Review of “The competition between fracture nucleation…” by McBeck et al.

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:

1. 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.
2. 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.
    a. 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.
    b. 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.
    c. 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”

3. 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.
    a. 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 III-IV 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….).
    b. 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.
4. 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. 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.

References:

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