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

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Dear Editor,

As requested, I have reviewed the manuscript titled "Micro- and nano-porosity of the active Alpine Fault zone, New Zealand" by Kirilova et al., please find my general and specific comments below.

Kirilova et al. present data from the analysis of a core recovered during the Deep Fault Drilling Project (DFDP-1B). Synchrotron X-ray microtomography (XCT) was used in conjunction with Transmission electron microscopy (TEM) to gain information about the porosity and its habit during the critical pre-rupture stage of a major fault's seismic-cycle. These results are used to discuss the potential differences in processes that

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are active in and around the Alpine fault and ultimately how this could effect a fault's transition to the next stage of its seismic cycle.

General comments:

The contribution from Kirilova et al. utilises a data set that is very special and provides a rare window into the microstructural state of a fault that is known to be approaching its next major earthquake rupture. Thus this contribution contains key information that brings us towards understanding how and why earthquakes can occur. The manuscript is well written, engaging and does a great job of orienting the reader in existing literature for hydro-chemo-mechanical feedbacks and the Alpine fault in general. It is clear that much effort has been made to make the contribution interesting and as such it was a great read.

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 $\sim$0.14% porosity between samples given the filtering methods used. The authors use this difference as part of the main discussion on how changes in porosity will affect a major fault's mechanical state and I think that the significance of the result needs to be unpacked and evaluated more in the text. Lastly, I think the authors should be careful with when they say they 'demonstrate' or 'show' that certain processes are active. The results of this contribution are almost singularly observations about the characteristics of each sample's porosity and in this sense the authors do not show but rather interpret the presence of fluids or the activity of pressure solution. I think this is an important distinction to be drawn.

I very much enjoyed reading the manuscript and think that, with the inclusion of some more visualisation of the XCT data and some clarifications on the limitations of observations, it provides another solid step forward in understanding fault rocks and their dynamics.

Best wishes,

James Gilgannon

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. 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

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stated.

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.

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.

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 fractures 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 ∼0.14% porosity? Stated more plainly, how do you know if the variation between samples is not just a function of the degree to which each lithology experienced the coring and retrieval? Alternatively, can you rule out that the variation of ∼0.14% may just be related to the uncertainty of the polynomial fitting used to cap the pore size for integration?

I am uncertain if it is correct to straightway interpret this difference of ∼0.14% as meaningful. I think that more interrogation of this result needs to appear in the discussion before it is taken forward as independent confirmation of other literature. It might be that the authors wish to use the bore hole and laboratory measurements of permeability that are mentioned in the text to quantitatively check if the difference of ∼0.14% in total porosity can account for these differences in permeability. I am aware that this would require some assumptions when calculating but it would provide a base to the interpretation that a difference of ∼0.14% porosity between samples is meaningful. As currently presented I think that the result only convincingly shows that each calculated porosity is of the same order of magnitude.

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 under-utilised 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.

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 increased 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 ultimately control fault rock stability.

5. Clarification of the word overpressure

As a last comment, I would recommend that the word overpressure is defined somewhere 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 referring 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.

Technical corrections:

Line 70: '…gouge zone with predominantly random fabric…' to '…gouge zone with a predominantly random fabric…'

Line 71: 'This cohesive but uncemented layer has significantly…' to 'This cohesive but uncemented layer has a significantly…'

Line 88: 'Detailed lithological and microstructural description…' to 'Detailed lithological and microstructural descriptions. . .’

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