

Revision

On the link between stress field and small-scale hydraulic fracture growth in anisotropic rock derived from microseismicity

Gischig, Valentin et al. 2017. Solid Earth Discussions (EGU)

I found this manuscript of great interest, and am glad to see practical demonstration from the ETH group of what promised this year at the Induced seismicity meeting in Davos.

The title matches perfectly the topic developed in the manuscript.

The authors refer to a wide range of references. The experiment set-up, purposes and results are described in a clear way. Results appear to be interesting, and might help to understand rock-breaking processes during geothermal stimulations in Switzerland.

I definitely recommend this work as a publication for the EGU journal. No doubt.

However, from MINOR TO MODERATE REVISIONS are nevertheless needed.

I wish my best, and proficient researches to the ETH group.

Enrico Caffagni

Following some major concerns I have found.

1. The Abstract is quite long.. in comparison to standard EGU Discussions abstracts.
2. The Discussion can be shortened, avoiding repetitions of concepts.
3. Some references are missing or written in an incorrect way.
4. Table 2 is present, but never mentioned in the text.
5. Figure 11 appears with overlapped labels.
6. I feel that a bit of clarification is needed when the authors described the “Microseismic monitoring”. I think it is more a question of terminology. The classical induced microseismic events range in frequency between 80 to 300-400 Hz. No more. Sure that the space-time scale is different. Yet you measure events at magnitude -2.5 for instance at KHz. Should we define these as “acoustic emissions”? You actually mention, page 6 line 208-209, “similar to those commonly used in laboratory acoustic emission”. For coherency, you should continue to call such events as acoustic emissions, throughout the paper.
7. I am not sure that your stress characterization study is in the ‘far-field’ domain, page 6 line 184, at least if this is only due to the “several tens of meter away from any fault”. What you are doing, I believe, instead is a characterization of the “local” stress field. Which is more useful.

8. It would benefit your paper to include, at a qualitative way, one or two sentences on the topic: what drives the initiation of an hydrofracture, and “when” or “why” it decides to stop? I suggest to refer to Dahm et al. (JGR, 2010), who discuss the effect of the pore pressure gradient, or pressure gradient, which in your experiment might be also responsible of fracture plane deviation or the trend in asymmetrical growth, page 16 line 544.

Better. Your pressure gradient might drive the fracture, but when this starts decreasing, well, your fracture “feels” the local stress field then re-orient itself naturally.

Another plausible explanation of fracture plane rotation is a high resilient tectonic stress, see Cooke et al. (2016, TLE).

(it is just an attempt of clarification; there are better interpretation; work on that)

9. I honestly do not see a strong connection between polarization through stacking and focal mechanisms. In Figure 11 in a few beach-balls a few events appear in different color (black and white) yet in the same section of the ball. This seems to be contradictory.. Actually, by itself, the stacking operations is for enhancing the signal arrival, not for polarization; unless one projects first the traces into already obtained polarization vectors then perform the stacking. By such methods one can locate microseismic events (see Caffagni et al. 2016, GJI).

Beside that, the lack in the majority of strike-slip events or a combination of different source mechanisms seems to be a kind of constant in induced seismicity. You can see and refer for instance to Baisch et al. (2015, BSSA; Figure 12).

10. The authors need to be careful when they declare “the observation of DC components..exclude 1” page 17 line 577-578. No, I do not agree. A tensile source mechanism has a DC component as well. In addition Figure 8 reproduces what to me is a tensile event, Cluster 2 (max P wave is bigger than S wave). If I had to sort hundreds of events automatically and visually, I would classify that event as a tensile one.

11. The authors mention in the Appendix, page 20 line 652, “a solid angle is also referred to as the take-off-angle”. The classical take-off-angle is not defined as a solid angle, see Stein and Wysession page 222.

12. More than else, since you as ETH group are currently leading the laboratory experimental research of hydraulic fracturing in Europe, it would benefit to develop on the topic: What’s for?

Switzerland might be soon venue of massive usage of geothermal exploitation. What can we learn at macro-scale from such experiments at micro-scale? Can just we simply “upscale” our results in cases of real large-scale stimulations? Perhaps the answer is yes..Injection values in pressure are much higher though and perhaps the effect of small-scale rock anisotropy might vanishes in comparison to big deformations due to pore-elastic effects or fluid diffusion or fracturing.

The paper you want to publish is called “EGU Discussion”. It would be appropriate if you could “unbalance” yourself and make some qualitative or even quantitative declarations on future perspectives from such mini-scale hydro-fracturing experiments.

In the following you will find my minor concerns. Please, do not reply in your response file to all of them. But make sure to read them and revise. I have also added additional important concepts missing.

Minor concerns

Abstract

Pg. 1

Line 15: “transverse” to what? To the radial? Usually P-waves propagates in the radial direction

Line 20: “ from the overcoring stress. An anisotropic elastic model..”

Line 21 “sigma1 is significantly..” to Line 24 “the north”. Is not clear and I would simply remove it, to shorten the abstract

Introduction

Pg. 2

Line 37: “Hydraulic fracturing or hydrofracking (HF)”

Line 37: Rephrase “Hydraulic fracturing induces artificial fracture networks in a rock mass by high-pressure fluid-injection. It has become an essential technique..., for instance to enhance the permeability..., and increase... . HF should not be confused with hydroshearing (HS). HS is a method of rock.. that uses fluid-injection....promoting shear failure, attendant dilation of pre-existing fractures, and fault slippage...”

Line 45: “criticality of the discontinuity sets” What do you mean? It is not clear

Line 47: “HS has been often exploited in... . HF small volume has been also utilized in stress..”
I would replace the “deep geothermal projects” with the EGS, the “Enhanced Geothermal systems”

Line 52: “etc; see Zang and...2010”

Line 52: “To better constrain the stress field...and sections where no pre-existing fractures are present”

Line 55: “water is injected at a constant rate until the ..down, initiating a fracture at the borehole wall...sub parallel to the principal...., and significant deviations are not expected due to the tensile strength anisotropy..”

Is this what you mean with “no complications from tensile...” If not, please provide explanations.

Line 57: “, then high-pressure fluid injected will tend to initiate an axial fracture” Is this what you mean? Is not clear what initiates the fracture.

Line 59: “minimum principal stress is close to be aligned to the borehole axis”

Line 65: “Injection volumes..” You do not mention here the injection rate (in litres per minutes), which is one of the constraining factors as well of the induced seismicity.

Line 67: “at which the breakdown occurs”

Line 70: “treatments (of importance here) it can be considered as the pressure...open. ISIP is thus interpreted as...”

Pg. 3

Line 71: “intended for HF or HS, can generate acoustic emissions and microseismic events” (see my comment n. 6)

Line 75: “regardless of scale” What scale? Time- space-scale?

Line 76: “monitoring has been routinely used..” Check you references. They are all in the past. You cannot use the present tense.

Line 78: you may include: Caffagni et al. 2016

Line 78: “At the other extreme of scale”. What scale?

Line 84: “indicate changes in the local stress condition”

Line 87: “controlled by..” Here you may develop on the pumped fluid, the pressure gradient (see Dahm et al. 2010, JGR)

Line 94: “direction σ_3 (Haering...), particularly for HF operation (Rutledge...; Zoback et al. 2012 SPE)”

Line 100: “Deichmann et al. 2014; Eaton and Caffagni, 2015, First Break)

Pg. 4

Line 107: “ Detailed moment tensor....have shown that most of the induced...with relatively a few...”

Line 111: “is very inefficient in radiating” No. Energy from acoustic emissions is radiated efficiently but with a classical monitoring systems of geophones it is not detectable. Please correct.

Line 113: “Thus,...do not necessarily...themselves, yet they contribute to illuminating the overall plane of fracture growth..”

Line 117: “there are a few..between meter-scale....and the ambient stress..”

Line 121: “though it would be desirable”

Line 135: “water injection” You mean the “injection history”? If so, please revise

Line 135: “Then, we detail our anisotropic...localization method.. The obtained results are then compared to the overcoring stress field observations” Is this what you mean?

Experiment context and study site

Pg. 5

Line 164: “Since in-situ stress is a relevant factor driving...” you cannot say that is the “major force”, also the fluid-injection effects, e.g., pore pressure diffusion and propagation are important

Line 176: remove “that serves”

Line 178: “yields estimate of the full 3D stress”

Pg. 6

Line 184: “The goal is to characterize the local stress condition” see my comment n. 7

Line 187-188: “It was intended...Pahl et al..” the meaning is clear. Rephrase with better English

Line 196: “HTPF”. Never mentioned, please spell it

Micro-seismic monitoring

Line 209: “experiments” Include at least one reference

Pg. 7

Line 215: “accelerometers” remove the Italic format

Line 231: “Signals were digitized with a 32-channel...” Is this what you mean?

Line 237: What is the reason of this “dead-time”? Please provide a few explanation

Pg. 8

Line 256: “this spans only 7 m” What you had in mind is the “array’s aperture”? If so, please revise

Line 262: “transverse isotropy” Is this correct? Or it is instead “transverse anisotropy”? You mentioned before the elastic anisotropy approximation of Thompson..

Line 282: “1000 time” Is this a peculiar number? Why not stopping the repetition at 100. What would happen? Please provide a few explanation. Also, what about the computational time? This would be important to reproduce the results.

Line 283: “principal component analysis” You can also include the accepted acronym “PCA”.

Pg. 9

Also in this section, it might help the reader to see an image of your procedure, a visualization of your located clusters. You can decide to include an additional figure here.

Also a number of questions raises up, such as are there events co-located? Did you identify repeated slips? You can provide a few explanation based on your results

Results

Line 319: “due to the lower noise-levels in the borehole” Have you really checked in the traces if what you argued is true? If so, what are the noise levels or the signal-to-noise-ratio?

Pg. 10

I would move Line 322-331 at the beginning of the Results section page 9. First you describe your HF treatment parameter, then you move to the induced seismicity description

Line 329: “not reached during the break-down cycle”. Why? Do you have an explanations? That is interesting

You comment about the injection volume, but I think you need to comment as well more on Figure 2, which brings very important information, that are not described in the manuscript.

I would add:

“Induced events occur mainly nucleating in time in correspondence of the peak in both injection pressure and rate.” First question here might be: Why we do not observe events later? Also, is there a preference between pressure and volume in inducing seismicity? It looks not..

“In the HF3 case, instead, events look generating also later with respect to the previously observed vertical propagation. This could be a manifestation of fluid-propagation effects and/or fracturing”.

It might help also to plot the injection rate with the seismicity rate, to see what are the minimum levels of the injection to trigger seismicity. Perhaps the results might be combined with Figure 3 or to replace Figure 3.

Line 337 “(see Doescth...)”

Line 342: “the impact of considering anisotropy produces variations in the spatially..”

Pg. 11

Figure 5 is neat. Why fractures grow in one direction? Is there a lateral stress gradient factor (see Caffagni et al. 2016, GJI)? Could you plot the direction of sigma H max, to see if there are mis-alignments?

Line 355-358: An explanation of this tendency could be the “Kaiser effect”, as observed for instance in the Cooper Basin, (see Baisch et al. 2009, BSSA). I would include this part with one sentence.

Line 379: attach “wave” and “forms” (waveforms)

Pg. 12

Line 402: remove the second “and”

Line 406: “but also accounting for”

In eq. 2. A_i is in the Fourier domain (frequency) or in time domain? It is not clear..

Line 412-414: Please, clarify this sentence..

Pg. 13

Line 435: replace “accord” with “agreement”

Line 436: replace “Figure 2g-i” (?) with “Figure 10 c”

Line 438: “even with $M_{ra} < -3.5$ could be located”. Ok, but what is your uncertainty in that range?

Pg. 14

Line 454: “the same source mechanism”

Line 457: “it was not...DC mechanism” Where do you shown this behavior?

Line 465: “of volumetric expansion, likely due to the fluid-injection..”

Discussion

Line 480: “The direction of propagation of HF3 was different from the other..it propagated downward. HF3.. HF3 also...differently in the instantaneous shut..(ISIP), which decreased with...”

“with cycle to stabilize...Figure 12” What do you really want to say? Is this significant or a minor factor which can be removed?

Line 485: “deepest measurements (HF1 at 18 m) to..., (HF3 at 8 m).”

Pg. 15

Line 490: Please spell “OPTV”, never mentioned earlier.

Line 495: “moved 0.3 m downhole, fluid could be injected in a way that fractures were expected to re-open”

Line 499: “they may have worked...hydrofracture test, since the injected fluid was able to penetrate... Here I actually would add: “Seismicity starts propagating from the packer but not for the HF3 case”

Line 501: “consistent with the evidence that..” Please also check. It is “decameters” or “meters”? In the mentioned figures, it looks like it is in meters..

Line 502: “The low recovery rate of HF3..either by assuming that the packer acts as a sealer of the created fracture after releasing... , or that fluid flows to the far field..”

Line 510: “ranked 5/5 and 4/5”..? Please provide a short explanation. Not all of your readers knows the overcoring technique in details as you do.

Line 516: “and the fault planes of the HF induced seismicity”

Line 523: remove 0 in “090”

Pg. 16

Line 531: “We have shown that micro-seismic monitoring..has provided essential..to obtain a final stress tensor estimate.”

Line 536: “may be due to fractures initiated..”

Line 547: “After the initiation, the fracture gradually re-orientes itself to become..to the direction preferred of the principal stress.”

Here you may develop arguments including the pressure gradient. See my comment n. 8

Line 556: remove “Once”

Line 558: occurred. This reorientation was not...seismic clouds, and it would seem...”
It would be interesting to know why this did not happen..

Pg. 17

Line 561. Did you compute the reduction of σ_n on the foliation plane due to the injection? I expect this to be very low..

Line 570: “We expect focal mechanisms to be in agreement with the stress field orientation... Hence the variability of the mechanisms, which we observe must be due to..”

Line 577: “associated to fracture propagation. In our case, the observation of DC.. exclude (1)”
No. I do not agree. Please see my comment n. 10.

Pg. 18

Conclusion

At the beginning, please, you should insert a sentence that recall the experiment (shortly), or a bit of context. “An experiment at the GTS has been conducted...with the purpose of...”

Line 590: “system to study the..at spatial scale from decimeters to meters. The workflow which we have implemented with standard seismological tools, such as..joint location by station corrections... For other seismological...their uncertainties (e.g., Kwiatek...). In the present case, micro-seismic...proved to be crucial to combine interpretation in the results of the stress...”

Line 599: “intervals. Such patterns have an EW strike and dip...”

Line 601: “deviated significantly from the normal to the seismic..”

Line 603: “discrepancy” Among what? Please clarify the two terms of the discrepancy

Line 611: “It is possible that stress..and pressure leak-off effects..influence.. Our observations..surveys conducted in moderately anisotropic rock. A combination of...is essential to obtain a reliable interpretation of the link between stress field and small scale HF growth.”

In this way you use words from the abstract and you close the loop.

Pg. 19

Appendix

Eq. 2 is meant to be a sum of the ray path contributions in all the layers that you have considered or not?

Line 627: revise “inverse”

Line 633: A verb is missing in the sentence.. Please revise

Line 640: remove the second “becomes”

Pg. 21

Eq. 13. You mention \cos^{-1} . Did you mean the $\arccos(x)$ or the $\sec(x) = 1/\cos(x)$? Please specify to avoid confusion

Pg. 23

References

ASTM (2008) is missing!

Pg. 24

Evans et al. It is 2005a or 2005? Please check in the text and revise

Pg. 25

Hollinger et al. You have written in the text “Hollinger”. Please, revise

Jeffrey, 2000. Not clear what is it.. a book or a paper?

Manthei et al. 2003. This reference is missing!

Pg. 26

Martinez-Garzon. Please write correctly this surname in the text and reference

Pine and Batchelor. There is written 2003 but also 1984.. Please revise accordingly

Pg. 27

Rutledge et al. 2004. There is another Rutledge and Phillips, 2004 in the text. Please, revise

Van As and Jeffrey (2000). There is another Van As (2000). Is the same? Please, revise

Warpinski et al. The two dates in the references do not match the date in the text. Please, revise

Thomsen and/or Thomson reference is missing! What date then? 1986 or 1989? Please, revise

Figures

Figure 3: Is the “Injected volume” a cumulative injected volume? If so, it is better to revise the horizontal label

Figure 4: Caption “c) Difference...models. It is shown the station..”

Figure 11: Caption: “agrees with one of the focal planes..”

Figure 13: Caption: “Comparison between the foliation plane, fractures....with the seismicity cloud directions...”