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Solid Earth
Discussions

Interactive comment on "Hydraulic fracture propagation in a heterogeneous stress field in a crystalline rock mass" *by* Nathan Dutler et al.

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This is a very interesting and really useful study to the geothermal community and the rock-deformation community given the rarity of such experiments in-situ and good monitoring of their impacts. This is highly novel work in that respect, and should be of great use and interest to both academia and industry. I strongly encourage publication of this manuscript after some minor revisions.

Most of my critiques are minor and I am sure can be easily addressed by the authors. I do have one pressing concern with the Discussion, particularly with how it is structured and written. Given the value of such studies like these, and their scarcity, I think the paper would really benefit from a rewrite of the discussion to more clearly lay out

the authors arguments and how they are supported by the data they have acquired. Currently the discussion is at times difficult to follow and a slight rewrite/reorganisation, and some careful rephrasing of parts would immensely improve this manuscript. I have tried to provide more detail below on where I think some useful improvements could be made, to assist the paper outputs in making a bigger impact.

There are places in the discussion where a strong conclusive statement or argument is made by the authors for a particular theory they support. While this is great to see, I would be interested to see the authors take some space in the paper to discuss possible alternative interpretations to their preferred theory based on the observations and present the arguments for and against them.

I have made several comments below specifically about stress features in borehole images that I would like the authors to address.

The results presentation in the manuscript is quite lengthy mostly due to extended and suffers textual description of the results. Perhaps the authors could look at refining and reducing the presentation/description of the data in the manuscript text, referring more heavily to their excellently designed figures? While this would not be essential for the eventual publication of this paper it would improve its readability.

The figures are fantastic though I have only suggested minor edits to improve their usefulness.

Minor correction suggestions:

Page 2, Line 6 – References to support the statement of massive HF technology in petroleum and geothermal should be provided here.

Page 2, Line 6 – 'Deep' geothermal is a little subjective, some conventional geothermal that requires no HF goes to depths of up to 4km. I would suggest refraining from the use of deep in this context and sticking with enhanced geothermal systems which are directly involved with HF processes.

Page 2, Line 12 – Another example of confluence of deep geothermal with enhanced geothermal. I would advise avoiding this as it does not fit with all examples of 'deep' geothermal globally, nor with all EGS which can also be explored at more shallow depths.

Page 5 Line 19 – what is the dip direction of the foliation?

Page 6, Figure 1 – While northing and easting are noted on the model axes, a N-direction arrow would make these much easier for a reader to visualise the 3D structural geometry. The blue circles in d) are very difficult to see.

Page 6 – What are the two fracture systems mentioned for the d-b S3 shear zone? A reference to Figure 2a would be useful here.

Page 7, Table 1 – The header suggests that the orientations are displayed as dip and dip direction, yet are written in the table the other way around. Make the table header consistent with the way the information is displayed.

Page 7, Line 7 – When you state the perturbed stress field are you referring to the stress state within the fractured zone associated with the S3 shear zone, or does it extend beyond this? It becomes clearer to the reader later in the paper but would be better to clarify this at this point.

Page 8, Figure 2 – a) what are the coloured triangles in stereonets. It would be useful to have the symbols used here be the same as those used in 2c to make the link between these two diagrams more obvious. B) Is the stress state in the corner the arrangement of the perturbed region or the unperturbed? A north arrow on the 3D model (the left diagram) would make it easier to read than having to go off the axes. C) Black lines with pore pressures on them are not explained in the caption.

Page 9, line 3 – By 'intact injection intervals' do the authors mean intact lithology with no brittle deformation visible? This could be written to be clearer to the reader.

Page 9, line 8 – This statement is only true for some of the fracture density peaks, or

some measurements of Tsz but not all. The correlation of the two does not always match up, perhaps the authors could rethink the strength of this statement and explain the variability observed here in the correlation, or refer to a paper that has discussed this?

Page 15, Figure 4 – Be specific in the caption about what the black lines on graph 4a represent.

Page 16, Figure 5 – The labelling of which bar chart is backflow and which is injection could stand to be presented a little more clearly. It was difficult to read at times.

Page 17, Line 1 – repetition of 'the'.

Page 18, Line 22 – change 'posterior' to 'post'.

Page 18, Figure 6 – The image log for HF1 (post-test) shows a pre-existing fracture in the test interval, albeit towards the bottom of the HF interval that was not imaged in the initial televiewer log. This needs to be addressed by the authors for any potential effect on the HF test results, or explained as an induced structure with proof that it is one. Are there other mechanical weakness in these intervals beyond fracturing (layering etc.), some of the image logs (HF5 in particular) suggest there may be a fabric in these depth intervals, is there any information from core about layering etc.? If so, has the possible effect of these mechanical weaknesses in the 'intact' rock on HF testing been considered? Do the orientations of the induced fractures agree with the existing stress field orientations i.e. strike perpendicular to sigma 3 (depending if they are DITF or petal centreline fractures)? It would be good to see this information presented somewhere in the manuscript or supplement for comparison to already published stress data. The authors mention later that some of the induced fractures formed during testing are opened along a foliation orientation – this is very important as the sigma 3 value derived from these tests is that required to open a plane of weakness which may not necessarily represent the tectonic stress magnitude. I encourage the authors to include further discussion of this and the potential implications of it on their

findings. Currently I think not enough consideration has been given to this.

Page 19, Line 4 – en echelon morphologies for the induced fractures are mentioned. These are usually a result of a well deviated with respect to the orthogonal stress tensor. Have corrections for the stress orientations determined from these image logs been run to account for the effect of the deviated well? What is the deviation of the wells in the HF intervals?

Page 19, Line 7 – Do the authors mean 730 events were located in D space? Change the word localised to located if so.

Page 19, Line 12 – Villiger et al. reference incomplete, missing the year.

Page 19, Figure 7 – Does the colour intensity of the seismicity dots reflect anything or is this simply due to a number of points of the same colour overlapping on the diagram? What are the 3D cylinders (various colours) around the wells?

Page 20, Line 30 – How was the best fit plane determined? The fit looks weak on the figure, how good is it as a representation? Can the authors comment on this?

Page 22, Figure 9 – the coloured points on the time series and the polar plots do not match (one is outlined the other is not). A small consideration but it would be nice to make these consistent between diagrams.

Page 20 – 'Injection executed south of S3 show in general smaller magnitudes than the one' – can the authors clarify what is smaller in magnitude? Is it tilt or seismicity?

Page 21, Line 6 – 'tiling of the tunnel floor away' – clarify away from what. This sentence is very difficult to follow. Please consider rewording for clarity.

Page 28, Line 15 – Villiger et al reference missing a year.

Page 29, Line 20 – have a similar trace orientation 'to' the HF experiments.

Page 30, Figure 14 – the labelling of the points on the stereonets is incomplete in

places and confusing. A key in the corner of this figure would be more useful perhaps? Please revise.

Page 32, Line 5 – is as 'follows'.

Page 32, Line 6-9 – An important part of this section of the manuscript. I advise the authors to rewrite this sentence or two with an emphasis on clarity of meaning, as it does not currently read as well as it could.

Page 32, Line 11-18 – This section could benefit from a rewrite with the aim of ordering the arguments made in a clearer and easier to read fashion. It is currently confusing and is not quite making the point the authors are trying to make which is that these results support the theory of the S3 being hydraulically connected to an open system with respect to fluid flow.

Section 6.1 – It is my opinion that this section suffers a little due to its structure. I think if the authors took some time to order their arguments better the scientific findings of this section would be more apparent to a reader. As it currently reads, it is confusing and I think it is very important the authors consider how to rephrase this section to better highlight their arguments.

Page 32, Line 33 – Does this actually cast doubt on stress orientation from well data only or is the 30-degree rotation result from another process such as pre-existing weaknesses affecting the propagation of the induced fractures? I would like to see a discussion on the other arguments for this observed orientation difference between the seismic cloud and the stress orientation from borehole data, with arguments for and against each before coming to a final conclusive statement such as the one currently made.

Page 33, Line 1 – How is the trace of a fracture scattered? I encourage the authors to revise the clarity with which they have written sections and statements of their discussion as they are often difficult to follow, and difficult to relate to any conclusion or

argument being made.

Page 33, Line 5-8 – I am unsure why the authors state the trace of the induced fractures are complex, they are actually quite clear in form and morphology from the images provided in figures. What is more complex is where these fractures nucleated during HF tests, i.e. at the abutting natural fractures or within the sealed section of well itself? The interaction of the induced fracture with natural fractures outside the packer would seem to infer that the fracture propagated vertically until it met a pre-existing structure and then ceased to propagate vertically. However, that is difficult to prove without directly observing the induced fracture growth. Finally, the comment on stress orientations from induced features being misleading in deviated wells is old news and there are numerous studies (Hickman, McNamara, Davatzes, Barton, Zoback) that have addressed this issue in a number of geothermal well scenarios. For example, McNamara et al., 2015 (Rotokawa geothermal borehole imaging paper) calculated minimal effect on using induced features for stress orientation in geothermal wells deviated at angles up to $\sim$22 degrees from vertical. I would like to see the authors acknowledge some of the points raised here in this section of the discussion as they have important implications to their work.

Page 33, Line 11 – to some 'extent'.

Page 33, Line 11-13 – Have the authors considered that the induced fractures do not grow in a penny-shape fashion due to the influence of the curved free-surface of the borehole wall?

Page 39, Line 22-34 – It is standard practise to assume that induced features in borehole images over small well lengths are not representative of the regional far field stress orientations but are in fact likely representative of localised effects or heterogeneities of the stress field orientation. This is reflected in the quality ranking system developed for such data by the World Stress Map project. So, while the data presented here by the authors may not agree or align with expected regional far field orientations they are

likely still providing accurate information on the local perturbations in the orientation.

Page 39, Line 29 – Please check the English of this sentence.

Page 40, Figure 16 – Please include a key for the coloured symbols on the Mohr space diagrams.

Page 40, Line4 – Conclusion 4 and conclusion 2 sound contradictory. 2: The growth of the hydraulic fractures is strongly influenced by natural fractures. 4: the spatial distribution of microseismic events associated with the fracture growth seems to be predominantly controlled by the stress state. While I appreciate these are subtly different data being measured I would encourage the authors to comment somewhere on which aspect, if any, is more controlling on fracture growth (stress or pre-existing weaknesses), or discuss somewhere the link between the stress, natural fracturing, and the effects this can have on induced fracture propagation.

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