Response to comments by Enrico Caffagni

Major concerns

1. The Abstract is quite long in comparison to standard EGU Discussions abstracts.

We shortened the abstract by removing the information on fracture initiation at the borehole wall that was probably driven by strength anisotropy, which is not a major overall scope of the paper.

2. The Discussion can be shortened, avoiding repetitions of concepts.

We have already shortened the discussion somewhat in response to reviewer 2. However, we feel that there are many aspects of fracture growth that is noteworthy and that we would like to discuss. Thus, we also added a short paragraph discussing the unidirectional fracture growth (referring to Dahm et al., 2010) as suggested by the reviewer further below.

3. Some references are missing or written in an incorrect way.

We corrected this.

4. Table 2 is present, but never mentioned in the text.

We added reference to Table 2

5. Figure 11 appears with overlapped labels.

Corrected.

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.

We are aware that typically high frequency seismic events are referred to as acoustic emissions. However, the distinction to seismic events is not clear and different frequency thresholds are given, above which events are supposed to be called acoustic emission. In fact, also events above ~20 Hz are audible and might be referred to as acoustic emissions. In our view, microseismic or just seismic is the more general term describing simply elastic waves. We thus prefer this term over acoustic emission.

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.

We agree that the term 'far-field' is misleading in this context. We changed the term to indicate that it is an estimate of the unperturbed (or less perturbed) stress field some distance away from the fault.

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-orients 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)

The work by Dahm et al., (2010) is indeed a possible explanation for our asymmetric fracture growth (although possibly not a sufficient explanation). In case of the work by Cooke et al., (2016), we are not convinced that it is transferable to our case.

We added a section discussing the asymmetric fracture growth observed based on Dahm et al., (2016)

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).

We agree that the waveform stacking to retrieve better polarization analysis is problematic and have removed this from the text and the corresponding figure (also in response to reviewer 2).

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).

Indeed, other authors have also observed focal mechanisms that deviate from the prevailing stress field, although in most of these cases (Baisch et al., 2015; Deichmann et al., 2014) the majority of the focal mechanisms does agree with the stress field.

We added a sentence mentioning these observations by other authors.

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.

We disagree on this comment, pure tensile sources do not have a DC component, otherwise they are not pure tensile sources as they require a shear component. Also, a P-wave that is stronger than the S-wave is not necessarily indicative of a tensile event: depending on where you are in the radiation pattern, the P-wave may be much stronger than the S-wave for a pure DC event (i.e. at 45° from the focal planes). In addition, the sensitivity to S- and P-wave in dependence of sensor orientation is not known. Generally, simple visual assessment of S to P-wave ratios may be misleading to distinguish DC from tensile sources; for this full moment tensor analysis is recommended, which is currently not possible with our data.

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

We changed the wording to say that the solid angle is a function – or is representative - of the so-called take-off angle.

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.

As the reviewer has already indicated that the discussion is rather long, we prefer to refrain from expanding it towards possible implications for experiment several scales larger than our hydrofractures. Although we agree that various observations may have implications for the large reservoir scale, we prefer to discuss our observations at their scale and leave it to the reader to transport insights to the larger-scale.

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

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

'Transverse isotropic' is a standard term in anisotropic elasticity theory. Transverse possible related to the symmetry axis.

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

We changed it to '...overcoring stress tests, provided an anisotropic …'. By adding a comma it becomes clear the second part after the comma belongs to the first part of the sentence.

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

The stress field characteristics described here are key outcomes of the paper. We prefer to mention it in the abstract. We shortened the abstract by removing aforementioned information (Comment 1 above)

Introduction

Line 37: "Hydraulic fracturing or hydrofracking (HF)"

We prefer to avoid the more colloquial term 'hydrofracking'.

Line 37: Rephrase "Hydraulic fracturing induces artificial fracture networks in a rock mass by highpressure 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…"

We partially adapted the changes in the manuscript.

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

We added ‘proximity to failure’ as explanation.

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”

We use the acronyms as suggested and use enhanced geothermal systems.

Line 52: “etc; see Zang and...2010”

Changed.

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

We prefer to keep our formulation for brevity.

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.

Changed as suggested.

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.

Changed as suggested.

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

Our formulation is correct.

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.

We here refer to the size of the hydro-fracture, which is constraint be volume and not injection rate.

Line 67: “at which the breakdown occurs”

Changed.

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

Changed partially as suggested.

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

We prefer not to distinguish between acoustic emissions and microseismicity as the distinction is not clear.

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

We refer to the HF scale.

We added this in the text.

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

Using the past is also not appropriate as microseismicity is still being used routinely.

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

We added the reference.

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

We refer to the HF scale.

We added this in the text.

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

We prefer to keep our formulation as it is more complete.

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

Here we only discuss impact of stress field and anisotropy on fracture orientation. However, we later added in the discussion section a discussion asymmetric fracture growth that includes the interesting study by Dahm et al., (2010).

Line 94: “direction sigma 3 (Haering…,) particularly for HF operation (Rutledge…; Zoback et al. 2012 SPE)”

Changed as suggested.

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

We added the reference as suggested.

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

Changed partially.

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.

We did not claim that acoustic emissions are inefficient in radiating energy, but tensile fractures. The notion is also supported by the fact that acoustic emissions mostly have double-couple sources, and only rarely are secondary opening components observed.

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

We feel our formulation is more precise.

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

Changed as suggested.

Line 121: “though it would be desirable”

Our formulation is correct, too.

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

Corrected.

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?

Changed as suggested.

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

Changed as suggested.

Line 176: remove “that serves”

Changed.

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

Changed.

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

Changed accordingly.

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

We changed the sentence to make it clearer.

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

Corrected.

Line 209: “experiments” Include at least one reference

We added a reference.

Line 215: “accelerometers” remove the Italic format

Changed (also for piezoelectric sensors further above).

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

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

We explain in brackets that this is related to the system being occupied to store the detected waveform.

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

Changed.

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

Yes, ‘transverse isotropy’ is correct and a standard term in literature.

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.

We add a short explanation that 1000 repetitions yields a statistically representative sample.

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

We prefer to spell out for clarity.

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

We here limit ourselves to the method description, while the results on located clusters and repeated events are covered later.

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?

We indicate that the noise-level is less than half of the one from the tunnel sensors.

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

We prefer to keep it as it is, as moving this section would compromise the text structure. We first describe general event statistics (number of detected/located events) and then describe when they occurred (breakdown cycles, reopening cycles or after shut-in).

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

Unfortunately, we do not have an explanation for it, and we do not want to expand the discussion towards this topic. We prefer to simply report this observation for future research to address the it.

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.

We do not observe that seismicity only occurs when pressure or rate are at their peaks. In fact the largest seismicity rate occurs somewhat after the peak pressures during breakdown or reopening.

However, we agree that it is interesting to state that seismicity rate somewhat depend on injection rate but not on pressure.

We added a statement describing these observations in the manuscript.

"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.

We do not follow the observation regarding HF3. Post-shut-in seismicity also occurs in the cases of HF1 and HF2. Clearly, seismicity rates and propagation reflects fluid-propagation and fracturing. We feel it is not necessary to additionally comment on this in the text.

Also as mentioned above, injection pressure and rate-dependence of seismicity can be observed from Figure 2 and does not require an additional figure.

Line 337 "(see Doescth...)"

We added the word 'see'.

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

We believe our formulation is more precise.

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 misalignments?

Currently, we do not have an explanation for the downward or upward propagation. The comparison with the stress field estimates from overcoring is done later in Section 5.2 and Figure 13. Note that it does not make sense to discuss the stress field in terms of sigmaHmax, as the stress tensor is rotated with respect to ground surface or the horizontal.

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.

We added a sentence on this.

Line 379: attach "wave" and "forms" (waveforms)

Corrected.

Line 402: remove the second "and"

Corrected.

Line 406: "but also accounting for"

Our formulation is correct.

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

We added that it is in time-domain.

Line 412-414: Please, clarify this sentence.

We understand that the reviewer has read the originally submitted version of the manuscript. The updated manuscript addressing comments from Reviewer 2, already addressed this sentence, too.

Line 435: replace “accord” with “agreement”

Changed.

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

We are actually referring to Figure 2g-i in this sentence and not to Figure 10c.

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

We indicate in the text that these magnitudes were located with an error better than 2 m.

Line 454: “the same source mechanism”

This part was shortened in response to reviewer 2.

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

This part was shortened in response to reviewer 2.

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

This part was shortened in response to reviewer 2.

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?

We prefer to keep our formulation. We here only state our observations. We prefer to not comment on their significance although we believe that they are significant as they are consistent throughout all cycles.

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

Changed as suggested.

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

Changed as suggested.

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

Partially changes as suggested

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”

We prefer to keep our formulation, which we believe is clearer.

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..

In fact, it is decimetres. We changed this in the text.

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...”

We prefer to keep our formulation.

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.

We added a short explanation.

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

‘Fault’ is not an appropriate term for fracture at a scale of meters. We prefer to keep our formulation.

Line 523: remove 0 in “090”

Changed.

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

Changed as suggested.

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

Changed.

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

We keep our formulation as it is more precise.

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

We added a short discussion on asymmetric fracture growth and pressure/stress gradients.

Line 556: remove “Once”

Changed.

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

We keep our formulation.

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

The goal of this calculation is to compare σ_n on the foliation plane to σ_3. If the injection pressure is used to compute an effective stress, both quantities would reduce by the same amount. Thus, we do not see how this changes the discussion.

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..”

The section has changed in response to comments by reviewer 2.

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.

Double-couple events – even if not pure and imposed by a tensile component – do require shear motion along the source plane. Such a mechanism is not in agreement with pure tensile fracture opening.

We reworded this sentence to explicitly state that pure tensile fracturing can be excluded for the double-couple events (but not necessarily in general).

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…”

We added such an introductory sentence.

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...”

We partially changed the formulation for better clarity.

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

We keep our formulation.

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

We keep our formulation.

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

It is the discrepancy explained in the previous sentence. We reworded the previous sentence to make it clearer.

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.

We prefer to use an alternative formulation for the conclusion.

Appendix

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

We state that it is the entire ray path length.

Line 627: revise “inverse”

Corrected.

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

Corrected.

Line 640: remove the second “becomes”

Corrected.

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

We meant arccos and corrected it.

References

ASTM (2008) is missing!

We could not find the reference ASTM (2008) so we changed the citation to Zang and Stephansson, (2010)

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

We removed the a.

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

It should be Holliger. We changed it in the text.

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

It is actually a patent. We reference it properly.

Manthei et al. 2003. This reference is missing!

No, it is actually there.

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

We corrected it in the text.

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

It should be 1984. Corrected.

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

Rutledge and Phillips, 2004 was changed into Rutledge et al, 2004 in the text.

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

Van As (2000) was changed into Van As and Jeffrey (2000).

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

Corrected.

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

We added the reference and corrected the year. It is Thomson, 1986.

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

We corrected it in the caption.

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

Changed as suggested.

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

The figure and caption has changed in response to reviewer 2.

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

We keep our formulation.