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Interactive comment on “Experimental evidence that viscous shear zones generate periodic pore sheets that focus mass transport” *by* James Gilgannon et al.

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I read with interest the paper titled “Experimental evidence that viscous shear zones generate periodic pore sheets that focus mass transport” by Gilgannon and coauthors. The paper presents microstructural analyses of marble samples deformed as part of a previously described set of laboratory experiments. Most of the authors were part of a recent paper (Gilgannon et al., GRL 2020), which also conducted a microstructural analysis of a sample from the same sample suite. In their previous paper, the authors suggest that cavities are nucleated as part of the recrystallization process. In the present paper, the authors examine the spatial distribution of those cavities and demonstrate that the distribution becomes increasingly anisotropic with progressive deformation. The authors then conclude that these observations demonstrate that rocks cannot be described as viscous in the same manner as fluids and that constitutive models for rock viscosity are therefore inherently flawed. This content is appropriate for publication in Solid Earth.

The central analysis in the paper involves the use of wavelet convolution to characterize the spatial distribution of pores. I'm not aware of comparable implementations of wavelet analysis in microstructural studies, and I think this is a very valuable contribution to the community. The anisotropic distribution of pores is clearly demonstrated, and the caveats of the method are usefully discussed.

I do have three main comments about the interpretations of the results and the statement about constitutive models of rock viscosity. First, I'm curious about the nature of the cavities, what the pore fluid is, and how many pores are inherent in the starting material. Second, I have some concerns about the inference that porosity increases during deformation, which stems from the details of the analysis. Third, the discussion of treating rocks as viscous fluids seems like an overstatement considering the nature of evidence presented in the manuscript. I discuss these three points below, followed by minor comments indicated by line number.

–The nature of the cavities:

My question in this context is whether or not there is a pore fluid, and if so, what is its composition? One item of concern is whether the pores are filled with CO2. That is, could there be some decarbonation of the calcite during initial pressurization and heating that generates porosity? Caristan, Harpin, and Evans (1981) presented evidence of calcite decarbonization during hot-pressing of calcite aggregates in a gas-medium apparatus, and it seems worth discussing whether or not something similar has occurred here. This question could be answered in some part by comparing the starting material (not shown) to the low-strain material (gamma = 0.4).

–The evolution of porosity and its measurement:

Considering that some porosity may be present at the onset of deformation, the fundamental question is whether or not the porosity increases with deformation. A primary conclusion of the previous work (Gilgannon et al., GRL, 2020), which analyzes one of the same samples as in this paper (PO422, gamma = 5), is that porosity increases due to pore nucleation associated with recrystallization. Indeed, the previous work presents excellent observations of pores associated with new grain boundaries, and a total porosity of ∼1%, but as far as I can tell, it is never demonstrated that deformation increases the total porosity.

The present work presents an excellent opportunity to make that demonstration since samples deformed to a variety of strains are analyzed. However, it is still not clear to me that there is a change in the total porosity. The wavelet analysis definitely demonstrates a change in the spatial distribution of pores, but as far as I understand, it does not present an increase in the porosity.

My conclusion here stems from the details of the measurement of porosity density, which is the fundamental measurement presented in the paper. As I understand the measurement procedure described in the appendix, only the centroids of identified pores are used in the construction of the porosity density maps. Thus, “porosity density” is apparently the number of pores per unit area, and this quantity may be better referred to as the “pore density”. I admit I had some confusion about how “standardized density” is defined, why it is unitless, and why it can have negative values. Some clarification here would be valuable in a revised version, but for now it seems to me to represent a normalized version of the “pore density” and not the porosity (pore volume per total volume).

Unfortunately, the “pore density” does not seem as valuable as porosity in regards to interpreting the hydromechanical effects of the observed microstructural evolution. A key inference in the paper is that permeability is enhanced (and becomes anisotropic) during deformation. The relationship between permeability and porosity is not trivial, but if the porosity were known, a back of the envelope calculation could be made to assess the potential changes in permeability expected. However, I don't think changes in permeability can be assessed qualitatively or quantitatively when using only the number of pores. Furthermore, from the data presented here, it seems entirely possible that the porosity could be constant with increasing deformation, even though the number and distribution of discrete pores evolves.

As a final note on this topic, there is a clear link to previous work on the segregation of melt during deformation of partially molten rocks (for a review, see Kohlstedt and Holtzman, AREPS, 2009). I'm surprised to not see any of that work referenced in the present manuscript. Those studies demonstrate that the (melt-filled) porosity can dynamically arrange into planar features not dissimilar to the "periodic pore sheets" described here. However, I'll emphasize that the average porosity is static in those experiments. Locally it may increase or decrease, some new pores are created, some old ones are destroyed, but the total porosity does not change. It remains to be demonstrated that a similar phenomenon is not occurring in the calcite samples analyzed here and that the overall porosity is actually increasing.

–Challenging the concept of rocks as viscous fluid:

The authors take the discussion beyond the development of a periodic array of pores to comment on whether or not rocks can be treated as viscous fluids. The paper is framed around the common treatment of rocks as viscous fluids at long timescales and suggests that that framework is flawed and a new paradigm involving creep cavitation is necessary. Emphasis is given to the transition from viscous to brittle behavior.

I suggest that these statements are overstating the case and detract from what is otherwise a useful paper about applying wavelet analysis to periodic pore distributions. My primary concern here is that the authors do not provide any evidence that the mechanical properties of the rocks investigated are modified by the formation of an anisotropic pore distribution. The mechanical data have already been published in previous work, so if these pore sheets are significant to the rheological behavior, then the authors can demonstrate that with the mechanical data. Furthermore, if these rocks can't be described as viscous fluids because of the porosity evolution, then the authors could again demonstrate that with the mechanical data.

In addition, I'll note that viscosity is a phenomenological description, and when discussed in the context of crystalline materials, is generally only taken to apply at macroscopic scales. For example, we often discuss rocks as fluids with non-Newtonian viscosities for which the viscosity is controlled by the dynamics of dislocations at the lattice scale. There has been much work in the materials sciences over the past several decades demonstrating that, at scales well below the grain size, dislocation propagation occurs in discrete bursts (key terms are "dislocation avalanches" and "jerky flow"). This behavior is clearly not viscous, but when averaged over many crystals of many orientations, the mechanical behavior can still be described as viscous.

Key to the authors' argument is that creep cavitation can lead to brittle behavior, which "can never be predicted by flow laws commonly used to model viscous deformation." An analogous case can be seen in the fatigue of metals. Constitutive equations have existed for some time to describe plastic deformation (i.e., having a yield stress and strain hardening) in metals. Similarly, the equations of plasticity cannot describe failure in fatigue. Does that mean that metals do not deform plastically? Does that mean that we need a new paradigm and must throw out constitutive descriptions of plasticity? I think the answer to both questions is "no" since, although we need additional physics to describe fatigue, there are many situations in which a plastic description of metals is totally appropriate. Similarly, I'd argue that there are many situations in which the description of rocks as viscous fluids is totally appropriate.

So, at the very least, I suggest the authors weaken their comments to note that rocks can reasonably be treated as viscous fluids in many situations.

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Discussion paper

As a final note, although the formation of pore sheets may require additional physics in our constitutive models, in many cases, these additional physics can be described as an evolving viscosity. A relevant example is the work of Holtzman, King, and Kohlstedt (EPSL, 2012), who present a framework for describing the evolution of the viscosity of partially molten rocks as planar features of high porosity are formed.

To summarize my primary comments above, I think the wavelet analysis is an important contribution, and the observation of anisotropic pore distributions will motivate much future study. However, I feel the interpretations of these data are overstated since 1) whether or not porosity increases is unclear, 2) it seems the effects on permeability cannot be assessed from these data, and 3) a departure from viscous behavior associated with the pore evolution is not demonstrated. Based on these comments, I suggest the manuscript would require major revision to be accepted.

–Minor comments:

Line 20: It's not clear to me that the term "frictional embrittlement" is appropriate here. Is the implication that friction leads to brittle deformation, in the manner that hydrogen embrittlement means hydrogen doping leads to brittle behavior? I have been unable to find another use of this term in the literature.

Line 23: This does not seem like an appropriate reference here. The book by Kocks, Argon, and Ashby is certainly a classic work, but it is primarily focused on the small-scale aspects of dislocation motion. Their treatise is focused on the mechanisms that lead to plastic deformation (as in deformation with a yield stress), and I don't think they refer to geological processes, viscous behavior, or creep.

Line 25: It seems strange to refer to these rocks as viscous when the primary argument of the paper is that we can't think of rocks as viscous.

Line 35: The authors state here that the evidence is unambiguous for the role of creep cavities. This phrasing seems a bit strong to me considering my main comments above.

Line 54: At this point in the text, I was looking for a description of the sample preparation and data collection procedures. I eventually found a lot of this in the Appendix, but I think this part of the main text should point the reader to the relevant sections of the Appendix.

Figure 1: It would be very useful to know what the starting material looks like. In other words, does the porosity evolve simply as a function of increasing the confining pressure and temperature?

Figure 1: It would also be useful to state in the caption how the images were collected (e.g., BSE images in SEM).

Line 55: I'm curious how the authors distinguish between a pre-existing second phase and a new precipitate. And on a related note, if some pores are filled with precipitates, then are they still considered pores in this analysis? Based on the text here and in the appendix, it is unclear to me if, and if so how, secondary phases are removed from the porosity density maps.

Figure 2: How come a similar analysis for the low-strain material isn't shown? That would be a useful comparison.

Figure 2: It took me a while to figure out that eta is a sort of measure of the total power for the whole map. It would be helpful to clarify this in the main text and point the reader to section A7.

Figure 2: Both panels b and c have three peaks labeled, but only two are discussed in the text.

Lines 73 and 74: The values given for the locations of the peaks seem too precise to me considering the broadness of the peaks. I suggest reducing the precision by at least one significant digit. Or present some measurement of error for these numbers.

Figure 3: Panel g seemed out of place to me here as it doesn't really have to do with the wavelet visualization. Isn't it better suited to Figure 1?

Figure 3: It took me a while to figure out how exactly the visualizations were made. Much of this information is in the appendix, but more detail can be given here. The caption can simply state that these images represent the convolution of the density map and the wavelet, or something along those lines.

Line 83: The authors state here a key thrust of the paper, that the development of anisotropic porosity is not accounted for in our conceptual models. But the question that arises to me as I read this line is...do we need to account for it? I think that need is what remains to be demonstrated. Is the permeability measurably affected? Is the mechanical behavior affected? The mechanical data were collected and published for these experiments. Is there in signature in those data of the porosity evolution?

Line 87: If an attempt is made in a future revision to quantitatively relate the observed porosity to permeability, then this seems like a good spot for a back-of-the-envelope calculation.

Line 113: The text states that "...the hydro-mechanical anisotropy presented here would...". However, hydromechanical anisotropy is not presented here. Microstructural anisotropy is presented, and the hydro-mechanical anisotropy is only inferred.

Line 138: Again, how does the algorithm deal with secondary phases? Are they initially marked as pores?

Line 138: Here and elsewhere "S" is used instead of "A" to indicate items in the appendix.

Line 161: There is a typo of some sort in this sentence.

Line 185: There is some explanation here, but the choice of L still seems pretty subjective. Isn't it simpler to just retry the analysis with different values of L and see how that affects the results?

Line 215: I'm a little confused about the definition of "mean power spectrum". Is this the mean of the power spectrum? The mean of several power spectra?

Section A6 and Figure A4: I think this is a useful analysis of the edge effects. Some aspects of this analysis could be highlighted in the main text. For instance, in Figure 3, the caption could note that the white space around the visualizations indicates the region subject to edge effects, the size of which is dependent on the wavelength. This also begs the question, for the maximum wavelengths investigated (∼600 microns), what proportion of the input image is actually useable?

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