Dear Editor,

I send you herewith our responses for K. Cashman’s review of our paper:

“The strength and permeability of tuffisite-bearing andesite in volcanic conduits”

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We greatly appreciate the comments we received from Professor Cashman. Her comments and critique have allowed us to tighten the focus of our discussion and improve the clarity of the description of our experimental methods and results. Based on her comments, and the comments of G. Giordano, we have changed our title to “Strength and permeability recovery of tuffisite-bearing andesite”, which better portrays the findings of our study. Our replies to her comments, and the suggested changes to our manuscript, are outlined below. The original reviewer comments are in blue, our replies are in black italic, and our suggested changes to the original manuscript are in black.

“The measurements show that, when put under uniaxial compression at magmatic temperatures (940°C), tuffisite-bearing samples show very little change in porosity (which starts at ≤ 10%) but an order of magnitude decrease in permeability (from ~ 10^{-16} to 10^{-17} m^2).”

We admit that, in the original manuscript, we perhaps did not state clearly enough that the high temperature, uniaxial compression experiments were performed on a different experimental setup to the experiments measuring both porosity and permeability. The uniaxial compressive strength tests were performed at high-temperature and atmospheric pressure at the Ludwig-Maximilians Universität in Munich. Whereas, the porosity, permeability and ultrasonic wave velocity measurements were made at room-temperature and under a confining pressure at University College London. Ideally, the measurements would be made under the same conditions (T, Pc and Pp) but our current experimental setup does not yet permit this. In order to clarify our experimental program we would like to add a new figure showing the different experimental setups. This way, we are confident that there will be no further confusion concerning our experimental methods. We hope that this adequately addresses Prof. Cashman's concerns. The figure caption will read as follows:

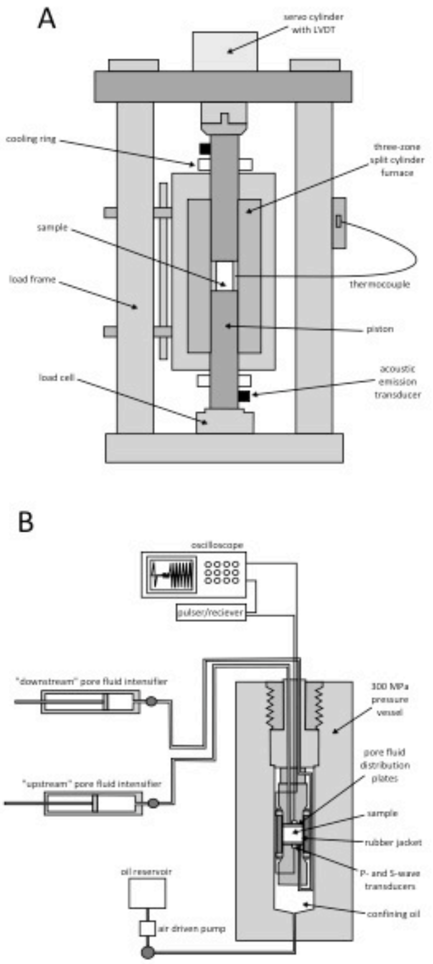
Figure 2: (A) Schematic of the high temperature uniaxial press at the Ludwig-Maximilians University in Munich (LMU), described in detail by (Hess et al., 2007). (B) Schematic of the 300 MPa hydrostatic pressure vessel equipped with two 70 MPa servo-controlled pore fluid intensifiers or volumometers located in the Rock & Ice Physics Laboratory (RIPL) at University College London (UCL).

As well as adding the new figure (see below) we would like to change some of the text in the manuscript to read as follows:

Sample porosity, permeability, and ultrasonic wave velocity measurements were made under increasing confining pressures in a 300 MPa hydrostatic pressure vessel equipped with two 70 MPa servo-controlled pore fluid intensifiers or volumometers located in the Rock & Ice Physics Laboratory (RIPL) at University College London (Fig. 2B).

And:

The uniaxial compressive strength (UCS) was tested, independent of the porosity and permeability measurements described above, in the high-temperature uniaxial press (Fig. 2A) at Ludwig-Maximilians University in Munich (see Hess et al., 2007 for details of the apparatus).



"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^2 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."

We agree that we should be more explicit regarding our interpretation of this point. We cannot measure, and cannot comprehend the original porosity. Instead we have used the works of Zou et al. (2011) and Shepherd (1989) to compute a range of permissive porosity and permeability values as a function of grain size distribution. We report and use a conservative range of porosity and permeability values based on the observed grain size distributions and degrees of compaction. Since no shear fabric is present in the tested rocks, we assume homogenous volume compaction rather than shear enhanced *compaction dominates in these rocks. We would like to clarify our discussion by adding the following to the manuscript:*

Direct measurement of the grain size distribution of tuffisite directly after formation is inhibited by their position within the edifice. The values reported above represent the span of permissive porosity and permeability for grain size distributions similar to that of the tuffisites tested. We suggest that the values of porosity and permeability of the tuffisite veins, immediately after their formation, should be within these ranges.

"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."

Both reviewers expressed their concern regarding using the term tuffisite when describing the observed structures. Standing back and looking at our initial manuscript anew, we do indeed concur that our original sample description was insufficient and as such, each reviewer proposed their own version of the trigger mechanism in their attempt at understanding what we had described. However, here, we certainly find ourselves in the presence of tuffisites – that is veins of solidified, polymictic, volcanic rock fragments (Lavallée et al., 2012a). For one, these veins are highly irregular and massive, unlike fault-related gouge material, which commonly show the presence of cross bed structures or C-S fabrics within fault planes (Cashman et al., 2008; Kendrick et al., 2012). Moreover, the veins are all found as lenses within individual blocks; that is, their lateral extent starts and ends within a block, refuting the idea that they form along a slip zone. At Mount St. Helens the fact that the gouge is holocrystalline is unsurprising as the groundmass of the initial spine contained 30% interstitial glass, but over a few months, the content rapidly decreased to 2% (Pallister et al., 2008). We would like to emphasize again, that the rocks tested in this study show no structures indicative of shear. Taking all our observations into account, we think that the rocks are best described as veins of massive, well sorted, polymictic tuffs, which can be termed tuffisite following Cloos (1941). In order to clarify the points made above we would like to expand the introduction as follows:

Tuffisites have been reported for a wide range of chemical compositions and diverse volcanic environments, including basaltic diatremes (Cloos, 1941), andesitic fossil conduits (Noguchi et al., 2008), and rhyolitic conduits (Tuffen et al., 2003). They were first defined by Cloos (1941) who described them as follows:

> *"The host rock seems **tuffisized**, i.e. infiltrated by the tuff along its finest cracks and crevices and intimately mixed with it."…"Appearance and color of such mixed rock types '**tuffisites'** is governed by the nature of the host rock"… "All observations point towards this **tuffisitation** taking place during the main phase of volcanism through penetration of gases into the surrounding rock".*

This makes them inherently different from rocks occurring within cataclastic shear zones enveloping lava domes and spines such as described at Mt. St. Helens, USA (Cashman et al., 2008; Kendrick et al., 2012; Kennedy et al., 2009) and Mt. Unzen, Japan (Nakada et al., 1999). Key differences include geometry and internal structures. Tuffisites create or use pre-existing fracture networks producing an anastomosing pattern of ash-filled veins (Stasiuk et al., 1996; Tuffen et al., 2003), whereas shear zones tend to form linear, or in the case of erupting lava spines, annular patterns (Cashman et al., 2008; Friedlander, 2012). Structurally, tuffisites are massive or show bedding structures resulting from the transport in a fluidized state (Tuffen et al., 2003), whilst shear zones are foliated and/or commonly develop a C-S-shear fabric (Kendrick et al., 2012). One does not however preclude the other; tuffisites can potentially be sourced from shear zones simply by later gas fluidization of the fine-grained cataclastic material.

"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."

In the introduction, we introduce tuffisites as veins comprising lithified mixtures of particles of juvenile volcanic material interspersed and intimately mixed with host rock fragments of similar size (Cloos, 1941). These veins represent cracks and crevices formed within the subsurface host rocks, and/or within the magma itself, that are simultaneously in-filled by transportation and deposition of material (juvenile and lithic) deriving from subsurface fragmentation. Tuffisites therefore do not display shear fabrics, which are inherent in faults. On that basis, the present tuffisites' strength and permeability recovery speaks of a complex evolution of the state of a host rock/magma from initially intact, to fractured, in-filled by gas and ash particles, and subsequently healed. Such changes in physical states undoubtedly announce significant changes in the properties inside the volcanic conduit. Please refer to the paragraph above for changes we would like to make to the manuscript.

"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 permeability 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."

All information given above about the MSH gauge enhances the differences between the gauge and the rocks tested in this study. None of the described textures are present in the rocks we described. The intrinsic differences between the source mechanisms of tuffisites and fault gouge make their porosity evolution incomparable. In order to address these points we would like to expand the introduction as proposed above.

"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."

Certainly, it will be very interesting to pursue a detailed analysis of the mechanics of the solidification of tuffisites. This is part of the plan for a larger geochemical/petrological investigation. For the purpose of this first study we decided to introduce the tuffisites through microscopic textural analysis, as well as through a study of the elastic properties of the rocks and their depth-dependent permeability, as no such physical data exist in the literature; a fact which inhibit our ability at assessing their potential importance in volcanic systems. In our opinion, the remarkable recovery of these tuffisites' physical properties is worthy of a stand-alone contribution to the geological community.

Some specific questions:

"What is a volumometer?"

A volumometer is a combination of a pore pressure intensifier and an LVDT. This combination allows for precise measurement of the volume of the sample during pressurization. We have added Figure 2 in order to show the experimental setup in detail.

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

The reviewer is correct. We would therefore like to change the wording in the manuscript to:

For the purpose of this study we apply the simple effective pressure law of $P_{eff} = Pc - \alpha Pp$, assuming that $\alpha = 1$ (Guéguen and Palciauskas, 1994). Where P_{eff} is the effective confining pressure, Pc is the confining pressure and Pp is the pore pressure.

"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?"

The samples were first vacuum-saturated with distilled water, and then placed inside the hydrostatic cell were they were connected to two pore pressure intensifiers. The pore pressure intensifiers then regulate the pore pressure inside the rock. We hope that any confusion regarding our experimental methods will now be suitably addressed by our new experimental apparatus figure (Figure 2).

"And how is isolated pore space measured?"

We did not measure the isolated porosity of these rocks. We only measured the connected porosity of our rocks (via helium pycnometry prior to testing, and via the water expelled from the sample during pressurization). Although the isolated porosity may provide some insight into a rock formation, we note that in such heavily micro-cracked materials (Petrakova et al., submitted), there is very little unconnected porosity (Mueller, 2006), and thus the measured porosity is by all means equivalent to the connected porosity which serves as a rough proxy to permeability (Mueller et al., 2005).

"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."

We agree that the effect of loading rate on the strength of dome building materials is both interesting and important, but the goal of this research, we stress, was to constrain whether the presence of tuffisite veins can influence rock mechanical behavior and *strength. As such, the investigation of tuffisite angles with respect to the applied stress direction was seen as essential. To study the influence of loading rate in an appropriate amount of detail would not only require a substantial experimental program, but also a vast quantity of material. Here, the samples we collected were wisely used to investigate the influence of geometry and stressing angle (with respect to the direction of the veins).*

"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."

Perhaps the fact that the permeabilities are similar is not surprising, since the tuffisites (in their present state) do not represent high porosity zones. However, without proving this assumption experimentally, it remains an assumption.

"A question about the samples with tuffisite veins at 45° - did the authors look at these samples after the experiment was completed?"

As described in section 3.2, the samples failed catastrophically, meaning they were pulverized and no coherent material could be recovered. Analysis of the run products was therefore impossible. Since the reviewer's comments elucidate the need of a better explanation of the post experimental state of the samples, we would like to enhance the manuscript by adding the following:

During failure the samples were essentially pulverized; it was therefore impossible to perform any post-experiment analysis on the textures within these rocks.

"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 way after failure?"

The permeability reduction was not permanent. At these modest pressures we are far below P (the onset of inelastic grain crushing and pore collapse under confining pressure increments), therefore we are still within the field of elastic compaction, i.e. recoverable deformation. If we had encountered P*, we would have observed a huge decrease in sample porosity. However, the pressures used are still well below those where we may expect P* in porous (13-23%) sandstones (Baud et al., 2006). As for the post-UCS test analysis (and as described in section 3.2), the samples failed catastrophically, meaning they were pulverized and no coherent material could be recovered. The grain size distribution after failure would therefore only characterize the fragmentation process. In our UCS tests, failure was purely brittle and no time-dependent deformation was noted from the mechanical response. In light of such mechanical behavior, no ductile* *deformation or the development of a shear plane would have been observed in our samples.*

"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 do some simple calculations to test this. For example, how much would crack width need to be reduced to explain this permeability decrease?"

Following classical studies on rock deformation in the brittle field, the strength of a rock decreases with porosity (Paterson and Wong, 2005); from this rule, one would expect the strength of a rock hosting tuffisite veins (forming through the process of failure and in-filling by fragmented particles) to decrease. Here, we therefore assessed this assumption and we surprisingly observed that the presence and/or orientation of a tuffisite vein can, under such extraordinary circumstances, carry no physical implication for the state of a host rock. The fact that the tuffisite is holocrystalline cannot account for this non-doctored behavior. We remarked that the samples are entirely brittle in our UCS tests; this is perhaps not a surprising observation, given their holocrystalline texture, but a full description of the experimental results/observations is key to any experimental paper.

As for the relationship between porosity and permeability, it certainly is complex. In such low-vesicle content materials, the permeability is likely to be governed by their pervasive microcrack network (Mueller, 2006; Mueller et al., 2005). However, in terms of total bulk rock porosity, the crack porosity represents only a small fraction compared to pore/bubble porosity. Therefore, a small decrease in porosity (we refute that the porosity "remains essentially constant" as alluded by the reviewer) can yield large changes in fluid flow properties. The closure of microcracks is actually the only plausible explanation for the decrease in permeability (pores/bubbles are not compliant at these pressures). Indeed, the closure of microcracks has been blamed for similar decreases in permeability in other volcanic rocks (Vinciguerra et al., 2005). Whilst the suggestion of calculating the decrease in crack width required to cause our permeability reduction is interesting, we are unconvinced that it furthers our understanding of the experimental data.

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

We agree that some statements were somewhat awkward. We have therefore gone through the manuscript very carefully and have smoothed the flow of the text.

Kind Regards,

S. Kolzenburg and co-authors

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