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 #2

Opening paragraph

The manuscript titled "Influence of reservoir geology on seismic response during decimeter scale hydraulic stimulation in crystalline rock" by Villiger and others is a dense description of a series of injection experiments performed at the Grimsel Test Site, Switzerland. The experiment was well-constructed and the seismic analysis is very complete. While I find the science aspects of this paper intriguing, the organization and in some some cases level of detail distracts from the main topic. The paper seems to be structured like a report more so than a journal article. A lot of detail is included whether it is relevant to the main goal or not.

We thank the referee for this criticism. We realize that the manuscript/report may be too bulky. We shortened and streamlined it to be more concise and readable and devoid of distracting details.

Aside of a substantial shortening of the introduction, we merged section "4.2 Spatial properties of seismicity clouds" and a shortened section "4.3 Propagation of seismicity". Left of section 4.3 are diffusivity values and general propagation characteristics of seismicity clouds, whereby the methodological part of these sections was transferred to the supplementary material SM6 and SM5, respectively. In addition, Figure 9, where we showed seismic triggering fronts of an experiment, as well as an overlay of seismicity with inferred velocity changes, was removed. Also, section "4.7 Network performance" was removed partially from the manuscript. From Figure 13 only d) is left and included in section "4.3 Frequency magnitude distributions". Finally, parts of the discussion in section "5.3 Aseismic deformation" and the outlook section was removed from the manuscript.

Along those lines, I am not sure that the outlook section describing the researchers next plans is appropriate here.

We removed the outlook section as recommended.

Overall, I find this an important paper with interesting and possibly significant results. However, had I not been reviewing, I probably would have quit reading, because it was hard to keep the track of the goal and relevant information given the organization and distracting information.

Introduction

like a lot to introduce the study at hand, and given the organization while the information is useful and interesting there is not a clear motivation between the introduction and what is to come. The title talks about geology, but this is not introduced until page 3.

We agree with the referee that the introduction covered a wider scope as the rest of the article. We shortened and restructured the introduction so that it is focussed to the main topics of the paper.

Some specific comments: Organization of paragraph starting Line 124 needs work: line 124 "seismicity rates might be linked to geologic setting"; line 127 "Seismicity may also be dependent on fault orientation"; and then line 131 returns to "seismic response . . .may also be linked to local geological setting". Seems like should talk about local geologic setting generally and then go into details like fault orientation. As written now is jumpy and distracting.

This whole paragraph was restructured and rewritten (now starting in L. 121).

Figure 1: what is Petrothermal? Should be defined somewhere, not a common term

Agreed, Petrothermal (i.e., injections into hot and dry rock volumes) is now defined in the caption of Figure 1 as well as Hydrothermal (i.e., injection into aquifers).

Methods

This section describes the experimental design for injection and methods used in the catalog construction. Table 1 is a nice concise collection of both the experimental design and results from the seismic analysis. I would suggest a second table that breaks out the seismic information by cycle. You later describe cycles but there is not a clear place to assess this analysis.

We understand the referee comment. We included tables in which located seismic events of HS and HF experiments are resolved in cycles and phases to the supplementary material SM8. We believe the tables are cumbersome to read and therefore placed them in the supplementary material.

The discussion of the AE sensors was really distracting. Strongly suggest moving this discussion to the supplement and just highlight the key points in a subsection "Integration of AE sensors"

Thanks for this comment. We assume that the referee is talking about section "3.2.1. Seismic monitoring" and the explanation of the installed hardware therein. We do not agree with the referee here. This is not the usual setup of a seismic network, therefore, we belief it is important to assign it some relevance and explain it in some detail (Starting in L. 369).

Specific comments: The pick errors seem very specific were they determined empirically? If not how did you decide on these values?

Yes, pick uncertainties were determined empirically. We added the word "empirically" at the location where we introduce the P-wave pick uncertainties (L. 432).

You spend a lot of time discussing pick weights and the velocity model, but when it comes to station corrections there is one sentence referring to another reference. Seems that there could be more here.

We agree with the referee, that the Joint Hypocenter Determination (JHD) approach which involves the determination of station corrections is not discussed in much detail. We have added one more sentences to the explanation of the station corrections. However, the weighting of the P-wave picks in the location of the seismic events, as well as the determination of the five velocity parameters using a genetic algorithm are introduced in this manuscript. The applied station corrections on the other hand follow the approach introduced by Gischig et al. (2018). Gischig et al. (2018) offer a nice summary in their appendix of the Joint Hypocenter Determination (JHD) approach

in an anisotropic velocity model, in which the determination of station corrections is a central part. Therefore, we decided to not explain the station corrections in more detail (L. 460).

The discussion of magnitudes and notation is confusing: The introduction of the three types of magnitudes paragraph starting line 418 is confusing. I recommend that you start with a direct sentence like "Here we calculate three magnitudes: . . ."

Yes, we agree with the referee here, this can be confusing. We inserted some introductory sentences in the direction the referee proposes (starting L. 477).

Line 513-514 discusses how you calculate the "adjusted amplitude magnitude" MA (as defined line 426), but in line 514 this is called the "amplitude magnitude". Please be consistent.

Yes, that is confusing, now M_A is called the "amplitude magnitude" throughout the manuscript (L. 480, 493, 582).

The equation to get the adjusted magnitude is to subtract 4 from the relative magnitude calculation. This seems really large. In the comparison how large was the spread in magnitudes. It would be worth seeing a figure of the Mr Mw comparisons

In our understanding the adjustment from relative magnitudes M_r which aims to describe the relative source strength with no absolute meaning, into more realistic amplitude magnitude M_A based on a physics based M_W is arbitrary. We included a moment magnitude M_W vs. amplitude magnitude M_A comparison to the supplementary material SM7.

Results

Specific Comments: Line 536 says "During HF injections, significantly fewer detections compared to HS injections (Figure 4c,d)". In Fig 4c, the cumulative number of seismic events is 2000 compared to Fig 4a with 500. Even Fig 4d has 600 which is more than 4a. True HF is less than what is shown in Fig 4b, but your statement is not supported by the figure. When I look at numbers in Table 1, I would still be pushed to use the word "significantly". There actually seems to be a lot of variability in the total number of detections, which should perhaps be investigated or at least commented on.

Yes, we agree with the referee, this may be confusing. Detected seismic events do not have to be mistaken with located seismic events. Seismic detections should only in a first approximation be considered as a measure for seismic activity. Seismic detections can be flawed because the quality control of location (i.e., at minimum 4 P-wave arrivals are needed, and the largest axis of the error ellipsoid should be within 1.5m) has not yet been passed. One reason why the number of detections can be flawed is the assumed flow through a seismic monitoring borehole and the triggering of stick-slip movement of AE sensors in the borehole (starting L. 608). For any analysis in this manuscript only located seismic events are used.

To make the difference between detected and located events more clear, the seismic detection rate was removed from the middle plots of Fig. 4 and the cumulative located events are shown on the second y-axis of each experiment.

Line 537: For HF "a comparably high percentage of detections (33%) were made during shut-in". This is not evident in Figure 4.

We also agree with the referee here; this is not obvious from Figure 4. Figure 4 only hosts the time evolution of a selection of HF experiments, to get a full picture, the supplementary material SM3 has to be considered. To the cross reference of the figure we added "for a selection of HF experiments" and "for a selection of HS experiments" for HF and HS experiments, respectively (L. 605 and L. 589).
Contributions to the 33% shut-in detections stem from experiment HF5 and HF8 in which a hydraulic connection was created to a seismic monitoring borehole (explanation starting in L. 608). We additionally indicated the period where we believe the detections are flawed by stick-slip movements of the sensors in the monitoring borehole (figure to HF5 in SM3, and figure to HF8 is Figure 4d).

Figure 5a would help to label which were S1 and which S3 injections

Yes, that is right! The information was added to the figure.

Instead of "fitted for" suggest "fit to" throughout text

That does fit better. In L. 687 the "fitted for" was exchanged by "fitted to".

Line 760, You introduce M0 displacement and M0 hydraulic and then immediately move to a discussion of seismic moment. It would be helpful to have a conceptual description of these parameters when you introduce the terms and how they differ before going into the details.

The three quantities (seismic moment, hydraulic moment and total moment release) used in this section are introduced in an introductory sentence. Then, the estimate of the seismic moment release is explained in more detail (starting in L. 873 onwards). In a next section (L. 890) the quantity equivalent hydraulic moment and its estimation is explained. Finally, the total moment (L. 898) is explained. We feel the structure will be clear to readers when the paragraph is read as a whole.

Line 801: is this really "a best guess"?

We agree with the referee here that "a best guess" is maybe not the best choice of wording here. We changed "a best guess" to "average estimate" (starting L. 879).

The section on Network Performance should come earlier in the discussion and be used to inform the discussion on b-value and the seismic cloud.

Thank you for this comment. We agree with the referee and included the discussion on a varying network sensitivity at the location where M_C is introduced (Section 4.3, L. 808). We also inserted a new figure showing the varying M_C in the experimental volume (Figure 9).

Line 868: Why is there a discussion of S-phases. You did not use them. You could simply add a sentence when the waveforms in Fig 3 that the S-phases were not of sufficient quality for picking

We agree with the referee, it is not common sense to discuss this limitation. However, we believe when displaying waveforms in Figure 3, some readers might ultimately ask themselves why no S-phases are observed and picked. Thus, we added some more information at the location where we explain that no S-waves were picked (starting L. 422).

Discussion, conclusions, and outlook

Given all the information in the Results, I was looking for a concise summary that linked back to main the questions of the paper. This could be an introductory paragraph before diving into the details.

Yes, we totally agree with the referee. An introductory paragraph was added at the beginning of this section summarizing the main results of this experimental campaign out of seismological perspective (starting in L. 1002).

Line 880: you mention "permeability increase, pressure propagation and rock deformation". These were not directly addressed in the paper

Yes, we agree, in this section we refer to the performed hydraulic stimulation experiments at the Grimsel Test Site in general. It is meant as introduction as the discussion brings together other observations made prior and during the experiments such as injectivity change or the influence of geology (L. 992).

Line 925 what does "the first 100 1 of fluid" mean?

Yes, we understand that this can be confusing. We mean by the first 100 l, the initial 100 l of fluid which were injected into the ductile shear zones S1. We replaced "first" with "initial" leading to "the initial 100 l of fluid" (now L. 1054).

Line 1077: what does "first 200 1 of injected volume" mean

Same here, we replaced "first" with "initial" (now L. 1214).