Author's reply se-2019-159

Dear Editor

We would first of all like to thank you and the reviewers for your valuable time you put into this manuscript. Your comments and constructive criticism have improved the quality of this manuscript. In the following, we address all reviewer comments point by point in blue color font. The reviewer comments are held in italic font. The line locations used in this reply letter are referenced to line locations with displayed track changes.

Anonymous Referee #1

The biggest question I have left after reading the manuscript is concerned with the (lack of) difference of the HS and HF stimulation responses. It seems the experiment was set up to tease out the differences in seismic behavior between the two different stimulation treatments. Looking at the structures that were activated seismically I wonder if the result really were ANY different. It seems that the same structures were activated and the variation among the 4 tests of each treatment was at least as large as the variation between HF and HS treatments. Although the differences of HF and HS tests are discussed in some length, I am missing a clear statement regarding this negative finding.

We thank the referee for this observation and agree that our experiments were also set up to tease the differences between HF and HS stimulation experiments. The differences may be less than expected, because HFs quickly connect to pre-existing fractures. We added a sentence explaining this assumption in the discussion section 5.2 (L. 1139). However, we also believe that several clear and detailed statements on the differences were already included in the first section of the discussion part (starting in L. 1036) as well as in the abstract (L. 30) of the manuscript.

To support this, I would like to encourage the authors to rework Fig. 4 as the pressure information cannot be discerned from that. Please add a second axis for pressure and scale it such that it uses the full range of the subfigure.

Yes, we agree with the referee, and in order to better discern the injection pressure from the injection rate an additional y-axis was added to Figure 4 and to the figures of the remaining experiments in the supplementary material as the referee suggests.

The last major comment is regarding the references in the text, many of which are missing in the list of references. I did not do a full check, but urge the authors to do so. Some missing references are McClure&Horne, 2013; Secor & pollard, 1975;Schoenball et al., 2019; Kwiatek et al., 2018; Goodfellow & Young, 2014, Brixel et al., in review; Villiger et al., 2019; Jung, 2013b;

We regret this mistake. A full check was performed and missing references were added to the reference list.

Minor comments:

L. 115: I would not say that alternative approaches to McGarr are more conservative, but rather say that the assumptions of McGarr may not be valid. We now have ample of data to discard the McGarr hypothesis.

Yes, we agree with the referee here. However, to shorten the manuscript for better readability we removed the part on estimating the maximum possible magnitude to a great extend, because our paper does not contribute to this discussion.

L. 261: It is unclear what "a possibly unperturbed stress state" should mean.

Yes, this can be confusing. We now added more information to the text to make clear what we mean by the unperturbed and the perturbed stress state (starting in L.315).

L. 537: Do you have an idea why so many events were detected after shut-in in this test? Any indications from the structures that were active. How about b-value, etc.? Is that something we should worry about for a full-scale test?

That is an interesting question! Unfortunately, we believe this high percentage of detections (33%) has no seismic origin. We think a direct hydraulic connection between the injection interval and the open seismic monitoring boreholes (termed GEO's, Figure 3) was created. The hydraulic connection led to flow-through of the seismic monitoring boreholes during stimulation and possibly to stick-slip movements of the AE sensors, which in turn led these detections (explanation starting in L. 604). Further evidence that these detected events were induced through flow-through provides the fact that these detections were often made on AE sensor pairs placed in the same seismic monitoring borehole. We remember that a detection is declared a detection as soon as a possible seismic event was observed on at least two AE sensors (L. 411).

Fig. 6: Would be helpful to remind what gray events represent

Yes, agreed, a sentence was added to the caption of Figure 6 to clarify what the gray events are.

L. 655: You substitute t by the injected volume for Shapiro's diffusivity estimation (SBRC). This addresses and important criticism of the SBRC method, namely that it disregards fluid injection rate (e.g. Schoenball et al., 2010, GJI). I believe this deserves to be discussed in some more detail here.

We absolutely agree and added a statement that the estimated diffusivity values are based on the SBRC which disregards the fluid injection rate (L. 733).

L. 694 & 696: -0.1%, -0.5%

To direct the reader more to the main topic of the manuscript and improve the readability we decided to delete Figure 9 in which we superimposed seismicity with velocity variations.

L. 772ff: Not quite clear. Do you mean the moment of all events combined into a single event?

Yes, exactly. We made adjustments to the text, the sentence now reads "…cumulating the moment release of all possible seismic events per injection experiment into a single earthquake would have induced a moment magnitude MW in the range of -3 to -1" (starting in L. 884).

L. 956ff: Nice discussion, important observation!

Thanks!

L. 991ff: In this whole discussion I am missing some statements whether the activated structures actually are hydraulic fractures. Based on the Schmitt plots and the seismicity plots it seems that most structures could be hydraulic fractures just as well.

We agree with the referee; these statements are missing. We added the information on what our definition of a hydraulic fracture is and assigned the probable experiments in which a hydraulic fracture was induced to it. We furthermore added the information to the text, that the combination of mode-I and mode-II,III deformation is possible for the majority of the experiment (starting L. 1120).

Section 5.3: Given the data presented in this manuscript this entire discussion is rather speculative. I can see that this point may well be made using complementary data discussed in some of the "in prep" manuscripts referenced throughout but it may not fit well in this manuscript.

We fully agree with the referee. We now just state the determined percentages of seismic to total deformation observed in our experiments, compare it to values form the literature and deleted the rest of the paragraph (starting in L. 1160).

L. 1089ff: This is an interesting discussion. However, there should be a limit to this given by the total volume per stage or the size of the activated rock volume. Let's call this elementary rock volume (ERV) for now. Once you break out of this ERV, your likelihood of activating a structure from a neighboring ERV with different seismogenic index grows very fast. So unless you are activating only the near-wellbore region of your ERV there may be limits to the zonal isolation approach.

Yes, we believe the referee is absolutely right here. We pose the question on how representative a pre-stimulation might be in the text. We conclude that zonal insulation and the ability to seal isolated zones no matter what offers more flexibility and opportunities to intervene an ongoing stimulation treatment (starting in L. 1237).