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Interactive comment on "The competition between fracture nucleation, propagation and coalescence in the crystalline continental upper crust" *by* Jessica A. McBeck et al.

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The manuscript entitled 'The competition between fracture nucleation, propagation, and coalescence in the crystalline continental upper crust' by McBeck et al. aims to illuminate the nucleation, growth, and coalescence of micro-fractures in crystalline rock prior to sample-size shear failure, and how this 'road-to-failure' varies in the presence/absence of pore fluids. To do so, three shear failure experiments (2 dry, 1 saturated) were conducted in a triaxial vessel, whilst obtaining a full 3D X-ray tomography model at set intervals of differential stress up to sample failure. The 3D X-ray tomography data was analysed to obtain measures for microfracture nucleation, propagation,

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and coalescence. The main conclusion of the manuscript is that under fluid saturated conditions, microfractures tend to propagate more in isolation rather than coalesce with nearby microfractures relative to the dry case. This interesting conclusion based on the observations and excellent data analysis warrants publication, but the manuscript first needs to address a number of significant problems. These problems comprise the lack of clarity on the main aim of the manuscript, and a somewhat tedious discussion section with unclear/inconsistent arguments. I hope that my comments below will be of help to improve the quality and the originality of the manuscript.

Major comments:

1. The main aim of the manuscript is not clear: Is it the ‘road-to-failure’ with an additional step in the X-ray tomography data analysis (i.e., quantification of fracture coalescence), or is it trying to elucidate the difference in pre-failure deformation for dry and saturated conditions? This duality is making it difficult to follow, especially in the introduction and the discussion sections of the paper, and the authors may wish to rethink this. Moreover, the ‘road-to-failure’ has been studied and presented by (some of the) authors in other recent manuscripts, partly on the same dataset, and the additional data analysis step feels like a somewhat meager addition to these previous works. I feel that the observations on microfracture development with/without fluids does contribute more significantly to progressing our understanding of brittle rock deformation between the yield point and sample-scale failure, and so I recommend to emphasize this as the main aim of the manuscript (studied with the approach of measuring of fracture coalescence, propagation, etc.).

2. Following on this, the title does not cover entirely the content of the manuscript, and should contain some mention of the dry vs. saturated conditions.

3. I believe that (part of) the data has been presented in other manuscripts, so it should be clarified better what the innovative aspect is of this manuscript in relation to previous ones. Also, indicate which sample data sets have been presented in which manuscripts

before.

4. The methodology section 2.1 is very short and lacks some basic experimental information: What deformation rig was used, what is the voxel size, what was the axial loading rate, how was axial shortening determined, and how much time was allowed for the pore fluid pressure to equilibrate across the sample prior to the onset of loading, and in between load steps?

5. Line 145: The authors may want to add some clarification on the meaning of nucleating fractures in their data: It seems to me that the fractures that appear within the resolution of the X-ray data at each step may have been there the previous step as well, only not detected due to their size/small volume. It is most likely that they nucleated from a preexisting defect (grain boundary, cleavage plane) that initially had no volume to begin with. This does not hamper the analyses here, but it would clarify that the term nucleation used here is somewhat relative to scale/resolution, and does not describe fractures forming out of the blue. This brings up the interesting point as well on what is actually measured: The volume of microfractures. Do the authors think there are many 'hidden' pure shear microfractures without much opening (i.e., volume) in their data?

6. The authors attempt to explain coalescence from a linear elastic fracture mechanics perspective, with the hypothesis that fractures near each other are more likely to grow because their fracture tip stress concentrations interact. This is introduced first in section 3.4, and further expanded upon in section 4.4. This hypothesis is not well explained or quantified: It seems to me that such an interaction depends on the length of the fractures involved (longer fractures, larger stress intensity) and on their orientation, as well as on the exact stress fields around them (e.g., mode-II fractures have reduced and increased stresses near their tip, whereas mode-I fractures do not). So I am not sure if I understand well or agree with line 208: 'The observations match the expectations of LEFM'.

Secondly, contrary to this hypothesis is the statement in the introduction that LEFM cannot explain well fracture coalescence (line 30-35), so why choose this as a framework to explain the observations on coalescence?

7. Section 4.2 discusses the competition between fracture nucleation and isolated propagation. This is somewhat tedious because the authors elect to use the analogy of sandstone deformation and models designed for layered sedimentary sequences for crystalline low porosity rock. I do not feel this is very informative: Triaxial deformation of granular aggregates is very different from low porosity rock, and the step from a small-sized crystalline rock sample to a sedimentary basin feels like a leap. Most discussion is summarized in the last paragraph of this section: This competition seems adequately explained by the fracture length dependent stress intensity factors, so that a few growing fractures shield shorter (nucleating) fractures. As a suggestion, the authors could analyse the lengths of the fractures in loading-parallel direction in their data to provide a somewhat more quantitative argument here.

8. I feel that Section 4.3 contains some similar problems as discussed above: The sandstone analogy is not a very helpful argument to explain competition between isolated fracture propagation and fracture coalescence in crystalline rock, and neither is fault damage zone evolution: The presented data is on an initially intact sample without a pre-existing fault zone. Further irrelevant excursions include the 316-318 on dilation in gouge materials.

9. Section 4.3 contains, in my opinion, the most interesting discussion: The influence of fluids on microfracture evolution prior to sample-sized failure. The authors first discuss the different confining pressures on all three samples as the source for the different microfracture evolution, and rule this out as a conclusion. Second, the chemical effect of water on crack propagation is discussed, followed by a discussion on the mechanical effect of a pressurized fluid. The authors conclude, rightly so, that these last two effects cannot be distinguished from each other and future research is necessary. I largely agree with the line of thought and the conclusion of the authors, but there are a few caveats and/or additional points that need to be addressed, starting at the level of the

experiment: How does the sample size (4x10 mm) influence the reproducibility of the experiments, especially given the relative large (450um on average) grain size of the material (for instance, some grains seem to have dimensions of > 1mm in the 3D CT models)?

Secondly, dilatancy hardening is presented as a mechanism to influence microfracture evolution, but how specifically is not clear. Dilatancy is often discussed on the scale of cm-size (and larger) shear fractures or fault planes, where fault roughness and microfractures around the shear plane accommodate the dilation. Here, the microfracture regime does not yet have such a centralized structure, but could it be possible that larger and coalescing microfractures have a larger dilatancy rate than smaller fractures, so that the former are more affected by dilatancy?

On stress corrosion: The authors could try to include a back of the envelop calculation on how long it takes for fluids to reach the crack tips during and after a deformation step – i.e., are the crack tips wetted during propagation, and has the pore fluid pressure equilibrated within the sample? I have measured hydraulic properties on these monzonites that may prove helpful to measure diffusion times. ("Variation of Hydraulic Properties Due to Dynamic Fracture Damage: Implications for Fault Zones", JGR 2020)

10. Maybe I have missed it, but I could not find an in-text reference to Figure 7.

11. I feel that there is some overlap in the discussion at the end of section 3.4, and the discussion on LEFM and coalescence in section 4.4. Consider cutting the part in section 3.4.

Minor comments

12. Line 31-32: I do not think this statement is correct: LEFM is scale-independent.

13. Line 38: Successful in what?

14. Line 40: Clarify what is meant with the mode of failure.

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15. Line 60-61: Does this not depend on whether the reaction is diffusion or precipitation controlled?

16. Line 108: The fractures have been simplified as ellipsoids, how realistic is this shape, especially for coalesced fractures?

17. Line 122: In the description of how fractures are tracked from one X-ray dataset to the next, would it be clearer to speak of fracture volume instead of fracture?

18. Line 126: Insert 'to' in between step and those.

19. The figure references are somewhat chaotic: I would refrain from referring to the figures until the results section, and not refer to results figures in the introduction.

20. Line 138, line 227: The term elastic is not correct here, because unloading at this point would not reproduce the near-horizontal stress-strain curve.

21. Section 3.1: How did the samples look like post-failure? Did they exhibit a single shear fracture at a 30-degree angle to the loading axis?

22. Line 147: Do I understand correctly that nucleating fractures from a previous load step are counted as propagating fractures in the next step (i.e., the nucleation counter is set to zero)?

23. Line 150: Repetition from section 2, can be removed.

24. Line 159: Was the volume of fluid expulsed from the pore pressure pumps measured? If so, do they match with the volume increase inferred from microCT?

25. Line 165: Are the exponents of the increase comparable between all three samples?

26. Line 170: develop a → used our developed (the method was already explained in section 2).

27. Paragraph 184-196: The technical part of how to define near and distant fractures

should move to the method section. Also, how were closing and growing fractures defined?

28. Line 217: in during → in.

29. Line 322: The word 'analyses' in this context suggests some calculations/quantification. Maybe 'Discussion'?

30. Figure 4: Would it be possible to indicate the four deformation stages here? Showing the fracture volume in mm3 instead of voxels would make it easier for readers to extract dilation rates. In panel (c), would it be possible to have the same scale, so that the logarithmic trends are easily comparable (also for figure 5b)?

31. Some referencing is incomplete or skips over classic papers:

- line 28: The development of microfracturing with stress was already well documented by earlier studies than those cited here, especially from the 70s onward. For instance, Tapponier & Brace, 1976 have performed excellent microstructural work on this (see Paterson and Wong, section 5.7.4, for more refs).

- Line 223-224: This statement is not correct: mechanical data, AEs, and microstructures show the development of microfracture networks past the yield point; Brace, Paulding, and Scholz 1966 inferred microcracking to be responsible for significant pore volume change during loading; Tapponier & Brace, 1976 and Wong 1982 show microstructures; AEs by Scholz 1968. These are examples of the older, more classic works that show this.

- Line 316: Martin III, 1980 ("Pore pressure stabilization of failure in westerly granite") may be more relevant here, as it shows the phenomenon in crystalline rock.

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