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Interactive comment on "Fluid-mediated, brittle-ductile deformation at seismogenic depth: Part II – Stress history and fluid pressure variations in a shear zone in a nuclear waste repository (Olkiluoto Island, Finland)" *by* Francesca Prando et al.

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We thank Georg Dresen for his constructive comments and suggestions, which will allow us to clarify and improve aspects of the manuscript. We agree that a more concise manuscript will make for easier reading and better communication of our key observations and interpretations.

Based on the points made by both reviews, we will restructure the paper with a clearer

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focus on the microstructural evidence of the increase in stress towards the centre of the fault zone, which is provided by the EBSD and grain size analysis of quartz. We agree that the conceptual model that we presented in the first version of the manuscript remains somewhat speculative in the absence of tight constraints of the P-T evolution experienced by the fault. As we reckon that we cannot improve significantly the P-T estimates (especially those of the cataclasites), we will focus the discussion more on the possible causes for the increase in stress and strain rate towards the shear zone centre and will reduce the section on the conceptual model of fault zone evolution previously presented.

Below we include a point-by-point response to the reviewer comments outlining our plans for the associated changes of the manuscript.

R: The authors provide a detailed structural investigation of a shear zone subjected to brittle overprint. The study is based on several core samples drilled through a mylonitic shear zone that shows brittle overprint of ductile deformation. The paper seems too long and could be more concise and organized. The observations are of interest to readers of SE and eventually deserve to be published. Here are a few comments: The paper is too long in general and should be shortened significantly. In particular, the introduction could be more to the point.

A: In the revised version of the manuscript, we will reduce the length of the first three introductory sections. The introduction (section 1) will summarize the current knowledge on the microstructural evidence recording stress variations during brittle-ductile deformation, to present the main focus of the manuscript. The geological setting (section 2), will be more concise on the regional deformation history of Olkiluoto, with specific reference to the existing literature in regards of the ∼1.8- 1.75 Ga time range, directly relevant to our study. Evidence for brittle faults exploiting ductile shear zones being a characteristic at Olkiluoto (e.g. Skyttä and Torvela, 2018; Marchesini et al., 2019) will be emphasised. The methods (section 3) will also be shortened, with some of the discussion of the limitations of the applied techniques moved to the supplementary materials.

R: As for the discussion, that could be significantly shortened as well since large parts are pure speculation and over interpretation not supported by observations.

A: We suppose that with over-interpretation the reviewer is referring to the discussion on the temperature estimates in section 5.1. Given the limitations of the chlorite and graphite thermometers (Beyssac et al. 2002, Vidal et al., 2016, Kirilova et al. 2018), and the lack of dating for the observed deformation events, we are aware that P-T conditions for the cataclastic event could not be fully constrained. To present the observed structural evolution of the studied fault zone we integrate our observations with regional constraints for Olkiluoto (Aaltonen et al., 2016 and reference therein). No specific study exists for the Olkiluoto area to better constrain exhumation occurring after the Svecofennian orogeny and this aspect of the regional geological evolution of south-west Finland is still open to several interpretations. In the discussion, we wanted to convey the limits on data availability and consider broader tectonic constraints. In proposing a wider regional model for the shear zone evolution we perhaps, with hindsight, obscured those interpretations that are supported directly by our new data and observations. To avoid misunderstanding of the main points of the discussion sections we will be careful to present separately the interpretations of the fault evolution based on microstructural observations and the derived model of stress evolution.

R: In their analysis the authors focus on quartz microstructure in host rock mylonite and cataclasite. It would be interesting and potentially helpful to know what potential differences exist also for other main minerals such as feldspar or micas.

A: The focus on quartz is due to the observed microstructures and mineral assemblage in the fault zone. Feldspars in the mylonite present brittle behaviour, with domino-type fragmented porphyroclasts, typical for a mylonite deforming under greenschist facies conditions. Evidence of ductile deformation of feldspar was not observed in the mylonitic foliation itself. We described neocrystallisation in the mylonite, which typically

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occurs in the pressure shadows around feldspar porphyroclasts (presumably reflecting dissolution-precipitation creep). Mica growth is parallel to the mylonitic foliation and form less than 10% of the host rock and mylonite, but may reach up to ~20% in the cataclastic matrix. Whilst we agree with the reviewer that considering the bulk rheology is a very important aspect when trying to estimate the flow stress, we think that assuming quartz as representative of the bulk rheology is appropriate here, given the low abundance of the micas and the microstructures of the feldspars. In revising the manuscript we will be more specific on the description of the observed microstructures that lead to the choice of focusing on quartz.

R: Based on the presented observations and thermal constraints for the different or possibly overlapping deformation episodes there remains significant doubt that the conceptual model presented in the discussion is sufficiently warranted. In particular temperatures in the cataclasites are not well constrained and the question arises to the innocent bystander if some of the deformations has to attributed rather to a much younger brittle overprint, possibly even due to reactivation during postglacial uplift.

A: Microstructures and quartz grain size variation clearly document an increase in flow stress towards the fault zone centre, which is eventually overprinted by the brittle fault core (cataclasite and pseudotachylyte). We agree, however, that the P-T estimates are not sufficiently robust to constrain the formation of the observe cataclasite. The parallel orientations of the stretching lineation and slickenline lead us to interpret that the cataclasite formed during a continuum of brittle and ductile deformation in Olkiluoto, rather than as a brittle overprint as part of a distinct, younger tectonic event (Mattila and Viola, 2014). Temperatures constraints of chlorites in the cataclasite matrix are not precise enough given the small grain size was close to the resolution limits of the electron microprobe. Overall, we are confident of the P-T constraints obtained for the ductile deformation, and we recognize the limitations that the poorer P-T constraints for other structures place upon our model.

R: The authors seem to contradict themselves when they exclude that the brittle deformation features where not subjected to ductile overprint but in the discussion forward the idea that ductile deformation was punctuated by brittle ‘events’.

A: There is a misunderstanding due to poor phrasing on our part. What we meant is that the vein emplacement, a brittle event, took place under ductile conditions and that the cataclasite, representative of brittle deformation, was not subjected to ductile overprint. We will rephrase the discussion in the revised manuscript to make this much clearer.

R: As for the grain size variations within quartz domains. There is no discussion of the potential effects of second mineral phases. It is also unclear, to what extent the cataclasites exploit the presence of fine-grained layers if at all.

A: We are aware that second phase pinning can have a strong effect on the grain size estimates. For this reason, the EBSD maps used for the grain size analysis were collected from monomineralic quartz layers, in order to avoid underestimates of the grain size due to pinning effect. We are confident that our grain size estimates are robust because the relict grains contain subgrains of the same size of the recrystallized grains. Thus, post-recrystallization grain size modifications (either grain growth or grain size reduction) did not affect our samples.

R: To conclude, I suggest radically shorten introduction and discussion focusing strictly on what is really supported by the observations. As suggested, I would also analyze the deformation microstructures of other main phases. I hope these, at this stage, rather general comments are useful to the authors.

Again, we thank Professor Dresen for his constructive comments and anticipate a greatly improved revised manuscript will be submitted along the lines indicated above.

Vidal, Olivier, Pierre Lanari, Manuel Munoz, Franck Bourdelle, and Vincent De Andrade. "Deciphering temperature, pressure and oxygen-activity conditions of chlorite formation." Clay Minerals 51, no. 4 (2016): 615-633.

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