Review of "Failure criteria for porous dome rocks and lavas: a study of Mt. Unzen, Japan"

Coats and her co-authors have addressed the majority of my previous comments satisfactorily, either by well-argued rebuttal or by making amendments to the text, for which I applaud them. In particular, they have improved the clarity of the manuscript in many parts, and performed a more in-depth interrogation of their data using the various micromechanical damage models discussed in the text.

I only have one outstanding concern, which relates to the balancing of units in Eq. 11. As highlighted in my original review, the units (as stated) do not balance out if b does not equal 1. This is a fundamental problem stemming from the use of an exponent model. The authors counter this comment by couching their constant *k* in units of $\sqrt{Pa.s}$ (i.e. $Pa.s^b$). While this is not particularly satisfactory (the "flow consistency index" has an ambiguous physical meaning if it is not in measurable units, i.e. Pa.s as it is currently explicitly defined in the manuscript), it does solve the immediate unit balancing problem. However, it is not a suitable solution as later in the manuscript their non-unity value of *b* appears again (in the Deborah number equation). The authors indicate that

$$De = \frac{\left(\sigma/_{k}\right)^{1/b}}{G_\infty}\eta_m.$$

If the authors use units of $Pa.s^b$ to define *k*, then the units balance thus:

$$De = \frac{\left(\mathrm{Pa}/_{\sqrt{\mathrm{Pa.s}}}\right)^2}{\mathrm{Pa}}\mathrm{Pa.s}$$

when *b* = 0.5, which is to say $De = \mathrm{Pa}$. The Deborah number is a dimensionless ratio (a timescale divided by a timescale), so presenting it in units of pressure is clearly not desirable, and I'm sure was not the authors' intention. Moreover, this assumes that *b* is a "neat" fraction, so that $1/b$ is resolved into an integer and the degree of the *k* radical is also an integer. Things become more complex if 0.5 < *b* < 1.0.

I urge the authors to look more critically at this problem, and perhaps reconsider the use of a power-law model, which propagates problems when incorporated into more involved analyses. Failing this, the authors should at least take care that their representation of *k* and *b* do not lead to errors later in the manuscript. For example, defining a critical strain rate λ such that λ = 1 s^{-1}, viscosity could be presented as

$$\eta_A = k\left(\frac{\dot{\epsilon}}{\lambda}\right)^{b-1}$$

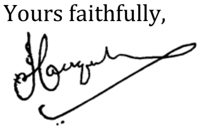
such that the units balance out without the need to redefine *k*:

$$\mathrm{Pa.s} = \mathrm{Pa.s}\left(\frac{s^{-1}}{s^{-1}}\right)^{0.5} \rightarrow Pa.s = Pa.s\sqrt{1}\,.$$

I acknowledge that this may not be a perfect (or even correct) solution, but it may be a useful avenue for the authors to explore. In any case, the authors ought to discuss some of the shortcomings of their power-law approach.

This point aside, I recommend this article for publication in *Solid Earth*.

Yours faithfully,



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