Comments to the Authors for the manuscript se-2020-137-manuscript-version2

I reviewed a previous version of the manuscript, and I am glad to see that the Authors answered in an exhaustive and very detailed way to the concerns of both Reviewers. The Authors' modification to the previous version of the manuscript effectively improved both the readability and the scientific content and structure of the paper. The overall length of the paper is right at the word count limit for a Short Communication. I wish to see these data and discussions published as soon as possible. Unfortunately, I still have some minor comments which the Authors may want to consider. The manuscript already fulfil the high-quality standards of Solid Earth and it deserves to be published after some very minor corrections. I provide below some comments and suggestions which I hope might help the Authors in shortening the text and clarify even more its content.

General comments:

1) Title: as it is now, the title is rather inconsistent with the whole bulk of topics discussed in the manuscript. I am referring to "focus mass transport", which subject is rather limited in the Discussion section. This comment relates also to the General comment 3) about Section 3.1.

   Perhaps a slight rewording of the title would make it effectively reflect the content of the whole manuscript (e.g.: "Experimental evidence that viscous shear zones generate periodic pore sheets: effects on fluid redistribution and mechanical behaviour"; something that include both topics. By the way, this is only a suggestion.)
2) Introduction and Discussions: there are two seminal papers, in my opinion, from Neil Mancktelow (2002, "How ductile are ductile shear zones?" *Geology* https://doi.org/10.1130/G22260.1; and 2008 *Lithos* https://doi.org/10.1016/j.lithos.2007.09.013) which must be discussed (or at least cited) in your manuscript. Both papers cover exactly the topics and paradigms you wish to discuss, and thus I am quite surprised in not seeing them even cited in your manuscript.

Briefly, Mancktelow (2002) shows that there is the necessity of a "pressure-sensitive plastic deformation" component during viscous deformation of ductile shear zones to explain melt–fluid flow within ductile shear zones from a continuum mechanics point-of-view.

Indeed, including this point-of-view in the Introduction (Lines 20-32) would strengthen and support your claim for the necessity of a "reappraisal of the community's perception of how viscous deformation in rocks proceeds with time", alongside with natural and experimental results. It would also support your discussion in Sections 3.1 and 3.2.3 (Lines 225-227).

3) "How mylonites could focus mass transport". I would like to see a clear discussion and separation between what is observed and inferred from the experimental data and microstructures and what is then extrapolated to occur in natural shear zones. I'll explain myself. The presented experimental data and microstructures show that there is the formation of a systematic, periodic and anisotropic pore network which allows for the mass redistribution within your sample, rather than an effective mass transport. The deforming sample in conjunction with the deformation apparatus cell constitute a "closed" chemical system.

   Given that the "system definition" is a matter of scale, if one considers the deforming sample and the confining medium as two separate entities, the ingress of Ar from the confining medium is a clear evidence for the occurrence of an effective mass transport between two media. However, this cannot be demonstrated with the presented data and I completely understand that demonstrating Ar mobility is far beyond the scope of the present manuscript.

   By contrast, mass transport in natural shear zones implies either gain or loss of chemical components in an "open" chemical system, which commonly includes two media: the shear zone and the some other rock (host rock, subducted or nearby tectonic units, e.g. Selverstone et al., 1991 JMG; Barnes et al., 2004 JMG) which act as either source or sink of the transported mass.

Therefore, I would suggest the Authors to clearly state that in the experimental case the porosity allows for a mass redistribution within the sample, which sample can be probably treated as a closed system. Then, if this process is extrapolated and adapted to the natural "open system" shear zones, where the deforming dynamic-porosity-bearing medium communicates with another medium, it can effectively promote mass transport. This can be easily addressed by the Authors with some rewording of the paragraph.

4) In my opinion, it would be better to present first the comparison with other experiments and then discuss how the porous anisotropy may affect the mechanics of such experiments (swop the order between section 3.2.1 and 3.2.2). This would also allow to extend the discussion about the Generalised Thermodynamic model to the other experiments and thus discuss the differences between the experimental results. Indeed, when reading the section comparing experimental results one question comes up: what about the boundary conditions (constant force vs. constant velocity) in these different experiments? Does it relates to the mechanics? This is only a personal suggestion, but it would probably ease the reading of the manuscript and its logical structure.

   Otherwise, the Authors need to consider the boundary conditions as one of the "difference and similarities" between experiments (Lines 183-197), given that they show the important role of boundary conditions on the mechanics and microstructure of experiments in the previous section (3.2.1).

5) Section 3.3: I really like the discussion about veining and fluid flux, which perfectly fits with the above-discussed "mass transfer" capability of shear zones related to creep cavitation and the dynamic fluid pump model. The reference to "recent experiments on calcite gouges" fits perfectly with the "earthquakes and tremors" topic discussed at the end of the paragraph and the discussion of experimental mechanics above. However, the discussions concerning dyking and frictional melting seem a bit out of place, they are still speculative (as stated by the Authors) and a bit disconnected to the rest of the paper in my

opinion (Delete Lines 230-233; 236-240). Therefore, I would suggest the Authors to limit the discussion about "speculative" topics, also in order to shorten the main text. A rapid rewording and swop of sentences within the paragraph will easily satisfy this suggestion, if the Authors agree on that.

Detailed comments:

Line 80: If you are not considering the mineral-filled pores in your quantification, then specify that these values are minimum estimates.

Line 165: Please specify what is the related boundary conditions in the Generalised Thermodynamic model (i.e. constant velocity = constant thermodynamic flux; constant force = ?).

Bologna, 04.12.2020

Alberto Ceccato