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Solid Earth Discussions

SED

Interactive comment

Interactive comment on "On the link between stress field and small-scale hydraulic fracture growth in anisotropic rock derived from microseismicity" *by* Valentin S. Gischig et al.

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General comments

The paper by Gischig et al. aims at characterizing the local stress field at Grimsel underground rock laboratory during the small-scale hydraulic fracturing experiment. To achieve this task, the Authors combine a variety of direct in-situ stress measurements with seismic data originating from high-sensitivity acquisition system consisting of in-situ acoustic emission (AE) sensors an high-frequency accelerometers.

The manuscript is an example of solid scientific work combining seismic data (here:

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extremely small seismic events recorded with the great effort using state-of-the-art high-frequency acquisition system) with geomechanical (local stress measurements), as well as geological data towards a unified, seismo-mechanical characteristics of the local stress field. There is not so many comparable projects, e.g. recent fatigue hydraulic fracturing experiment in ASPO (Zang et al., GJI, 2017), or at even smaller scales in the laboratory, and they all posed a significant challenges in data acquisition and interpretation. As such, the presented work contributes to the scientific progress in understanding the seismic response and stress field evolution in geo-reservoirs subjected to hydraulic fracturing. The applied investigation methods, although not innovative, are absolutely valid and appropriately applied, as well as appropriately referenced whenever necessary. The paper is well written and structured. The quite lengthy introduction could be likely slightly reduced by focusing more on actual topic of the paper (stress field characterization). The number of figures is appropriate. The readability of figures could be improved (I provided some comments later on). As such, I think the manuscript is acceptable for publication after addressing proposed comments.

Specific comments

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?

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.

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.

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.).

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.

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.

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).

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

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

L83 Manthei

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 fluid-injection protocol and extensive sensor array, Geophys. J. Int., 208(2), 790–813, DOI: 10.1093/gji/ggw430)

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.

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".

L226 SBH-1

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

L244 Correct the reference

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

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

L286 Is it 95% confidence interval?

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

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

L380 all 112

L411 \pi f_0 L412 P-wave

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?

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.

L425 This part is hard to understand. Please refer to exact figures. The caption of figure 8 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!!!

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)

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

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

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.

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.

L493 Nice observation

L500 Some crossed-out words

L530-532 Some more crossed-out words

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

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

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.

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

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)

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.

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

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