# Review of Coats et al.

Dear Editor and authors,

I have read the manuscript *Failure criteria for porous dome rocks and lavas: a study of Mt. Unzen, Japan* with great interest. By way of a targeted experimental campaign, Coats *et al*. map out the failure conditions for suites of variably-porous crystalline andesite as a function of different temperatures and strain rates. The conclusion of the study is an empirical threshold for the failure of these materials derived from their data.

The article is assidious and well written, the experimental protocol appears rigorous, and the study yields a wealth of interesting new data. Overall, this article represents a commendable research effort from the authors. Where the authors perhaps do themselves a disservice is in the analysis of their data, which could be more comprehensive. Below I outline minor comments or concerns that I feel the authors should address or clarify. Pending these changes, I recommend this article for publication in *Solid Earth*.

Yours faithfully,

Jamie Farquharson

## Comments

(Please forgive the clumsy formatting, I was forced to write this report on my phone)

Lines 39-56: Sparks (1997) is highly relevant to this study, and is a surprising omission here.

> Sparks (1997) The causes and consequences of pressurisation in lavas done eruptions. *Earth and Planetary Science Letters* 150(3-4): 177-189

Line 66: how do the authors define the "temperature range of interest"?

Line 101: it might be useful to provide the equation for *Ca* here.

Lines 134-144, and elsewhere: the authors describe two inclusion models which they highlight may explain their data: the pore-emanating crack model of Sammis and Ashby, and the sliding wing crack model of Ashby and Sammis. However, the authors do not go on to employ either of these models subsequently. As I intimated previously, it seems something of a shame that there is not a more involved analysis of these data. Analytical solutions for both these models are provided by Zhu *et al*. 2010 *JGR* and Baud *et al*. 2014 *IJRMS*, respectively. Previous authors have utilised one or other in order to describe the failure behavior of volcanic materials or analogues, for example Zhu *et al*. 2011 *JGR* (for tuffs), Vasseur *et al*. 2013 *GRL* (sintered glass), Heap *et al*. 2014 *JGR* (andesite), Zhu *et al*. 2016 *JGR* (basalts). Moreover, Zhu *et al*. 2011 extend the analytical solution to a dual-porosity medium, and Zhu *et al*. 2016 combine both models so as to have a representative element volume comprised of an effective medium including a pore surrounded by many cracks. If Coats *et al*. were to interrogate their data in a similar manner, they may be able to glean valuable information about the governing microstructural elements in their samples contributing to failure (for example by contouring for different values of KIc/√[πr]).

Line 146: the authors should state clearly that they here define "lava" as something at high temperature (volcanic rocks may often also be referred to as lava).

Line 184: in which direction has the lobe moved (*i.e.* rotation, inflation, advance)?

Line 279, and elsewhere: is this a change of 1 % or 1 vol.%?

Line 296 and 298: which software was used to run the scripts? Are they publicly available?

Line 322: the observation that in dense materials the connectivity is higher than in porous materials is somewhat counter-intuitive, and runs counter to results from previous researchers (Farquharson *et al*. 2015 *JVGR* and Collombier *et al*. 2017 *EPSL*, both of which noted that connectivity generally increased with increasing porosity). Do the authors have any comment on this difference?

Line 342: cristobalite isn't a polymorph of quartz. Rather, both quartz and cristobalite are silica polymorphs.

Line 355-359: did sample volume change upon heating? This really is surprising, as one would anticipate thermal cracking upon heating and subsequent cooling, due to thermal expansion mismatch between the constituent microstructural components (*e.g.* Browning *et al*. 2016 *GRL*). This may be especially pronounced given the existence of cristobalite in your samples, which undergoes a significant volumetric change as it transitions between its alpha and beta forms (a function of temperature: see for example, Damby *et al*. 2014 *JAC*).

Line 378-380: it is not immediately clear from the figure, but does the rock stiffness increase with thermal stressing? Here you identity microfractures as the culprit, yet previously you indicate that thermal stressing doesn't affect the porosity of the samples. Moreover, I would think that induced cracking would serve to decrease the material stiffness, rather than increase it. I suggest the authors re-word this section for clarity and consistency.

Line 400-401: as previously, it would by useful if the authors were to distinguish between % and vol. %.

Line 431: Young's modulus ought to be capitalised. Also, there is a full stop missing at the end of this sentence.

Line 441-445: see comment above concerning these inclusion models.

Line 470: could this observation be due to pore anisotropy (*e.g.* Bubeck *et al*. 2017; Griffiths *et al*. 2017)?

Line 480-481: do the authors have any information on the pore size distribution or pore anisotropy that could help explain this?

Line 525: shear-induced development of pore connectivity was also shown experimentally by Kushnir *et al*. 2017 *EPSL*.

Line 535-560: as with my previous comments concerning the inclusion models of Ashby and Sammis and Sammis and Ashby, I believe the authors could extend this Deborah number analysis some more.

- As far as I can tell, the dimensions of Equation 10 only balance out if *b* = 1. Pa = [Pa.s × s^-1] is fine (*b* = 1), but Pa = √[Pa.s × s^-1] is not, as would be the case were *b* = 0.5. To me, this highlights a serious shortcoming in the empirical approach adopted here, especially as there has been more recent work on the topic which adopt physical rather than empirical parameters, for example the Wadsworth *et al*. 2017 chapter referenced elsewhere in the manuscript. For example, that chapter addresses the physical scaling of *De* with crystal content. Ultimately, a similar physical approach could yield a much more generally applicable failure criterion for porous materials.
- Line 535: using *exp* as a subscript is a little ambiguous (at first glance I presumed it signified an exponential).
- Line 546: Oswald should presumably be Ostwald.
- Line 546: here, the authors state values of *k* and *b* of 1653 and 0.5, yet in the caption for Figure 10, the values are *k* = 1606 and *b* = 0.7755. Which of these are correct? What is the sensitivity of the the following analysis to variations on *k* and/ or *b*?
- Equation 11: Based on equation 9, shouldn't σ relate to strain rate × viscosity (or equivalently, *De*× *G*∞)?

Line 618: this is in contrast to existing theory, models, and experimental data. Perhaps this effect is masked in your data by sample heterogeneity? I would be wary of including this point as a key conclusion of the study.

Figure 4: there appears to be some data obscured by the legend in panels *a* and *b*.

Figure 6: for clarity, perhaps the authors could plot viscosity as a function of strain rate (similar to Figure 10a; perhaps with symbols coloured for time).

Figure 10: in panel *a*, the authors state that the equation is shown on the figure, but it is missing. In panel *b* (and line 555), is the relation given by the yellow line based on only three data (i.e. the transitional data)? What is the r2 value of this relation? Can the authors comment on the theoretical value of *De_c* for a nonporous material? Would this fit on the trend? Likewise, how do the authors anticipate *De_c* evolving for highly porous materials?