

Response to Reviewer 1:

Dear editor,

You asked me to review the manuscript entitled “Hydro-mechanical processes and their influence on the stimulation effected volume: Observations from a decameter-scale hydraulic stimulation project” authored by Hannes Krietsch, Valentin S. Gischig, Joseph Doetsch, Keith F. Evans, Linus Villiger, Mohammadreza Jalali, Benoît Valley, Simon Loew, and Florian Amann submitted to Solid Earth.

From my perspective, the topic of the study “What happens when fluid is injected into boreholes?” is suitable for publication in SE. In principle, it bears implications for fundamental research and industrial applications, alike. In fact, I consider the performed fantastic experiment at the Grimsel site as a milestone of geoscientific research and congratulate the people involved for the work they have performed. However, my general impression of the submitted manuscript is that it requires substantial rewriting. Unfortunately, I feel incapable of judging the soundness of the made interpretations based on the submitted manuscript.

Below, I try to emphasize some of my thoughts reflected by the digital annotations of the attached pdf-version of the manuscript. Please note, that, as ample as they may be, the comments on structure, wording etc. are meant as examples, in cases to be correspondingly applied to the entire manuscript. In addition, I would like to stress that my digital comments document my immediate response when reading the manuscript but have not experienced any retrospective polishing regarding diplomatic wording etc. No offense meant whatsoever.

When opting here for “major revision”, though I think that most of the manuscript has to be rewritten (in the wake of which shortening to maybe half of the current length seems possible and should be aimed for), I do so because the experimental observations do undoubtedly deserve publication. They, however, should first at all be presented without mixing in interpretations and then be accompanied by a plausible and sober account of their significance.

Thank you for providing me with the opportunity to read about the results of this spectacular fluid-injection experiment. My apologies for taking long to provide this report; the review is not as thorough and constructive as I probably would have liked it to be, but I really did not want to delay matters any further.

Kind regards,

Joerg

Response:

Thank you for your comments on our manuscript. Following your request, we reorganized and shortened the manuscript to improve the visibility of the key observations and their importance. The most important step in this direction was the shortening of the introduction in a sense that we removed the long literature review. The literature review was also removed from the methods part, when it comes to the description of picking the jacking pressure. In addition we stepped back from the “Q-strategy” and derived the research questions more directly from the introduction text. Moreover, we cleaned the manuscript for statements that are written multiple times in the manuscript and separated results and interpretation more drastically. Thus, the manuscript has been extensively rewritten, which makes it difficult to respond in detail to your comments in the document pdf. We are sorry for the

inconvenience. However, we respond to the main points you raised in the referee letter and the comments within the pdf that are about scientific content, which is still in the manuscript.

In fact, we feel that the manuscript reads much better now. Thanks for requesting this re-organization!

terminology:

To me, many aspects of the chosen terminology (and/or nomenclature) are barely instructive for the reader (e.g., nomenclature for injection zones, injection cycles etc.) or are close to presumptive –if not to say misleading- when it comes to “shear zones” and their sub-units. The latter is an odd mix between addressing the paleo-character and the current state of the shear zones penetrating the investigated rock volume.

From my perspective, the rock volume’s current state is of primary interest for the current experiment, i.e., the fluid injections: What are the features and structural elements seen today that are relevant for fluid flow? The genesis of the structural features maybe of importance when discussing the results and their implications. I found the terminology regarding the two major paleo-shear zones and the hierarchy of elements in them today quite confusing (and actually unnecessary). It should simply be reported which type of “pre-existing hydraulically relevant features are intersected by the bore-holes/enclosed by packers”.

Response: We agree that the terminology in the manuscript is not very easy. However, we argue that it is important to keep it for sake of precision. Also, the subunits of the two main shear zone types are important to keep, as those illuminate the variation in responses of similar shear zones during stimulation experiments. Additionally, we argue that the brief description of shear zones is important, to interpret the differences in response to stimulation. Nevertheless, we agree the terminology could be simplified and we tried to keep terminology as simple as possible. The removal of unnecessary statements about the shear zones, the intervals etc. should make it easier to follow the text.

To me, the notion of “re-activation/stimulation of these paleo-shear zones” is misleading. The study is supposed to report on “hydro-shears”, so please simply report the orientation and number of pre-existing fractures in an injection interval (and put their orientation in perspective to the local stress state). (Sure, their orientation characteristics etc. might be related to their association with a larger feature, i.e., S1 or S2, but that is of subordinate importance in the current injection experiments.)

Response: It is true the re-activation can be a misleading term. But, reactivation does not necessarily imply that a structure (fracture / shear zone etc) was re-activated under similar conditions, as under initial activation. In fact, we describe the difference between reactivation in terms of induced shear dislocation and in-situ stress field estimate. The number of reactivated fractures is reported in Table 1.

the Q-strategy:

The authors try to build the manuscript around explicitly phrased questions. In principle, such an approach may be viable, but here it did not work out well. If the questions are supposed to steer the discussion, “brief versions” of them have to head the subsections of the discussion. One aspect that I think cannot be addressed well by the Q-strategy is a clear formulation of an objective of the study. One might say that answering the questions is the objective, but –at least to me- they are way too general for this purpose. The authors might want to consider making the “zone-zoo” the central objective, i.e., juxtaposing the method-oriented constraints on the spatial extent of injection processes on the one hand and a synoptic process-oriented zone characterization on the other hand.

Response: As already mentioned above, we stepped back from the q-strategy. As we rewrote and shortened the introduction, it was easier to define clear research questions for the manuscript. Similarly, we changed the style of the interpretations, as we did not need to stick with the “Q’s” anymore. We think this was very beneficial for the manuscript.

presentation:

The manuscript requires, unfortunately, very extensive rewriting. The authors should aim for concise, shorter and strictly separated presentations of their objectives, the methods, the results, and their discussion/interpretation. For example, I consider the “background” sub-sections of the introduction obsolete; they rather indicate the lack of focus on a specific objective. Similarly, individual subsections are headed by lengthy reviews of already presented work, undoubtedly relevant for the current work, but pointing to the literature and rephrasing the essential outcome should suffice here. Organizational problems occur however down to the level of paragraphs; I found it often strange when which information is given. Many sentences are unnecessarily complicated and long. A large number of –to me– obsolete “logistical statements” about the existence of figures are made.

Response: We agree that the manuscript was not organized very well. Indeed, we found some statements in strange locations within the manuscript. We realized, that there were some “relicts” in the manuscript, that were important for us, when we tried to get some sense into the data, but which were not required (or even unnecessary) now. We screened the manuscript for those sections/statements and removed them. Additionally, as already said above, we shortened the introduction and improved the focus on the research question and the related important observations. Last but not least, we remove most “logistical statements”.

technical:

The presentation is essentially devoid of uncertainties of reported values (e.g., the hydraulic properties in Table 1).

Response: We added a paragraph to the conclusion that emphasizes the network geometry and its influence on measurement accuracy. For most of our measure, we describe the accuracy and/or detection limit of the sensor. Additionally, we referenced the “experiment description” by Doetsch et al. 2018 for more detailed information on the monitoring system.

In terms of overall uncertainties of the rock mass responses, we argue that variability with respect to target geology is presented by showing the observations from the different experiments per shear zone type (i.e. S1 and S3).

scientific conclusiveness/interpretation:

While I thought that the paleo-features are overstressed to some extent (my comments on terminology), in another direction I miss an account for them. The structures identified as two different types of paleo-shear zones, today likely correspond to heterogeneities in the physical properties of the investigated rock volume (again irrespective of their genesis). What is the effect of the variation in physical properties

* on stress state? (see for example the stress modification reported by Dan Faulkner and coworkers for changing elastic properties near a fault)

* on the velocity model for event location?

* on strain distribution in the volume (in fact stress and strain are inseparable ...)?

Admitting that I might have gotten it wrong, the authors' perspective on the stress state seems problematic to me. For a specific fracture, into which fluid is injected, first of all the local stress state is relevant. The authors should clarify, which stress tensor applies to which of the selected injection intervals. The parallel consideration of "perturbed" and "unperturbed" stresses seems inappropriate.

Maybe I overlooked it, but it does not seem that the issue of stress heterogeneity ever comes up in the discussion of the "zone extent". Also, I miss that the problem of network sensitivity is addressed. The heterogeneity in current physical state might considerably affect from which part of the investigated volume one "hears" activity or not. Probably, the authors will legitimately refrain from an extensive sensitivity study at this point, but an appropriate "disclaimer" is warranted when discussing the "seismically active zone". Similarly, when discussing the spatial extent of fluid-pressure diffusion the authors should comment on the (in)validity of classical scaling relations for heterogeneous media. They (convincingly) argue for "channelized flow", for which a general "scaling" statement can hardly be made (but maybe for hemiradial, bilinear, etc.). To me, it also is not trivial how to define a "pressure front"; unfortunately, apart from the underlying conceptual question also the "storage capacities of the recording points" play a role in that. A cool outcome of the study would be that despite the specific shortcomings of all monitoring methods their combination allows one to constrain the "permanently altered" rock volume. To me, "permanent modification" of some sort would be the diagnostic feature for "stimulated volume".

Response: The effect of their influence on physical parameters, such stress state and elastic parameters was discussed by Krietsch et al. 2018 and Doetsch et al. 2020. To better emphasize the influence of the target geology on the stimulation response, we added some sentences in the discussion part. It should be clearer not. The influence of the shear zones on the seismic velocity model and induced events are presented by Schopper et al. 2020 and Villiger et al 2019.

The parallel consideration of "perturbed" und "unperturbed" stress state is indeed inappropriate and represents an abovementioned relict from initial process of manuscript writing. In the reviewed version we argue that the perturbed stress state is suitable for near-field observations, whereas the unperturbed stress state fits the far-field observations best. This statement agrees with observations from induced seismicity and hydraulic testing.

We agree that the overall permanent modifications within the rock mass induced by stimulation are a very important point to capture. However, that main focus of the manuscript are the dynamic processes happening during stimulation. The detailed comparison of the pre- and post-stimulation rock mass conditions is current work in progress and will be published elsewhere.

Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2019-204/se-2019-204-RC1-supplement.pdf>

Response: As already mentioned above, the manuscript was strongly rewritten. Thus, we cannot answer to your comments in detail. However, we tried to incorporate all your requested changes in the reviewed manuscript. Thank you, for the very detailed comments!

Response to Reviewer 2:

The paper summarizes some key results of a series of stimulation tests performed at the Grimsel test site. Thanks to very comprehensive monitoring of a broad range of hydraulic and mechanical parameters this project has yielded a wealth of new results, partly published, and in this paper the authors provide a rather comprehensive overview of the results pertinent to the interplay of hydraulic and mechanical processes.

The results presented are very interesting and the discussion/conclusion is straightforward. However, I found the text in part too verbose and not always well organized. Maybe it could be condensed a bit more and the graphics cleaned up. My comments are however just minor and left at the discretion of the authors.

Response: Thank you for the kind introduction. As it was also requested by the other reviewer, we re-organized and shortened the manuscript to make it clearer. This included the removal of the extensive literature review in the introduction and in some parts of the methods sections. Actually, we found some relicts in the manuscript, that were important while getting sense into the data, we are unnecessary now (or might make things even more complicated). Also, the main research questions are not better derived from the introduction and the discussion part was tailored to the key observations.

Some minor comments:

Page 4 Line 17-22 is not very clear, possibly wrong. The entire volume should be affected by poro-elastic stress perturbation but obviously the extent of the fluid pressure front and the poro-elastic deformation front likely differ in space and time.

Response: You are right. We had an inconsistent statement about poro-elasticity in these lines. We corrected it.

Fig. 1 is not very instructive and needs to be more clear concerning shear zone position and in particular sensor locations wrt to target volumes remain rather obscure.

Response: The 3D geometry is difficult to show in a 2d figure. For more detailed information about the monitoring network, we referred to Doetsch et al 2018 (Grimsel Experiment Description). Additionally, we have the borehole log figure in the appendix which shown the sensor placements in the individual boreholes.

Fig. 2 left hand side not very clear, maybe one could reduce size of pole projection and enlarge photos.
Response: The figure was modified after Krietsch et al. 2018 (Scientific Data). We provided the reference for more detail and added few words to the caption that hopefully make it clearer.

Fig. 3 is a somewhat busy and should have a more comprehensive caption explaining what we see.

Response: Yes, the figure is rather busy, but we improved now the figure caption, to make it clearer.

Starting with 3 Methods there are several typos, missed references etc. at least in my pdf.

Response: According to your initial request, we rewrote the manuscript in a clearer way and simultaneously cleaned up typos, missed references etc.

Fig. 9 is very busy but the caption needs a more detailed explanation.

Response: We improved the caption for better clarity.

Fig. 12 lower part is not very clear.

Response: We improved the caption for better clarity.

The Interpretation/Discussion part is not easy to read. Not sure if all observational details need an interpretation here. Maybe it would help to focus on the key results and observations from the experiments. The schematic Figure 14 illustrating complex deformation in the 'primary' (?) stimulated zone is too busy. Maybe it is possible to summarize key aspects of the hydraulic and mechanic responses to the injections for the two target shear zones in two schematic diagrams. The scientific content of the manuscript absolutely deserves publishing but I believe that the manuscript and the potential readers would benefit from some 'polishing' of text and figures. I hope my suggestions are useful to the authors.

Response: As already mentioned above, we re-organized the manuscript in a way, that not every tiny detail is interpreted anymore, but that the focus is now clearly on the main points according to the introduced research questions. Thus, we argue that the overall readability is highly improved and confusing terminology minimized.