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Solid Earth Discussions

# *Interactive comment on* "Inelastic compaction and permeability evolution in volcanic rock" *by* Jamie I. Farquharson et al.

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Review of Farquharson et al.

This manuscript deals with a central question in Earth science related to fluid migration in our volcanically and tectonically active lithosphere. Namely, the evolution of fluid permeability during deformation. The hypothesis that is here tested experimentally is that the evolution of the fluid permeability in volcanic rocks is highly dependent on the inelastic strain accumulated when the deformation occurs in the compaction regime. There is little or no analysis of the data presented beyond computing the inelastic component of the bulk (or axial?) strain and presented measured values of properties that evolve with strain. The is the only shortcoming of this work. Otherwise, the manuscript is well illustrated, well written and meets the standards of novelty and quality for publi-



cation. Below I submit some constructive comments which I hope will guide the authors in their revision of this article.

(1) In the abstract line 19, the term "high strains" is used. I think that throughout the manuscript the authors should be explicit and clear about what strain they are talking. Axial strain, pore strain, and inelastic strain are all introduced at some point and so referring to strain more generally is not clear. (2) Page 2 line 10; do the authors mean to say "siphoned"? Or do they simply mean that the volatiles are able to escape? Throughout the manuscript there is some flamboyant language that perhaps precludes a clear understanding of what is meant in places. Another example is perhaps the use of "amoeboid" as an adjective to describe a pore shape on line 26 of the same page. (3) Page 2 line 14; not all of the papers cited here are explicitly in support of the statement made. (4) Page 2 line 31; the authors state that porosity is the reciprocal of bulk density, which is not true. It's clear what the authors mean but, as stated above, they should be very clear in the language they use and check throughout the manuscript that they all agree on each sentence. (5) Page 2 line 40; by this point in the manuscript I thought it would be illustrative to have a diagram that schematically explained the concepts of this paragraph. If the authors felt inclined to provide one I think it would strongly improve the understanding of the regimes to which they refer. (6) Page 3 line 2; the reference to Farquharson et al., 2016b should occur at the end of the sentence – in a few places the authors put a reference in the middle of a sentence without it always being necessary. (7) Page 3 line 7; do they authors mean "monotonically" when they say "monotonously"? (8) Page 4 line 1; on all measurements of a value, the authors should give an experimental uncertainty. The density should have an uncertainty quoted with it, please. (9) Page 4 line 3; if the authors use a correction for raw permeability data, could they please show it explicitly. As far as I know they are far from any length limit associated with this journal and so they could take the space to be as thorough as possible. An equation for the correction and a plot to which the reader could refer to see how the correction works would be valuable. (10) Page 4, line 9; what does a poroelastic constant of unity mean in physical terms? It seems to

imply that the pressures are isotropically distributed in the solid matrix. Does this imply something about the compressibility? Could the authors unpack this useful equation a little in the text that follows? (11) Page 4, line 11; the authors state that they apply a pore pressure. Presumably they do not apply this to any isolated pores. This would need stating clearly here. (12) Page 4, line 18; I am familiar with the justification of a strain rate of 10ˆ-5 sˆ-1 in rock mechanics studies on the grounds of "standard practice". However, do the authors perhaps have some justification for using this rate on more physical grounds? For example, Heap & Wadsworth (2016) among many other studies showed that a dominant control of strain rate in permeable rock is to shift regime from effectively permeable on the timescale of deformation, to effectively impermeable. It's ok to say that this rate was used for experimental expediency but it would more satisfying to see if it could be justified. (13) Page 4, line 20; do the authors mean d\phi and not \delta\phi? I suspect they do. In which case please change throughout. (14) Page 4, line 38; if \alpha is different for different properties when everything else is the same, then it is not the same coefficient – i.e. it is not \alpha. In which case don't use the same symbol. This took me a few reads to understand and would be clearer if the authors simply referred to, for example, \beta, and named it another poroelastic constant that is different from \alpha. This does beg the question of why it's different and what physics \beta is tracking that \alpha is not. (15) Page 5, line 6; why would you need the mean radius? (16) Page 5, line 10; this equation begs an explanation. Why is this the answer to an effective cross-sectional area in a deformed sample? This is not explained at all. (17) Page 5, line 32; it's clear here that the authors mean an axial strain. Please can they go carefully through the manuscript and be specific about strain definitions. Otherwise they could say up front that throughout this work "strain" is used to mean "axial strain". This is important as they talk about other forms of strain. (18) The discussion is robust and I have no commentary. (19) The figures are well presented. However Figure 5, which is the take-home message of the work, is not clearly presented. I would recommend some analysis be explored, such as a "change in permeability" as a function of inelastic strain or some metric that scales with the effective



pressure of deformation? The reader might be looking for some take home conclusion which they don't find here. I do not see clearly, for example, that permeability increases and then decreases, as the authors state. (20) A central conclusion of the work seems to be this: that in the brittle regime permeability increases because strain is accommodated by discrete fractures. And that this is different from the compaction regime in which pore crushing produces diffuse areas (compaction bands) of granular material. The novel idea here is that the effective pressure controls the particle sizes in these zones which in turn controls the permeability limit during progressive compaction. However, this idea is left hanging without much additional support or exploration. There are many permeability models for granular media, including polydisperse examples (e.g. Wadsworth et al., 2016), which could be used to confirm this idea. It's exciting and therefore a little bit of a shame to leave it unexplored with simply a statement that it would make a good future study. If the authors felt inclined to unpack this idea a little, it would be worthwhile.

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