Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-5-AC3, 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.

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Received and published: 31 March 2019

Response to Anonymous Referee comments (Referee #2 - se-2019-5-RC3)

We thank the reviewer for the thorough and constructive review, which will no doubt help us prepare a greatly improved version of our manuscript, especially with regard to the quality of the figures and the discussion cantered on the fluid inclusion data. Most of the comments and suggestions are being taken into account to finalise a revised version of the paper. There are a few points raised by the reviewer, however, that we

would like to clarify and discuss in this letter to the benefit of the open and constructive scientific discussion that Solid Earth facilitates. In the following, we report first the reviewer's comments followed by our replies.

16: Reviewer: "BDTZ: Please spell out throughout the text. Abbreviations of this sort create unnecessary jargon that impedes understanding" Authors: Our study describes and characterizes in great detail a fault zone exhumed from the brittle-ductile transition. The acronym BDTZ – standing for brittle ductile transition zone – is used 13 times throughout the text, indeed a remarkable number. We spell the acronym out in the first line of the abstract (line 12) and again in the introduction (line 58) and are certain that this is sufficient to clarify it the reader. Furthermore, this abbreviation (or its short version, BDT), is quite common in the literature and not really a form of ultraspecific jargon. We believe that its use will not impede the general understanding of the text and, in fact, can actually smoothen the reading.

16: Reviewer: "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." Authors: We propose to modify that section to better focus on the main outstanding issues in that broad theme. We wish to convey the message that uncertainties remain as to the role of fluids in permitting and facilitating broadly coeval brittle and ductile deformation. Moreover, in the Introduction we focused the readers' attention on the possibility to obtain information about temperature and fluid pressure conditions from fluids trapped in mineral phases during brittle-ductile deformation. In particular, we highlight that there are several studies that use fluid inclusions and other geochemical techniques to retrieve information on the burial history and deformation depth at which specific structures form, whereas constraints on fluid pressure and fluid composition in fluid-rich faults are comparatively rare (but see e.g. Cox et al., 1995). This is indeed what we try to better understand with our study. We will explicit better all this.

22: Reviewer: “Please explain multi-scalar or avoid this term (at least in the abstract if you don’t have the space to explain it).” Authors: Thank you for your suggestion. We will change the text at line 22 to “Structural and microstructural analysis”.

24: Reviewer: “homogeneous: Do you mean single-phase?” Authors: With the term “homogeneous fluid” we mean a fluid that was in a homogeneous physical state (liquid, in this case) at the time of trapping. The establishment of this state (or, alternatively, a heterogeneous state) is a fundamental step of fluid inclusion studies, and without this fluid inclusion data would be essentially meaningless (e.g., Bodnar et al., 1985). The approach of determining a homogeneous or heterogeneous state of a fluid is a well-established approach in the fluid inclusion literature (Diamond, 2003b) and provides a very efficiently tool to discern paleo-fluid properties and geochemical processes. The identification of homogeneous vs. heterogeneous fluids in a sample is carried out by finely discriminated, petrographically associated, groups of fluid inclusions that are trapped together, i.e., by fluid inclusion assemblages (Goldstein and Reynolds, 1994). In our study we worked exclusively with FIAs; therefore, we are in the position to discern the properties of the paleo-fluid(s) of the BFZ300 fault. This claim is very important and should be considered carefully for the evaluation of our dataset and interpretation.

28: Reviewer: “delete “comprised”” Authors: Done.

28: Reviewer: “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.” Authors: We agree with the reviewer, but have to stress that the 10-160 MPa range is not meant to represent the variability of fluid pressure during one single deformation phase. It neither refers to estimated values of lithostatic and hydrostatic pressure or vice versa. That range represents, instead, only the maximum and minimum estimated pressure attained by the fluids involved in

the cyclic deformation, as documented by the fluid inclusion record. We will certainly clarify this important aspect in the revised manuscript. We have presented fluid inclusion data that can be ascribed to four distinct reactivation events (Qtz I damage zone based on secondary FIAs, Qtz I fault core based on secondary FIAs, Qtz II fault core based on pseudosecondary FIAs and Qtz II fault core based on secondary FIAs). We analysed FIAs that present the less petrographic evidence of post-entrapment re-equilibration (see also schematic figure in the supplementary material). Our microthermometric analysis suggests, though, that fluid inclusions experienced post-entrapment re-equilibration and/or that different fluids entered the fault zone (see also reply to Reviewer #1). In either case, we interpreted that the lowest estimated pressures are probably derived from fluid inclusions trapped during the latest fracturing event at shallow conditions, near the surface. Conversely, our peak pressures reflect higher fluid pressure conditions from earlier deformation at greater depth.

29: Reviewer: “. . .physical conditions. . .” Authors: We will add “. . .. physical conditions (T, Pf). . ..”

51: Reviewer: “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 unfavourably oriented.” Authors: Although the “fault-valve behaviour” concept by Sibson is mostly suited for misoriented inverse or normal faults, it is by no means only restricted to those faults typologies. Fluid pressure counteracts the normal load on fault planes in all geological environments and all fault geometries. Significant fluid pressure build-up to high and even transient supra-lithostatic conditions is just more easily attained in compressional and extensional settings compared to strike-slip settings. This is because fluid flow is easier along the $\sigma 2$ direction and therefore fluid overpressure is difficult to attain in strike-slip faults with sub-vertical $\sigma 2$. Nevertheless, extremely heterogenous lithologies, combined with large volumes of circulating fluids, as well as the combination of sealing processes and/or crystal-plastic deformation and recovery may help the formation of compartmentalised fluid overpressure also in strike-slip deformation zones.

51: Reviewer: “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.” Authors: In this paragraph (we believe the reviewer is referring to line 67, not 51), we argue that the relative abundance of veins changes from “fluid-rich” to “fluid-poor” faults, with the former predominantly characterized by dense networks of extensional and hybrid veins and the latter by only minor veining and more significant volumes of wear products. Large volumes of hydrothermal fluids and the related precipitated economic minerals associated to fault zones, are excellent examples of significant coseismic, deformation-controlled fluid flow in the mid to upper crust (Cox et al., 2001).

68: Reviewer: “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.” Authors: We humbly point out that this is not the main issue at discussion and that we do not see any particularly controversial statement in what we have written. Although we agree with the Reviewer that the presence of veins alone is by no means sufficient to conclude high fluid pressure and large fluid fluxes, we point out that we wished to simply introduce two end-member types of fault systems formed at the brittle-ductile zone (fluid-rich and fluid- poor). A significant number of published studies have documented the presence of veins in hydrothermal fault systems exhumed from the BDTZ. To produce fluid-rich faults, possibly associated with ore mineralization, large fluid fluxes are indeed needed (e.g. Cox, 2016; Sibson, 2017). We discuss later in the manuscript that our case study qualifies as a fluid-rich fault because the entire fault structure is fundamentally formed by multiple vein generations.

81: Reviewer: “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. Authors: (we think the reviewer is referring to line 50) We respect the opinion of the reviewer and do agree that many examples might exist where fluids are only passively involved in deformation and coseismic rupture. It is, however, well known that fluids do have the potential to steer and trigger deformation in an active system (e.g. Sibson, 1985, Nature; Miller et al., 2004, Nature to cite two out of a huge number of papers going in this direction). Our statement only highlights that fluids are important variables in the seismic cycle, something that we consider undisputable. We do not agree with removing this part or rephrasing it.

84: Reviewer: “analysis of fluids or fluid inclusions? Did you sample and analyse water samples?” Authors: We used the generic term “fluid” because we analysed fluid inclusions and synkinematic minerals, precipitated from a fluid percolated through the fault, we did not only analyse fluid inclusions.

89: Reviewer: “Reference?” Authors: This is an introduction to the system studied in the present manuscript.

93: Reviewer: “. . .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? Authors: One of the main conclusions of our study (which emerges by reading the entire paper down to the final discussion) is that the deformation zone that we document has accommodated repeated brittle deformation episodes under overall ductile background conditions. Transiently high fluid pressure in a ductile environment where crystal-plastic processes dominate, is believed to be the trigger of the alternating cycles of brittle-ductile deformation that we have documented and that we propose as the evolutionary conceptual model for the studied deformation zone. Quartz precipitation in opened fractures plus crystal-plastic processes and viscous recovery helped the system to regain strength and permit fluid pressurization, thus triggering more hydrofracturing events. In light of the Reviewer's comment, however, we will modify the text to clarify even better this aspect.

99: Reviewer: "meaning of "metric levels"? Authors: meaning up to several meters thick. We will rephrase the text.

135 Reviewer: Please be specific what "this" refers to. Is the deformation zone discordant to the old ductile fabric or not? "This" is confusing. Authors: The studied fault zone did not exploit a ductile precursor. To avoid misunderstandings, we will change the text with "Other faults, such as the studied BFZ300, do not show any clear genetic relation to the older ductile fabric and cut it discordantly".

136-142 Reviewer: 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. Authors: Lines 136-142 are a preview of the studied fault zone and to the conjugate structure to BFZ300, which is the subject of the paper forming Part II of this study.

146 Reviewer: "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? Authors: Part II is totally focused on the presentation of the conjugate sinistral fault zone (BFZ045). We see no harm in creating a conceptual and editorial cross-referencing to the second paper of this set of companion articles.

149 Reviewer: Why are they limited? Mine exposures can be the best 3D exposures. Explain. Authors: The reviewer certainly knows that underground facilities and mines are not unlimited. The exposure of the studied fault zone was (as it is now even covered) limited to the actual excavated volume of rock. The outcrop is only a few meters

wide.

153 Reviewer: Do you mean surface outcrop? Authors: The studied outcrop is at about 480 m below sea level. To avoid misunderstanding we will change the previous sentence with "collected at the studied underground outcrop".

165 Reviewer: FIA is strictly petrographically defined and generally interpreted to be cogenetic Authors: We agree with the Reviewer about the cogenetic nature of fluid inclusions in a petrographically defined assemblage and this is essentially confirming our statement of lines 163-167 of the manuscript. The cogenetic nature of the group of inclusions is a fundamental requisite for the definition of the physical-chemical properties of the entrapped fluid (see reply to "comment 24"); therefore, it is also useful to discriminate possible stages of post-entrapment re-equilibration (Bodnar, 2003, and refs. therein), which is the reason for which we have referred to that paper.

170 Reviewer: 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. Authors: We used the verb "consider" because this is anyway an interpretation of millimetric structures observed in thin sections, even though we performed careful petrographic analysis with the aim to correlate millimetric structures with mesoscale observations. We considered as co-genetic, i.e. proper FIAs, those trails that exhibited both similar orientation and petrographic characteristics at the scale of the thin section. We will clarify this even further in the revised manuscript.

176 Reviewer: 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. Authors: We agree with the Reviewer about quartz being a good host for fluid inclusions due to its hardness. However, many papers describing experimental deformation of quartz crystals hosting FIAs have documented that textural re-equilibration of fluid inclusions occurred alongside the deformation of the host. With “as similar as possible” we only meant that we used petrographic analysis to identify FIAs whose textures preserve those of the pristine FIAs, i.e., for our analyses we deliberately avoided FIAs showing re-equilibration textures due to intense deformation of the host quartz. However, to avoid misunderstandings, we will change the sentence “. . .working on FIAs that are as similar as possible to those preserving the pristine fluid conditions:..” to “ working on FIAs that show the lowest possible degree of textural re-equilibration. . .”. We will also refer to the paper of Fall and Bodnar (2018); however, see comment 190 below.

184 Reviewer: subscript Tmice and Thtot Authors: Done.

186 Reviewer: 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. Authors: This comment is in contrast with the best practice of fluid inclusion microthermometry reported in all handbooks. For instance, in Chapter 7 of the classic handbook by E. Roedder (1984) – and specifically in its “Sequence of Measurements” – the procedure for microthermometric analysis states that low-temperature phase changes should be measured first using the freezing-heating cycling technique. Another practical guide to microthermometry published later than the Roedder’s handbook (Belkin, 1994, his p. 14) reports explicitly that “Low-temperature phase changes should be measured first on all inclusions in the mineral chip because heating may decrepitate or stretch the host . . .”. Thus, our approach follows the best practice approach of fluid inclusion analyses. There are only two exceptions to the rule given above, which correspond to the cases of microthermometry carried out in high-density H2O inclusions hosted by easily deformed minerals such as fluorite, calcite, barite, and sphalerite, or microthermometry carried out in diagenetic fluid inclusions (Lawler and Crawford, 1983; Roedder, 1976; Goldstein and Reynold, 1994). Apart from these cases, excessive heating caused by the determination of final homogenization temperatures generate inclusion stretching and decrepitation, and indeed this is considered the typical cause of decrepitation of inclusions in most studies (Goldstein and Reynolds, 1994).

189 Reviewer: Is 3 degrees C your cycling range? Please indicate that Authors: As reported in the manuscript, 3 °C is not the cycling range but the accuracy we determined for the microthermometric measurements. We determined the accuracy of Tmice and Thtot using synthetic fluid inclusions for which these properties are independently known.

190 Reviewer: 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. Authors: We interpreted our dataset taking into consideration a wealth of literature that considers natural and synthetic FIAs that experienced a number of post-entrapment re-equilibration regimes (Bakker, 2017; Bakker and Jansen, 1990, 1991; Diamond et al., 2010; Tarantola et al., 2010; Vityk and Bodnar, 1995a; Vityk and Bodnar, 1995b, 1998; Vityk et al., 1994), some of which is cited in the manuscript. We appreciate the work of Fall & Bodnar (2018); however, the focus of that paper is the interpretation of Thtot data of FIAs from a number of geologic environments, and not the study of re-equilibrated FIAs. This limitation notwithstanding, the method presented and discussed by Fall & Bodnar is in agreement with that of our manuscript. Figure 16 of Fall & Bodnar shows a method to evaluate the uncertainty associated with the Thtot of re-equilibrated FIAs. Using a comparison with experimental data on synthetic FIAs (Vityk and Bodnar,

1995a), Fall & Bodnar show that outliers of Thtot histograms of re-equilibrated FIAs – which are often ignored during data interpretation – are actually important to define the most probable "original isochore". That approach is exactly the one we tried to follow, that's why we wrote the sentence "care was taken in recording the minimum and maximum values for each assemblage" in the manuscript. However, we did not record only the lowest and highest final homogenization temperatures of each FIAs, but the entire set of microthermometric properties (Fig. 10 of our manuscript). This approach is consistent with the method used in several studies on re-equilibration experiments of synthetic FIAs, for which mean, range, standard deviation, extreme values, etc., all have a significance with respect to the P-T history of a fluid inclusion sample (e.g., Diamond et al., 2010; Vityk and Bodnar, 1995b, 1998). We also carefully selected FIAs having 25-30 fluid inclusions with the aim to have a statistically representative number of measurements. Based on these considerations, we conclude that we followed a correct procedure to collect and interpret the FIA data. The reviewer probably misinterpreted our text.

233 Reviewer: 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) see 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. Authors: Synthetic (R) and antithetic (P) shear surfaces have only millimetric offsets and do not cut the Y (or M) slip surfaces (please see Figure 2 of the manuscript). This is both consistent with the common knowledge that R and P shears nucleate at incipient stages of shear deformation along the master slip surface (Tchalenko, 1970, to go back to the origin). Our R and P structures are

indeed second-order to the main BFZ300 Y slip planes and do not cut them across. The geometric and kinematic set-up of BFZ300 is very standard and does not require complicated models. There is no need, therefore, to question the validity of this fundamental mechanical approach to the unravelling of brittle fault zone architecture in terms of structure hierarchy and kinematics. The work by Myers and Aydin (2004) is indeed very interesting but is irrelevant to our structural analysis in that it describes the details of the structural reactivation of joints during subsequent shear deformation.

239 Reviewer: What means hybrid fractures? Shear and opening? Sheared opening-mode fractures? Or sheared fractures? Or dilatant faults? Authors: Hybrid fractures are fractures forming at differential stress conditions larger than 4 T (where T is the tensile strength of the rock) and smaller than 5.7 T. It is a standard terminology for fractures that form in response to a combination of dilation and shear, across and along the fracture planes, respectively, see e.g. Hancock, P.L., 1985. Brittle microtectonics: principles and practice. Journal of Structural Geology 7, 437–457. https://doi.org/10.1016/0191-8141(85)90048-3.

241 Reviewer: Without dip information, it is difficult to interpret these structures. Authors: We disagree with the reviewer about the difficulty to interpret the here proposed structures. All the relevant data is presented and discussed in the paper. What the reviewer is after, is already presented in the text. Fracture orientation data from BFZ300 are plotted in the stereonet shown in the inset of Figure 2a and discussed in the relative text. In the stereonet it is evident that mode I veins and joints bisect the acute angle formed by the conjugate fault systems, thus constraining the direction of $\sigma 1$. In the revised manuscript we will explicitly mention the average strike and dip of the four sets of structures also in the text.

243 Reviewer: Joints by definition are barren. Use: Quartz-cemented fractures Authors: We will amend the text to "quartz-filled joints juxtaposed against".

245 Reviewer: translucent? White? Authors: Translucent means diaphanous, semi-

transparent, glossy. All these adjectives describe the typical appearance of fine-grained recrystallized quartz. We leave the original text.

246 Reviewer: Opening-mode Authors: We do not understand what the reviewer wants to say and with reference to what? We leave our text unchanged.

248 Reviewer: Do you quantify this? Authors: We did not quantify fracture density and only relied on visual inspection. Fracture density variation, however, was sufficient to catch our attention and have us write this sentence.

252 Reviewer: . . .vein cemented with. . . Authors: We disagree. The vein is not cemented. The vein is the infill material that seals a fracture. We will leave the original text

254 Reviewer: “microstructural analysis reveals that. . .” State what observations (facts) reveal this, not the type of analysis. Authors: We will change the sentence to “These shears are formed by cataclastic bands formed at the expense of the host migmatitic gneiss”.

263 Reviewer: Figures to support these statements? Authors: The chlorite textures are already reported in Figure 3. We will insert a reference to Figure 3 at line 263.

290 Reviewer: juxtaposed by Authors: We will amend the text to “juxtaposed against”

293 Reviewer: Viscous is a term used to describe a constitutive (stress-strain) behavior. I suppose crystal-plastic is a better term here. Authors: We agree with the Reviewer and we will change it.

294 Reviewer: crystal-plastic; plastic is a constitutive term. Authors: Agreed.

315 Reviewer: sealed and healed: these terms are used differently in the literature. Best to define if they carry specific meaning here. Authors: The fractures are sealed by the newly formed nucleated grains. We removed healed.

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

image of Qtz I grains from the damage zone appears in Figure 4d. We will add a reference to this figure at the end of the sentence (line 353).

379 Reviewer: What is phi? Figure 9h is missing. Authors: We defined phi in section “3.1 Fluid inclusions and Mineral Chemistry”, line 191 (Volume fractions of individual fluid inclusions determined as % of the ratio φ=Vv/Vtot (cf. Diamond, 2003). We will add to line 379 a new reference to the definition of the parameter. Figure 9h is not missing. Panel h is presented as a small insert inside Panel g. To avoid misunderstanding we will change the structure of the figure.

394 Reviewer: The ranges in these fluid inclusion values is too large to be interpretable. See general comments above. Authors: We repeat in her the comment we posted to the other Reviewer, Prof. Olivier Vanderhaeghe, who wrote: “The homogenization temperatures and salinities obtained on the analysis of what is presented as a consistent assemblage of fluid inclusions display rather wide ranges of values”. We give here a detailed discussion of this issue in order to critically consider the degree to which our dataset is interpretable in light of the current knowledge. A comparison between the 0-5 wt% NaCleq salinity range reported in our manuscript and that of typical crustal reservoirs is useful on this regard. Figures 1 and 2 presented in reported in Yardley & Graham 2002 show a number of such reservoirs, namely sedimentary and metamorphic fluids from shallow marine basins and continental margin rocks, from which is clear that the estimated 0-5 wt% NaCleq salinity range is not too large as claimed. The salinity-temperature ranges that mark the 1-31 boxes of Fig. 2 of identify fluids from some large reservoirs, like those of the sediments above the Salt Dome Basin of the Mississippi (box 5) and fluids from the Southern and Central North Sea formation waters (boxes 6 and 7, respectively). These fluids were generated in geological environments that cannot be compared with Olkiluoto, and their salinities were determined with techniques that were distinct from those we used here. However, the boxes 16, 18, 19, 20, 25, and 28 of Fig. 2 of Yardley & Graham consistently identify syn- to post-metamorphic fluids from quartz veins that were studied with fluid inclusion micro-

analysis. These fluids can be directly compared with our dataset. It is clear that the fluids that formed the veins within the Waterville Fm. metasediments (box 19) and the veins of the Connemara schists of Ireland (box 25) show bulk salinities that are comparable with the BFZ300 fluid. The others have ranges that are actually larger than that of the BFZ300. Hence, the salinity range determined at Olkiluoto cannot be considered large. In terms of textbook classification (e.g., Pirajno, 2009), the fluids sealed within the FIAs of the BFZ300 can be classified as low-salinity fluids, and their range is comparable to that documented in several metamorphic veins from a large number of geological environments. From Fig. 1 of Yardley & Graham it can be seen that fluids similar to those of Olkiluoto formed the veins of the Shimanto accretionary complex of Japan, the veins of the Otago schists and Torlesse metasediments of New Zealand, and the veins in the Hill End Goldfield of New South Wales-Australia (boxes 11, 13, 14, 33, 15, respectively). In the new version of the manuscript, we will make a comparison between the fluid data reviewed by Yardley & Graham and those of the BFZ300, in order to emphasize this aspect. It is important to stress that although several events of fluid entrapment occurred within the BFZ300, they did not necessarily correspond to sealing of exactly the same batch of fluid. Several generations of distinct – but all low-salinity – fluids must have been inject into the fault and formed the quartz-chlorite assemblage, as suggested by distinct chlorite compositions and textures (see supplementary figure). Regarding the ranges of Thtot measured in the FIAs: we show that the 150-200 °C ranges measured in individual FIAs of the BFZ300 can be explained by an effect of re-equilibration during post-entrapment deformation. The Thtot histograms lack specific modes (line 487 of manuscript) and no statistical data including mean, range, standard deviation, extreme values, etc. could be used to evaluate the regime of post-entrapment deformation. This makes the application of the interpretation criteria of the relevant literature (see reply to comment 190) difficult to apply. Our suggestion whereby the 1-5 wt% NaCleq fluid inclusions homogenizing in the 200-350 °C interval reflect the characteristic properties of the vast majority of damage zone and of the fault core fluids is coherent with the established notion that a number of inclusions survive

virtually intact the modified post-entrapment PT conditions (Diamond et al., 2010; Vityk and Bodnar, 1998). This range is in line with the temperature ranges estimated with the quartz-chlorite and sphalerite-stannite geothermometers. Thus, the strength of this dataset is the number of concurrent geothermometric data, rather than the rigorous validity of the Thtot histograms.

445 Reviewer: 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. Authors: In the captions of Fig. 11b referred to the stannite-sphalerite geothermometer, we say that "The region of the plot that was calibrated with this geothermometer lies between the 250 and 450 °C isotherms. Hence, compositions corresponding to T<250 °C should be interpreted with caution", which is exactly what the reviewer claims. However, to avoid misunderstandings we will rephrase lines 444-445 and we will change the label "220-305°C" in Figure 11 with "250-305°C".

455 Reviewer: I think you mean single-phase aqueous liquid and that the inclusions homogenize into the liquid phase (bubbles disappear also into the vapour phase and at the critical point). Or at least, no vapor-rich inclusions were trapped. Authors: We have in part replied to this comment in a previous section. By reporting "bubble disappearance" we conform to the glossary and terminology proposed by Diamond (2003a, b), which mean that bubbles disappear in the liquid phase. However, to avoid misunderstanding, we will change the contentious sentence to "The fluid was in homogenous liquid phase at the time of entrapment, as testified by the consistent final homogenization into the liquid phase (i.e. bubble disappearance)."

458 Reviewer: 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? Authors: Because the reviewer suggests using the paper by Fall & Bodnar (2018) as a benchmark for our study, we reckon he/she suggests plotting our data like the box and whiskers plots of Figs 4-9, 15, and 20 of Fall & Bodnar. We find these plots extremely useful; however, we think they would not be effective in our study. Box plots are very efficient when large datasets from tens of FIAs must be compared with each other, which is the case of Fall & Bodnar's study; however, they do not work well when microthermometry data from individual natural and synthetic FIAs need to be compared. In their Fig. 16, Fall & Bodnar use actually histograms to show the effects of post-entrapment re-equilibration in an individual FIA from an orogenic Au deposit by making a direct comparison with a re-equilibrated synthetic FIA. In our work we followed the same approach by plotting the histograms of Fig. 10 (recording the minimum and maximum Tmice and Thtot for each assemblage) and making comparisons with the literature data on synthetic FIAs. Thus, histograms are useful in our case. The reviewer claims that there could be many reasons for the wide data spreads of the studied FIAs, one of which would be a supposed mix-up of FIAs. This possibility can be excluded by the fact that the petrographic work carried out before microthermometry is used to map (see supplementary material) and record each inclusion of a FIAs before microthermometry starts. Regarding the primary and secondary FIAs: we have shown that the studied quartz crystals are characterized by a complex network of healed cracks and, indeed, also by many FIAs arranged as trails, cross-cutting and superimposed on each other and apparently arranged in clusters. Hence, we could not confidently identify any primary undeformed fluid inclusions. We only identified a few clusters of primary fluid inclusions within Qtz II (line 382), but they showed irregular and decrepitated morphologies. Thus, microthermometric determinations were not possible in those inclusions and we decided to not emphasize their presence. Secondary and pseudosecondary FIAs are reported in Fig. 10 with the acronyms Type S1, Type

S2, Type S4, and Type PS, respectively. Regarding the bivariate Thtot-salinity plots: as suggested by the reviewer we prepared a single diagram where we reported all the microthermometric datapoints divided for structural domains, in accordance with the scheme we proposed in the manuscript (please, see supplementary material). This new plot is of difficult interpretation, also in the light that our FIAs were subjected to post-entrapment modifications as clearly highlighted by frequency diagrams reported in the manuscript. Moreover, the proposed plot, it excludes fluid mixing within the FIAs based on the scatter of the data. Indeed, the Thtot and the calculated salinities of all the FIAs from BFZ300 plot within large ranges of the diagram and overlap with each other, suggesting that there is no systematic co-variation of the fluid properties that was caused by clear distinct fluids or fluid mixing. The co-variations or correlations mentioned by the reviewer are therefore not clear evident in our samples. However, they are evident in Fig. 3 of Fall & Bodnar, which makes this type of plot for 20 FIAs entrapped within a sphalerite crystal from a vein of the well-known epithermal Zn-Ag-Pb deposit of Creede, Colorado (1: oldest FIA; 20: youngest FIA). It is clear that the 20 FIAs entrapped within the Creede sphalerite define very tight Thtot-salinity intervals, which differ from each other and form a trend starting from a high salinity/high T "end member" fluid (Thtot: 260-270 °C; Salinity: 9-11 wt% NaCleq) and ending with a low salinity/low T end member (Thtot: 200-220 °C; Salinity: 6 wt% NaCleq). Such trend can be explained – at the scale of a single crystal – by progressive mixing of the two end-member fluids that generated the 20 FIAs at progressively later stages of entrapment. All these features are completely missing in the Olkiluoto FIA dataset.

491 Reviewer: 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. Authors: There are of course other ways to interpret our dataset. We stress again that we used fluid inclusions that were the least deformed (petrographically) and combined this dataset with field suggesting fluid overpressure. The maximum values of fluid pressure proposed in our dataset are consistent with lithostatic overpressure at 6 km depth (but see below) and hydrostatic fluid pressure at 16 km depth. However, we also stress two other points: 1) Peak fluid pressure estimates represent minimum fluid pressures during brittle cycles that followed the initial hydrofracturing, in the case of both Qtz I and Qtz II, because they are related to secondary and pseudosecondary FIA. We presented a dataset of 385 measurements, collected from FIAs that showed the less degree of re-equilibration and also arranged as secondary trails with the aim to propose a convincing dataset. Despite the large work on FIA selection, we had some evidence of re-equilibration in our dataset, also in the samples from the damage zone, which should be the less deformed in the light of the fault localization and documented microstructures. 2) In a strike-slip system, with vertical $\sigma2$, the fluid pressure can only be as high as the $\sigma3$, which is typically less than $\sigma2$. This means that the estimate of 6 km depth for lithostatic overpressure is only the minimum depth at which hydrofracturing occurred in our fault.

496 Reviewer: 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. Authors: The isochores reported in Fig. 12 are based on homogenization temperatures and salinities. In the figure caption we say that the light-coloured areas represent the T/P ranges of FI trapping considering the entire values of salinity and

homogenization temperatures measured for each structural data. On the other hand, the darkest areas are the T/P ranges of FI trapping estimated from the most statistically representative salinity of the pristine fluid (0-5 wt% NaCleq). However, we did not take in consideration the Th spread because of the impossibility to determine a clear mode in the Th curve. The 0-5 wt% NaCleq salinity range is given considering that 80% of our salinity measurements are within this range. Using this statistical approach, we were able to reduce the uncertainty about the fluid pressure. This is also why we decided to report in the figure the ranges constrained from the entire dataset, without salinity filtering, and also areas with the most probable salinity. Moreover, of course the two geothermometer applied are used for pressure correction purposes. Accordingly, we will also reword text and legend.

509 Reviewer: 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? Authors: The presence of multiple generations of quartz veins (which are characterized by different mineral phases) documented in the field and the evidence of mutually overprinting brittle-ductile structures suggest episodic fracturing and flow within the system, followed by a mechanical hardening of the system. In light of the Reviewer’s comment, we will add to the previously presented text “As also independently illustrated by microstructures described above, we propose that. . .”

517 Reviewer: 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)? Authors: We already showed and commented the data of Yardley and Graham (2002).

523 Reviewer: 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. Authors: We agree with the reviewer that microstructures

and fluid inclusions alone are insufficient to demonstrate faulting by hydrofracturing. Our interpretation of successive (at least two) faulting events steered by overpressure conditions is mostly field based. At the outcrop scale, the fault walls are completely separated by major veins, only later reworked by cataclastic bands and recrystallization. Also, the quartz-veins cut at high-angle the foliation instead of opening along dilatational jogs, suggesting an active role of the fluid phase in forming the fault. As to the possibility that the BFZ300 is a large-scale dilatational jog, we refer to the schematic representation reported by Skytta and Torvela (2018) in their Figure 5c. In that figure, they reported also the BFZ300. However, if the BFZ300 represented a dilatational jog linking BFZ045 and BFZ100, it should have had a sinistral kinematics, which is not consistent with field data. Therefore, we conclude that fluid overpressure was needed to form the structures observed in this study.

532 Reviewer: Possibly, but I don't see the clear supporting evidence. Authors: Cementation by fluid precipitation in open fractures (or in fracture meshes) causes hardening of the system. This is also the base concept used to explain and justify the formation of crack-seal structures.

537 Reviewer: Please cite some more recent geologically work on this topic (1921!). Authors: We cited this work because it was the first proposing this mechanism of fault growth. In accordance with the Reviewer's request we will also insert the reference to Sibson (1996) and Peacock (2017).

615 Reviewer: 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. Authors: We used the presence of a pseudotachylyte vein in the sinistral conjugate fault (object of the Part II of this study) as possible but valid evidence of seismicity in the studied system (Cowan, 1999; Rowe and Griffith 2015). At the moment, there is only a single abstract presenting data about pseudotachylyte vein in the studied system (Menegon et al., 2018,

cited in the paper references). However, the complete separation of fault walls during hydrofracturing and fault wall implosion suggest a fast stress drop, which is likely to be associated to seismic failure (e.g. Sibson 1985, Nature). Moreover, fluid-overpressure is believed to extensively occur at the nucleation depth of earthquakes (e.g. Saffer and Tobin, 2011, Annual Reviews of Geophysics). In addition to that, earthquakes (and micro-earthquakes) are widely associated to artificial fluid injections (Healy et al., 1968, Cox, 2016 and references therein). In the light of this, we think is reasonable to discuss the possibility that the studied fault was seismically active at the BDTZ.

625 Reviewer: 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. Authors: Ellsworth (2013, Science) reviews a large number of studies dealing with fluid-induced seismicity. In particular he highlights that largest earthquakes in central Oklahoma and elsewhere are triggered by injections of very large volumes of water that communicate pressure perturbations directly into pre-existing (and likely critically stressed) basement faults. We discussed this case of fluid pressure perturbation on a pre-existing fault in the previous sentence. Ellsworth (2013) and other studies (e.g. Cox, 2016; Rubinstein and Mahani, 2015 among many others) report that during fracking operations of intact reservoirs (i.e. without large critically stressed faults) swarm seismicity, without well defined mainshocks, is normal. We are simply evaluating an additional hypothesis about the seismic behaviour of our fault system in the light structural characteristics of fluid-rich faults (Cox, 1995). 771 Reviewer: Abstracts should be avoided as citable source. Authors: We have already partially replied to this comment in the previous section. We generally agree with the Reviewer about this topic, but we cited this abstract because at the moment there are no published data about the sinistral conjugate fault associated to BFZ300. The sinistral fault hosts a pseudotachylyte vein, suggesting its seismic character. The sinistral conjugate is object of the Part II of this study (Prando et al., in prep.).

Figure 1 Reviewer: 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. Authors: We indicated the study site with a light green star. We will change the colour of it. From our point of view, Figure 1a is useful to insert the study area in a regional geological framework, which is also briefly introduced in Section 2- Geological Setting. Regarding Panel b we accept the suggestions proposed by the Reviewer and will change the figure in the revised version of the paper.

Figure 2 Reviewer: 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. Authors: We agree with the Reviewer and have decided to split the Figure in two. New Figure 2 will show: I) mesoscale observations; II) enlarged version of the stereonet; III) two panels with horizontal striae documented in the field. These are added also in light of the Reviewer's suggestions and comments. New Figure 3 will only represent the entire scheme of the fault with its details (Panels b-g of old Figure 2). As already commented in the previous section, we prefer to maintain our definition "quartz-filled joint" to highlight that they are joints, without lateral displacement.

877 Reviewer: correct astep Authors: Corrected with "At a step-over"

Figure 9 Reviewer: Figure h is missing Authors: Panel h is not missing. Please, see also comment for line 379.

Figure 4d Reviewer: Too dark. Authors: This is a cathodoluminescence image and we are not able to change the quartz luminescence. We may try to carefully brighten the image with a common graphic program.

Figure 5c Reviewer: Do you have a CL image that confirms crack-seal? Authors: CL image for Qtz I from the damage zone is in Figure 4d. Please, see also the comment above.

Figure 11 Reviewer: last sentence: see comment for line 445 Authors: We accept the modification and we will change it also in the caption.

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Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-5/se-2019-5-AC3-supplement.pdf

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