Referee Comment (RC) 1

We are grateful to Christoph Schrank (CS) for his in-depth, well-structured and critical review. We think that in addressing his comments we will substantially improve the manuscript. He presented three major criticisms of the work (points RC1 – 1 to RC1 – 3). We respond to these first by repeating the referee's comments in bold font, then our responses are in plain text. Christoph Schrank also provided additional specific comments in an annotated pdf. We have also copied these comments into this document (except for typos, which we just corrected) so that it is easy to demonstrate what modifications are made in the revised manuscript in response to them.

RC1 – 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.

We agree with the reviewer that data sparsity likely imposes observational bias in our study. However, we do not claim to provide a complete model of the Alpine Fault's architecture. Instead, we highlight that our microstructural and geochemical data from four key sites suggest that the architecture of this particular fault could be more complex than previously thought. However, some upscaling/extrapolation of our results to other scales is feasibly accomplished by discussing our observations in the context of existing seismic investigations (see lines 546 – 549).

Our methodology is similar to that employed by previous studies of the Alpine Fault rocks, which, because of the nature of outcrop, have to rely on detailed investigations of one or only a few locations (e.g. Boulton et al., 2012; Sutherland et al., 2012; Toy et al., 2015). Barth et al. (2013) employed a very similar approach to ours (in terms of methods used and amount of locations studied) to investigate the architecture of the Alpine Fault's southern segment. Our study essentially extends the work of Barth et al. (2013) to the fault's central segment.

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).

Regarding the first part of this comment we agree that our statement "locations with thinner PSZ contain more clasts in the hanging-wall, which tend to be larger, compared to locations with thicker PSZ" is problematic because the outcrop situation provides only two locations, where the relationship between amount / size of grains and PSZ thickness can be investigated. The intention of this statement is to describe an observation. We highly encourage others to provide additional observations supporting or disproving our statement. Furthermore, we like to emphasize that this statement is not further exploited in the discussion of this manuscript, i.e. no general conclusions are based on this statement, which reflects that it describes an observation.

The decrease of amount and size of clasts towards the PSZ described in the manuscript is based on thin section analyses and is in accordance with outcrop observations (i.e. can be observed at all four outcrops) and previous work (e.g. Boulton et al., 2012; Toy et al., 2015). We will state this in the revised version of the manuscript. Please note, while PSZ-proximal detrital clasts have not been sampled, matrix clasts have not been observed at Martyr River.

The second comment, that "PSZ width can easily vary by a factor of 10 on along-strike scales of tens of meters without changes in host rocks" has motivated us to take a closer look at previous studies that examined PSZ width and consider comparisons to the work we present.

A few past studies report dramatic thickness variations in fault gouges. Shervais and Kirkpatrick (2016) and Kirkpatrick et al. (2018) report extreme thickness variations over short distances within the most recently-active of 13 slip zones that make up the so-called "inner fault core" of a fault that accumulated a total displacement in the order of 10 – 30 km (Shervais and Kirkpatrick, 2016). While the fault's total displacement is fairly-well constrained, it is unknown how much displacement each individual slip layer accumulated. It could easily be that the particular layer ("layer m") with thickness variations only accommodated a couple of meters of slip. Other studies of faults that accommodated displacements ranging between 1cm and 1km have reported large thickness variations over short distances parallel to displacement (e.g. Sagy et al., 2007; Sagy and Brodsky, 2009; Brodsky et al., 2011; note: it is debatable if these three studies are at all comparable in this context – see response to aspect RC1 – 3[c](2)).

With ~470km (Sutherland et al., 2007) the Alpine Fault has accommodated a much larger total offset than the slip zones investigated by e.g. Shervais and Kirkpatrick (2016) and Kirkpatrick et al. (2018). This difference suggests that variations in fault thickness are not as comparable as they appear on first hand. In addition, many studies dealing with thickness variations as function of displacement generally note that "faults smooth with increasing slip" (Brodsky et al., 2011; see also Sagy and Brodsky, 2009). In this context, thickness variations of e.g. *layer m* from Shervais and Kirckpatrick (2016) and Kirckpatrick

et al. (2018) appear to reflect rough contacts between the slip zones and the surrounding units. The Alpine Fault, with its substantially larger amount of slip accommodated, displays – where sufficiently exposed – smooth contacts between fault gouge and hanging- and footwall, respectively (see Figures 4b-d and 5b-d in the manuscript; see also Sagy and Lyakhovsky, 2019). These observations further suggest that thickness variations reflect different amounts of accommodated displacement.

Sagy and Brodsky (2009) document a fault where there is "a monotonic, nonlinear positive correlation between the displacement on the fault and the thickness of the cohesive granular layer". In other words, they observe that thickness increases with displacement (see also their Figure 10), so assuming comparable incremental displacements, an individual fault plane that accommodated more earthquakes will be thicker than a fault plane that accommodated fewer events. This is in accordance with previous studies (e.g. Evans, 1990; Faulkner et al., 2010; Ben-Zion and Sammis, 2003), and also one of our major conclusions (see paragraph lines 519 – 529).

Mineralogical differences could be another factor limiting the comparability of results described by e.g. Sagy and Brodsky (2009) or Shervais and Kirckpatrick (2016) and our study. Sagy and Brodsky (2009) describe a fault with layered damage architecture. In their setting, thickness variations are not related to the layer hosting the principal slip surfaces but to a layer containing a cohesive granular material mostly (> 90%) composed of plagioclase. This layer is interpreted to be "more cohesive than the surrounding damage zone". This contrasts with the Alpine Fault gouge, which is phyllosilicate-rich (between ~30 to ~60wt% in investigated samples; see also Figure 8 in the manuscript) and incohesive (Boulton et al., 2012). Slip zones described by Shervais and Kirckpatrick (2016) contain phyllosilicates but amounts are typically smaller compared to the Alpine Fault.

While we are still convinced that – despite the limitations of poor outcrops – it is appropriate to discuss variations in Alpine Fault gouge thickness in the context of fault zone architecture, we acknowledge that variations in fault zone thickness should be discussed in a broader context than it has been done in the manuscript. Consequently, we will include the above discussion in the revised version of the manuscript.

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.

Similar to our statement "locations with thinner PSZ contain more clasts in the hanging-wall, which tend to be larger, compared to locations with thicker PSZ" the statement "the type of contact between hanging-wall cataclasites and fault gouge correlates with PSZ thickness […]" here describes an observation. However, this statement is repeated in the

discussion section (lines 492 – 493). While we consider this statement as a valuable observation to be (not) confirmed by succeeding studies, we agree that data are too scarce to derive any conclusions from it. We will remove this statement from the discussion.

RC1 – 2

The isocon analysis as conducted here is problematic.

Before replying to the individual aspects of this comment, we realized that equations 1 and 2 of the manuscript contained typos. Eq. 1: there should be super- instead of subscripts to index concentrations of altered and host rock, respectively; Eq. 2: it should have been m^{-1} instead of m (compare to equation 6 in Grant, 1986). We will change this in the new version. Fortunately our original calculations already employed the correct equation.

[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 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.

We agree that the protolith geochemistry is poorly constrained. In order to improve this, the revised version of the manuscript will include all other published geochemical analyses of the Alpine Schist, namely Tables 1 and 2 of Roser and Cooper (1990) and the data provided by Grapes et al. (1982). These additional data in combination with the ones already used in the manuscript cover the entire central segment of the Alpine Fault.

These additional data are in agreement with those already reported in the manuscript and do not substantially change the results of the isocon analyses (Figure 1). On average, slopes derived from analyses with better constrained protolith geochemistry are 0.045 larger than slopes solely based on protolith geochemistry provided by Toy et al. (2017). These modifications do not affect the overall behavior; i.e. all outcrops with the Alpine Schist as protolith are still affected by mass gains owing fluid-related alteration but absolute gains are on average 7.6% smaller (new: 26.7% vs. old: 34.3). However, slightly larger isocon slopes imply that some individual elements in some samples are less enriched or even depleted (e.g. Al is now depleted in 3 out of 6 samples in the DFDP-1A core).

[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).

In the preceding study of Schuck et al. (2018) we also performed an isocon analysis. In this, we assumed either Al or Ti to be immobile, respectively. However, results revealed that both phases are mobile (up to 28% enrichment of Ti based on immobile Al; up to 22% depletion of Al based on immobile Ti). We will refer to this previous study more explicitly in the revised manuscript. For a detailed evaluation of the process providing the isocon please refer to the reply to aspect c (below).

[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?

Grant (2005) discusses several possibilities to derive the isocon: (I) clustering of C_i^A/C_i^O (concentration of an elemental species i determined from an altered sample A divided by the one determined from the protolith O); (II) a best fit of data forming a linear array through the origin in an isocon diagram (the graphical equivalent of I); (III) the a priori assumption that certain components were immobile; (IV) the assumption of constant mass during alteration; (V) the assumption of constant volume during alteration.

By using option (III) one often assumes immobile Ti and / or Al. This assumption has been shown to be invalid for the fault in our past work (Schuck et al., 2018) – see reply to [b] (above). Assuming other elements than Ti and Al to be immobile (such as Zr as in Schleicher et al., 2009), implies that concentrations of these elements are commonly determined. However, Zr is not always measured (see for example Toy et al., 2017) limiting the amount of potentially available protolith data. Furthermore, it is debatable if the elements assumed to be immobile are actually immobile.

Options (IV) and (V) appear to be inconvenient, because increasing amounts of phyllosilicates towards the PSZ (e.g. Sutherland et al., 2012) suggest mass changes and substantial formation of calcite veins within the PSZ suggests significant volume increases (see Schuck et al., 2018).

Option (I) and its graphic equivalent option (II) are worthy of a closer examination: the four data points (Si, Ti, Al, Cu) chosen by Grant (2005) to determine the isocon cluster around 1 (maximum difference between individual points: 0.057; standard deviation: 0.026; Table 1, *Panel A*). However, Fe, Mg, Na and Zn cluster around 0.73 and appear to better constrain the isocon as their spread is 0.029 (standard deviation: 0.013). Furthermore, while the option chosen by Grant (2005) does not suggest substantial mass changes, the second option presented here suggests mass changes. In our opinion, this highlights that even data-based methods to derive the isocon may be biased.

In order to minimize the bias related to the determination of the isocon, we decided to derive the isocon with a best-fit of all data as this approach does not require any assumptions.

Results of isocon analyses with better constrained protolith geochemistry still show Al enrichment / depletion. However, absolute values are smaller: up to 15% enrichment / up to 5% depletion with better constrained data vs. up to 44% enrichment in the original manuscript. Comparison of Tables 6 and S2 of this manuscript with Table 4 of Schuck et al. (2018), where immobile Ti has been assumed, indicates that the results of both isocon analyses are in general agreement with each other.

Furthermore, results of isocon analyses with better constrained protolith geochemistry now indicate Na depletion except for the cataclasites at Waikukupa Thrust. Na depletion is in accordance with alteration of albite to phyllosilicates (see Figure 6a of the manuscript). However, Na enrichment in the cataclasite at Waikukupa Thrust (Table 6 of the manuscript) is not easily explained.

[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).

In the caption of his Figure 1 the reviewer states that "the given linear fit implies that SiO_2 is less mobile than Al_2O_3, which may be an artefact of the fitting method." However, isocon analyses indicating relatively immobile SiO_2 are not necessarily an artefact of the fitting method. Mineral dissolution simultaneous with phyllosilicate formation is a better explanation for the apparent immobility of SiO_2.

Data scaling would not resolve a potential bias resulting from linear fitting: by taking the data presented in Tables 6 or S1 (summarized in the reviewer's Figure 1) and scaling SiO_2 concentrations by a factor of 0.5 (see Grant, 1986) one would still get the same slope for the isocon.

We agree that isocon analysis should not be performed in log-log space. Fortunately, ours was not, as can be seen in our equations 1 and 2 of the manuscript. After having derived all important parameters (namely the slope of the isocon, mass changes and element enrichment / depletion) we plot element concentrations and the isocon in a diagram and use a logarithmic scale for its axes. This has the advantage that now there is no clustering of data points with concentrations smaller than 10wt% (compare Figure 1 from the reviewer and Figure 9 of our manuscript; see also Figure 6 in Schleicher et al. 2009). This means that we derived mass changes and element enrichment / depletion from mathematical operations and not graphically from the isocon. The plotted isocon diagrams (Figure 9) are a condensed, hence simplified, visualization of Tables 6 and S1.

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.

We agree that the choice of the protolith or the isocon might have substantial impacts on the results of the isocon analysis. However, as indicated in the reply to aspect [a], better constraining protolith composition by using all available data has an impact on some of the elemental species analyzed (absolute values of enrichment / depletion change), but has no impact on the overall behavior. This meets our expectations: Isocon slopes of analyzed fault rocks being substantially smaller than 1 indicate substantial amounts of fluid-related alteration, which lead to mass gains (e.g. formation of authigenic mineral phases). This is in accordance with microstructural observations of this and preceding studies (e.g. Sutherland et al., 2012) which demonstrate pervasive authigenic mineralisation. On these bases, we would have been very surprised if slightly modified protolith compositions resulted in isocon slopes of 1 (no fluid-related alteration) or greater than 1 (mass losses associated with fluid related alteration). Note that the situation at Martyr River differs, as already discussed at lines 401-402 of the manuscript.

We conducted our isocon analysis based on the equations provided by Grant (1986). Our equation 1 corresponds to equation 5 in Grant (1986) and our equation 2 corresponds to equation 6 in Grant (1986). In this, the approach is comparable to the one used by Schleicher et al. (2009). However, there are a few key differences: Schleicher et al. (2009) assume immobile Ti and Zr and derive the slope of their isocons by applying a best-fit to these two data points, whereas we chose a best-fit to all data points; as we justified in our reply to aspect [c]. Furthermore, they assumed constant densities, which we do not, because we expect that volume and mass change during authigenic alteration.

Schleicher et al. (2009) report two mass changes: (I) one for the entire fault rock (their Figure 6) and (II) one for individual elemental species (their Table 1). These mass changes of individual elemental species are calculated following equation 6 of Grant (1986), which corresponds to equation 2 of our manuscript. However, the results do actually not reflect mass changes but "change[s] in concentration of a component relative to its concentration prior to alteration" (Grant, 1986); i.e. element enrichment (positive values) or depletion (negative values).

In contrast to Schleicher et al. (2009), we decided to not perform a mass transport calculation, because this calculation is not necessary to assess the general pattern of fluid-related alteration, which was our motivation for the performance of the isocon analysis.

As will be discussed below in our reply to the comment in line 408 of the annotated manuscript, major differences between the study presented here and the one presented by Schleicher et al. (2009) – predominant element enrichment vs. predominant element depletion – probably reflect that the hydrological systems and host rock compositions of the Alpine Fault and the San Andreas Fault differ, so the nature of fluid-related alteration also differs. In this context we do not see any need to discuss our results with regard to the methodology / results presented by Schleicher et al., 2009.

RC1 – 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.

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.

We address these concerns below, where we reply in detail to comments provided in the annotated pdf.

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

We appreciate the reviewer pointing out that what we had written in lines 471 – 485, which relate to Figure 10, was confusing (see also RC3). The figure we presented is correct and indeed implies that most strain is accommodated in the FW-proximal gouge.

Lines 471 – 476 describe briefly observations of Cowan et al. (2003) of low-angle detachment faults in the Death Valley. These faults juxtapose strong footwall rocks on weakly consolidated hanging-wall sediments, and have layered fault gouges.

Lines 477 – 485 aim to explain that the Alpine Fault is comparable to these low-angle detachment faults, in having a layered fault gouge and juxtaposing strong hanging-wall rocks on weakly consolidated footwall sediments (see also Biegel and Sammis, 2004). In both cases the fault gouge layers in contact with the weakly consolidated sediments (Death Valley low-angle detachment fault: hanging-wall; Alpine Fault: footwall) are microstructurally comparable (fine-grained, clast-poor).

Cowan et al. (2003) concluded that the gouge layer in contact with the weakly consolidated hanging-wall sediments accommodated more strain than the gouge layer in contact with the stronger footwall rocks (see their Figure 11b). Guided by this, we hypothesised that – when adjusted for the different kinematic situation (i.e. thrust vs. normal movement) – in the Alpine Fault Zone the footwall-proximal layer in contact with weakly consolidated sediments accommodated more strain than the hanging-wall-proximal layer. This situation is sketched in our Figure 10.

We will include this revised and – and hopefully clearer – explanation in the revised manuscript.

[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.

As discussed above, we agree that data are sparse. However, we have described all known fault gouge outcrops NE of Martyr River. Data coverage can only be improved by more expensive approaches (such as drilling projects). We will clearly acknowledge this in the revised manuscript.

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.

As outlined above, we agree that recent work on PSZ thickness variations (e.g. by Sagy et al., 2007; Sagy and Brodsky, 2009; Shervais and Kirkpatrick, 2016; Kirkpatrick et al., 2018) needs to be discussed in the revised manuscript.

However, we also note, that it is questionable, if results of e.g. Sagy et al. (2007), Brodsky et al. (2011), and Sagy and Lyakhovsky (2019) are comparable with ours. These studies investigate fault planes and analyze their roughness by examining the geometric properties of bumps and depressions. For us, it does not imply that these geometric features directly link to fault gouge thickness variations as there are no data along transect oriented normal to the fault plane (compare with Sagy and Brodsky, 2009, where fault plane bumps and depressions have been attributed to deformation in a layer located structurally below the fault gouge).

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.

We agree that it would be optimal to study the fault zone architecture of the Alpine Fault in outcrops being located closer to each other. Nonetheless (and as outlined above), in absence of more outcrops than studied in the investigated fault segment, we consider it useful – though not ideal – to compare thickness variations at locations being as apart as they are in our study. While this approach does not allow to provide a detailed model of the fault zone architecture, we are confident that we are able to demonstrate that the Alpine Fault is more complex than previously thought (e.g. Norris and Cooper, 2007), and think that it is valuable to highlight this.

Our work is also valuable, because the Alpine Fault has accommodated so much more displacement than faults examined in previous studies of fault principal sip zone geometry

(see references introduced in our reply to point 2), which mostly focused on displacement-thickness- and displacement roughness-relationships.

[d] More generally, I feel that the pre-existing ideas from the mechanical literature on fault-zone width are not as well conidered 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.

We agree that there are other properties than mineralogy, which could affect rheology. However, the grain shapes and alignment we were able to examine appear to display only minor variations between individual outcrops. In the revised version, we will highlight that there could be other factors causing the observed thickness differences but that it is not possible to identify them owing to the scarce data.

We disagree with the reviewer that "this statement is not surprising or new and not well supported by references to existing literature". In the past it has been proposed that the Alpine Fault's architecture is consistent with the conceptual model of Caine et al. (1996) (e.g. Williams et al., 2016). However, we argue that this view is most likely an oversimplification and that the conceptual model presented by Faulkner et al. (2003) might be more suitable to describe the Alpine Fault (lines 566 – 568). We realize that this conclusion needs to be pointed out more explicitly in the revised manuscript.

The criticized sentence at lines 512 – 513 should be considered in context with its preceding sentence "[…] unlike other fault zones worldwide, the Alpine Fault displays a negative correlation between phyllosilicate content and fault core thickness", which is one of the key-messages of this study.

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.

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.

There is no doubt that the experiments reported in Schrank et al. (2008) are valuable to understand strain localization in general. However, their use to understand the geometry evolution of the Alpine Fault might be limited, because these experiments assume isochoric, isothermal, isochemical conditions and no changes in deformation mechanisms.

After having been uplifted along a single fault plane from a detachment at ~35 km depth (Little et al., 2005), the typical fault rock sequence encountered at the Alpine Fault (Figure 2c of the manuscript) records (a) progressively increasing strain from the hanging-wall towards the fault plane and (b) a change from ductile (mylonite formation; distal to the fault plane) to brittle (cataclasite formation; proximal to the fault plane) deformation mechanisms. We agree that the basic architecture of the fault might have been fixed at the onset of faulting. The Alpine Fault Zone rocks were deformed ductilely at depth then this was overprinted by brittle deformation. It is very likely the planar architecture and specific mineralogical arrangement imposed on the mylonites during creep have an impact on the fault gouges we focus on. This is dissimilar to the models the reviewer presents, but it is probably important in localization of deformation. However, our observations do not address this aspect. We are not looking at the results of early shear (i.e. fault zone evolution). Instead we focus on the description of the result of fault gouge evolution.

Fault rocks encountered in DFDP-1B (Figure 3a in the manuscript) demonstrate the complexity of the Alpine Fault Zone in the brittle regime. There, two PSZs were sampled, both surrounded by brittle fault rocks. We can interpret these two PSZs in two ways: (I) displacement was localized at more than one PSZ, which is dissimilar to the common view that Alpine Fault Zone uplift occurred along a single fault plane, and /or (II) that one PSZ

was abandoned and displacement transferred to another PSZ (fault gouge dating could help to resolve this issue, but is beyond the scope of this paper).

By writing this down, we realize that this line of reasoning is poorly explained in the manuscript (see lines 531 – 532) and needs revision.

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.

Below we respond to the specific comments on the manuscript except for cases where e.g. typos are highlighted. Numbers refer to the line in the pdf where the reviewer placed his comment

l. 45: It [the Punchbowl Fault] has a single PSZ.

CS: In Chester and Logan 1986, it can be seen that the longest continuous along-strike outcrop has a length of < 600 m. All other outcrops expose much shorter segments. I am mentioning this here to highlight that your study looks at a very different length scale. Moreover, one cannot be entirely certain that the Punchbowl fault consists of a continuous PSZ/core.

Our response: We briefly review published work on the Punchbowl Fault, because it is the classical example for the fault model proposed by Caine et al., (1996). However, we do not try to directly compare our observations with those obtained from the Punchbowl Fault, because the displacement on that structure and its overall geometry is dissimilar to the Alpine Fault.

l. 47: The core of the Punchbowl Fault is surrounded by an approx. 15 m thick damage zone […].

CS: In C&L 1986, they state 30 m as average DZ width.

Our response: Chester and Logan (1986), p. 83, last paragraph states: “These features increase in intensity towards the main gouge zone and define a zone of intense deformation that is laterally continuous but variable in width, averaging 15m.”

l. 60: Atypical for fault zones hosted in quartzofeldspathic protoliths […].

CS: I am not aware of a study that convincingly establishes this link in such general terms. Could you please add the related citation?

Our response: Here is an example: Faulkner et al. (2003) in their study comparing the quartzofeldspathic Punchbowl and San Gabriel Faults with the phyllosilicate-rich Carboneras

Fault state that “extreme localization, such as that seen more commonly in quartzo-feldspathic fault zones, suggests strain weakening and/or velocity weakening” (section 3.2, 1st paragraph).

l. 242: These [PSZ] thickness variations appear to be not systematically controlled by protolith lithology […].

CS: Can you really assess this? How much lateral displacement has occurred in each outcrop? What constitutes "lithology" in your case? Composition? Grain size? Grain shape? A relatively large number of microstructural properties determine the mechanical properties of a material - not just composition - and, of course, finite strain is quite important for the PSZ thickness.

Our response: Figure 2(b) shows that four out of five sample locations have the Alpine Schist (Torlesse Terrane) as protolith. In this case “lithology” refers to mineralogical composition. However, to acknowledge the referee’s valid concern, we will reformulate the sentence to: “Except for Martyr River, these locations have the same protolith (Fig. 2b.; see section 4.3). Furthermore, there is no systematic variation of PSZ thickness along-strike. This highlights that the fault core has a very variable character at different locations.”

l. 279: In addition, some microfaults have cores made of coarser-grained matrix and clasts or calcite veins surrounded by fine-grained gouge.

CS: Do you know the relative timing between microfaulting and calcite veins?

Our response: We re-examined the images displaying microfaults with calcite veins. We definitely cannot derive relative ages from cross-cutting relationships in them. Consider Figure 6i (below “B” of “BSE”) – here the microfault-matrix contains calcite clasts, which may originate from comminution of a calcite vein, which would imply that vein formation predated microfaulting. However, calcite veins are also seen in the cores of some other microfaults (no images shown in the manuscript), which indicates that vein formation postdates microfaulting. So… there are no systematic cross-cutting relationships and we cannot define one simple sequence of events.

l. 306: Where the PSZ is thinner, clasts tend to be more abundant and sizes tend to be larger.

CS: One could simply conduct a statistical test to quantify the correlation strength. Please, feel encouraged.

Our response: These observations are qualitative not quantitative.

l. 340: There are no discrete slip planes within the fault gouges.

CS: You did not sample any – preservation / recognition potential are an issue…

Our response: We will reformulate this sentence.

l. 364: They [Qtz and Fsp phases] do not exhibit any clear trend towards the PSZ.

CS: I find it difficult to discern trends from looking at tables. I am also uncertain of the meaning of "towards the PSZ" - as a function of orthogonal distance to PSZ?

Our response: Prior to the initial submission of the manuscript, we considered generating a figure demonstrating mineralogy. However, the number of samples investigated as well as sampling distance with respect to PSZ varies so much between locations that such a figure would be very hard to read. Also, there are only minor qualitative compositional changes between individual locations. The most important result of the mineralogical analysis is actually that thicker PSZs appear to contain fewer phyllosilicates than thinner ones. This is clearly shown in Figure 8. However, we have now realized that a description of the mineralogy of the footwall gravels was missing. It will be included (new section 4.3.3) for completeness in the revised manuscript.

l. 408: In summary, both microstructures and the predominant enrichment of elements show that fluids are not responsible for stress-driven dissolution processes or for substantial mass transfer out of the fault zone.

CS: Given my methodological criticism of the isocon analysis as well as potentially overlooked slickolites/stylolites in other samples, I believe that this point remains to be tested. Moreover, what about the large amount of authigenic micas? Your conclusions here at complete odds with the SAFOD paper by Schleicher et al. 2009, which you cite below but in a different context.

Our response: Please refer to the comment on Figure 6 below for our discussion of potentially overloocked slickolites / stylolites. Better constrained protolith composition did not substantially affect the general results of the isocon analysis (i.e. predominant enrichment of elements; see reply to RC1 – 2). While we admit that this is basically the opposite of what Schleicher et al. (2009) found for the San Andreas Fault ("most of the elements are depleted in the fault-related grains compared to the wall rock lithology"), we emphasize, as we did in our response to this referee's major comments, that this difference could originate from the fact that the San Andreas Fault has a different hydrological systems compared to the Alpine Fault (compare Xue et al., 2016, describing the San Andreas Fault hydrological system with Menzies et al., 2016, analyzing the Alpine Fault hydrology). Furthermore, the hydrological system of the hanging-wall is very active leading to intensive fluid-related alteration (Sutherland et al., 2012). One example of this process is shown in Figure 6a of the manuscript where plagioclase is altered to phyllosilicates. The same alteration mineralogy was shown by Schleicher et al. (2009) in the San Andreas Fault ("[…] albite/anorthite minerals […] altered into I-S clay minerals"). In this context, the "large amounts of authigenic micas" (which increase towards the PSZ; Sutherland et al., 2012) reflect rather element enrichment than dissolution.

l. 480: Fault gouge contains fewer and preferentially smaller clasts than the overlying hanging-wall-proximal layer […] and bounds mechanically weak gravels.

CS: What is your evidence?

Our response: See response to comment on line 306.

l. 483: Consequently, layered fault gouges and associated microstructural evidence demonstrate that strain localization and associated structural complexities within the Alpine Fault core are […] affected by shallow, hence late, processes.

CS: I am not convinced that you present clear evidence to support this statement. It appears rather difficult to figure out when the PSZ/gouge was active

Our response: This statement is supported not by observations of the gouge itself, but by the fact that the PSZ gouge cross-cuts all other structures in the fault core, so it must be the last active part of the fault system, and accommodate coseismic displacements. Furthermore, we know that Alpine Fault slip displaces the ground surface, because there are offsets of Quaternary features (e.g. Cooper and Norris, 1990; Berryman et al., 2012). So the gouge must accommodate slip in the very near-surface. We will include this argument in the revised manuscript.

l. 493/494: It has been suggested that such variations [in PSZ thickness] may results from different mechanisms of deformation (Hobbs et al., 1990; Schrank et al., 2008).

CS: In this context, it is important to look at the paper of Kirkpatrick et al. 2018. The references therein point to other relevant papers in this field. These papers, more correctly, speak about the effect of the constitutive model - which is an upscaled response of a microphysical deformation mechanism.

Our response: Will be elaborated in the revised version.

l. 500: These explanations do not appear to be responsible for observed variations as there is no correlation between PSZ thickness and sample location along-strike.

CS: Given your poor data situation – mainly due to exposure conditions and perhaps an inappropriate choice of length scale of observation – you cannot test this notion.

Our response: We agree that observations might be biased by outcrop situation and availability. However, the statement is based on the five locations investigated, where we do not observe a systematic increase / decrease of PSZ thickness from SW to NE.

l. 505: This is astonishing, because phyllosilicate-rich fault gouges generally tend to be wider (Faulkner et al., 2003; Schleicher et al., 2009) owing [to] the frictionally weak nature of these phases (Moore et al., 1997; Moore and Lockner, 2007; Lockner et al., 2011; Carpenter et al., 2015.

CS: These papers do not explicitly establish the statement made by the authors. I have other comments noted in the letter to the authors.

Our response: Faulkner et al. (2003), section 5.1: "The thickness of the zone of deformation […] is thought to be due to the mechanics of deformation of phyllosilicate-rich fault gouge." We agree, we erroneously cited Schleicher et al. (2009) here, and will remove that citation.

l. 508: The very narrow fault gouge at Martyr River contains frictionally extraordinary weak serpentine phases which elsewhere have typically dominated in fairly thick fault zones (Moore et al., 1997; Moore and Rymer, 2007, 2012).

CS: I feel that these statements and citations do not align very well. For example, Moore and Rymer 2007 show that serpentine is too strong to explain the weakness of the SAF - they find talc as the culprit for weakness. So speaking of "extraordinary weak serpentine phases" with these papers is not a great idea. Moreover, the fault dimensions noted in their 2012 paper do not vary much from the range of APF discussed here. Please, revisit.

Our response: We re-examined the cited literature with this specific comment in mind. It is debatable that talc causes the weakness of the San Andreas Fault (see e.g. Warr et al., 2014: "The hypothesis that talc causes creep at the depth of the borehole (Moore and Rymer, 2007; Wibberley, 2007) has not gained support due to the rare occurrence of this mineral in SAFOD fault gouge"), and thus removed the reference to Moore and Rymer (2007).

Also, we acknowledge that while serpentinite can have a quite low coefficient of friction (Moore and Reyhnolds, 1997; Moore and Lockner, 2004; Moore and Rymer, 2012), it is not "extraordinarily low", so we will remove that term. However, it is true that Moore and Rymer (2012) describe a 3 – 50 m wide "tectonic shear zone of serpentinite", which is more than two orders of magnitude thicker than the PSZ at Martyr River, so this part of the comment was correct. We also realize that we should change the emphasis to highlight the fact that the gouge from Martyr River is phyllosilicate- rather than serpentine-rich, as this probably has a greater influence on the way the gouge accommodates deformation, and we will make this change in the revised manuscript.

l. 510: Conversely, fault zones with similar quartzofeldspathic composition to the Alpine Fault are typically narrower (Chester et al., 1993; Faulkner et al., 2003). Consequently, unlike other fault zones worldwide, the Alpine Fault displays a negative correlation between phyllosilicate content and fault core thickness.

CS: Another example of somewhat tenuous alignment of the author's statement and the literature cited in support of it. Both papers examine a single particular field example of one fault. Thus, it seems quite bold to claim that they "are typically narrower". Similarly, I'd be curious to see the references for the statement in the next sentence regarding "fault zones worldwide".

Our response: We agree that we generalized the statement too much and will revise it.

l. 514: In addition, orientation of the fault (Figs. 2a & b), magnitude of stress and the stress field itself (Boese et al., 2012; Warren-Smith et al., 2017) are fairly constant along the central segment of the Alpine Fault.

CS: See my comments in the letter to the authors – I don't think this is true. Local geometrical perturbations can easily induce large local stress differences.

Our response: We stated this because the overall tectonic plate motions driving slip and generating stress fields within the Alpine Fault are fairly constant along its length, and the fault planes we investigated are all similar in orientation so the traction vectors resolved on them will also be similar. However, we recognise there are substantial topographic variations along strike of the Alpine Fault as it is cut by large glacial (and post glacial) valleys. Recently Upton et al. (2017) modelled the topographically-induced variations in stress and

demonstrated these indeed yield substantial local variations. We realise that we need to acknowledge this possibility, and will consequently add to the revised manuscript "However, it is still possible that the extreme variations in topography typical of the Alpine Fault hanging-wall generated quite different stress fields at our investigated sites (cf. Upton et al., 2017)."

l. 518: Biegel and Sammis (2004) suggested to explain along-strike variations of gouge thickness as record of rupture arrest. However, our observations (and the nature of Alpine Fault outcrops in general) do not easily lend themselves to a systematic analysis of this kind in the Alpine Fault zone.

CS: Absolutely correct – but that does not mean that you can rule out that these effects mattered. It would be instructive and convenient for the reader if you stated, which thickness variations this effect can cause.

Our response: We addressed a variety of potential mechanisms explaining PSZ thickness variations. We are quite confident that our proposed explanation (we actually investigated different faults planes having accommodated different amounts of displacement) is reasonable.

However, we are aware that the difficult outcrop situation prevents us from completely ruling out other explanations, which is not satisfying. Thickness variations as consequence of rupture arrest are mentioned for completeness. However, we do not see any mechanism explaining how rupture arrest could cause these variations. The original reference suggests that Biegel and Sammis (2004) had the same issue: "It is well known that the thickness of the gouge and breccia layers vary significantly along strike. Is it possible that these variations record the arrest of rupture?"

l. 521: However, quantitative models of fault core evolution have so far failed to reproduce datasets compiled on natural faults (e.g. Blenkinsop, 1989; Evans 1990; Sibson, 2003).

CS: Another example of poorly chosen references: these papers do not provide quantitative models of fault core evolution – they deal with empirical scaling relations and do not offer physics-based forward models. This problem is unsolved!

Our response: We disagree that these references are poorly chosen. Scholz (1987) summarizes empirical relations showing that thickness increases with displacement and "offer[s] a simple model that quantitatively accounts for this behavior". Hull (1988) states that he "[presents] empirical relationships between thickness and displacement" and quantifies this relationship by stating that "faults exhibit a linear correlation […] and an average displacement / thickness ratio of 63". The references we cited criticize the Scholz and Hull models; for example Evans (1990) states: "Conceptual and quantitative models which rely heavily on thickness-displacement relationships should be considered with caution until further data are collected on the topic. […] The linear and logarithmic plots of these data indicate a general increase in displacement with thickness, but the scatter of data show that this is a qualitative relationship […]." In the revised version we will emphasize the empirical character of these relationships.

l. 524: In other words: the more earthquakes a fault has seen, the thicker its PSZ may be (Ma et al., 2006; Li et al., 2013).

CS: There are alternatives. Slow creep along the fault can also modify the width to growth when the fault core is strain-hardening. See Schrank et al. 2008 as an experimental example.

Our response: We are only referring to relationships previously empirically-determined in earthquake-generating faults that slip episodically and have strain-weakening fault cores. The alternative indicated in Schrank et al. (2008) is not very relevant for the Alpine Fault, which is seismically locked down to 12 – 18 km (Beavan et al., 2007) and has displayed no measureable creep in historic times (see Schuck et al., 2018 for a discussion).

l. 526: […] the Alpine Fault's central segment, which is considered to rupture entirely during an earthquake (Sutherland et al., 2007; Howarth et al., 2018) […].

CS: This is a big assumption - how can you be sure that this was the case over the lifetime of the structure?

Our response: Berryman et al., (2012) showed the Alpine Fault accommodates earthquake ruptures, with ground displacements sufficient to dam rivers and create peat bogs, every ~300 years, and has done so for the last >5000 years. Coffey (2014) demonstrated evidence for three earthquakes of sufficient magnitude to create sieches in pro-glacial lakes with similar return periods c. 17ka. It is more difficult to understand why the fault would have behaved in a significantly different way prior to that than it is to assume it behaved the same. However, we will acknowledge this possibility by adding to the revised manuscript "and to have done so for at least the last ~5000 years (Berryman et al., 2012)".

l. 527/528: PSZ thickness variations suggest that studied fault gouges accommodated different amounts of displacement. This implies the studied PSZs are not part of the same shear plane, and that the Alpine Fault zone hosts multiple fault strands […].

CS: I disagree. Shear displacement is the integral of shear strain over sz width. See my simple numerical model. As soon as you have the slightest change in thickness, shear strain is no longer constant in the PSZ even if the applied displacement is the same at the far field. In other words: your statement is only true if the shear strain is identical everywhere in the PSZ - and one can rule out this assumption almost categorically at the scale of investigation. If you accept my criticism on the preceding sentence, this logical conclusion is no longer valid. However, I would agree that the statement is likely correct but for different reasons: it would be incredibly amazing and surprising to observe a fault on the scales of tens to hundreds of kilometers which features only on fault strand!

Our response: The comment inspires us to be more explicit here, and so we will modify the sentence to read "If we employ the simple relationship that shear strain in a fault zone is equal to zone width multiplied by boundary displacement, and assume constant strain distribution within the PSZ gouges, then the PSZ thickness variations we describe suggest the studied fault gouges accommodated different amounts of displacement. This implies the PSZs are not part of the same shear plane, and therefore that the Alpine Fault zone hosts multiple fault strands."

l. 534: This is unexpected given the small separation between boreholes and thus sample spacing of less than 100m.

CS: I have seen many rock, which are compositionally heterogeneous, and in terms of fabric, on the 10 to 100-m scale. Again, the opposite would be a surprise in schists and gravels as host rock. A flood basalt may be different...

Our response: We agree, compositional and microstructural heterogeneities on the 10 to 100 meter scale are not uncommon. However, the situation encountered at DFDP-1A and DFDP-1B differs, as indicated by Boulton et al., 2014: "Because each PSZ gouge has a distinct mineral assemblage representative of high or low temperature alteration, experimental results quantify how frictional properties vary within mineralogy, temperature and pressure". In our view, these dramatic differences appear conspicuous, if one assumes that both PSZs (less than 100m apart) are part of the same fault plane sharing the same record of P and T. We do realize now that it is necessary to explicitly paraphrase the observations of Boulton et al. (2014) in our revised discussion.

l. 543: […] which is atypical for quartzofeldspathic faults but typical for carbonate or phyllosilicate-hosted ones.

CS: I would appreciate more careful linking with existing literature to establish this point. At the moment, I feel doubt that the literature and your data support this notion firmly. But I'd be keen to be falsified in this feeling!

Our response: Please refer to the reply to our comment on line 510.

l. 553: Conclusions […]

CS: Given my criticism regarding the isocon data/method and mechanical interpretation, etc., I can expect the need to echo this in the conclusion. Forgive me if I do not do this for the sake of avoiding redundancy!

Our response: We will modify our conclusions to reflect the revisions we make to the discussion to accommodate the reviewer's major comments about this aspect.

Figure 6: The reviewer suggested we had overlooked the presence of stylolites in the images shown Figures 6c and 6e. We have systematically reexamined these images and still think we were correct in our interpretation sated at lines 404 – 405 ("furthermore, except for the slickolite observed in the DFDP-1A fault core (Fig. 7g), there is no microstructural evidence for pressure-solution such as dissolution seams or indented and embayed clasts").

Figure 6c of the manuscript contains a feature of undulatory shape that resembles a stylolite, but closer examination reveals no unambiguous evidence for pressure-solution, such as indented and embayed clasts. The feature undulates where it abuts clasts in the gouge it transects, but does not truncate them. We therefore prefer the interpretation that the featured formed by fracturing of a heterogeneous material.

The same is valid for Figure 6e of the manuscript, where there are definitely no indented and embayed clasts. The corresponding optical image (Figure 6g of the manuscript) shows the seam the reviewer indicated is a crack (white / transparent line in plain polarized light).

As recommend by the reviewer, where possible (locations WT, GC and MR), we will indicate the orientation and shear sense of the shear plane relative to the images in Figures 6 and 7.

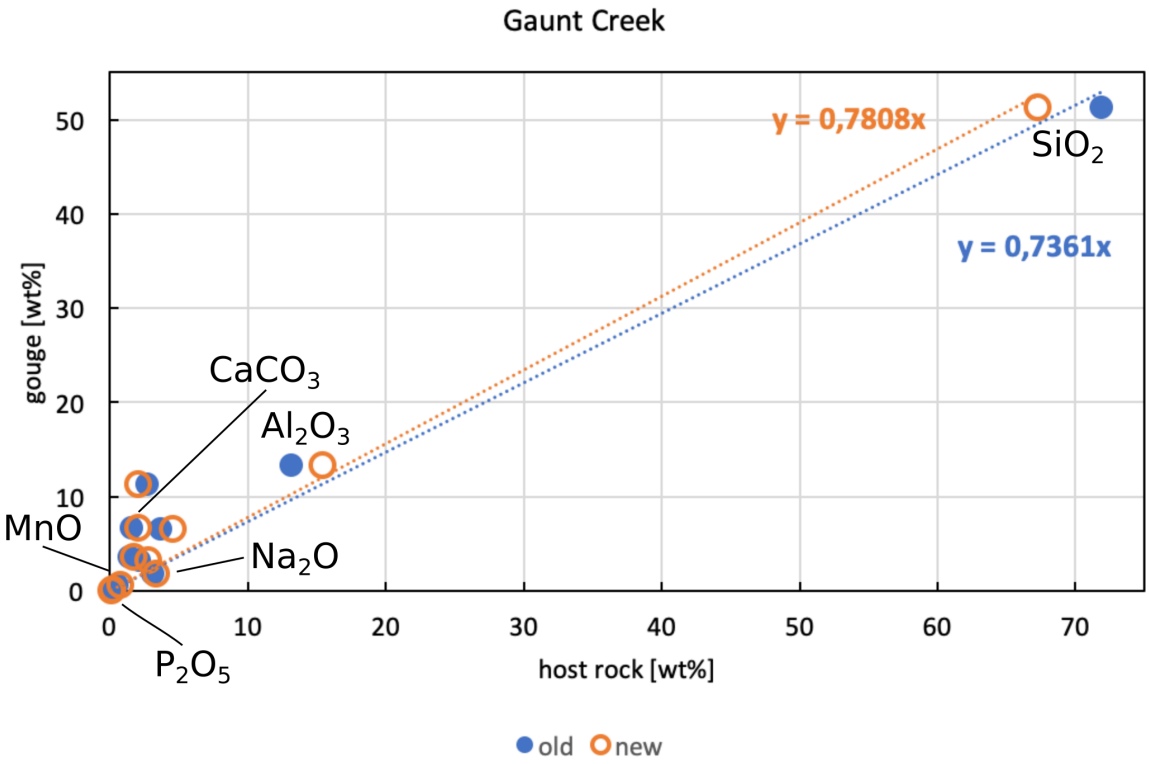

Figure 1: Isocon analyses of Alpine Fault PSZ at Gaunt Creek with host rock composition used in the initially submitted manuscript ("old") and the updated, better constraint one ("new"). The slightly larger slope of the new isocon implies a slightly lower mass gain (new: 28.1% vs. old: 35.9%).

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