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Solid Earth
Discussions

Interactive comment on "Silica diagenesis-driven fracturing in limestone: an example from the Ordovician of Central Pennsylvania" *by* Emily M. Hoyt and John N. Hooker

John Hooker

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Dear Referee,

We thank you for your time that you invested in reading our paper and submitting such productive criticism. We have reviewed your comments and will use them to enhance our paper's presentation and content. We are currently visiting the structure, grammar, and background issues you mention and improving them to ensure a streamlined and detailed presentation that can be well-understood by all readers. In the spirit of open discussion, we would also like to comment on a few of your remarks as follows:

1. Conclusions not supported/ hypothesis-driven: Our approach was to present multiple hypotheses that could potentially explain the observed fractures. Although we cannot concretely prove that any one of our hypotheses is the mechanism for our stratified fracture set, most proposed hypotheses fail our tests, such that we are left with the concept of silica diagenesis. We cannot disprove this explanation, and as well, it is supported by our petrographic and XRD data results. To summarize, we allow that we cannot prove silica diagenesis was the cause, but our main aim was to disprove other hypotheses and suggest the viability of silica diagenesis as an alternative. We welcome arguments that either (i) uphold the viability of alternative hypotheses, or (ii) negate the viability of silica diagenesis.

2. Optical petrography/ point counting: We would like to clarify, and will also revise this in the presentation of our paper, that we performed point-counts on field photos and not on thin sections; but we otherwise used the standard method that is normally applied to thin section images. We would also like to explain that our quartz composition percentages were quantitatively derived using XRD analysis. We agree with the referee that XRD cannot distinguish diagenetic from detrital quartz; let us clarify that our interpretations of the type of quartz (biogenic or detrital) was based on qualitative petrographic observations.

3. Terminology: fracosity: We recognize the potential misunderstanding and unfamiliarity of this new term. We would like to clarify that the use of this term is to emphasize the difference between fracture porosity, which is not our focus in this paper, and our method of point-counting field photographs. Fracosity differs from the conventional method of scanlines used to calculate fracture intensity in that fracosity represents the volume fraction of the host rock that is occupied by fractures, including fracture cement. This quantity was determined by dividing the fracture points in the field photo by the sum of the fractured and unfractured points in the photo and then multiplying that by 100 to produce a percent. In that sense it is an area measurement, just as porosity, although a volumetric fraction by definition, is commonly measured as an area in point

counts of thin sections. We concede that without spending more time on the subject, it is perhaps not wise to go with a little used term, but on the other hand, we are unsure of another term that encapsulates the volume fraction of rock comprising fracture pore space and cement. We will be sure to focus on this issue to improve how we introduce and reference the term, or abandon it, to avoid such confusion. We welcome opinions on the matter.

Again we thank you for your thoughtful review, which will clearly improve the work overall.

Sincerely,

Emily Hoyt and John Hooker

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