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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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Gregory Ballas

Comment

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 con-

traction regime. This is the main result of this study and is potentially linked to the mecanostratigraphy 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.

Response

We thank Gregory Ballas for reviewing this manuscript and for providing valuable critical insight and analysis that will allow us to improve and clarify the manuscript. We appreciate and are grateful that the reviewer thinks that this work deserves to be published within the special issue. Three main points are raised by the reviewer.

The first point refers to one of the main findings of the research that has not been given enough focus and description within the article. We thank the reviewer for pointing this out and have amended the manuscript to have a larger focus on the presence of localised faults and shear bands under a contractional regime. The role of mechanostratigraphy has been given more focus and discussion throughout the manuscript.

The second point highlights the documentation of SECBs within the normal fault regime. Upon reflection of the comments from the reviewer we have reassessed the data and have come to the conclusion that the structures do not represent SECBs but rather CSBs that have variable displacement along strike. While the structures do in places seem to have no apparent shear offset, this may be a sectioning effect. Furthermore, we observe that the lithology impacts the displacement associated with a structure with greater shear offset observed as bands propagate through clay-rich layers and reduced offset when propagating through shell-hash rich layers, for example. Therefore, in light of the comments from the reviewer, we have changed the results to show only CSBs present during horizontal extension, not SECBs, consistent with other studies examining deformation bands formed under an extensional regime (e.g., Rotevatn et al., 2008; Saillet and Wibberley, 2010; Solum et al., 2010; Soliva et al., 2013; Ballas et al., 2014; Soliva et al., 2016).

The third main point is regarding the methodology, data description, and paper organisation. In regard to methodology, issues surrounding analysis of complete datasets rather than individual outcrops has been addressed. We now discuss both scales and highlight a problem with only focussing on one of the two scales. The figures have been re-made to align with the order of data description within the text. Data has been described in more detail where necessary and appropriate. However, in our opinion, most of the data is described to a level that is required for the key points of the article and more description would result in lengthening of an already long manuscript.

Main comments:

Comment

*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?);

Response

We have introduced the Nubian sandstone and sandstones hosted in the North Sea, in addition to providing more insight into the lack of studies focussing on DBs within subduction wedge settings. In doing so, we have also expanded the section referring to the lithology and how the lithology studied in this research is unique regarding DB studies, thereby further exposing the originality of the research.

Comment

*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);

Response

Sections regarding classification of DBs have been shortened and incorporated into the introduction and a brief synopsis of 2.4 added into introduction. We did not, however, incorporate Figure 3 into the discussion as suggested because we believe that a figure to explain the cam-cap yield envelope is required in the introduction to provide a background into the idea that tectonic setting influences the band kinematics and orientation.

Comment

*I recommend to show outcrop image mapping used for scan-line distribution analysis. Why not considered spacing > 20m?

Response

We had already included outcrop images of the maps used for scan-line distribution analysis in Figs. 17 & 18. Spacing >20 m has not been considered as it represents areas of no exposure along beaches associated with stream mouths. To include such data would influence the spacing results erroneously.

Comment

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);

Response

We respectfully disagree with the reviewer and would like to point out that statistical approaches to characterising fracture patterns are common and very important in Structural Geology (add long list of references to methods papers in this field, including those from Sanderson's work, FracPaq, etc.). Most commonly, the aim of reporting a mean is to determine the central tendency of a distribution or its most probable, common value. This approach makes a lot of sense when one considers constant spacing with random noise (which should be normally distributed). However, it is not useful when considering spacing that varies systematically with position, especially when this variation is nonlinear. Our simple statistical approach can be seen as a simplified version of that of "Sanderson, D. J. and Peacock, D. C.: Line sampling of fracture swarms and corridors, Journal of Structural Geology, 122, 27-37, 2019.". It is designed to test if a deformation band distribution is a function of position (mean not useful) or if it is constant with random noise (mean useful). We have kept the section regarding theoret-

ical structure distribution in the text because it is needed to understand our simplified statistical approach to the mapping and can act as a check for other researchers to compare their datasets with. In terms of additional figures: we provide three representative examples for bands showing periodic and aperiodic spacing. While many others were measured (see Figs. 9 & 19 for statistical results), we did not feel it appropriate to include all examples within the manuscript. If the editorial team indicates a requirement for such data, it can be added to an appendix.

Comment

*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;

Response

Regarding fault patterns, we have used the approach of Healy and Jupp (2018) to test if the fault orientation distribution is bimodal. This is a published, mathematically sound workflow that can be compared to other datasets and is reproducible and reliable. It also minimises observer bias because it is quantitative.

Unfortunately, the reviewer does not state explicitly which specific problems can arise from the use of the objective statistical methods employed in our paper. Thus, it is difficult to respond to the criticism concisely. We are certainly aware of, and explicitly mention and discuss, the most important source of sampling bias in our study: exposure bias. The hinterland outcrops are rare and poorly preserved. Therefore, our work is restricted to the coast which has excellent outcrop. However, the coast is subparallel to the dominant strike of major thrusts and folds. As a result, it is expected and not surprising that we observe fewer D3 thrust (mentioned in the paper in section 4.4.2.) than D2 faults, which strike at a high angle to the coast. In addition, most of our study sites sit on the back limb of a large D3 syncline, and because of this particular exposure bias, we cannot study how deformation band and fault distribution varies with position across large-scale folds. Moreover, the coastal outcrops are (fairly unstable) cliffs bordered by rock platforms. So not every single bed featuring deformation bands yields good opportunities to document them in 3D. Nevertheless, exposure bias is a common problem for all geological research. At the coast, exposure is > 80%, which we consider excellent. We do not see how the use of objective statistical methods in the analysis of fault/DB orientation data and line maps constitutes a major problem.

Comment

*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;

Response

This dataset will be removed.

Comment

*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);

Response

We agree with this comment and have changed the figures accordingly. We thank the reviewer for this comment as it has enabled us to better describe the data with clearer figures.

Comment

*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.

Response

Agreed and implemented.

Comment

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

Response

The Ds/Dc values have inherent error because many bands do not show offset in the thin section, therefore, the offset value has come from the field measurements. The error in the measurement will then be carried into the estimate. Additionally, the porosity is measured in 2D for the DB and the HR so there is error. However, the Ds/Dc values do suggest more CSBs with large shear offset and that has been addressed in the text. We thank the reviewer for their detailed analysis of the research to highlight this.

Comment

*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 microgeometry 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.

Response

Clearer figures have been made for the microstructure (Figs. 8, 12, 14). There is always significant grainsize reduction in the bands accommodated by cataclasites.

Therefore, the bands are generally cataclastic, even when they transect layers with high clay content. See the new Fig. 6 in the revised manuscript, please.

Comment

*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.

Response

We agree that quartz cementation has not been addressed properly. To alleviate this problem, substantial additional analyses such as high-resolution cathodoluminescence imaging would be required. Considering that this paper focuses on outcrop scale descriptions of DB and that the present BSE images permit to identify cataclasis as certainly the dominant deformation mechanism, we removed the images indicating the presence of quartz cement and this will be the focus of further research.

Comment

*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.

Response

Figures have been added to the manuscript and previous image panels have been reordered to split the images into their respective deformation phases. The figures now align with the descriptions.

Comment

*Explain how damage zone of fault thickness is defined.

Response

This has been addressed in lines 660-661.

Comment

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

Response

Done as suggested.

Comment

*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.

Response

We thank the reviewer for this comment. As aforementioned, these structures cannot be definitively identified using multiple methods of identification. However, CSBs can. Therefore, we have changed the text to show CSBs associated with horizontal extension.

Comment

*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.

Response

In lines 893-914 we discuss in detail why it is very challenging to determine the stress path for our rocks of interest and state that it is beyond the scope of our study. We explicitly highlight that the simplistic model discussed in Fig. 21 is purely speculative and requires further testing based on an inversion of the stress path. All we do is to state that there is a non-zero possibility that the mean stress and the strength of the rock package increase from D2 to D3, and thus one could be tempted to apply similar arguments as those in literature to explain the difference in D2 and D3 bands. This issue is already discussed at great length and with due care in lines 904 to 913. We do not believe that it requires further discussion.

Comments from within the text

The reviewer has provided numerous other references to add into the text and to broaden the scope of the research. We have implemented these changes and read the relevant papers to improve the manuscript and thank the reviewer for adding the references to the comments.

The reviewer has identified the grouping of clusters to represent one band as a bias. It is certainly a simplification. However, as shown now in Figs. 6 & 13, a band can turn into a cluster downdip and back into a single band again. This is a function of host rock and now mentioned in the text. We follow the approach of Main et al (2000) and consider clusters as a single band. Any error introduced by this averaging is usually smaller than the pattern variability captured by using the scanline approach.

The reviewer has asked why we normalised spacing data. We did so to enable analysis and comparison of multiple outcrops.

The reviewer suggested that figure 20 be made more realistic. We have kept the original figure because this is a schematic to show a possible order of events in any tectonic setting and would like this to be simple and understandable. The process of overprinting structures within damage zones is very complex and we do not believe that there is a great enough understanding of the process for the image to be made more realistic because in doing so, it would be less realistic.

References

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