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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

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GENERAL COMMENTS ABOUT THIS STUDY In the submitted paper authors provide an interesting field example of bed parallel joint sets in limestone and furnish an attractive explanation of a possible jointing model which is based on chemically driven fracturing (namely, silica diagenesis). The study is well reasoned and informative, as well as corroborated by field data. As this topic may be of interest for a wide audience of potential readers, I would advise publication of this manuscript after a minor revision, according to the following comments. SPECIFIC COMMENTS - It is unclear to me why authors state as tectonic strain or fluid overpressure, hardly can explain hori-

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Discussion paper

zontal joint formation. Although a clear negative correlation between silica content and occurrence of horizontal joints is evident, I wonder whether this is sufficient in order to exclude the effects of a compressive tectonic and fluid overpressure or a combined effect of these latter and chemical driven fracturing. Authors should further clarify this point. - Introduction: although Introduction is rather illustrative, authors should depict more thoroughly here the state of the art in the field of chemically driven fracturing. - Section ‘Point Quantification’: the described method of fracture porosity quantification is interesting, nevertheless authors may illustrate a comparison with more traditional methods such as scan line and scan area and the (eventual) advantages of the use of this criterion. - Section ‘Point Quantification’: It’s not clear to me whether the described method is applied over field or thin section images. Furthermore, authors should clarify if a lower threshold is adopted for fracture size (according to Ortega et al., 2006; Guerriero et al., 2010) or all existing fractures within the sampled area are accounted . - Section ‘Point Quantification’: authors may explain more thoroughly this method. Maybe, an illustrating picture might be opportune here. - Section ‘Point Quantification’: it’s not clear to me why authors prefer the use of fracosity, which provides an estimation of porosity associated to all fractures falling within the investigated image/area, rather than fracture porosity associated to a single fracture set (e.g. horizontal set). - Sections ‘Introduction’, ‘Discussion’ and ‘Conclusions’: such sections are well reasoned and illustrative, nevertheless I suggest to point out more explicitly as horizontal and vertical fracturing are two mechanical problems substantially different. In orthogonal to bedding jointing, layers of different size and mechanical properties are constrained to show the same horizontal strain whereas, in case of horizontal jointing, the involved beds are independent mechanical systems. As a consequence, all theories about joint filling are here unusable. - Section ‘Discussion’: authors provide an interesting strain energy based analysis in order to justify a vertical extension of horizontally fractured beds, nevertheless it is unclear to me how the two cited terms Sf and Sc are compatible. In my opinion we can observe two alternative scenarios: (i) Sc different form 0 and Sf = 0, in case of contraction strain and (ii) Sf different form 0 only in case of extensional strain. Probably I have misunderstood something in discussion and, so, I suggest to make clearer such section. - Section 'Discussion': It would be interesting to consider the following as a possible alternative model for horizontal jointing: under hypothesis of early silica migration, and consequent shrinking, some internal/residual stress can be induced by heterogeneous chemical induced alterations. Namely, if some portion of a rock layer experiences shrinkage, the remaining part may bear the overburden, so prohibiting vertical contraction. This may justify the condition of vertically pinned rock layer. Should be noted that, as frequently rock compressive strength is larger than tensional strength of about one order of magnitude, a small portion of a rock layer (slightly over the 10% of the total) which is not subjected to shrinkage is sufficient to induce tensional fracturing within the remaining part of such layer.

MINOR TECHNICAL CORRECTIONS - line 62 – 63: the word 'layers' appears too much times within one sentence. - line 173: 'joint abundance' is rather generic if this term denotes a function of bed thickness; the sentence 'joint spacing is proportional to bed thickness' is more appropriated; - line 181: 'hydrostatic pressure' is generic, whereas 'overburden stress' is more opportune; authors may substitute with: '. . .a state where the fluid pressure exceeds the overburden stress at a given depth. In this latter instance horizontal fractures can form . . .'; - line 273: the sentence '. . . the host rock is prohibited from vertical contraction . . .' may be misleading as it might lead the reader to think about some boundary conditions or external constraint which hinder subsidence or vertical compaction; - Fig. 14: It is not clear to me whether or not the observed fractures are orthogonal to bedding. - Table 2: also in table captions authors might explain that Total Area is expressed as number of all grid intersections falling within the inspected image.

CITED REFERENCES Ortega, O., Marrett, R., Laubach, E., 2006, Scale-independent approach to fracture intensity and average spacing measurement. AAPG Bulletin 90, 193–208. Guerriero, V., Iannace, A., Mazzoli, S., Parente, M., Vitale, S., Giorgioni, M., 2010. Quantifying uncertainties in multi-scale studies of fractured reservoir analogues:

implemented statistical analysis of scan line data. Journal of Structural Geology 32, 1271-1278, doi:10.1016/j.jsg.2009.04.016

With my regards and best wishes for your work, Vincenzo Guerriero

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