

Interactive comment on “Hydro-mechanical processes and their influence on the stimulation effected volume: Observations from a decameter-scale hydraulic stimulation project” by Hannes Krietsch et al.

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Received and published: 23 March 2020

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

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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

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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 may be 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 boreholes/enclosed by packers”.

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.)

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.

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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.

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.

technical:

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

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

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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 "channeled 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".

Please also note the supplement to this comment:

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

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