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Interactive comment on "Seismicity during and after stimulation of a 6.1 km deep Enhanced Geothermal System in Helsinki, Finland" *by* Maria Leonhardt et al.

Anonymous Referee #1

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The manuscript "Seismicity during and after stimulation of a 6.1 km deep Enhanced Geothermal System in Helsinki, Finland" brings an extended results of processing the seismic monitoring data set obtained during the hydraulic stimulation carried out in 2018. Data from different available seismic stations were combined to extend the number of detected and located events and to display the seismic moment release in time. Cross correlation technique was used to estimate focal mechanisms of the largest possible number of events whose variability was analyzed and used to determine the stress field components. The instability of fault planes was then used to assess the most prominent faults.

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Discussion paper

The study deals with very interesting data on injection induced seismicity in a unique experiment and gives some valuable results. These are in particular the extended catalog, the focal mechanisms and principal stresses. Providing these data to the scientific community will undoubtly help better understanding the induced seismicity in geothermal projects in hard rocks. However, despite of reasonable language (as I can assess as non-native speaker), the study is not easy to read. This holds e.g. to the parts on catalog methodology and results, which is not easy to understand. One of the reasons is structuring the paper to Methodology and Results sections. It is a good approach in general, but in some cases it breaks the individual topics and makes the paper longer and understanding more difficult. So I recommend to describe only the more sophisticated methods like 2.3, 2.3 and the location part of 2.1

I also think that the spectrum of methods applied is too wide with no clear focus. The authors should decide if they present new high quality extensive seismic catalog whose parameters are characterized by a set of suitable (statistical) methods or they present a seismological study including interpretations. The point is that despite the catalog is the most valuable output, it is never characterized by at least Gutenberrg-Richter distribution and similar methods. The authors also spent a lot of effort determining focal mechanisms using quite sophisticated method to get maximum number of mechanisms, they however do not show the whole set of FM and assess their quality. I am also not sure about the improved quality of locations in terms of their asymmetric position to the borehole. As a result I believe the paper should be restructured according to its main focus - presentattion of new data. Details of my comments which should be adressed in a major revision are summarized below.

Particular comments

Ln 109-120 (Methodology). The explanation about different subsets of larger and smaller events and their relocation is not very clear. E.g. how many events were above Mw 0.7; were the 3464 events chosen from this subset?; did these events occur during stimulation because you added 321 post-stim events?; did 68 events com from

this subset?. . .

Ln 172 - 176. Please explain the SVD application in more detail. The point is that SVD is usually used to find a common pattern in a data set. For this you would need more polarity patterns for each event that just one, which you have as a result of cross correlation. âĂĺThe next question is whether the polarity matrix (eq. 2) shows the polarity fit between the target and template events as indicated on Ln 171 or the fit of polarities themselves. In the first case, it could not be used for calculating focal mechanisms.

Ln 178. The way you reduced the polarity ambiguity is not clear; by considering manually picked events one can verify the automatic picks, I believe.

Ln 185 The final sentence mentioning the resulting reverse faulting fits rather to the Results than Methodology section

Ln 195 Please argue for using this distance metrics - what is the reason for 1.5 in the denominator? And which type of cluster analysis did you use? What is the difference to the published method of moment tensor clustering of Cesca (2014)?

Ln 209-216 (Results) I think that the VSP based model deserves more attention. The present way is not appropriate - to show the model as a result without any more details. If it is considered as a result of this study, the data, methods and results should be shown. In the opposite case, the VSP model can be cited from a different study or as a personal communication from its author.

Ln 218-.. The description of seismic catalog update appears too detailed and technical and overlaps with the similar section in Methodology. Please consider unifying, making it more clear and concise.âĂĺâĂĺAnother point concerning locations is the (mis)fit of the hypocenters with the borehole trace. IN the depth sections of Fig. 3 it appears that most hypocenters lie below the borehole trace, which is rather unlikely. Please compare e.g. Fig. 3 in Kwiatek et al (2019) where the hypocenters occur almost symmetrically

around the borehole.

Ln 364 It is interesting that the post-stimulation seismicity does not show any systematic migration. This observation should be supported by a sort of distance-time or coordinate-time plot. In fact, even the existing papers of Kwiatek and Hillers on the Helsinki stimulation do not show such data.

Ln 381 To see the events at perimeter these should be shown on top of the others, e.g. in grey

Ln 393 Please argue for the highest expected pore pressure perturbation at the bottom of the permeable zone

Ln 400 The depthward migration is not visible in Fig. 3. And further, it is very unlikely that water would flow down in the expected lithostatic conditions of the rock formation where no open fractures are expected. On the contrary, water tends to flow up due to the buyoancy effect cause by the difference in density of water and rock.

Ln 442 In the Summary, the authors mention seismic catalog as a result of the study provided to he community. This sounds great, however I would welcome to see some quality analysis of the catalog, at least to show the Gutenberg-Richter distribution distinguishing the original catalog, the newly detected and newly located events.

Ln 449 The statement “The temporal behavior of the poststimulation seismic moment release until bleed-off is still similar to the moment release observed during individual stimulation phases” sounds a bit vague.

Ln 474-476 According to the unclear description of some parts I am not sure if all of the coauthors did really contribute to the manuscript (by e.g. the manuscript correction indicated in the Author contribution section).

Figures

Fig S1 should be included as Fig. 1; this is much more informative than the present

Fig. 1 which could be moved to Supplements

Fig. S2 overlaps with Fig. 2 and using different time scale (absolute vs. relative) makes it different to compare. Why not combining Fig.S2 and Fig.2 in a single plot?

Fig. 2 is missing reference in the text. The caption does not explain the meaning of time - from which moment the days are counted? It is also not clear why you do not show also the time period during the stimulation as indicated in the manuscript title and also shown in Fig. 3.

Fig. 3 The caption should be better specified; e.g. mentioning the name OTN3 of the borehole is missing and the legend does not explain the colored bands along the borehole trace. Are these the stimulated sections and should their color correspond (at the moment it does not) to the colors of hypocenters?

Fig. 4 and 5: the yellow line is hardly visible.

Fig. 5 The three CM0 plots could be better shown with common Y axis, which would spare space and make them more legible, Also a single legend would then suffice.

Fig. 9 The black stress component are not visible enough, consider using different color.

Fig. 10 Please indicate in the caption that the stress ratio R 0.53 determined in the stress inversion is used. And shift the Px markers a bit to the right, these are very hardly visible now.

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