Solid Earth Discussions

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Comment on se-2021-127

Michael Heap (Referee)

Referee comment on "Transient conduit permeability controlled by a shift between compactant shear and dilatant rupture at Unzen volcano (Japan)" by Yan Lavallée et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-127-RC1, 2021

This manuscript presents a study that describes (using field observations, microstructural work, and laboratory measurements) conduit shear zones at Unzen volcano in Japan. The manuscript contains observations and data that will surely interest the volcanological community. I have five main comments and a series of line-by-line comments.

Mike Heap (University of Strasbourg, France)

Main comments

1. When discussing the textures/microstructure of the samples, what is described in the text is often difficult/impossible to see in the accompanying figures. Instances that require attention are described in the line-by-line comments below.

2. The XCT data are used sparingly, and qualitatively. It is possible, using the Avizo software used by the authors, to provide more quantitative analyses to support the statements in the text that refer to the size, shape, and connectivity of the void space. Such analysis would, in my opinion, elevate the manuscript. This is discussed in more detail in the line-by-line comments below.

3. The authors state that the bulk of the one of their conduit shear zones is the result of ductile deformation. Since the entire zone is an impressive manifestation of brittle deformation (it’s a shear zone containing cataclasites and fault gouge), I find this statement, at least the way it is written, a little confusing. I’m also left confused by the discussion about diktytaxitic textures, a texture commonly observed in lavas that contain little to no evidence of shear (this texture can be the result of late-stage gas filter pressing of a silica-rich melt phase, which leaves behind a microlite scaffolding), and ductile

deformation. Another way to look at this – as discussed in the recent paper of Ryan et al. (2020, JVGR) that describes a very similar structure – is that large-scale brittle deformation resulted in the formation of fault rocks (cataclasites and fault gouge) that were subsequently lithified by solid-state sintering, and then re-sheared and re-fractured. Did the authors observe any evidence for sintering in their samples? Are the diktytaxitic textures discussed actually sintering textures? I don't want to impose a certain school-of-thought onto the authors, as it's their manuscript, but I consider that sintering-fracturing cycles to be a simpler way to explain the observations/results. Further, the ductile deformation of porous lavas very often results in the formation of compaction bands. If there is ductile deformation, driven by cataclastic pore collapse, where are the compaction bands? Did the authors observe anything like this? Finally, ductile behaviour, driven by cataclastic shear-enhanced compaction, in a very stiff/crystallised magma with a porosity of about 20% would require an effective pressure of about 30 MPa, or depth greater than 1 km. Is this in line with the authors' interpretation? Something to think about.

4. Similar observations/data exist for Mt St Helens (Gaunt et al., 2014, Geology) and Chaos Crags (Ryan et al., 2020, JVGR). It would be interesting, in my opinion, to compare and contrast the various systems somewhere in the discussion section. Are the permeabilities similar? Is the evolution of permeability as a function of distance from the fault/margin similar?

5. There are various aspects of Figure 9b that I disagree with. First, I'm not sure why the authors have added "high pore pressure" and "low pore pressure" to the x-axis. The effective mean stress is a term that neatly encapsulates the three compressive principal stresses and the pore fluid pressure (i.e. a low effective mean stress doesn't necessarily mean that the pore pressure is high) and so it not needed/incorrect to refer to the pore pressure on the x-axis. Second, the authors have also added "low strain rate" and "high strain rate" to the y-axis, effectively giving the diagram two y-axes. However, a high differential stress does not necessarily mean high strain rate, and vice-versa. As a result, the graph shows that, at high strain rate and high effective mean stress, the material will be ductile. However, magma will likely be brittle at high strain rate, no? Similarly, at a high effective mean stress, the deformation mechanism can be changed from viscous flow to cataclastic flow by increasing the differential stress. Why would increasing the differential stress change the micromechanism of deformation at a constant effective mean stress? Increasing the differential stress at low effective mean stress can change the behaviour from viscous flow to shear rupture, and increasing the effective mean stress can also change the behaviour from viscous flow to shear rupture. In other words, increasing the confining pressure could change the behaviour from viscous flow to shear rupture. I don't see how this can be true. In fact, there are numerous instances in which the diagram suggests an unlikely/incorrect material behaviour for a certain set of x- and y-coordinates. It is not possible on a graph like this to simply exchange the y-axis (differential stress or strain rate) to explain certain aspects of the diagram. Finally, the authors have also indicated the influence of strain rate directly on the diagram, by providing a series of curves. However, while the influence of strain rate on the brittle failure of materials is reasonably well-known, the influence of strain rate on the onset differential stress for cataclastic flow is, to my understanding, largely unexplored. How are the authors sure that increasing the strain rate increases the differential stress for the onset of shear-enhanced compaction? In short, the diagram combines aspects of rock behaviour and fluid behaviour that I don't consider to be compatible. I strongly suggest that the authors reconsider this diagram.

Line-by-line comments

Line 118: Another relevant, and very recent, study is that of Ryan et al. (2020, JVGR). These authors document (field observations, microstructural work, laboratory measurements of porosity and permeability) a conduit-parallel shear zone within crystal-rich dacitic magma at Chaos Crags (USA).
Ryan, A. G., Heap, M. J., Russell, J. K., Kennedy, L. A., & Clynne, M. A. (2020). Cyclic shear zone cataclasis and sintering during lava dome extrusion: Insights from Chaos Crags, Lassen Volcanic Center (USA). Journal of Volcanology and Geothermal Research, 401, 106935.

Lines 195-197: Brittle deformation does not always lead to increases in bulk sample porosity. As shown in the cited Heap et al. (2015, BV) paper, samples that developed macroscopic fractures can contain a lower bulk porosity at the end of the experiment. I would subtle reword this sentence.

Line 207: "strain rates are…".

Line 210: Additionally, efficient compaction can shunt the magma into the brittle regime once porosity has been reduced sufficiently to promote brittle behaviour at the imposed effective pressure.

Line 237: "…the source of debate…"?

Line 275: CSZ was already defined.

Line 288: What was the length, or range of lengths, of these samples?

Line 321: Distilled or deionised water? Or tap water?

Line 323: Since water is expelled from both cracks and pores, I suggest the authors change "pores" to "void space" or "porosity".

Line 325: Did the authors wait at each pressure increment to ensure microstructural equilibrium? I guess the authors waited until the porosity change stabilised before measuring permeability at a given effective pressure?

Line 361: Was there any evidence of alteration?

Line 393 and elsewhere: What is shown in blue on Figure 4 is all connected, or are isolated pores also shown? It is possible using the Avizo software to provide pore size and shape statistics. Why not provide histograms showing pore size distributions for these materials, for example? This type of analysis would use these data more effectively, and provide some quantitative statements to support the authors' claims.

Line 404: It's difficult to observe the irregular vesicles in Figure 4e and 4f. Can the authors isolate one or two of these irregular-shaped pores and show them alongside the reconstruction of the entire sample? The Avizo software could also be used to provide the average circularity of the pores, to provide some quantitative statements to support the authors' claims.

Line 405: How are the authors sure that these vesicles "enhance the connectivity"? I'm not sure that changes in connectivity can be assessed using Figure 4.

Line 411: Can the authors label the S- and C- fabrics in Figure 3?

Line 422: It's somewhat difficult to see what the authors are describing in Figure 3. Perhaps it's worth showing a zoomed image showing the pulverised phenocrysts within the more porous bands?

Line 432: Unless I'm mistaken, the authors don't refer here to the XCT reconstructions shown in Figure 4c and 4d. What do these data show?

Line 444: Did the authors observe any evidence for the sintering of particles in the fault gouge?

Line 447: It's difficult to see the alignment of pores in Figure 4a and 4b. Can the authors show some additional images that show this alongside the reconstruction of the entire sample?

Line 454: I would say "large", rather than "important". It might also be worth noting that these porosities correspond to the porosity at ambient pressure.

Lines 455-446: Although up to the authors, I suggest that they reword this sentence to improve clarity. I had to read the sentence several times to understand its meaning.

Line 458: Be careful with percentages. Are the authors talking about a relative change or a change in percentage points?

Line 467: The permeability values shown in Figure 5 were measured at what effective pressure? 5 MPa?

Lines 481-487: Should some of this text not be moved to the discussion section?

Line 518: The surface of the fracture is quite rough. Are the authors sure that they had a good seal between the field permeameter and the rock surface? I say this because the permeabilities within the fracture are very high.

Line 533: Diktytaxitic textures are also documented in dome lavas from Merapi volcano in the cited Kushnir et al. (2016, JVGR) paper. These authors interpreted this texture as the result of late-stage gas filter pressing of a silica-rich melt phase, which left behind a microlite-supported groundmass and cristobalite in neighbouring vesicles.

Line 576: “in conjunction”?

Line 599: Kushnir et al. (2017, GRL) appears in the reference list, but perhaps more relevant, at least here, is Kushnir et al. (2017, EPSL).
Kushnir, A. R., Martel, C., Champallier, R., & Arbaret, L. (2017). In situ confirmation of permeability development in shearing bubble-bearing melts and implications for volcanic outgassing. Earth and Planetary Science Letters, 458, 315-326.

Line 600: The cited Heap et al. (2017, EPSL) paper also measures porosity changes during the deformation of andesite at high pressure and high temperature (above and below the threshold glass transition temperature at the imposed strain rate).

Line 623: See also the aforementioned paper by Ryan et al. (2020, JVGR).

Line 647: The authors mean “magmas” rather than “lavas”?

Line 653: The Coats et al. (2018, SE) study only provides uniaxial deformation experiments. This sentence, however, discusses the influence of effective pressure on rock failure mode.

Line 730: “determinations”?