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Solid Earth
Discussions

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

Alberto Ceccato (Referee)

alberto.ceccato@unibo.it

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General comments: In this Short Communication, Gilgannon et al. present a series of microstructural data from experimental deformation of natural rock samples which demonstrate the systematic and spontaneous development of microstructurally–organized transient porosity, i.e. dilatant microstructural sites, during viscous deformation. This dataset and the following discussions allow the Authors to tackle the validity of the common assumption that dilatant processes and porosity development are suppressed during viscous deformation of rocks. From the title, the main discussion points to address the effects of this transient porosity on the mass transfer capability

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Discussion paper

of viscous shear zones. Then, in the main text, the effects of this porosity development are unfolded and extended also to other topics, such as deep seismicity, slow earthquakes and tremors. In a quantitative way, the Authors demonstrate that the development of transient porosity is a systematic characteristics of viscous shear zones developed in an homogeneous starting material and they demonstrate that the extent of such process is relevant to the behaviour of the whole shear zone and not only to the grain–scale interactions.

Overall, this preprint represents a substantial contribution to the scientific progress within the topics of the deformation of the solid earth, tackling quantitatively a topic which was previously only postulated or treated in a qualitative way.

The manuscript is concise, well written and straightforward. The introduction drives the reader clearly and straightforwardly to the main scientific question the Authors want to tackle in the main text. Then, data and methods are presented in a concise way and focussed on selected points necessary to the reader to follow and understand the subsequent discussions. The microstructural dataset and analysis described here are novel, scientifically sound and statistically robust, providing a sound base on which the Authors can build their discussions. The Method section is extensively unfolded and integrated in the Appendix.

However, the Discussion section suffers from just a couple of generic and vague sentences (as outlined below in the Specific Comments section), which the Authors need to recast a little bit in order to support their inferences and conclusions. I understand that the Authors are clearly speculating on some of the discussed topics, but some sentences present a very large logical jump between the presented data and the conclusions they want to support. Most of my observations and comments focusses on the speculation about pore–fracture evolution, not the mass–transfer capability, which seems to be the main topic of the communication.

I want to highlight the novelty and the importance of the quantitative approach the

Authors have adopted to tackle the topic, being a fundamental point of the present paper that makes the difference with previous publications. Moreover, I have found this Short Communication really stimulating, triggering in me a lot of new questions and interesting points of discussions. I think the present preprint need some moderate–minor revisions in order to make it suitable for publication in the Solid Earth journal. I think the paper will be of high interest for large part of the structural–experimental–geophysical community of the readers of SE.

In the followings, I provide some comments and observations, which I hope will help the Authors to improve the present preprint. Well done, congratulations!

Specific comments:

Lines 13–14: please add some references supporting this statement.

Lines 32–34: I partially disagree with these two sentences. It is true that much of the past advances on the creep cavitation subject results from experimental works and micro–scale analysis (as the cited references report); but it is also true that many other authors have evaluated the extension and occurrence of creep cavitation and related phenomena in natural polymineralic shear zones deformed at geological conditions and have tried to qualitatively extrapolate their results from the thin section–scale to crustal–scale shear zones (e.g. Giuntoli et al., 2020 SciRep https://doi.org/10.1038/s41598-020-66640-3; Preciguot et al., 2017 NatComm https://doi.org/10.1038/ncomms15736). I understand that your samples are "natural" geo–materials, but you are presenting the results of experimental deformation at lab conditions, not geological conditions. I suggest the Authors to recast these two sentences taking into consideration this observation. I would focus on two main points that, in my opinion, are the strong points and the novelty of the paper: 1) the quantitative approach to evaluate the extension of creep cavitation processes; 2) the occurrence of creep cavitation in monomineralic shear zone.

Line 40: please, specify what you have revisited of the set of classical experiments (the microstructures of the deformed samples).

Line 55: please, specify if you have considered these grain–filled pores as “porosity” during the following density quantification or not. I guess you have considered it as “porosity”, but this is not clear neither from the main text, nor from the Method section (or perhaps I missed it).

Line 89: I would replace “microstructure of ...” with “the spatial arrangement of syn–kinematic pores”. In my opinion, the definition of the effective microstructure would imply the analysis and characterization of pore typology, connection, morphology, etc...

Line 94: Even though I agree with the Authors about the importance of the spontaneous development of a systematic pore pattern, this statement is rather speculative and not fully supported by the data presented in the manuscript. “Bulk material properties” is rather generic and vague. What are these bulk material properties and initial heterogeneities? Grain size? Mono–polymineralic composition of the sample? (What about the experimental boundary conditions? Shear strain rate? confining pressure? Sample/Room humidity and fluid availability?) I understand that this could lead to further discussion that goes beyond the scope of this short communication, but I would like the Authors to be a little bit more specific on the term “bulk material properties” if they want to keep the sentence. On the other hand, if the Author want to work around this comment: highlight the lack of any initial heterogeneity and state that, at present, the genetic causes of this microstructure need further investigations...

Lines 104–106: These two sentences are rather speculative and vague. Unfortunately, it seems to me that a logical connection between the occurrence of strain invariant mechanical parameters and the possible evolution of pore sheets into creep fractures is missing. In my opinion, this paragraph suffers from two main problems: (1) there are no direct logical links between the observation of strain invariant mechanical parameters and the evolution of pore sheets into creep fractures, i.e. the maintenance of strain invariant mechanical parameters is not a sufficient conditions to prove (or just speculate) that also your pore sheets will evolve into creep fractures. (2) Even though the experiments of Dimanov et al. and Rybacki et al. have been performed under similar conditions to those reported here, there are some fundamental differences that might undermine your inference: a) different lithologies; b) occurrence of creep fractures at relatively low strains if compared to shear strains obtained in Barnhoorn et al.; c) lack of any evidence of creep fractures in the set of experiments of Barnhoorn et al. (2004). Rather than a speculation on the possible evolution of these pore sheets under "variable" natural conditions, I would suggest the Authors to discuss and compare in a more detailed manner their experimental results and inferences with those presented by Dimanov et al (2007) and Rybacki et al. (2008). Then, a speculative extrapolation to the natural conditions might be attempted. Indeed, there might be some microstructural similarities between the cited references and the data reported in the manuscript that might better support your speculation about the "possible evolution of pore sheets into creep fractures". In addition, "natural conditions are more varied" is a rather vague and generic statement. What are these variable natural conditions? Transient shear strain rates? Wet vs. dry conditions? Please specify if you want to keep the sentence.

Technical Corrections:

Figure 1: please, enhance the brightness/contrast of the BSE images OR enlarge the images (this might help the reader to instantaneously capture the porosity distribution even if the image is very small).

Figures in the supplement: There is a discrepancy between the figure call–out in the text (Figure S*) and the Figure captions (Figure A*). Please change one or the other to be consistent throughout the text.

Line 245: There's probably a reference typo [(18) ???].

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