform with certain published theoretical guidelines and therefore very likely overestimate the permeability. Indeed, the cited Filomena et al. (2014) paper shows that measurements (nozzle diameter = 9 mm) made on 1-inch diameter cores are between 34 and 36% higher than those measured on the parent block. I therefore also strongly suggest that the authors use "permeability estimate" (this tool is designed for quick field assessments of permeability) rather than "permeability measurement" when referring to these data.

2. Related to the previous point, estimations of permeability using the hand-held permeameter on the laboratory samples will not be sensitive to permeability anisotropy. This explains why no permeability anisotropy was observed when using the hand-

held permeameter, but was observed when the permeability was measured in a pressure vessel using conventional techniques. If the authors choose to keep their hand-held permeameter data, they should clearly state that assessments of permeability anisotropy on laboratory samples is not possible using the hand-held permeameter and it is for this reason why no permeability anisotropy is observed (Lines 477-482). Therefore, the discussion on Lines 732-736 and 747-763 should also be reduced.

3. Due to the high sample-to-sample variability of the blocks collected, robust assessments of water-weakening are not possible using only one wet uniaxial compressive strength or one wet indirect tensile strength measurement. The addition of wet UCS and tensile strength measurements is certainly a plus for the manuscript, but the authors should clearly inform the reader (in the results, discussion, and conclusions sections) that, due to sample-to-sample variability, robust assessments of water-weakening are not strictly possible with these data, but that pilot wet experiments are suggestive of water-weakening. Some of the discussion, which tries to explain why water-weakening is observed in compression but not in tension (starting on Line 790), should also probably be reduced. Unless the authors are willing/able to provide more wet experiments?

4. On Line 858 (and elsewhere) the authors state that “failure forecasting may be more effective in tensile regimes”. Can the authors elaborate on what they mean here? Which parts of, or when is, a volcano typically in a tensile or a compressive regime? Is it possible to tell? Or, it is possible to infer the stress conditions from the recorded volcano seismicity?

Line-by-line comments

Line 83: Not only the porosity, but the connectivity of the porosity.

Line 136: I think this should be “. . .the strain of the material in response to loading. . .”

Line 136: I wouldn’t say that Young’s modulus correlates “poorly” with porosity. The

scatter is quite similar to that for UCS as a function of porosity (“Volcanic rock strength inversely correlates with porosity. . .”; Line 103).

Line 223: The authors previously state that there is “substantial hydrothermal alteration in localised areas of the dome” (Line 190). No hydrothermally altered rocks were collected during the field campaign? If hydrothermally altered rocks are not considered representative of the dome, perhaps the authors should provide some information as to the extent of the alteration? Is it restricted to only a small volume of the dome?

Line 331: How long did a typical experiment last?

Line 357: “The”.

Line 522: “uniaxial”.

Line 559: Is it useful, here or elsewhere, to compare this empirical relation with that presented in Heap et al. (2020; already cited) for a range of volcanic rocks?

Line 768: Missing space.

Line 796: For water-weakening in rocks, see Baud et al. (2000) “Failure mode and weakening effect of water on sandstone”.

Line 838: Not only strength, Heap et al. (2020; already cited) show that Young’s modulus is also considerably reduced when one takes large-scale fractures into account.

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