Response to reviewers

Reviewer 1

We thank to reviewer 1 for the positive comments on our manuscript.

Reviewer 2 (G. Kwiatek)

We would like to thank to G. Kwiatek for his thorough review, which we led to great improvement of the manuscript. We were able to address almost all of his comment as detailed below.

The Authors put a lot of efforts in constraining the hypocenters of AE activity. However, I am puzzled why they did not use the S-waves to improve the location quality? From Figure and the paper itself it is clear the S-waves were efficiently recorded and they could help to constrain the locations. S-phases have been applied in previous studies using similar acquisition system with success (JAGUARS project, ASPO FHF experiment, see appropriate papers). Could you comment on that and also inform the Readers on your choices?

Although in many events the S-waves are clearly discernible, we found that the onset times of only a few of them could be accurately picked, while most of them are associated with considerable uncertainty. Furthermore, the S-wave velocity of the rock mass is not well described in the literature, and our S-wave arrival times are too few to determine a reliable S-wave velocity model. For these reasons, and the fact that the locations are already well determined with P-wave arrivals only, we decided that the effort to pick and use S-waves arrival is more trouble that it is worth.

In introduction (L145-149) I think it would be fair and beneficial to mention in this context a concurrent Aspo FHF experiment (Zang et al., 2017; also: Kwiatek, G., Martínez- Garzón, P., Plenkers, K., Leonhardt, M., Zang, A., Specht, S., Dresen, G., and M. Bohnhoff. Insights into complex sub-decimeter fracturing processes occurring during water-injection experiment at depth in Äspö Hard Rock Laboratory, Sweden, JGR, submitted, or equivalently a similar, but already published contribution at the Schatzalp workshop on Induced Seismicity). The aim of ASPO experiment was to optimize the hydraulic fracturing procedure and limit the occurrence of undesirable LME. Otherwise it seems your introduction is incomplete.

We included the proposed references in the introduction.

The calculation of average cross-correlation coefficient (L293-294) seems to be not conducted fully appropriately. The ensemble of correlation coefficients should be first transferred to the Fisher's domain (Z-domain) and then averaged. The resulting average can be transferred back to the "regular" domain by the inverse Z-transform (variance stabilizing transformation). Please correct.

We included the Fisher transformation in our cluster analysis procedure (also mentioned in now in the manuscript) and found comparable results to the previous analysis without the Fisher transformation. We updated Figure 8 and 9 with the new results and adjusted the text accordingly. As the events for which focal mechanisms were computed do not fall into the same clusters as before, we removed the grouping into events belonging to different clusters. The results and interpretations remain the same.

Regarding the observations related to shear/tensile events (L390-L392). I would definitely appreciate here more extensive presentation of results in exchange for the pure reference to Eaton's paper. What is the range of ES/EP energies (or S/P amplitudes) you observe? Do the observed values of ES/EP or S/P amp. ratio allow to suggest the existence of tensile components? (cf. discussion in Kwiatek, G. and Y. Ben-Zion (2013). Assessment of P and S wave energy radiated from very small shear-tensile seismic events in a deep South African mine. J. Geophys. Res. 118, 3630-3641, DOI: 10.1002/jgrb.50274.).

We expanded the short discussion on possible presence of tensile source components by giving the range of observed S/P-wave amplitude ratios, and mention that low values may point to a tensile component, as found by Kwiatek and Ben-Zion (2013) (added as reference).

In lines L473-L475: Stress rotations due to pressure changes were already reported for geothermal reservoir (Martínez-Garzón, et al., GRL, (2013), DOI: 10.1002/grl.50438) and also confirmed recently by synthetic modellings (see Ziegler et al., Schatzalp 2017). If you want to keep this section (see minor comments), that could benefit to the discussion.

We moved this section to the discussion section and added the suggested reference Martínez-Garzón, et al., (2013). We also added the earlier paper on stress perturbations due to hydraulic fracturing by Warpinski & Branagan, Altered stress fracturing, JPT,1998.

The comparison of stress estimations (_L530-532). I am really puzzled why the Authors did not perform the stress tensor inversion using the polarity data, e.g. following the nonlinear stress tensor inversion (MOTSI Abers/Gephart), and rely their results on the spatial orientation of the fault planes. I believe stress tensor inversion would provide additional information supporting your findings.

Stress inversion from polarization data is not the primary goal of this paper and may in fact produce very misleading results. Instead, we strive to obtain reliable orientations of the seismicity clouds that provide an indication to the σ3 direction, and to compare these to the results of other stress measurements. Clearly, the found focal mechanisms do not match well to the the stress field orientation estimates from both the seismicity clouds and the overcoring measurements. As discussed later, the focal mechanisms may indicate the stress perturbations around the propagating fracture, which in our case would invalidate the use of polarization data to infer stress orientations.

I am afraid I am not fully understanding your discussion on mechanisms (L570-580). Looking at your mechanisms the variability of the fault plane solutions does not seem extreme. Why don’t you plot a Mohr circle and discuss how (even the composite) fault plane solutions fits to the stres field derived from other measurements. It may be that the fault planes (regardless of whether they are normal, strike slip or thrust) may actually be critically stressed in this complex environment. This was successfully applied for analyzing ASPO data (see Kwiatek et al., 2017, Schatzalp proceedings).

By plotting the stress conditions along the focal planes in a Mohr-Coulomb diagram, we indeed find that pressure leak-off is sufficient to explain the different focal mechanisms. We added such a figure to the text and adjusted the discussion on the topic accordingly. Thanks for the great idea.

Minor comments

L65 I understand that the volume is related to small-scale hydrofracturing projects. However, it would be good to write it down here once again explicitly to distinguish it from larger-scale industrial projects

Corrected.

L73 Naming convention for "microseismic" is not consistent. You either use microseismic or microseismic. Please unify.

We replace micro-seismic by microseismic throughout.

L83 Manthei

Corrected.

L124 Zang et al., 2017 (and everywhere else, the appropriate reference is: Zang, A., Stephansson, O., Stenberg, L., Plenkers, K., Specht, S., Milkereit, K., Schill, E., Kwiatek, G., Dresen, G., Zimmermann, G., Dahm, T., and M. Weber (2017), Hydraulic fracture monitoring in hard rock at 410m depth with an advanced fluidinjection protocol and extensive sensor array, Geophys. J. Int., 208(2), 790–813, DOI:10.1093/gji/ggw430)

Corrected.

L215 The Wilcoxon does not seem to work very efficiently above 17kHz (cf. appendix in Kwiatek et al., 2011) and our experience was that the transfer function was not flat above this range.

Corrected.

L217 You use "piezo-sensor", however the well-defined term in the field (cf. Kwiatek et al. 2011 and other follow-ups) is the "(in-situ) acoustic emission sensor" or simply "acoustic emission sensor".

We prefer to keep this term (that is also used in other articles) as we feel it is less ambiguous. Acoustic emission in our understanding refers to a target frequency band. Thus, the term 'acoustic emission sensor' does not say anything about the sensor technology; both piezosensors and accelerometers may be called 'acoustic emission sensors' as they can cover the acoustic frequency range.

L226 SBH-1

Corrected throughout.

L229-230 This is in surprising agreement with what was observed at ASPO FHF (reference?)

I did not find such an observations stated by Zang et al., 2017 or any other ASPO reference.

L244 Correct the reference

Corrected.

L251 How the P-wave velocity was estimated (any details, reference, variability?)

We already describe that the P-wave velocity model is estimated using a grid-search approach minimzing the differences between observed and predicted P-wave arrival time. We feel the information given is already appropriate

L279-L284 It is not clear what this procedure reflects. Are you concerned about velocity mismodelling, or P-wave mispicking. Please clarify.

We now clearly indicate in the manuscript that we are concerned with P-wave arrival time uncertainties.

L286 Is it 95% confidence interval?

Yes. We added this in the manuscript.

L323-325 This is similar to what has been observed at ASPO, please refer.

We added reference to Zang et al., 2017.

L357-L385 The sentence starting with "Clear rings..." is not understandable, please rephrase/modify/extend it.

We formulate this statement more clearly now.

L380 all 112

Corrected.

L411 npi f_0

Corrected.

L412 P-wave

Corrected.

L413 What is the range of dominant frequencies you have and how this correspond to the resonant frequencies of the AE sensors? Does it not lead to overestimation of magnitude, as you enhance the resonances at high frequencies due to Q correction?

The range of dominant frequencies is between 1 – 10 kHz as not indicated in the manuscript. To avoid artificial enhancement at (currently unknown) sensor resonance frequencies, we follow the recommendation of Zang et al., (2017) and filter data between a narrow band-pass filter between 3 and 7 kHz and use nominal frequency f_0 = 5 kHz to correct for attenuation.

L420 Unfortunately to the project, this suggest putting accelerometer sensors on tunnel walls does not allow to effectively record such small seismic events despite of reasonable source-receiver distances.

Although we agree that this might be the case, it does not naturally conclude from our study, as we did not compare borehole versus tunnel wall installation of accelerometers. Thus, we refrain from commenting on this in the manuscript.

L425 This part is hard to understand. Please refer to exact figures. The caption of figure 10 is poor - it is hard to understand differences between a) and b) (stress drop...), Please explain all the details of the figure in the caption!!!

We expanded the caption with details so that figure 10 and caption become self-explanatory. We also improved the details of this computation in the manuscript.

L426 Guess there is a typo here. I have no idea how you calculated so small source radii for the assumed stress drops of 1MPa. Using Eshelby's formula bring me to way larger values of the order of meters (0.8-2.5m i.e. 80 to 260 cm, not 8 to 25 cm as you write) for the stress drop of 1MPa. Anyway, calculations for 0.1MPa seems to be fine. It is also more reasonable assumption regarding the frequencies and expected source radii as well as the low expected confining pressure (stress drop seems to be dependent on average stress)

Indeed, we must have made an error in the calculations. The source radii lie between 1.4 and 4.3 meter for stress drop of 1 MPa and 0.3 – 0.9 m for 0.1 MPa stress drop. We changed the numbers in the text and figure 10 as well as the corresponding discussion accordingly.

L440 What is the b-value? Is it comparable with what is observed for other fracking project (high values)?

Due to the limited number of events with magnitudes and associated high uncertainty in b-value computation, we decided to report only report a robust lower bound for b-values that is estimated to be >2.

L453 Isn’t it simply the composite fault plane solution?

Due this and the following comment we decided to remove these focal mechanisms based on stacked waveform polarizations.

L460-L461 I think it is over-intepretation. I guess if you stack multiple double-couple sources with even slightly different focal mechanisms you may achieve spurious non-DC components (e.g. Frohlich, 1994). Therefore, i am not sure if your reasoning do not go to far. I guess waveform stacking is not the way you can get a reasonable message with respect to the mechanism of faulting (shear/tensile). Please reduce L455-L465 to solid observables and reduce the discussion.

We agree with the reviewer that this is possibly beyond any reasonable interpretation, as it is not clear what such ‘stacked focal mechanisms’ represent. We therefore remove them from Figure 11 and the corresponding text in the manuscript, and thank the reviewer for pointing out a reference that has already considered the problem.

L467-L475 This paragraph should go to discussion, if you are concerned about the (apparent) lack of non-DC events. I am not concerned and I do support your findings, though, and believe your results are solid without too much speculations on why you don’t observe tensile openings. As you write, the tensile openings are energetically ineffective, and likely limited to the very small earthquakes that likely cannot be handled properly by AE system.

We agree and moved the paragraph to the discussion, where parts of it are repeated anyway (we kept the first sentence as it goes well with the end of the previous paragraph). We also included the reference to Martinez-Garzon et al., (2013) that was recommended earlier.

L493 Nice observation

Thanks!

L500 Some crossed-out words

Corrected.

L530-532 Some more crossed-out words

Corrected.

L559 Stress magnitudes and stress field should be presented earlier in the manuscript
(just before section 3 i suggest)

We moved the table with the stress information to the end of the section on stress measurement.

L594 "readily be applied to failure at such scales"? Is that what really want to write?

We changed the sentence to ‘can be applied to seismicity at such scales’

Figure 2 "d)" is misplaced. The idea of coloring per HF is a good one, but the problem is you re-use these colors for painting other curves (e.g. Injection pressure and rate in Figure 2). Maybe it is possible to take advantage of other colors or patterns (e.g. dashed lines) in this and other figures while you NOT refer to the particular stimulation. Finally I suggest to repeat HF# label in each panel and also paint dots in g,h,i with appropriate color reflecting HF.

We improved the figure as suggested by the reviewer.

Figure 4. Hard to read... Why not to put simply a 2D version of this plot (e.g. view from top)?

A 2D plot would conceal the borehole seismic array, which nicely shows the impact of the anisotropy on station corrections. Indeed the 3D view is difficult to read, however, we could not find a better view angle.

Figure 6. Mark HF1 HF2 and HF3 labels in a). I suggest to change colors in b) to not confuse reader with what is in the subplot a)

Changed as suggested.

Figure 10. Please rewrite the whole caption. The content of the figure is quite badly explained. What is R? Please enhance stress drop messages, or just explain in caption what is the difference between a) and b) subplot.

We changed the figure and caption accordingly and added more information (see comment further up).

Figure 11. Remove "for a few events". I suggest to put shading to distinguing thrust from normal focal mechanisms more easily.

Caption changed as suggested. We feel the focal mechanisms are more clear without shading.