Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-71-AC3, 2020

Solid Earth
Discussions

Interactive comment on "Analysis of deformation bands associated with the Trachyte Mesa intrusion, Henry Mountains, Utah: implications for reservoir connectivity and fluid flow around sill intrusions" *by* Penelope I. R. Wilson et al.

Penelope I. R. Wilson et al.

p.wilson@kingston.ac.uk

Received and published: 14 October 2020

Dear Laurel

Many thanks for your detailed and highly constructive comments on the manuscript. We've tried to address all items you have raised, and think that the updates make for a much improved manuscript.

Following your recommendation, we have replaced the visual qualitative porosity estimates with more quantitative values derived using ImageJ. This had been part of our

Printer-friendly version

Discussion paper

original work plan, but did not have time to do this prior to our original submission! We've added a figure highlighting the basic workflow used in ImageJ, updated the graph in Figure 8 (formerly 7) and all porosity values within the text to refer to these new values. Note, all porosities are markedly lower using this image analysis method, but the overall trends are consistent with earlier observations.

With regards the reference to "Kozeny-Carmen equation fundamentals", this is simply a discussion point and we have not actually attempted to estimate permeabilities ourselves. We've therefore added a few more words to highlight that applying this equation to deformation band permeabilties is a gross over-simplification.

Below are a list of the point by point remarks you raised and the actions we have taken.

Kind regards, Dr Penelope Wilson

Reviewer Comments – The authors indicate that the outcrops studied are all part of "massive" sandstone roughly 10 m thick, implying that the samples collected are all part of a single host rock unit. The term "massive", however, is applied by sedimentologists to strata that are structureless, either from the time of deposition or due to post-depositional processes such as bioturbation. However, it is evident from the images and descriptions of sedimentary features provided by the authors that the outcrops studied are neither structureless nor uniform. Figure 2 beautifully illustrates both lateral and vertical variations in sedimentary structures, as well as subtle differences in color and resistance to weathering, consistent with variations in grain size and/or cement mineralogy or percent. In addition to removing the term 'massive' from the paper, I propose the authors explicitly state that although it is not possible to trace a single bed across the margin of the intrusion, their analyses suggest they have sampled rocks with relatively similar grain size, grain rounding, and mineralogy.

Author Response – Removed the term "Massive" and added additional wording as proposed above.

Reviewer Comments – Figure 8 is very attractive, but not designed for ease of understanding. I'm a microstructure geek, and I found it hard to navigate because part of the information that would normally be provided in the caption of a single image is given in the text, some is in the caption, and some is beneath a single figure. Some of the labels on images are very difficult to see. For example, I searched for Fe labels after I saw in the caption that Fe referred to 'iron staining' (staining of what? does this mean iron oxide grains or cement or coating?). The dark text does not show up on dark background. Red labels are hard to see; DB labels should be backed with white boxes to stand out and arrows generally need to be larger (only the TMFS-6 arrows really stand out). In general, it would be better if labels were bold; those imposed on dark areas of thin sections should be white. In short, it is not possible to glean all of the important information about an image from the figure and caption alone. Because the data acquired from thin sections are important to this story, I suggest a different approach. Move the partial captions beneath each image into the main caption and add information. For example: TMFS-1 (20 porosity), TMFS-2 (15-20% porosity), and TMFS-3 (30-35% porosity) are all well sorted, subrounded, subarkoses with local poikilotopic calcite cement. Only TMFS-3 includes deformation bands. Porosity is reduced to <5% in the deformation band, within which small, angular grain fragments provide evidence of limited cataclasis (example highlighted with a bold arrow). Walk the reader through the rest of the photomicrographs in a similar way. Be sure to clearly state what you see as well as what you infer. You don't see pressure solution; you infer it from embayments in grains at point contacts (which can be better highlighted with bold arrows). You don't see cataclasis, you infer it from angular grain fragments. You don't see compaction; you infer it from reduced porosity and preferred alignment of elongate grains (which you don't mention anywhere, but should). In other photomicrographs you can see alignment of elongate clasts parallel to cross laminae or deformation bands. It's good to point that out. Also, I personally like the fact that you haven't drawn lines over deformation band boundaries. For readers less familiar with what these features look like, you may wish to provide some guidance in either words or arrows that mark top and bottom boundaries to a band.

Author Response – We have now separated this photo montage into two separate images (now Figs 9 and 10) showing Host Rock and Deformation Band examples respectively. Individual photos are now larger, and we have increased the font size and added a yellow fill to the labels so they are clearer. We've also added edge markers for the deformation bands in Fig. 10. Additional details are now in the Figure caption, rather than embedded in the figure. New figures attached.

Reviewer Comments – You note 'indistinct "fuzzy" boundaries to larger grains' beneath your last photomicrograph. Most of the grains are quartz and have sharp margins. Your labeled plagioclase grain has "fuzzy" margins, which are also locally brownish in color. Without being able to zoom in further or look at this on an SEM, I would say that there are several things that could contribute to this appearance. Top on my list is margins that are oblique, rather than perpendicular, to the surface of the thin section. Where the edge of a grain dips away from the grain center, it will be increasingly out of focus with distance. With extensive cataclasis, you may be looking through a zone of fine grain fragments on that grain edge. I think this is what you are referring to, but I'm not sure. If it is, spell it out and highlight the specific margin. If I were you, however, I would focus on more obvious evidence of cataclasis: a high percentage of angular grains that are substantially reduced in size with respect to subrounded grains evident in host rock.

Author Response – We agree there may be a number of reasons for seeing "fuzzy" edges to grains. However, the examples in question appear to be associated with feldspar grains, while adjacent quartz grains show very clear distinct edges. We make the observation, but have not expanded this in any detail, and yes, have made more effort to emphasise the basic key observations, both in text and figure captions.

Reviewer Comments – On p. 8, you also discuss 'early development of sub-grain boundaries', and follow that on p. 9 with observations of 'clear sub-grain boundaries parallel to deformation band orientations.' In general, we use the term 'subgrain' (with

Printer-friendly version

Discussion paper

no hyphen) to refer to a part of a larger grain separated from the host grain by a dislocation wall. Production of subgrains is part of the process of rotation recrystallization; it is not a brittle process. Subgrain boundaries are only visible with crossed polars, which causes differences in orientation of the crystal across these dislocation walls to show up as differences in exteniction (grayscale). The only features I see oriented subparallel to deformation bands appear to be cracks. Please revise the text for clarity and accuracy.

Author Response – We've added an XPL image to show an example of this (Fig 10d). There are only a few examples, and by far the dominant process is brittle (intragranular cracks and fractures, and shear fractures); however, we felt it was worth highlighting that this more plastic deformation was also apparent.

Reviewer Comments – Add a reference to the list of studies of deformation band impacts on flow (line 40): Sigda, J.M., Goodwin, L.B., Mozley, P.S., and Wilson, J.L., 1999, Permeability alteration in small-displacement faults in poorly lithified sediments: Rio Grande rift, central New Mexico: In Haneberg, W.C., Mozley, P.S., Moore, J.C., and Goodwin, L.B. (eds) Faults and Subsurface Fluid Flow in the Shallow Crust, AGU Monograph 113, 51-68.

Author Response – Done

Reviewer Comments – Change lines 51-53: "Deformation bands preferentially form in more poorly lithified layers within quartz arenite to arkosic sandstones (i.e. those lacking in lithics) at shallow depths (1–3 km; Fossen, 2010)" to: "Deformation bands within quartz arenite to arkosic sandstones (i.e. those lacking in lithics) preferentially form in more poorly lithified layers at shallow depths (1–3 km; Fossen, 2010)." The former suggests deformation bands are restricted to poorly lithified layers of specific composition.

Author Response – Done

Reviewer Comments – In lines 195-196, the authors refer to ‘cycles enclosing blocks’ and note common features of ‘networks with lots of cycles’. The discussion of cycles refers to Figs. 6c and 2b&c, but it is not possible to understand how the reader is supposed to connect this information to the images. The term ‘cycle’ is not defined, and it is never mentioned again. If it is important to the story, the authors should define what they mean and why it is relevant. If it is not, they should remove references to ‘cycles’.

Author Response – This was a term used in past publications describing the general methodology. We have now removed it here and replaced it with branches for consistency. i.e. branches bound an isolated segment.

Reviewer Comments – In line 218, the authors refer to ‘a slightly coarser grained bed within the sandstone horizon’. I am not aware of a definition of ‘horizon’ used in this context. It appears to be a way to suggest associations between samples collected. Does it refer to the 4 m thick section of sandstone shown in Fig. 2? Please clarify.

Author Response – Replaced ‘horizon’ with ‘unit’, which we then introduce earlier to describe the sandstone unit sampled.

Reviewer Comments – I would like to see the authors replace references to ‘weak’ deformation or cataclasis with more specific information regarding observations rather than interpretations. I suspect they mean that evidence of fracture and associated grain-size and porosity reduction is present, but not as extensive as in other samples, as suggested by higher estimates of porosity.

Author Response – Done

Reviewer Comments – I suggest the authors replace ‘grain crushing’ with ‘distributed microcracking’ in places like line 233. I think it is a more accurate representation of the variable amounts of grain-size reduction via fracture illustrated in their thin sections. Their photos show a range from deformation bands in which the majority of grains are subrounded and similar in size to those in the host rock to deformation bands in which most of the grains have been reduced to relatively small angular fragments and relatively few original grains remain.

Author Response – Done, and have also added additional text within the figure captions for the microstructure figures (now Figs 9 & 10).

Reviewer Comments – On line 235, replace ‘Calcite is also present’ with ‘Calcite locally fills pores’.

Author Response – Done

Reviewer Comments – Line 245 refers to early development of subgrain boundaries. I addressed misconceptions re: subgrain boundaries in the previous section on Specific Comments Linked to Figures. The authors should make appropriate changes to the text here also.

Author Response – We’ve added some XPL images to the microstructure figures which show that some higher strained quartz grains within deformation bands do appear to exhibit sub-grain boundaries (e.g. Fig 10d), though this is not a common feature.

Reviewer Comments – On line 248, the authors discuss embayed contacts. I think it would be helpful to clarify what is meant by ‘embayed’, with reference to more clearly annotated examples in thin section images.

Author Response – Added notes on Figs 9 and 10.

Reviewer Comments – The sentence beginning on line 256 states that ‘Haematite is also incorporated into the matrix within deformation bands as a result of quartz grain crushing. Note the brownish-staining of deformation bands in Figs. 7a and 8”. What evidence supports this interpretation? Is it possible that hematite was precipitated after formation of deformation bands? Please provide evidence (: : :and you don’t need to hyphenate brownish & staining).

Author Response – Sentence removed as not relevant.

Reviewer Comments – Line 305 refers to 'minor cataclasis as evidence for shear'. Minor cataclasis can occur by compaction alone. It doesn't require shear.

Author Response – Re-worded.

Reviewer Comments – On line 309, the authors propose that evidence of compaction in sandstone suggests confining pressure may increase with proximity to the intrusion. It is certainly a sign of shortening, consistent with intrusion, but that suggests an increase in margin perpendicular stress, not an increase in confining pressure. Note also that intrusions, particularly shallow crustal intrusions, cool very rapidly. The temperature gradient between thin sheets of partially crystalline magma and wall rock so shallow it still has high porosity is very high, and temperature dissipates rapidly at cool shallow temperatures. If you know the thickness of individual sills and likely depth of intrusion, you can do a back of the envelope calculation to determine the cooling rate for a normal geothermal gradient (or even a slightly elevated one, which would not produce high temperatures at relatively shallow depths where you see high porosity sandstones). 'Pressure solution' actually has nothing to do with pressure. It is caused by a stress-induced chemical potential gradient. I suspect that what you are seeing is that the deformation bands that have accommodated the greatest deformation have the highest number of high-stress point contacts between grains, where solution mass transfer is facilitated.

Author Response – Re-worded in line with reviewer's comments.

Reviewer Comments – Line 311: Crush breccias do not consist of fragments that are only visible with a microscope. You do show clear evidence of cataclasis, which could be defined as distributed microcracking and rotation and translation of resulting clasts. You might want to provide a definition like this where you first introduce the term in the paper, to facilitate discussion here.

Author Response – Re-worded in line with reviewer's comments, and also added additional wording in the figure captions in Figs 9 & 10 (microstructures).

Reviewer Comments – Lines 313-314: If a principal slip surface or fault core were present, you would call it a fault or a deformation band fault and not a deformation band.

Author Response – Re-worded to make the point that we are discussing deformation bands and not faults in this study, but that deformation band faults are observed in elsewhere on the intrusion.

Reviewer Comments – Line 317: I am not familiar with the term 'permeability pathway'. Deformation bands are features that can influence flow pathways, but they cannot be considered in isolation. In this case, the elephant in the hydrologic room is the extremely low permeability intrusion. Regional flow will take the 'easiest' route around the intrusion, which will be influenced not just by deformation band distribution and connectivity but also by the permeability of the surrounding undeformed rocks. This paragraph also reflects a misunderstanding of the hydrologic significance of microstructural observations. The fundamental misapprehension is that tabular structures that formed by different processes (e.g., compaction bands vs. cataclastic deformation bands) can influence flow differently even if they have the same permeability. Your intrusion is effectively an impermeable wall. Your deformation band networks may redirect or inhibit flow in a shell around the plutons, or the main effect may be created by the intrusion itself. The best way to determine these effects would be to measure the permeabilities of cores cut in different directions through the networks, then work with a hydrogeologist to model flow. Without those data, I think you are restricted to providing a clear description of the structures at different scales. Please appreciate that the description itself is a significant contribution.

Author Response – We've modified the terminology here to state 'permeability and flow pathways', and made subtle changes to the wording in the paragraph. We've also added additional sentences to address the "elephant" that is the intrusion itself! Good point well-raised. With regards to any misunderstanding of processes, we'd like to highlight that we have simply referred to points raised by other authors which have suggested that different deformation band types may impact fluid flow in subtly different ways. We agree that if two bands have the same permeability, then they will of course impact fluid flow in the same way. The point being made here is that two bands with the same porosity reduction may not have the same permeability due to different microstructures.

Reviewer Comments – Lines 327-328: I do not know if anyone has published evidence of magmatic fluids of appropriate composition to precipitate calcite. I think this would be a more compelling suggestion if the authors could cite a study indicating it was possible. I do know that the solubility of calcite decreases with increasing temperature, so I suspect that heat introduced by the intrusion could facilitate precipitation of calcite from surrounding groundwater of appropriate composition.

Author Response – Re-worded to emphasize this latter point, which is what we were envisaging rather than the fluids being magmatically derived.

Reviewer Comments – Lines 335-336: I think it is particularly important to replace 'fractures' with 'deformation bands' in these sentences.

Author Response – Done

Reviewer Comments – Line 342: I don't know what the authors mean by "However, this assumes a homogeneous development of the grain-scale processes". Please explain.

Author Response – We've added additional wording here to highlight that the application of the Kozeny-Carmen equation here is an over-simplification as deformation bands are intrinsically heterogeneous!

Reviewer Comments – In line 351, the authors state "At Trachyte Mesa, deformation bands decrease markedly from ∼5 to 10m above the intrusion margin.." I assume they mean deformation band intensity decreases. However, they do not present data showing vertical variations in deformation band networks. Is this a personal observation? If the authors have data that show this variation, they should provide it. If relevant data have been published, they should cite the reference.

Author Response – Yes, this is a personal observation, but as stated, outcrops are limited, so detailed analysis may be challenging. The purpose of making this observation was to bridge the discussion. Adding additional data/ figures may detract from the key messages in the paper, particularly as the vertical variations have not been analyzed to the same extent as the horizontal variability. We agree this is an interesting area for further analysis, but we do not currently have the data available to expand on this further right now.

Reviewer Comments – I suggest the authors modify lines 354-357 to state: "In addition to reducing the bulk permeability of the reservoir, the deformation bands largely strike parallel to the intrusion margin (Wilson et al., 2016), producing an anisotropy in permeability similar to that of a fault zone (e.g. Farrell et al., 2014).

Author Response – Done

Reviewer Comments – Line 360-361 should be modified to state: 'Gaining a better understanding of these emplacement-related deformation structures may have important implications for fluid flow, hydrocarbon reservoir connectivity / deliverability, hydrology, geothermal energy and CO2 sequestration: : :'

Author Response – Done

Reviewer Comments – I suggest the authors modify line 378-379 as follows: "The increase in margin-parallel Y- and X-nodes with proximity to the intrusion is likely to inhibit flow perpendicular to the intrusion margin, as well as potentially forming non-producible reservoir zones."

Author Response – Done

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-71, 2020.

Printer-friendly version

Discussion paper

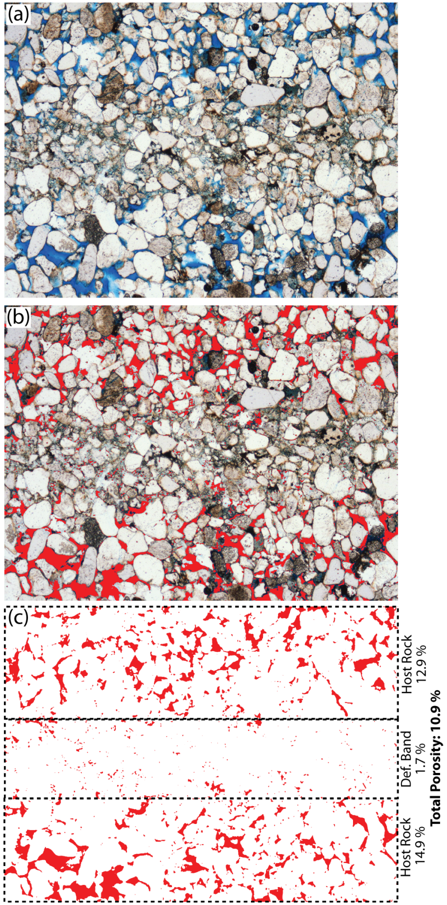


Fig. 1.

Printer-friendly version

Discussion paper

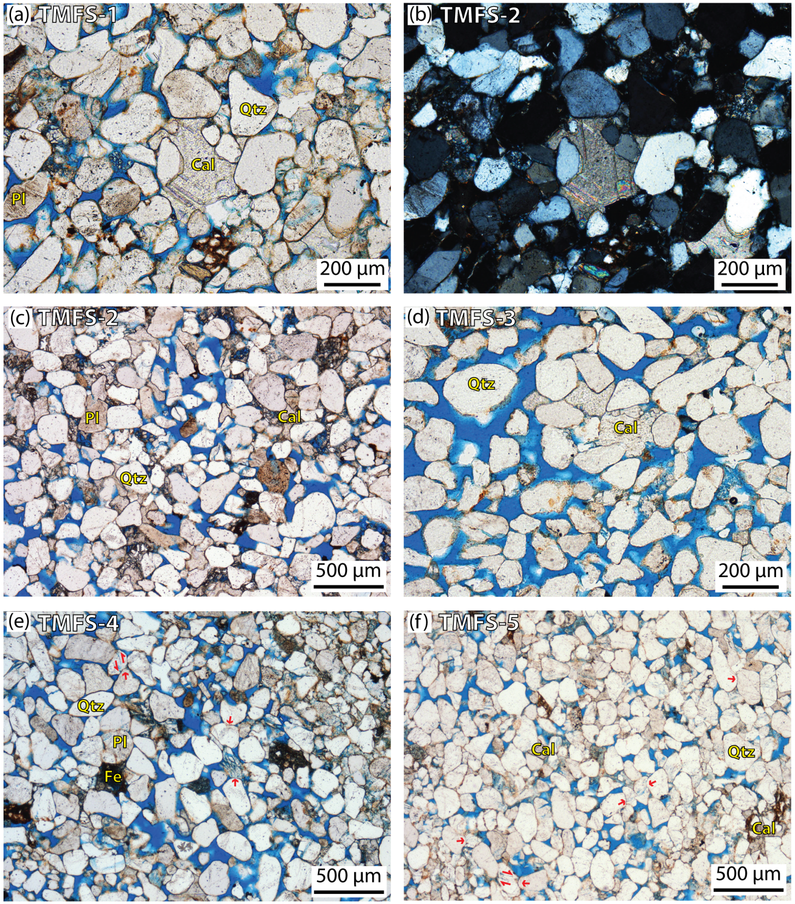
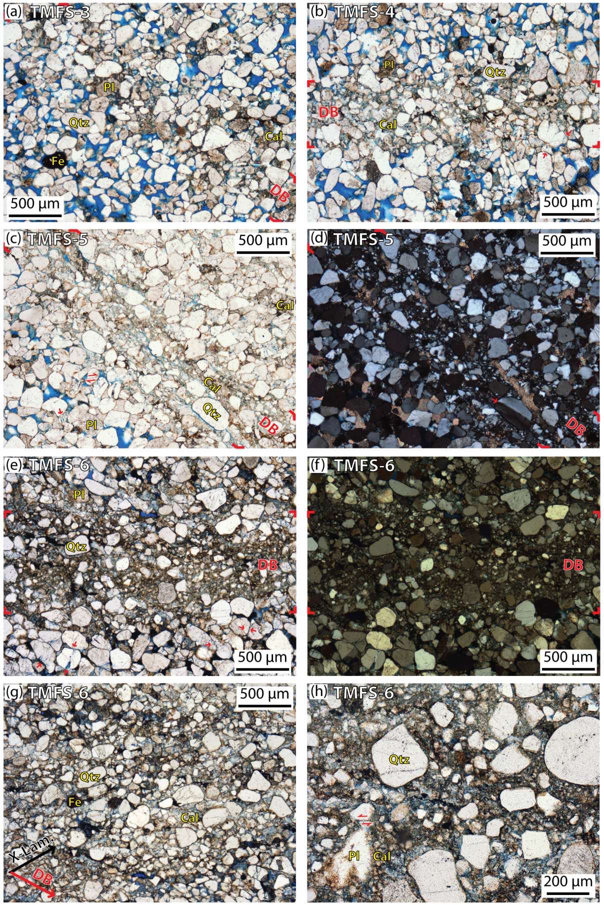


Fig. 2.

Interactive comment

Printer-friendly version

Discussion paper



(a) TMFS-3
Pl
Qtz
Cal
Fe
DB
500 µm
(b) TMFS-4
Pl
Qtz
DB
Cal
500 µm
(c) TMFS-5
500 µm
Cal
Cal
Qtz
Pl
DB
(d) TMFS-5
500 µm
DB
(e) TMFS-6
Pl
Qtz
DB
500 µm
(f) TMFS-6
DB
500 µm
(g) TMFS-6
500 µm
Qtz
Fe
Cal
X-Lam
DB
(h) TMFS-6
Qtz
Pl
Cal
200 µm

Fig. 3.