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Interactive comment on "Distribution, microphysical properties, and tectonic control of deformation bands in the Miocene accretionary prism (Whakataki Formation) of the Hikurangi subduction zone" *by* Kathryn E. Elphick et al.

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This work is interesting as new field examples of deformation bands are described in the poorly investigated context of accretional prism. This work confirms some recent results concerning tectonic regime controlling deformation band patterns in sandstone and adds two uncommon patterns: (1) Localized faults and shear bands under contraction regime. This is the main result of this study and is potentially linked to the mecano-stratigraphy especially marked in this geological context. This point deserves to be better developed with additional description of bed stratigraphy and fault architecture.

(2) Distributed SECB under normal regime, potentially formed by burial (overloading).

However, these structures are not consistently described (not show in figures, some problems with dihedral angle, distribution is missing…) which affects the impact of this result. I underline that different consistent approaches are used (field mapping, microscopy, image analysis) and the number of data appears adequate to clearly characterize the fault/DB patterns. The literature appears also extensive and consistent with the paper aims.

Because of these reasons, this work deserves to be published in this special issue "Faults, Fractures and Fluid flow" of Solid Earth and could be of interest for any scientists dealing with mechanisms of deformation in porous materials or reservoir evaluations. However, this work contains numerous important issues in the methodology, the data description and the paper organization which have to be managed before any possible publication. I propose major revisions with numerous comments (see below and attached .pdf file with minor suggestions). A second review is certainly needed.

Main comments: *The introduction is in good shape with consistent references. However, the authors exposed the originality of their work with the fact that their study material is not Aeolian sandstones. That's right but I find the geologic context of accretional prism and permuting stress field rather original. At least, modify the text of this section to be consistent with the literature (sandstone of Provence are not Aeolian, maybe introduce also Nubian sandstone in Egypt? Or North Sea?);

*The section 2. Background presents lot of repetitions. I propose to remove or displace the 2.2, 2.3 and 2.4 and only preserve the 2.1. Geologic setting. Move some sentences of the 2.2 concerning the classification of deformation bands from micro mechanisms to the 1. Introduction, thus remove this section 2.2. and the table 1. Remove the 2.3 Spatial distribution already explained in the introduction. Move the 2.4 Conceptual mechanical model to the discussion (also fig. 3);

*I recommend to show outcrop image mapping used for scan-line distribution analysis.

Why not considered spacing > 20m? The general shape of the spacing distribution is generally discuss using Pearson coefficient which is considered as the principal parameter to discuss band patterns. This approach could be interesting if accompanied by precise field observations and descriptions but it can introduce wrong interpretation if consistent field investigations are not done. I encourage you to develop description of mean band spacing, if possible from field measurement, and show several examples in figures. Remove the section concerning theoretical structure distribution (l.288-304 and section 4.5.1);

*The Pearson coefficient is also use to describe fault patterns (bimodal – polymodal). Again, this approach can introduce major wrong interpretation as a function of the measurements were done on the field. I recommend to better developed observations and field description before to use this statistical approach;

*The microfracture density have to be quantify from surface mapping (on SEM image) and not from scanline orientated normal to the band. This introduces an important bias as micro-cracks strike along force chains with specific angle to the bands;

*The description of structures is confusing as the text is following a chronological order, whereas figures are classified by type of data (macro, micro, grain size analysis, petrophysics). I encourage you to clearly separate data of your 3 events D1, D2, D3 to match with the text description (just keep the figure 9a on porosity with the total dataset for comparison);

*Indicate more precisely which part of each figure (a, b, c, d, e…) is concerned by citation in the text will help. Start your figure in the consistent chronology (generally D3 structures are firstly described in figure). I encourage you to add some indications on the figure.

*The Ds/Dc values appears inconsistent with data description, please check them;

*Extend the description of micro-mechanisms of D2 structures, the image you shown

fig. 7f is too limited to clearly expose the deformation process and the band micro-geometry you described in the text. Concerning these bands, you described a cataclastic process but this is not consistent with the negative relief they show on the field and their dark color. How explain that? It is not evident also in SEM images. Any important of clay (phyllo bands?) or disaggregation, or cementation (Organic Matter)? The observations of Fig. 6a-c rather argue for disaggregation bands.

*Qz overgrowths are not consistently described (wrong interpretation in the data description), add more precise observations. It could be of interest to constrained how evolve the mechanical properties and the petrophysics of the material.

*Clearly separate DB in fault Damage Zone and DB out of fault DZ in the description of D3, as done within the following distribution description.

*Explain how damage zone of fault thickness is defined.

*Use figure 14 within the data description and remove the figure 15 (not used).

*Develop the description of normal-sense SECB if you want to maintain the discussion concerning band type and distribution vs. tectonic regime l.720-736 and l. 776-784. I encourage you to do it, these normal-sense structures potentially formed by burial increase could be very interesting. If it is not possible, remove this part of the discussion.

*The mechanical approach exposed in figure 17 is not enough constrained to be consistently discuss (both stress path and yield envelope are not estimate from data). The hypothesis of compaction between D2 and D3 appears inconsistent with description. Think about strengthening by the D2 band pattern or cementation process to explain a potential increase of the yield envelope. However, the change of boundary stress conditions (extensional regime – contractional regime) and the presence of localized faults could explain this change of band properties from D2 to D3.

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Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2020-80/se-2020-80-RC1-supplement.pdf

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