Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-5-RC3, 2019

Solid Earth Discussions

Interactive comment on "Fluid-mediated, brittle-ductile deformation at seismogenic depth: Part I – Fluid record and deformation history of fault-veins in a nuclear waste repository (Olkiluoto Island, Finland)" *by* Barbara Marchesini et al.

Anonymous Referee #2

Received and published: 27 February 2019

The paper presents an integrated structural, petrographic, and fluid inclusion study to a nicely exposed deep crustal fault zone in Finland. The paper overall, and the microstructural description in particular, are nicely written. Other parts of the paper are in need of significant revision. Most significant is the fluid inclusion section. In my view, the data, as presented here, are not interpretable in a meaningful way. This may in part be an issue with how the data were collected (different FIAs inadvertently mixed together, freezing runs before heating runs) but more likely reflects a fluid inclusion record that can't be deciphered with standard fluid inclusion techniques (e.g.

multiple stages of overprint, FIAs that cannot be separated). Given the difficulty of interpreting the fluid inclusion record, I suggest that the authors either re-evaluate their analysis, and/or rely less on these results for their overall analysis. The way I understand the pressure estimates, the fluid inclusion data result in a wide range of inferred pore fluid pressures that exceed the physical limit between hydrostatic and lithostatic at any depth.

The structural interpretation relies on geometric concepts (Riedel shears) that in this structural context are superseded by concepts of fracture growth by propagation, coalescence, and shear reactivation that result in observable crosscutting relationships and thus kinematic sequences that should be described in detail. Some suggested references are listed below.

I also encourage the authors to sharpen the problem statement and present this work in the framework of multiple testable hypotheses. There is a large body of existing work on the involvement of pore fluid pressure changes on seismic (and aseismic) fault slip that is only incompletely covered in the cited references. Given this wide body of published work, a more focused problem statement will help guiding the analysis and make the paper more relevant to the current discussions on this topic.

Detailed comments below refer to line numbers.

16: BDTZ: Please spell out throughout the text. Abbreviations of this sort create unnecessary jargon that impedes understanding.

16: The problem statement "uncertainties remain as to the role of fluids in facilitating deformation in this zone" is too vague. There is a plethora of prior work on this general topic. To be considered novel, the problem needs to be framed more tightly. I encourage presenting the problem through multiple hypotheses that can be tested in the discussion section using the data presented in the data section.

22: Please explain multi-scalar or avoid this term (at least in the abstract if you don't

have the space to explain it).

24: homogeneous: Do you mean single-phase?

28: delete “comprised”

28: 160 and 10 MPa: This is physically not possible because the pore pressure can only range from hydrostatic to lithostatic. If the pore pressure is 10 MPa, the lithostat is about 27 MPa. It the lithostat is 160 MPa, the hydrostat is about 60 MPa for fresh water, and higher for saline water.

29: “. . .physical conditions. . .”

51: “fault-activated valve”: It is important to emphasize that the Sibson fault valve concept is applicable to high-angle reverse (or low-angle normal) faults, i.e. faults that are highly unfavorably oriented.

67: “veins . . . attest to the relative abundance of aqueous fluids” is a mischaracterization. Except in special cases (e.g. hydrocarbon systems, CO2-rich systems) rocks are fully saturated with aqueous fluid, regardless of the presence of veins.

68: The presence of veins can also not be equated with large fluid fluxes. Quartz and carbonate can easily be locally sourced and transported by diffusion. A possible indicator of large fluxes is the economic enrichment in elements above background in the host rock, e.g. Au in gold-quartz veins.

71: I disagree with this statement.

72: What are H2O-rich minerals? Zeolites? Also, see comment above about “hydrous conditions”. Anhydrous conditions would be difficult to envision in the upper crust.

81: I am not convinced at this point of the introduction that fluid flow events necessarily trigger seismic fault slip. I would propose that it is generally the opposite, i.e. that the abundant evidence of episodic fluid flow is a result of frictional slip instability, and that fault valving as proposed by Sibson is the exception for highly unfavorably oriented

faults. Without a compelling problem statement, I don’t find that the research question phrased in the following sentence is sufficiently justified.

84: analysis of fluids or fluid inclusions? Did you sample and analyze water samples?

89: Reference?

93: “. . .new insights into the mechanisms steering deformation. . .”: That is a very generic statement. For a paper that is published in a high-profile journal please try to be more specific. What is fundamentally new in this paper?

99: meaning of “metric levels”?

135: Please be specific what “this” refers to. Is the deformation zone discordant to the old ductile fabric or not? “This” is confusing.

136-142: Are these statements a preview of what you will describe, or is this already known? If the latter you need to reference. If the former, you should preface with “as will be shown in the following section. . .” but keep it to one sentence or two to avoid duplication with later text.

146: “will be described in a separate paper”: If it is important to understand the findings in this paper, you should include this material here. If not, why refer to an unpublished paper that I cannot access?

149: Why are they limited? Mine exposures can be the best 3D exposures. Explain.

153: Do you mean surface outcrop?

160: striae, not strie

165: FIA is strictly petrographically defined and generally interpreted to be cogenetic.

170: I don’t follow this sentence. You need to show, using petrographic evidence, that these inclusions are synkinematic. “Consider” implies that you assume that, but they may not be. This also applies to the second part of the sentence: Synkinematic and

post-kinematic fluid inclusion trails may have the same orientation, so you need to be careful with this assumption.

176: Quartz is a very good host for inclusions. As long as you get consistent results within FIAs you have a good quality criterion. The quality check is built into the FIA workflow. This sentence is not only unnecessary but actually misleading. What means “as similar as possible”? That adds subjectivity to where you actually have very rigorous approaches to quality control. Please refer to the recent Fall and Bodnar (December 2018) paper on this.

182 delete by

184: subscript Tmice and Thtot

186: Homogenization temperatures should always be determined before freezing. This is to avoid damage to the inclusion through the expansion while freezing. Although high-temp inclusions are less prone to this effect (the presence of a large bubble reduces this risk), you add the possibility that your datasets are contaminated with anomalously high temperatures due to leakage associated with freezing.

189: Is 3 degrees C your cycling range? Please indicate that.

190: Recording the lowest and highest homogenization temperature per FIA is not sufficient to evaluate the internal consistency within an FIA. To take advantage of the FIA approach for internal quality control, you need to measure multiple inclusions within an FIA (ideally more than 10 or so). Again, please refer to Fall and Bodnar 2018.

233: Do you observe any actual offset on the main fault or subsidiary fault surfaces? The Riedel terminology is generally misused because it only applies to very specific loading conditions (basement-induced faulting)âĂŤsee papers and book by Mandl. Frequently, as in this case, these terms are used based on simple geometric relationships rather than well documented cross-cutting and timing relationships. I refer the authors to papers by Aydin and co-workers (e.g. Myers and Aydin, 2004, JSG) that explain fault

evolution as a process of opening-mode fracture propagation, reactivation in shear, coalescence, and linkage. This approach should be based on rigorously documented crosscutting relationships that provide a relative timing sequence of structure formation and reactivation.

239: What means hybrid fractures? Shear and opening? Sheared opening-mode fractures? Or sheared fractures? Or dilatant faults?

241: Without dip information, it is difficult to interpret these structures.

243: Joints by definition are barren. Use: Quartz-cemented fractures.

245: translucent? White?

246: Opening-mode

248: Do you quantify this?

252: . . .vein cemented with. . .

254: “microstructural analysis reveals that. . .” State what observations (facts) reveal this, not the type of analysis.

263: Figures to support these statements?

275: Section 4.2 is overall well written!

290: juxtaposed by

293: Viscous is a term used to describe a constitutive (stress-strain) behavior. I suppose crystal-plastic is a better term here.

294: crystal-plastic; plastic is a constitutive term.

315: sealed and healed: these terms are used differently in the literature. Best to define if they carry specific meaning here.

352: Do you have CL images to confirm this interpretation?

379: What is phi? Figure 9h is missing.

394: The ranges in these fluid inclusion values is too large to be interpretable. See general comments above.

445: The lower temperature range obtained using this geothermometer is outside the calibrated range. If this geothermometer is not calibrated below 250 degr C, all you can say is that temperatures are calculated to be 305 degr C and lower. The 220 degr number is not valid.

455: I think you mean single-phase aqueous liquid and that the inclusions homogenize into the liquid phase (bubbles disappear also into the vapor phase and at the critical point). Or at least, no vapor-rich inclusions were trapped.

458: Based on your histograms I can't evaluate this statement. Histograms are not informative representations of fluid inclusion data. Box and whisker plots are preferred. For instance, you could plot salinity and Th for each FIA next to each other so that we can check if Th and salinity ranges somehow correlate. As shown, there could be many reasons for these wide data spreads: Mix-up of FIAs that formed under widely varying conditions (perhaps most likely); partial resetting or necking (although difficult to explain the Tm ranges); partial leakage during freezing (would explain range in Tm but be visible during freezing run); or a combination of all these. Which inclusions are primary, which secondary?

491: Perhaps, but this is fairly speculative. It is certainly a way to interpret the fluid inclusion results, but that does not imply that the fluid inclusion results are conclusive enough to support this interpretation. To make a convincing case, you would need at least a few well constrained FIAs at the low and some at the high end of the datasets. Overprint is often not complete, so we expect some vestiges of earlier events/conditions preserved somewhere (often not in the zone of highest deformation but at the edge of the fault damage zone). That is obviously hit or miss, and some structure are not suitable for this type of work.

496: It is not clear from your description if the plots in Figure 12 are based on the FI homogenization temperatures or not, but I suspect they are (in which case, the legend should say: "The light colored areas are defined by the slope and position of the fluid inclusion isochores as determined by measured inclusion salinity and homogenization temperature range"). In that case, your pressure range would be a function of the spread in Th (reduced by the geothermometer constraints), correct? Without more constraints on the reason for the wide range in measured homogenization temperatures I don't think you can make any reasonable inference about pressure variability. But maybe I misinterpret these plots, in which case you should reword the text and legend.

509: I don't know if this line of evidence is strong enough. Do you have other observations that are indicative of episodic flow? Cement textures, isotopic zonation?

517: I would consider a 0-5 wt% salinity range as quite significant. At least it would be in upper crustal environments. Can you cite some reference values from the literature (e.g. Yardley and Graham 2002)?

523: What is the evidence for hydrofracturing? This would imply, fracturing by an increase in fluid pressure rather than a decrease in total normal stress. That is very difficult to demonstrate (e.g. using very good fluid inclusion pressure data, e.g. Fall et al. 2015, GSA Bull). The observation of quartz-filled veins does not necessarily demonstrate this process.

532: Possibly, but I don't see the clear supporting evidence.

537: Please cite some more recent geologically work on this topic (1921!).

615: What is the evidence for seismic fault movement? There is now a significant body of work on structural indicators of seismic fault slip (e.g. work by Rowe at McGill). We also now know that significant fault slip can be aseismic (slow earthquakes etc) and that brittle does not necessarily mean seismic.

625: With the Oklahoma events, we have now a large body of recent papers on induced seismic events. Many are very discrete earthquakes. Your statement is not correct.

771: Abstracts should be avoided as citable source.

Figure 1: Where is the study site on (a)? Is this map needed? Inset in (b) is too small. Please enlarge. Add lat and long information to all maps for proper georeferencing of this figure.

Figure 2: This figure looks very busy. It would be easier to grasp if the photographs (b and d, e and f) would not overlap with each other and with the map. I assume this will be page width, so no need to bunch everything together. Joints by definition are barren, thus quartz-filled joint would be better termed quartz-cemented fracture. See comment about Riedel fractures above.

877: correct astep

Figure 9: Figure h is missing

Figure 4d: Too dark.

Figure 5c: Do you have a CL image that confirms crack-seal?

Figure 11, last sentence: see comment for line 445.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-5, 2019.