



## ***Interactive comment on “The strength and permeability of tuffisite-bearing andesite in volcanic conduits” by S. Kolzenburg et al.***

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*Overview:* This paper presents some interesting (although not particularly surprising) data on changes in the porosity, permeability and strength of tuffisite-bearing samples of holocrystalline dome rock from Colima Volcano, Mexico. However, I'll confess that I found the paper a bit difficult to follow because of the lack of clear articulation of the conceptual model behind, and framework for, the measurements and interpretation. Particularly confusing to me was the apparent distinction made by the authors between “initial” (or “recovered”) and (I gather) “disturbed” values of physical properties. Explanation of this distinction at the outset of the paper would therefore be very useful.

The measurements show that, when put under uniaxial compression at magmatic tem-  
C168

peratures (940°C), tuffisite-bearing samples show very little change in porosity (which starts at  $\leq 10\%$ ) but an order of magnitude decrease in permeability (from  $\sim 10^{-16}$  to  $10^{-17}$  m<sup>2</sup>). The authors interpret these data to show that the tuffisite has “recovered” its initial, or magmatic, value. I'm not exactly sure what this means, although I infer that the authors believe that the tuffisite was once highly porous and permeable. In fact, they state that the tuffisite must have started with a porosity of 17-27% and a permeability  $> 10^{-13}$  m<sup>2</sup> by analogy to other types of materials. I can see no real justification for this assumption, wherein lies the source of my confusion, particularly as the grain size distribution of the tuffisite veins is not reported.

In part, I think that some of my problems with this paper may derive from confusion in both the nomenclature and the genetic implications of nomenclature. The tuffisite described from Volcano Colima appears to be very similar to the fault gouge that we observed at Mount St. Helens in 2004-2008 (e.g., Cashman et al., 2008). In both cases, the material has a range of grain size, is holocrystalline, and has clasts of variable shape; both appear to originate from both comminution and abrasion during transport. I suspect we're talking about the same thing. However, there seem to be some implicit assumptions about tuffisite formation that aren't spelled out (and that are different perhaps from the genetic implications of fault gouge). It is these underlying assumptions that come into play when the tuffisite-bearing material is interpreted by the authors to have recovered some initial value.

Some background observations on the Mount St. Helens fault gouge: although we have not measured the porosity or permeability of the fault gouge produced during the spine eruption of Mount St. Helens directly, I think the UCL folks have. Our measurements do show that the gouge has a fractal dimension in particle size distribution of close to 3, which is similar to that observed in the core zone of faults. It also indicates dense packing (that is, very little pore space). In fact, the outer part of the gouge zone (an ultracataclasite) was virtually porosity-free and sufficiently consolidated to form striae. More generally, fault gouge (or even deformation bands) commonly has very low per-

meability relative to the surrounding material because of the extensive comminution and the ability of small particles to pack and fill space (as illustrated by the high fractal dimension). Thus I find the stated assumption of initially relatively high porosity and very high permeability to be potentially very misleading. This leads me to other questions about the interpretations presented here. If the tuffisite material had “recovered” from a previously more porous and permeable state, then shouldn’t evidence of that recovery be visible in SEM and/or element maps? Certainly any mineral precipitate should be identifiable. Moreover, I’m not sure that “hot pressing” or solid state diffusion can be called upon if the individual grains show signs of comminution and abrasion, but it would be useful to look at the nature of grain-to-grain contacts.

*Some specific questions:*

What is a volumometer?

All terms need to be defined (e.g., Pc and Pp).

I’m not clear on the porosity measurements. . . the authors describe porosity measurements based on the amount of water expelled from a sample when put under confining pressure – how was the water introduced in the first place? And how is isolated pore space measured?

All experiments were run at the same pressurization rate. . . it seems like it would be useful to re-do a few experiments at different loading rates to get a sense of the effect of loading rate on failure.

It doesn’t seem surprising that samples with tuffisite oriented perpendicular to the transport direction have permeabilities that are the same as the host rock. A question about the samples with tuffisite veins at 45° - did the authors look at these samples after the experiment was completed? I’m wondering if the permeability reduction was permanent, and if there was evidence for grain size reduction because of loading. I have the same question about the strength tests – were the samples evaluated in any

C170

way after failure? Again, I don’t find the assessment of brittle failure at high temperatures to be surprising, as the samples are virtually holocrystalline (we have seen the same behavior at Mount St. Helens in what has also been inferred to be high temperature formation of fault gouge).

It’s interesting that the permeability is reduced by an order of magnitude while the porosity remains essentially constant. The authors attribute this behavior to closure of microcracks. . . I agree that this is a plausible explanation but it would be useful to do some simple calculations to test this. For example, how much would crack width need to be reduced to explain this permeability decrease?

Finally, the manuscript needs editing in places – there are some incomplete sentences and others with awkward wording.

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C171