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Interactive comment on "Fault zone architecture of a large plate-bounding strike-slip fault: a case study from the Alpine Fault, New Zealand" *by* Bernhard Schuck et al.

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*** Note, please: I also uploaded the text below including my two figures in the supplementary zip file - just in case ***

This interesting paper presents new structural, microstructural, and geochemical data on the fault core and its immediate host rocks of the Alpine Fault, New Zealand. An impressive array of state-of-the-art analytical methods covering a broad scale range has been employed to analyse the samples. The resulting data are unique and very interesting. I strongly support their publication. The manuscript is generally well written and structured. The tables and figures are useful and well designed.

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However, I would like to offer three general points of constructive criticism, the addressing of which would strengthen and focus the paper – in my opinion. I shall attempt to summarise them as follows:

[1] The attempt to correlate microstructural and hand-specimen-scale geochemical data to fault architecture on scales of up to > 100 km is problematic, mainly due to data sparsity and observational bias.

[2] The isocon analysis as conducted here is problematic.

[3] The discussion linking geometrical characteristics of fault core and principal-slip zone to microstructures, geochemistry, and rheology is partially not well aligned with the cited literature. It would also benefit from consideration of additional articles on this field. Finally, some physical/mechanical processes of potentially high relevance are mentioned for the authors' kind consideration.

My main points for suggested improvements are explained in some detail below. Smaller points of interest are supplied in the annotated PDF of the manuscript. I hope this is not too much of an inconvenience for the authors and editors.

Most general critical comments

1. Correlation of fault geometric characteristics with host rocks In my opinion, the authors are a bit too optimistic when it comes to drawing rather general conclusions in regards to correlating geometric properties of the studied fault to (micro- and mesostructural) host-rock properties. Since this correlation constitutes a core focus of the manuscript, it deserves particular care. Let me give some examples in the following.

Lines 271 – 273: "Amount and size of clasts generally decrease towards the PSZ and vary systematically with PSZ thickness (Table 1): locations with thinner PSZ contain more clasts in the hanging-wall, which tend to be larger, compared to locations with thicker PSZ."

I admit I find this statement problematic. Table 1 provides hanging-wall observation for three locations. However, PSZ-proximal detrital clasts and matrix clasts are only sampled at two out of the three locations. Two data points with different values always display an apparent linear trend. I do not think that such a general statement is warranted based on the very sparse data. Moreover, it has been shown in continuous fault outcrops that PSZ width can easily vary by a factor of 10 on along-strike scales of tens of meters without changes in host rocks (see for example the very nice recent paper by Kirkpatrick et al. 2018 – highly relevant to the work at hand).

Lines 294 – 295:" The type of contact between hanging-wall cataclasites and fault gouge correlates with PSZ thickness: where the PSZ is thicker, contacts are transitional manifested by decreasing grain sizes (Table 2) and correlate with increasing amounts of phyllosilicates [. . .]".

The cited table also reveals that only two of the five studied locations expose, or allowed sampling of, the contact between hanging-wall cataclasites and PSZ. So again, two data points only are available and do not inspire tremendous statistical confidence in the validity of the above statement.

Other examples are highlighted and commented upon in the annotated manuscript.

2. Isocon analysis

I feel that the application and discussion of the isocon method in this manuscript should be improved. I am not a geochemist, so bear with me, please. Nevertheless, I shall attempt to offer some constructive food for thought in the following.

[a] Line 214: The authors emphasize that the choice of host rock is important for isocon analysis. In this context, it would be interesting if they added an explanation for their choices. For example, for the Alpine schist, they averaged compositional data from three samples obtained across a linear distance of < 20 m in a single drill hole. This drill hole is far away from most of the sites studied in this manuscript. Hence, one

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wonders: how heterogeneous is the Alpine schist chemically? Even in the three host rock samples used for averaging certain oxide proportions vary by a factor of 2, for rocks just a few meters apart. How large is this variability on the scales of tens of kilometres? In this context, it would be instructive, as a first test, to check if the chemical variability implied by the presented isocon plots is of the same order as that observed in the chosen (and ideally other) host-rock samples.

[b] Moreover, it may be interesting to consider additional constraints on the slopes of the isocons. Given that the deformation pT-conditions are quite well known, it appears potentially useful to identify elements, which can be considered as relatively immobile a priori, for the choice of an isocon rather than a simple (least-squares?) linear fit (compare Schleicher et al. 2009).

[c] Conversely, it would be instructive to discuss/consider if the best-fit isocon obtained in the present manuscript is consistent with geochemical expectation. For example, Na appears to be immobile while Al is relatively mobile according to the isocon example in Fig. 1 below. The same trend is implied for quartz relative to Al. Does this really make sense?

[d] On this note, linear fitting can entail a bias towards high-concentration elements (depending on the mathematical fitting method), which can constitute a problem. This issue is explained in the caption of Fig. 1 in more detail. This bias can be avoided by data scaling, as discussed in Grant's seminal paper of 1986. It is also interesting to recall that Grant (2005), the principal developer of the method, recommended to avoid the use of log-log-diagrams in isocon plots in this review paper. I echo his concerns here. It is easier to recognize elements not fitting the isocon on a linear scale (Fig. 1 below).

CAPTION Fig. 1: Linear isocon plot for Gaunt-creek PSZ example. Points of equal geochemical concentration (red circles) can be easily differentiated from those with mobility relative to the host (green points mark some examples). The given linear fit implies that SiO2 is less mobile than Al2O3, which may be an artefact of the fitting method. After all, the presented microstructural data show quartz dissolution, which is usually enhanced in the presence of micas. If the data point for SiO2 is excluded during standard least-squares polynomial fitting, one obtains a much steeper isocon tied in by Al2O3 indicating quartz depletion – and a slope > 1, which reverses the mass-gain/loss interpretation!

Finally, in the light of the comments above, the following statement (line 226 – 227) appears problematic: "The method nevertheless allows relative geochemical changes within the fault zone at this location to be accounted for." This is probably not the case for elements where the variability within the host-rock samples is of the same order as those measured in the altered rock, or, even worse, for which the host-rock composition or isocon are chosen incorrectly. In summary, I recommend a careful revision, replotting, and subsequent reinterpretation of the isocon analysis. If one accepts this recommendation, section 5.1 of the discussion will most likely have to be rewritten substantially. It is important, but not discussed, that the isocon analysis conducted here differs significantly, both in methodology as well as outcomes, from a similar study on SAFOD samples conducted by Schleicher et al. 2009.

3. Mechanical interpretation

I have a number of concerns related to section 5.5.2 – Alpine fault core.

[a] I noticed a few occasions where certain statements and the associated references do not align very well. I point them out in the annotated PDF.

[b] Fig. 10 contains a mistake – please, see annotated PDF for explanation.

[c] As already explained in section 1, I have some reservations in regards to generalisations about factors controlling PSZ thickness presented here. In a nutshell: 1) Due to sampling difficulties, data are too sparse to conduct statistically meaningful tests – two or three data points are not enough. 2) Relevant existing literature (Kirkpatrick et

al. 2018 and references therein; the work of Emily Brodsky and Amir Sagy, etc.) on thickness variations of PSZ is not considered but highly relevant to the study at hand. 3) Given thickness variability of PSZ on the scale of metres (see literature in point 2), the question arises: is it at all useful to compare thickness variations at locations of up to tens of kilometres apart? This question warrants some careful discussion.

[d] More generally, I feel that the pre-existing ideas from the mechanical literature on fault-zone width are not as well considered as they could be. I choose one sentence to kick off my little discussion of this criticism (noting that there are other statements which inspire further critical thought): Lines 512 – 513: "This suggests that strain localization within the fault core might be governed by processes insensitive of rheological variations caused by differing fault rock composition." Problem 1: It is well known that materials of identical composition can have very different rheological properties because of, say, differences in microstructure (porosity, grain size, grain shape, grain alignment or lack thereof), fluid pressure, strain rate, etc. So this statement is not surprising or new and not well supported by references to existing literature.

Problem 2: One important assumption in this statement is that the process of strain localisation within the PSZ postdates the formation of fault rock containing it quite significantly. There is good experimental literature on monophase, homogeneous materials with strain softening that shows that the localization process of the first fault itself happens during the transient strain-softening response of the material. During this period, the fault width and basic architecture are basically fixed and remain constant for quite some time during steady-state flow (provided that there are no other huge external changes such as a change in plate velocity or the arrival of an exotic block of wall rock with vastly different properties). I am going to do the terrible, terrible deed and sneak in some advertisement of our own work in this context (Schrank et al. 2008, Schrank and Cruden 2010), mainly because it is very easy to understand since the experiments are very simple but well controlled. So, once the fault core has formed, one obtains a weak material with irregular interfaces to the adjacent wall rocks, which in turn quite likely have different material properties. Such a state can easily lead to highly heterogeneous stress and strain distributions, which in turn control further localization within the fault rock. I illustrate this point with a (really quick and dirty) numerical simulation shown in Fig. 2.

CAPTION Fig. 2: Plane-strain isotropic linear-elastic shear experiment on a three-layer system with complex geometry. The elastic moduli and model scale are given in the upper panel. Total shear strain is ca. 0.05 (see black outline for initial geometry). Note that the results would be essentially identical for a linear-viscous model (just rescale the stresses according to your favourite viscosities and strain rates). The lower horizontal model boundary is fixed, the upper horizontal boundary is translated in the horizontal direction by 0.1 m. The vertical sides are subject to symmetry boundary conditions. The weak layer has a sinusoidal shape at the top with an amplitude of 5 cm and a wavelength of 2 m. The upper panel shows finite shear strain. Note its heterogeneous distribution within the weak layer. The lower panel shows Mises, which also shows significant perturbations imposed by the interface shape alone. Now imagine a real material! Pushing the Alpine schist across gravels with large clasts will always entail a highly rough interface and thus large local stress/strain perturbations. This problem is well known and also touched in some of the papers cited below (and many others).

In summary: local geometrical perturbations can play a very significant role in strain localisation and, accordingly, PSZ geometry. Moreover, the first localisation step likely occurs in the transient strain-softening domain – which is poorly understood mechanically because it is very difficult to run good experiments in this domain at geological conditions and it is also painful mathematically – and can easily introduce complex geometries even in simple materials with simple loading geometry. There is little doubt that the fault gouge at some stage of its history was weaker than its parent rock, and thus strain softening must be considered in interpreting localisation geometries. These points highlight some issues I deem very relevant to the work at hand and well established in the literature but not considered here. I also noted a few smaller but relevant issues with the logical chain of arguments leading to some of the proposed conclusions in this discussion section in the annotated PDF.

In conclusion: due to the concerns outlined above, I believe that most of the discussion section would greatly benefit from a substantial overhaul, informed by revisiting and reassessing already cited and new literature, and considering the issues I mentioned in sections [1], [2], and [3]. It follows that the conclusion then should also be rewritten.

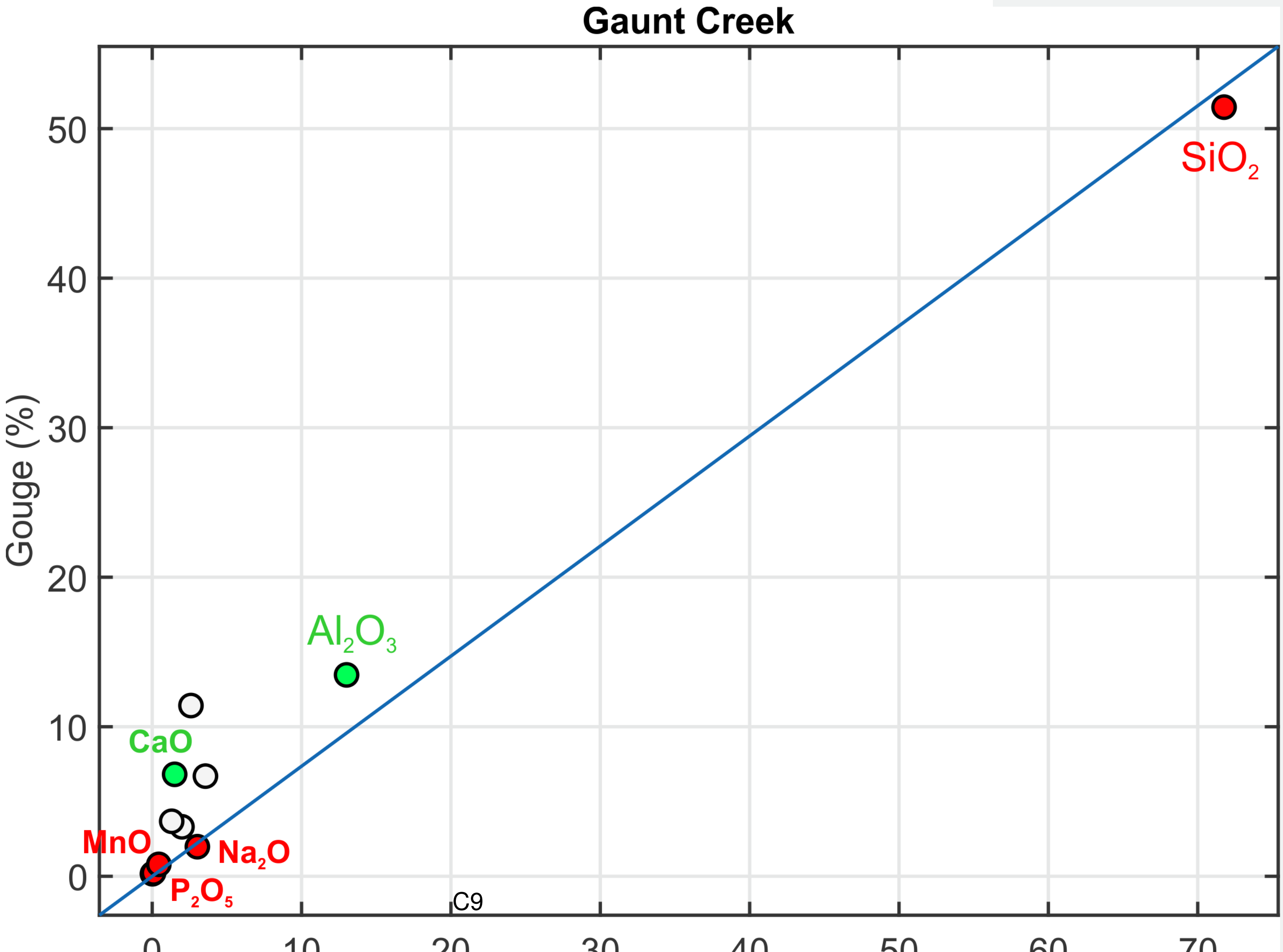
I hope that this review provides some interesting food for thought. Thank you very much for the opportunity to study this interesting paper.

Best wishes Christoph Schrank

Some references Grant 2005: Isocon analysis: A brief review of the method and applications; doi:10.1016/j.pce.2004.11.003 Kirkpatrick et al. 2018: Spatial Variation in the Slip Zone Thickness of a Seismogenic Fault; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018GL078767 Sagy, A., Brodsky, E. 2009, Geometric and rheological asperities in an exposed fault zone; https://doi.org/10.1029/2008JB005701 Sagy, A. et al. 2007: Evolution of fault-surface roughness with slip; https://doi.org/10.1130/G23235A.1 Schleicher, A., Tourscher, S., van der Pluijm, B., and Warr, L.N.: Constraints on mineralization, fluid-rock interaction, and mass transfer during faulting at 2 – 3 km depth from the SAFOD drill hole, Journal of Geophysical Research, 114, https://doi.org/10.1029/2008JB006092, 765 2009. Schrank et al. 2008: https://www.sciencedirect.com/science/article/pii/S019181410700212X Schrank and Cruden 2010: https://www.sciencedirect.com/science/article/pii/S0191814109002430

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-109/se-2019-109-RC1-supplement.zip

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-109, 2019.



Gaunt Creek
Gouge (%)
0
10
20
30
40
50
0
10
20
30
40
50
60
70
SiO2
Al2O3
CaO
MnO
Na2O
P2O5
C9

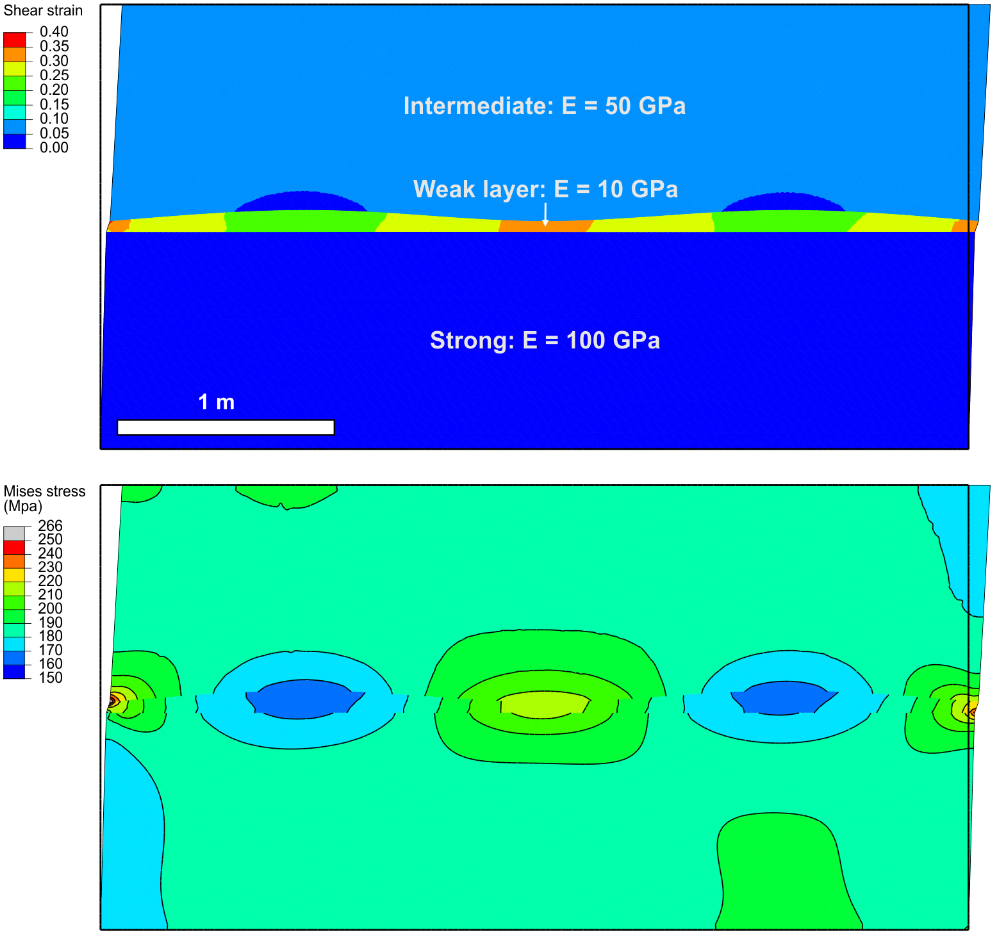


Fig. 2. Fig. 2 - see text for true caption

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