Topical Editor Decision: Reconsider after major revisions (20 Dec 2020) by Andre R. Niemeijer

Comments to the Author:
Dear authors,

I have now received 2 reviews of your revised manuscript, one original and one new and both reviewers have similar comments about the work presented.
It is clear that the work presented is appreciated by both reviewers and warrants publication, but some revisions are needed to make the manuscript acceptable. Specifically, both reviewers question the statement of a competition of fracture nucleation, propagation and coalescence and rather view the process as an evolution. Additionally, both reviewers indicate that the extrapolation of the presented results to the crystalline upper crust, i.e. many other rock types, is not justified.
I encourage you to revise your manuscript according to the excellent comments and suggestions of the reviewers.

Dear Editor Niemeijer, Dr. Aben and Dr. Reches,

Thank you for these helpful comments. We have made significant changes to the discussion sections, following your suggestions. Regarding the semantic arguments about the meaning of "competition", please see the responses to comments #8 and #17. We respond to your concerns point-by-point below in bolded font. We numbered the comments for clarity and indicate where we modified the manuscript with Word Document comments that contain the corresponding comment number (i.e., C#01 for comment #1).

Best,
Jess McBeck

Review #1

I have read the revised version of the manuscript entitled "The competition between fracture nucleation, propagation, and coalescence in dry and water-saturated crystalline continental upper crust" by McBeck, Zhu, and Renard, and their responses to the reviewer comments. The authors have made substantial improvements based on the comments provided by reviewers and editor, which have improved the readability and clarity of the manuscript, particularly the aim and the conclusion. However, I have comments on some of the author's responses, primarily on the discussion section which remains overly cumbersome and distracting due to many tangents and unnecessarily cited literature. I have detailed my comments below.

Kind regards,

Frans Aben

1. Comments 7 & 8, Section 4.1, and majority of section 4.2: On the excursions to literature on other lithologies and larger scale systems:

   I understand the reply from the authors to some degree, but I continue to feel that a disproportionate part of the discussion is devoted to this generalisation and is distracting to the story. I support the idea of the authors on: "Our general view is that rock deformation analyses benefit from reasonable generalization between different rock types", but the data presented in this study simply cannot help to achieve such a reasonable generalization without doing (or finding in literature) similar types of analysis on different rock types. Without this, the discussion will remain qualitative and "hand-wavy" and does not contribute to informing the reader on the main aims and conclusions of the paper. This may result in an enumerating literature study rather than a focussed scientific manuscript. I suggest to keep these discussions short and concise rather than write lengthy paragraphs, and use analogous sparingly.

First, for section 4.1, the last paragraph should be placed after the 1st sentence of section 4.1; it adequately explains why isolated propagation is favourable to nucleation of smaller flaws. I understand from lines 258-276 that the authors attempt to discuss the initial flaw distributions in other rock types, and how that may influence the outcome measured on monzonites? This discussion does not provide an informative conclusion (line 268), and may at most provide a hypothesis for future experiments (line 275). I could not be convinced on how the discussion on the amount of stress concentrators in sandstones is germane to the main outcome of this section that isolated propagation of larger fractures is favourable to nucleation of smaller ones in crystalline rock.

The next paragraph continues the discussion with sedimentary volumes (with different lithologies, different scales, and different boundary conditions relative to the experiment) as an analogue to the experiment. I do not see clearly how this serves the main explanation of why isolated propagation is favourable to nucleation of smaller flaws; as the authors state, this is well explained by LEFM and examples from the LEFM literature may serve as better analogues/examples (e.g., Weibull theory).

We have now shortened this section (4.1) to focus primarily on fracture development in crystalline rock. We have removed the discussion of crustal sedimentary sequences.

We continue to think that this manuscript benefits from describing the link between this work and the previous work on fracture development in sandstone. The link between these analyses is that both aim to understand the driving factors of fracture nucleation and propagation, and compare the dominance of these behaviors. In particular, we conclude section 4.1. with the concrete statement "in a given sandstone volume there will likely be a greater number of sites of significant stress concentrations than in a monzonite or granite volume, and thereby a larger number of sites suitable for fracture nucleation. Consequently, we may expect a greater dominance of nucleation in sandstone and other rocks with strong strength heterogeneity than observed in these monzonite rocks". We think it is valuable to mention how our results (the proportion of fracture propagation relative to nucleation) may differ between crystalline rock and granular rock.

2. For section 4.2:
   Line 306-316: Here, the authors describe the fairly well established evolution of macroscopic failure evolving from tensile to shear with increasing confining pressure. Why is it important to understand this, and the relative proportion of shear and tensile deformation, in light of the results that were obtained before macroscopic failure?

   We think that this topic is relevant for this study because it highlights a well-recognized link between confining stress and fracture network development. In the same way that this study finds a link between confining stress and the proportion of propagating vs. coalescing fractures, previous work has

observed a link between confining stress and the proportion of shear vs. tension. We have added an additional topic sentence describing this link more explicitly.

In addition, we think that this topic is relevant because the mode of deformation of the fracture (i.e., proportion of tension vs. shear) may help determine whether it propagates in isolation or coalesces with a neighbor. We provide further details in response to the next reviewer's comment (#3).

3. Line 320: Why does a tensile fracture enable greater access to preexisting fractures than a shear fracture? Line 322: Why do mixed-mode fractures have a larger surface area than shear fractures? Is their roughness larger? How does that relate to the aperture?

   We hypothesize that a tensile fracture may provide greater access to preexisting fractures because opening likely (necessarily?) increases the fracture aperture, as hinted by the reviewer. We have modified this paragraph accordingly to specify the link more clearly.

4. Line 323: I am not convinced that fault damage zones need to be mentioned here: A fault zone is a shear fracture that may be compared to the macroscopic shear fracture developed at failure in triaxial experiments. The analysed fractures in this study are all tensile, near-zero offset microfractures, so do not directly compare with a shear fault. At failure, the macroscopic shear rupture and subsequent slip will create additional microfractures surrounding the shear fault by dynamic transient stresses and slip over a rough interface, but these microfractures damage zone (or meso-fracture damage zone in the field) have few to do with the pre-failure microfractures studied here.

   We have removed this sentence, as suggested.

5. Line 345: Saturated gouges: These are shear systems, opposed to mode-I opening of microfractures. Gouge-filled fault systems with dilation may be described not by fracture mechanics, but by frictional processes.

   We have removed this point, as suggested.

6. Comment 4: What was the axial loading rate, how was axial shortening measured? Loading rate is an important parameter, as it may influence strength and whether the system will be (partially) undrained during a load step.

   We have now added this information.

7. Comment 9: The comment on the representative elementary volume has been addressed by the authors, but I would recommend to remove the general remarks on the existence of an REV for softening materials and the upper limit of a REV for glass beads – both are not applicable to the rheology tested here. Without these remarks, the authors already show that they have considered this problem, and support the reproducibility by previous work.

   Corrected as suggested.

8. On some linguistics: The authors aim to track which mode of fracture network development is dominant as a function of axial load, presenting it as a "competition". I am not convinced this should be presented as a competition, as one mode of

fracture network development naturally leads to the next: If all fractures continue propagating in isolation, at some point the fracture population will have grown to fracture lengths where it is not possible anymore to stay within isolation, and all fractures are near to each other. This eliminates the mode of propagation-in-isolation. Similarly, when sufficient fractures have reached a substantial length, nucleation of smaller fractures becomes unfavorable as the longer fractures "shadow" them (this is all explained in the manuscript as well). Thus, in my view, rather than a competition between modes, it is an unavoidable sequence of modes as a function of load that have some intervals of differential stress in which both modes may contribute to fracture network evolution before one of the modes is eliminated by evolving geometrical properties (fracture length, fracture spacing). This sequence seems, pardon the pun, set in stone, so that the "winner" of the "competition" is known, so is it not a sequence rather than a competition, with the main aim of quantifying in terms of load the transition from one mode to the other?

The central focus of this paper is to "investigate the relative contributions of three endmember deformation modes to fracture network development" (line 79-80). According to Oxford Languages dictionary, a competition is "the activity or condition of striving to gain or win something by defeating or establishing superiority over others". So a competition can be defined as any situation in which the expression of one behavior/characteristic limits the expression of another. Because we categorize the modes of fracture growth into three non-overlapping modes, the success of one limits the success of the others. For example, if a fracture is propagating in isolation, it necessarily is not coalescing. We agree that one fracture can transition between these modes, but it need not experience all three, as suggested by the reviewer. Our experiments show that a fracture can nucleation and then grow in isolation, but never coalesce with another fracture. We are thus interested in how fractures transition between the modes, and the evolving dominance of these modes. Because the modes are non-overlapping, at a given point in time in an experiment, we are able to quantify which fracture development mode is the most dominant, and thus is "winning" the competition. Thus, the sequence from nucleation to propagation to coalescence may also be framed as a competition with a different winner at various stages of deformation/differential stress. And this analysis further demonstrates that this winner changes due to differential stress and the inclusion/exclusion of fluids. Thus we think it is appropriate to frame this analysis, and comparison of the dominance of varying modes of fracture growth, as a competition.

9. Line 24: shortly before failure close to the peak stress

Corrected as suggested.

10. Line 9: Specify what behavior. Also, state in the first sentence that the paper looks at fracture development in crystalline rock.

This sentence states: "The continuum of behavior that emerges during fracture network development may be categorized into three endmember modes: fracture nucleation, isolated fracture propagation, and fracture coalescence.". Thus, the type of behavior is listed in detail after the colon.

We have now modified the sentence to specify that this work focuses on fracture development in crystalline rock.

11. Line 37: The word “struggle” implies to a reader that LEFM tries to describe interaction between fractures, but fails at it. Since LEFM does not attempt to describe this at all, I suggest to replace it by “does not”.

Corrected as suggested.

12. Line 38-39: The transition from dispersed to localized networks: This is not very clear, and may need some additional explanation. First, what is the driving force for evolving a network of fractures (e.g., continuous deformation, thermal cracking, etc), and which one will this study target? Second, how can a distributed disperse network become a localized network of connected larger fractures? What happens to the smaller fractures from the dispersed state that did not develop in larger fractures, are they healed or do we zoom out to the scale of larger fractures only for the localized network, ignoring the smaller fractures?

We have modified the manuscript to describe the process of localizing fracture networks in greater detail, and what factors control this evolution (i.e., the driving force). We only briefly describe these processes in the introduction, and go into greater depth in the discussion.

13. Line 85: Methods –> method

Corrected as suggested.

14. Line 193: stage VI stage IV

Corrected as suggested.

15. Line 231-234: This part of the data analysis should be mentioned in section 2 (method section).

We prefer to describe the specifics of this analysis here, rather than only in the methods section as the reader could forget these specifics by the time they reach the results.

16. Line 245: Be aware of the positive feedback through fracture length, which essentially does not allow for “far” fracture couples to exist anymore!

We have now described this caveat explicitly, as suggested.

Review #2

Dear Dr. Niemeijer,

This manuscript presents an experimental analysis of microfracture development in a low porosity rock under dry and wet conditions and under confining pressure. The authors systematically presented the methodology and the experimental observations of the state-of-the-art technique. The authors attempt to simulate in-situ conditions in the upper crust. This is an important topic with significant implications to rock mechanics and natural fluids production. While the topic is important, the paper suffers from a few central weak points that need to be revised. As I worked on related topics, my revision is somewhat biased, and I apologize for the frequent self-citations.

Ze’ev Reches

Major comments:
First to the good parts. The experimental approach, procedures and observations are carefully described and explained. While the technique is non-trivial, the description also refers to previous publications as expected. This is the central core of the work, and should remain intact. The experimental observations provide a unique, quantitative perspective of rock dilation processes under in-situ conditions of the upper crust in terms of confining pressure and water presence. The methodology is most suitable for such important problem, and it is suggested to limit the introduction and interpretation to this topic. While this strength of the analysis is clear, the authors attempt to give the impression that the paper delivers more that it actually can. Some suggests are listed below.

17. One issue is reflected in the title that reads: "The competition between fracture nucleation, propagation and coalescence in dry and water-saturated crystalline continental upper crust." This needs to be revised including the related discussions of "competition" throughout the paper. Note these two main reasons.

    This study presents the "evolution" of microcracks in experiments, but it does not present a "competition" between processes. The "competition" point is also a major issue with the interpretation throughout the paper. To claim that two (or more) processes compete with each other, the authors have to quantify and compare the processes on the basis of mechanical quantities like stress, strain or energy. The paper presents the evolution with general statements with no mechanical analysis. In this respect, it is similar to Reches (1988) (citing myself, apologies) that described the "Evolution of fault patterns in clay experiments" in terms of time/deformation evolution of the faults without mechanical analysis. Mechanics is mentioned in the discussion in general terms as a potential interpretation for fault propagation. Later, Reches and Lockner (1994) presented a detailed stress analysis of microfracture evolution. In summary, the authors present well documented evolution history of nucleation, growth, dilation, and coalescence of microfractures, and they speculate about the controlling mechanisms. Competition is not analyzed.

    With all due respect, the analysis is limited to four samples of 0.4 cm diameter of rock with 0.045 cm mean grain size, and this is perfectly fine. However, claiming that these observations are valid for the "..crystalline continental upper crust" without an quantitative scaling attempt is not justified.

    For these two reasons, an appropriate title could be something like: "The evolution of nucleation, propagation and coalescence microfractures in dry and water-saturated crystalline rock"

    The use of the term competition is justified in this work because we categorize fracture network development into non-overlapping modes. Describing the varying dominance of these modes as a competition does not require that we compare them in terms of stress, strain, or energy. Instead, we compare them in terms of fracture volume and number, and thus use a well-constrained quantity, rather than the influence of these fractures on the internal stress, strain or energy field, which necessitate far wider error bars than our calculations of fracture volume and number.

    In addition, please see the response to comment #8 of the first reviewer above.

    We have modified the title accordingly by removing "continental upper crust" so that we do not imply that we have considered the influence of scaling.

18. The present experimental method is an excellent tool to monitor dilation by microfractures, and the authors clearly demonstrated this capability by the number of microfractures and the associated global dilation (Fig. 4-6). However, the present method is ‘blind’ to shear fractures unless they associated with dilation, for example, wing-cracks with dilating fractures at both ends of a shear microfracture. The evolution of shear microfractures was analyzed extensively by acoustic emission (Lockner and many others), as well as by thin-sections mapping of multiple rock deformation stages (e.g., Katz and Reches, 2004). The authors carefully, and correctly, use only the term fracture, which is commonly (not exclusively) applied to extension fractures, and the authors correctly did not refer to faults, joints or shear fractures in their experiments.

    This inherent limitation of only dilation detection by this experimental technology can be partly eliminated by mapping and inspection of the mapped microfractures. As the authors mentioned, it is expected that the microfracture will parallel Sig1, and indeed many fractures do. In addition, there are zones and fractures that are inclined 10-20 deg relatively to Sig1, and which can be interpreted as shear-zones or faults. For example, zones in lower-left of fig. 2a, and most fractures in fig. 3, and most fractures in stage IIIIV of fig. 7. In this respect, the present observations are in very good agreement with the evolution presented in Fig. 5 of Katz and Reches (2004), and Fig. 5 in Reches (1988) (I could not resist the self-citations….).

    In continuation of the above, here is a suggestion that will be a significant contribution. Figs. 3 and 7 are schematic presentations of the dilated microfractures without scale and position in the sample. This presentation is fine as general display, but insufficient for evolution and certainly not for competition. It is suggested to use the detailed experimental data to prepare accurate maps (cross-sections) of the microfracture patterns. The experimental data will allow to produce maps with resolution of 10 microns that will be a new contribution of the evolution of microfracture networks, in addition to the global dilation in figs. 4-6. My bias is to use the mapping approach of my works mentioned above.

    We acknowledge that this segmentation method highlights fractures with aperture above or at the spatial resolution of the tomogram, and thus may miss some fractures that primarily host shear, with sufficiently small apertures. We mention this point in Section 2.3. We are interested in the proportion of tension vs. shear and thus have performed digital volume correlation on tomograms from our experiments (e.g., McBeck et al., 2020). This method provides tighter constraints on the proportion of tensile vs. shear strain than the fracture orientation-focused analysis suggested here.

    In other past work, we have been more specifically interested in the orientation of fractures in these experiments. Accordingly, we have described these evolutions in previous work (Renard et al., 2018). In particular, Renard et al. (2018) track the orientation of fractures and find that they evolve toward 60 degrees from the maximum compression direction (Figure 7). Renard et al. (2018) also provide detailed maps of the fracture patterns of these experiments.

    In addition, constructing these fracture maps would not help answer the central question of this paper of how the three endmember modes of fracture growth vary in dominance throughout loading, and vary due to confining stress and the exclusion/inclusion of fluids.

Renard, F., Weiss, J., Mathiesen, J., Ben-Zion, Y., Kandula, N., & Cordonnier, B. (2018). Critical evolution of damage toward system-size failure in crystalline rock. Journal of Geophysical Research: Solid Earth, 123. https://doi.org/10.1002/ 2017JB014964

McBeck, J., Ben-Zion, Y., & Renard, F. (2020). The mixology of precursory strain partitioning approaching brittle failure in rocks. *Geophysical Journal International*, *221*(3), 1856-1872.

19. Discussion: Sections 4.1 and 4.2 in the discussion emphasize the inappropriate issue of 'competition' discussed above. This part should be revised to focus on the evolution of microfracture patters at the sub-millimeter scale.

    We have revised these sections following this comment, and the comments of the first reviewer (#1-5).

20. Section 4.3 is a highly speculative jump of many orders of magnitude to crustal scale without the required mechanical analysis. It dilutes the quality of the hard, important observations of the paper. It is suggested to delete section 4.3.

    Section 4.3 includes only one sentence that describes work focused on km-scale faults. This section, and corresponding analysis, are concerned with how nearby fractures can perturb the local stress field and influence fracture growth. Thus, this one reference is applicable as it describes the influence of fault spacing on earthquake arrest. In the same way that we observe a relationship between fault spacing and fracture growth, previous work has observed a relationship between fault spacing and fracture dynamics. We now have added a sentence to more clearly specify this link.