Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-90-AC1, 2020

Interactive comment on "Micro- and nano-porosity of the active Alpine Fault zone, New Zealand" *by* Martina Kirilova et al.

Martina Kirilova et al.

martina.kirilova@uni-mainz.de

Received and published: 2 July 2020

Dear James Gilgannon, Thank you for your positive feedback and for taking the time to review our manuscript. We have carefully considered your comments and acknowledge their significance for beneficial and successful revision of this manuscript. Thus, we copy pasted and addressed each one of them in the text below.

General comments: This being said I have a few suggestions for the authors to consider in my specific comments. The majority of which relate to the methods section, where I think that some of the explanation should be reformulated and details added, alongside a suggestion of a figure to help the reader. Additionally, I think there is a need to better link the XCT and TEM data sets to enrich the results and hence shore

up the discussion. I have one less trivial concern that I would like the authors to address: stated here briefly, I am uncertain about how much can be made of a difference of 0.14

Response: Thank you for this summary and the constructive comments. We have addressed each one of them following the specific comments below.

Specific comments: I have tried to group the comments into blocks and they are ordered mostly in sequence with the order of the manuscript.

1. Comment on methods As it stands I think the methods section needs bolstering in places. Below I have noted where I think the manuscript could benefit from this: Lines 104 - 134: Analyses of XCT datasets The structure here gives the feeling that you tired one method but subsequently chose another over it. After reading the manuscript over a few times I can see that this is not the case and you actually use both methods: in a first step, you use the 'connected components' method for visualising pores in space and then in a second step you characterise the porosity histograms with your MATLAB code. I would recommend reformulating how section 3.3 is written to make it more clear that you did two things. I would go so far as to make subsections: 1) Segmented porosity for visualisation and 2) Quantifying total porosity. In this way it becomes clearer that you did both and the reason for using the integral of the pore volume histogram becomes clearer.

Response: We used Avizo software for initial processing (i.e. rescaling and filtering) of the data and thresholding. "Connected components" with limitation of the data (i.e. limiting the connected components up to 200 connected voxels) was applied only for visualization purposes. This is described in lines 105-114. The remainder of the pore analyses (both total porosities and shape analyses) were performed by implementing Matlab scripts using the whole threshold range in numerical format (look at lines 116-117 "Instead, the volumes of segmented materials (including cracks) were exported from Avizo software in numerical format" We acknowledge the fact that the reviewer was confused about our methodology. Thus, in order to make the text easier to follow, in line 116 we will add the following clarification: “Therefore, “connected components” with limit of 200 connected voxels were used only for visualization purposes.”

Of course then you would require a further subsection for the description of pore geometry (ie. the use of the covariance matrix), of which I presumed you have used pores from the ’connected components’ methods but limited to the size range you stated.

Response: No, as stated in lines 116-117 (“Instead, the volumes of segmented materials (including cracks) were exported from Avizo software in numerical format”) the pore sizes were not limited by the function “connected components”. Any further data manipulation was performed on Matlab. To clarify this, we will change the statement in lines 116-117 to: “Instead, the volumes and shape characteristics of segmented materials (including cracks) were exported from Avizo software in numerical format” And also, in line 123 we replace “Pore shapes were analyzed on bivariate histograms.” with “Pore shapes were analyzed on bivariate histograms plotted on Matlab by using the numerical pore characteristics, previously extracted from Avizo software.”

Lines 123 - 130: Pore shape descriptors The manuscript would significantly benefit from a figure illustrating the relevant aspects of the use of the covariance matrix. For example, I do not understand the author’s characterisation of sphericity. I may have misunderstood the description but the ratio of two eigenvalues, which are both contained within a plane, surely cannot describe the deviation/tendency to a sphere, or have I misunderstood the metric you present? I am more familiar with sphericity being the ratio of the equivalent surface area of a sphere with the same volume as the pore volume over the actual surface area of the pore volume (e.g. Wadell, H. (1932))? For this reason, I think that the section would benefit enormously from an example figure that corresponds to, and visualises the explanation of the metric. I imagine this would be best done with some specific examples of pore volumes from your data set. If the authors have not come up with the method themselves then I think that a citation for the more curious reader is also necessary. Wadell, H., 1932. Volume, shape, and

roundness of rock particles. The Journal of Geology, 40(5), pp.443-451.

Response: Thank you for this comment. No, we have not come up with the methods ourselves. All the shape analyses we performed are based on functions embedded in Avizo software that yield volumetric and shape characteristics for each segmented material in numerical format (lines 116-117). We simply plotted the results on bivariate histograms by using Matlab as stated in line 123. We do not find it necessary to include in the manuscript a description of how the software produces those results as Avizo software is trusted source and every user/reader can refer to their library. However, here we provide a brief explanation of the functions we have used: The covariance matrix is built on the basis of the moments of inertia. By using this matrix, the software computes the three eigenvalues by using a Singular Value decomposition. In an elongated ellipsoid the largest eigenvalue will describe the longest axis of the 3d object. In this context, the deviation of the spherical form (i.e. anisotropy - a value extracted from Avizo software) is measured as 1 minus the ration of the smallest to largest eigenvalue. In a 3D object if the smallest and the longest axis are equal, the medium will have the same value as well, describing a spherical object and having numerical value = 0.

Lines 132 - 134: Pore density calculation I think that it should be briefly mentioned how the density calculation was made. Was a kernel used? and if so how was the bandwidth chosen to account for number of data points? Or was it a point density calculation, if so what neighbourhood was used? I think the amount of information currently given is too sparse.

Response: We plotted the orientation of the longest eigenvalue of each pore on a lower hemisphere equal area stereographic projection. Thus, these stereonets do not represent pore density calculation but clusters of pores with preferred orientation. The data was plotted by using bivariate histogram bin counts implemented in Matlab (i.e. histcounts2), where: [N,Xedges,Yedges] = histcounts2(X,Y,Xedges,Yedges) The bivariate histogram results in bins with a predefined set of edges and the number of pore orientations that fall within each bin. This partitions X and Y into bins with the bin edges specified. For contouring we used the countouring algorithm implemented in Matlab. Relevant lines from the script used are below:

```
[cs,cLats,cLons] = histcounts2(Lats,Lons,-95:10:95,-100:20:100);
cLats(end)=[]; cLons(end)=[];
cLats = cLats + (cLats(2)-cLats(1))/2;
cLons = cLons + (cLons(2)-cLons(1))/2;

COLORBAR = 0:25:225;
contourfm(cLats,cLons,cs,'LevelList',COLORBAR(2):COLORBAR(2):COLORBAR(end));
caxis([COLORBAR(1) COLORBAR(end)])
contourcbar
```

2. Questions/concern regarding total porosity calculations My questions/concern is regarding the uncertainty associated with the filtering of pore data used and how this translates into the discussed differences in the magnitudes of the total porosity from different samples. Your TEM results show that very small tures exist, which you identify as fracture porosity and, by the general argument of the paper, could have resulted from coring. While these fractures shown in fig. 8d are below the XCT resolution, I am brought to wonder how many slightly larger pores exist that are actually induced fractures. For example, the fact that so many small pores identified by XCT are almost completely flat in shape (fig. 6) might reflect that many small fractures, that are not syn-kinematic, are retained in the analysis. Therefore for me a question that presents itself is; does a simple size threshold, as you have used, have an appropriate amount of filtering information to allow a discussion about a difference of 0.14I am uncertain if it is correct to straightway interpret this difference of 0.14

Response: We understand the reviewer's concern that big pores and small fractures could get easily misinterpreted/mislabeled in XCT datasets. This exactly is the prime reason why we decided against calculating total porosities in these samples by simply using 'connected components' and instead we fitted the data to a polynomial curve

(mentioned in lines 115-121). We believe that implementing a mathematical approach is much more trustworthy than limiting the data based on the interpreter's bias. Furthermore, our total porosity calculations (by using the polynomial fit) roughly coincide with the total porosities yield by calculating the total porosities based on connected components with up to 200 voxels. You can see these numbers on the table below:

DFDP-1B	polynomyal fit	200 limit%
DFDP-1B 58_1.9 (Sam73)	0.10	0.10
DFDP-1B 69_2.48 (Sam79)	0.12	0.11
DFDP-1B 69_2.54 (Sam19)	0.10	0.09
DFDP-1B 69_2.57 (Sam69)	0.24	0.17

The reviewer also expressed concern about the fact that some of the very flat pores may represent fractures. We acknowledge the validity of this statement. However, we believe that our approach of excluding cracks is efficient and possibly the best methodology for analyzing these samples (i.e. fitting the data to a polynomial curve). Furthermore, the shape of these pores is also very likely to result from their distribution along grain boundaries, especially of clay minerals (lines 190-191). The authors of the manuscript are in favour of this second scenario. And last but not least, the difference of 0.14

3. Comment on linking XCT and TEM observations The manuscript has a well crafted 'red thread' for the reader to follow but I feel that there is a gap in the current argument that requires some attention. The current formulation of the results goes from core/log scale to four very focused pictures of nano features by way of some abstract shape metrics at the micro scale. I am aware that figure 2 is supposed to bridge this gap by visualising the XCT data but it gives far too little information and doesn't allow the reader to see that your chosen TEM images are actually representative. The reader is left trusting the authors on things that can be evidenced with your current data sets. To address this I think that there needs to be a more tangible link between the records of

the microstructure in the XCT and TEM data sets. For example, the XCT and TEM data sets should be used for comparison/corroboration of the porosity/mineral associations. The XCT data is underutilised with respect to showing the microstructure and the discussion would benefit from the evidential support that would come from the inclusion of a figure that visualises slices through the XCT data. In a very broad sense, this information showing what the microstructure looks like in the XCT data set is needed to provide a more convincing argument for the general habit of porosity (for example, that they occur 'especially' proximal to clay minerals). Currently, there are assumptions or logical jumps made by the authors in the discussion which are not necessary because the data sets at hand have information to support or falsify these suppositions. Additionally, the absence of this data was what partly led me to my comment/questions in point 2 because I was not given enough information to understand how the differences in total porosity estimates related to the different sample microstructures. Even with this aside, I would personally like to see a figure that better contextualises the micro-scale pores and their associations. Mostly I recommend this because, as I said in the general comments, your data sets are very special and as a curious reader I would like to be furnished with as much information of what the rocks look like as possible.

Response: As we mentioned in lines 184-187 the TEM images focus mainly on nano-scale materials, however, the largest pores observed on those images are also captured by (or comparable with) the smallest resolution of the XCT data. This justifies the validity of our argument that similar mineral – pore distribution is present both on nano- and macro-scale. This is further supported by the fact that both TEM (Fig. 8) and XCT shape analyses (Figs 4, 5 and 6) indicate the presence of predominantly elongated, flat pores (lines 188-191). Therefore, we do not agree with the reviewer's comment that there is a gap in our arguments. Instead, we think we have provided sufficient data to demonstrate to the reader the validity of our interpretation rather than asking them to trust our judgement. Furthermore, we disagree that we have underutilized our XCT datasets. Instead, most of our interpretations are based on porosity estimates, and shape analyses yield from the XCT datasets.

TEM images were merely used to relate the distribution of pores in respect to different minerals and to give a microstructural context to our porosity analyses. Regardless, here we show two Avizo snapshots (figures 1 an 2) that demonstrate that pores are distributed along grain boundaries. The examples are taken from sample DFDP-1B $69_2.54.Wearehappytoincludetheseimagesinfigure2.$

4. Question about section 5.3 and the concluding sentence of the manuscript Is the porosity change not a consequence of the activity of other processes rather than a controlling factor? In the sense of your argument that the operation of mineral precipitation will lead to evaluated pore fluid pressures or fault rock weakness due to clay precipitation. Phrased as is, section 5.3 and the conclusions seem to make two arguments at the same time: the first giving the impression that porosity can provide a driving force for change and the second that its change is just a marker for the in- creased activity of other processes which will drive change. I would argue, within the framework of your manuscript, that changes in porosity only chart the activity of other processes that actively dissipate energy and the activity of these other processes ulti- mately control fault rock stability.

Response: This is a very good point, thank you. In section 5.3, we aim to demonstrate to the reader that porosity is very closely interlinked with fluid circulation and mineral precipitation, both of which may change the mechanical behaviour of the rocks, and thus trigger an earthquake. However, the amount of porosity and/or the presence of porosity in these rocks defines how these processes may evolve. Therefore, the state of porosity in these rocks plays a key role, and thus we conclude that the porosity is actually a controlling factor on the mechanical behaviour of the Alpine Fault.

5. Clarification of the word overpressure As a last comment, I would recommend that the word overpressure is defined some- where in the introduction. It is featured prominently in the first sentence and second last sentence of the abstract as well as the manuscript's conclusions but I am not sure to what the authors mean by it. I ask because it was my understanding that the bore hole fluid pressure measurements of

Sutherland et al. (2012) found that, while fluid pressure was compartmentalised around the fault, the fluid pressure was never above hydrostatic. It may be worth a sentence or two that elaborates if the authors are re- ferring to elevated fluid pressures or fluid pressures that exceed hydrostatic or some other meaning. Alternatively, the authors may not need to use the word overpressure as I think that the word is never mentioned in the discussion. Sutherland, R., Toy, V.G., Townend, J., Cox, S.C., Eccles, J.D., Faulkner, D.R., Prior, D.J., Norris, R.J., Mariani, E., Boulton, C. and Carpenter, B.M., 2012. Drilling reveals fluid control on architecture and rupture of the Alpine fault, New Zealand. Geology, 40(12), pp.1143-1146.

Response: We are familiar with the work of Sutherland et al. (2012), and we do agree with it. In addition, our work further supports the conclusions in their study (lines 237-239). We do not state anywhere in our manuscript that fluid overpressure has been achieved in these rocks. We only speculate that the very low total porosities in these rocks and the processes affected by them (i.e. mineral precipitation and fluid circulation) can eventually lead to fluid overpressure, and thus trigger an earthquake. But in order to avoid confusion caused by different terminology, we will replace “fluid overpressure” with “elevated pore fluid pressure”.

Technical corrections: Line 70: ‘. . .gouge zone with predominantly random fabric. . .’ to ‘. . .gouge zone with a predominantly random fabric. . .’ Response: Thank you. We will modify the text. Line 71: ‘This cohesive but uncemented layer has significantly. . .’ to ‘This cohesive but uncemented layer has a significantly. . .’ Response: The correction will be introduced in the text. Line 88: ‘Detailed lithological and microstructural description. . .’ to ‘Detailed lithological and microstructural descriptions. . .’ Response: We will modify the text accordingly.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-90, 2020.

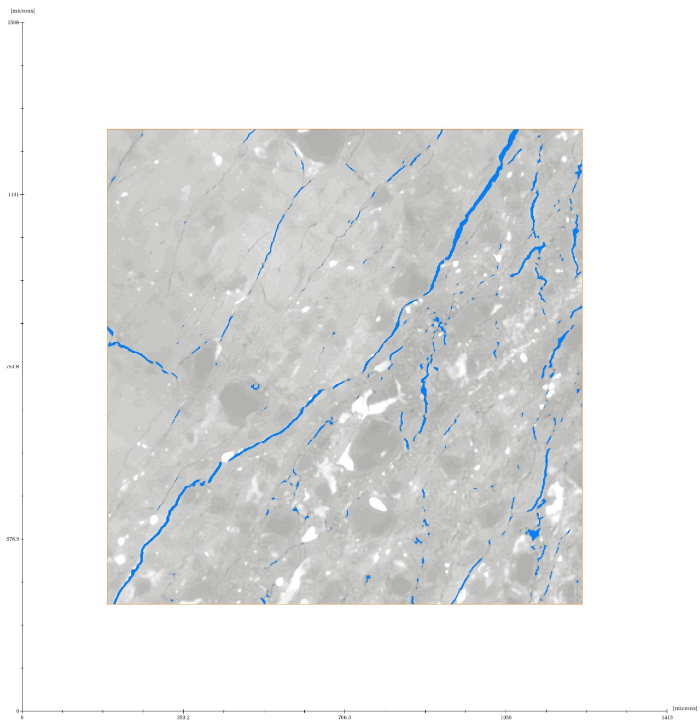

Fig. 1.

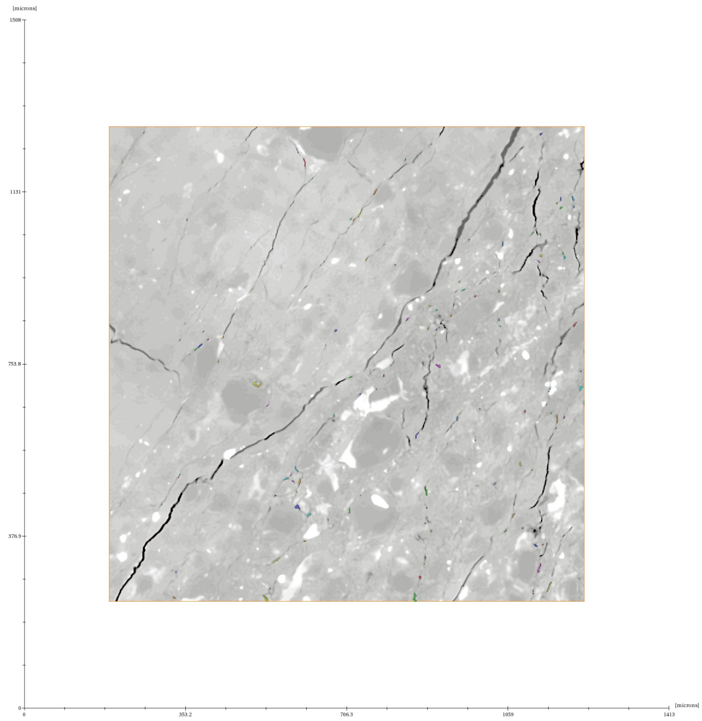

Fig. 2.